TRIAD Drug Treatment Evaluation Project
Six-Month Interim Report

Federal Bureau of Prisons
Office of Research and Evaluation

January 31, 1998

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ACKNOWLEDGMENTS

Numerous Bureau of Prisons staff members, past and present, have contributed to the TRIAD drug treatment evaluation project since its initiation in 1989. As project director I would like to acknowledge the individuals who contributed in the past or who are currently involved with the project. Sue Wallace manages the Community Corrections Center and post-release data collection efforts, supervising both data collection staff and computer programmers. Joyce O'Neil manages the field data collection efforts, which involves supervising field research staff, coordinating site visits, and ensuring quality control. Prior to Joyce O'Neil's involvement with this project, Connie Hobbs performed this role. Initial plans for the project were developed in association with Dan McCarthy, with additional assistance provided by Barbara Owen. Anita Arcidiaconco provided the administrative leadership at the outset of the project.

Field researchers served as the backbone of the very intensive in-prison data-collection process. I would like to thank the field researchers who spent many hours traveling to prisons to interview staff and patiently administer surveys and lengthy interviews to the inmates serving as research subjects. Researchers included Linda Adams, Tim Brooks, Solla Carrock, Karen Childress, Christine Escobedo, Marty Hill, Anthony Iwaszko, Jamie Hart French, Neal Langan, Tom Marsh, Natalie Merriweather, Holly Millner, Brenda Moore, Nicki Pate-Allen, Robin Pool-Harris, Connie Stevens, Michelle Taylor, Sharon Tittemore Suppa, and Laurie Westman. Dana Cyra administered the staff surveys and assisted with quality control procedures.

Equally important were the individuals involved in the collection of Community Corrections Center and post-release data. Rhonda Parker coordinated all the data collection from Community Corrections Centers and from the transitional services managers. Sharon Tittemore Suppa coordinated the interviews of U.S. Probation officers, and Stacey Kappral coordinated the collection of NCIC data (automated arrest records). Other individuals involved in post-release data collection included Dana Cyra, Christine Escobedo, Jamie Hart French, Holly Millner, Jennifer Pearson, Constance Stevens, and Laurie Westman.

The complex programming tasks for developing and updating tracking systems were coordinated by Sue Wallace and John Cashwell. David Smith, Suzy Vanyur, Chris Lehman, Peter Brustman, and Doug Yearwood wrote computer programs to manage the data collected for the project. Jamie Hart French provided administrative support, with Deena Walter Heikkinen and David Cramer previously responsible for such support. William Saylor provided statistical consultation to all project staff during the last few years. Gerry Gaes and Scott Camp contributed both by providing methodological advice and assisting in the data analysis and writing of this report. Sue Wallace and Joyce O'Neil coordinated the staff efforts in obtaining information contained in this report and wrote sections of this report. David Smith, Phyllis Newton, and Judy Gordon edited this report. The data collection required for this report and the report itself could not have been accomplished without the benefit of each staff member's skills and the teamwork exhibited by all of the persons cited above.

In addition, the invaluable contributions of the many Drug Abuse Treatment Program (DAP) staff who helped coordinate the data-collection trips is greatly appreciated. Community corrections staff, transitional services managers, and the numerous Probation officers nationwide who provided the information on inmates after their release from institutions are also to be thanked for their important contributions to this evaluation.

Members of the TRIAD advisory board provided suggestions during project implementation and also provided useful feedback on earlier drafts of this report. The contributions from advisory board members Doug Anglin, Helen Annis, Jeffrey Hoffman, Jim Inciardi, and Michael Maltz are greatly appreciated.

Lastly, I would like to thank those who provided the administrative and financial support for this project. Bureau of Prisons Director Kathleen M. Hawk and Assistant Director Thomas R. Kane provided the administrative support within the BOP. They approved policies facilitating the data collection and provided continued project funding. The National Institute on Drug Abuse (NIDA) provided the initial funds that made possible a project of this magnitude. Bennett Fletcher from NIDA has provided useful feedback and guidance throughout the project.

TRIAD DRUG TREATMENT EVALUATION
SIX-MONTH REPORT
EXECUTIVE SUMMARY

Introduction

The Federal Bureau of Prisons (BOP) has provided drug abuse treatment in various forms for almost two decades. With the passage of the Anti-Drug Abuse Acts of 1986 and 1988[1] and an increased emphasis on and resources for alcohol and drug abuse treatment, the BOP redesigned its drug treatment programs. This design was completed after careful review of drug treatment programs nationwide. The treatment strategy addresses an inmate's drug abuse by attempting to identify, confront, and alter the attitudes, values, and thinking patterns that led to criminal and drug-using behavior. The current residential treatment program also includes a transitional component that keeps inmates engaged in treatment as they return to their home communities.

The Bureau of Prisons (BOP) recently undertook an evaluation of its residential drug abuse treatment program (DAP), designed to monitor inmates up to 3 years following release from BOP custody. This interim report is based on inmates who had been released from BOP custody into the community for 6 months. Findings suggest that the program is effective in reducing recidivism and substance abuse. The evaluation, conducted with funding and assistance from the National Institute on Drug Abuse, reveals that offenders who completed the drug abuse treatment program and had been released to the community for a minimum of 6 months were less likely to be re-arrested or to be detected for drug use than were similar inmates who did not participate in the drug abuse treatment program. Specifically, among inmates who completed the residential drug abuse treatment program, only 3.3 percent were likely to be re-arrested within the first 6 months in the community compared to 12.1 percent of those inmates who did not receive such treatment. In other words, treated inmates were 73 percent less likely to be re-arrested than untreated inmates. Similarly, among inmates who completed the residential drug abuse treatment, 20.5 percent were likely to use drugs within the first 6 months in the community compared to 36.7 percent of those who did not receive such treatment, suggesting that those who received drug treatment were 44 percent less likely than those who had not received treatment to use drugs within the first 6 months.

The findings of this preliminary evaluation are noteworthy because prior research indicates that the first 6 to 12 months of an offender's release back to the community are particularly difficult and often are critical to a successful reintegration. These findings, which suggest that drug abuse treatment assists inmates during this initial resettlement period, offer encouragement for the conclusion that another correctional program "works," making a difference in the lives of offenders and reducing the likelihood of future criminal conduct. In addition, the findings in this

[1] The Anti-Drug Abuse Act of 1986 laid the groundwork for the drug treatment programs and the Anti-Drug Abuse Act of 1988 contained provisions for the funding of these programs.

study are strengthened due to the large sample size (1,800), a rigorous research design, and the uniqueness of using a multi-site sample.

Paths to Treatment Service

This evaluation project addresses residential drug abuse treatment programs in the Bureau of Prisons for inmates who receive a substance abuse diagnosis. However, any inmate interested in drug abuse treatment can receive services through various means, ranging from drug education to non-residential to residential depending upon individual inmate need. Treatment services are primarily available at three different stages while in Bureau custody. Treatment can continue when an inmate is released from Bureau custody to the supervision of U.S. Probation.

In the custody of the Bureau of Prisons:

Stage 1: Inmates participate in residential drug abuse treatment within the confines of a designated drug abuse treatment unit generally for either 9 or 12 months (i.e., unit-based treatment). The treatment strategies employed are based on two premises: the inmate is responsible for his or her behavior, and the inmate can change his or her behavior.

Stage 2: Upon successful completion of the unit-based drug abuse treatment program, inmates are required to continue drug abuse treatment for up to 12 months when returned to general population. During this stage of institution drug abuse programming, known as institutional transition, inmates meet with drug abuse program staff at a minimum rate of once a month. Ordinarily, institution transition is conducted as a group activity consisting of relapse prevention planning and a review of treatment techniques learned during the intensive phase of the residential drug abuse program.

Stage 3: All inmates who participate in the residential drug abuse program are required to participate in community transitional services when they are transferred from the institution to a Community Corrections Center (halfway house). In the community, the Bureau contracts with community drug abuse treatment providers who provide group, individual, and/or family counseling that meet the needs of the individual inmate. Generally, these contractors offer the same type/philosophy of treatment offered in the institution. In addition, community transitional services also are offered to inmates who have not completed any drug abuse treatment in the institution or who have received treatment other than the residential program.

Out of Bureau Custody:

In addition, inmates leaving Bureau custody for supervision with the U.S. Probation Office may remain in drug abuse treatment with the same treatment provider the Bureau used during the community transition program. U.S. Probation provides a wide-range of treatment services for its

offender population, although not all releasees are required to participate in post-release treatment.

Unit-Based Residential Treatment

This evaluation effort focuses on two types of residential treatment programs for alcohol and other drug problems. The first offers 1,000 hours of treatment over a 12-month period with a staff-to-inmate ratio of 1:12. The second offers 500 hours of treatment over a 9-month period with a staff-to-inmate ratio of 1:24. Most of the subjects in this study participated in the 9-month program.

All residential DAP's are unit-based, that is, all program participants live together — separate from the general population — for the purpose of building a treatment community. Each unit has a capacity of approximately 100 inmates. Ordinarily, treatment is conducted on the unit for a half-day in two, 2-hour sessions. The other half of the day, inmates participate in typical institution activities (e.g., work, school). During these times, as well as during meals, treatment participants interact with general population inmates.

The goal of these programs is to attempt to identify, confront, and alter the attitudes, values, and thinking patterns that led to criminal behavior and drug or alcohol use. Most program content is standardized and the following modules comprise 450 hours of programming, both in didactic and process groups: Screening and Assessment; Treatment Orientation; Criminal Lifestyle Confrontation; Cognitive Skill Building; Relapse Prevention; Interpersonal Skill Building; Wellness; and Transitional Programming. The remaining program hours are structured at the discretion of each program.

All admissions to drug treatment are voluntary. At the outset of program implementation, there were no incentives for residential drug treatment program participation. However, over time various incentives were implemented. These included nominal financial achievement awards, consideration for a full 6 months in a halfway house for successful DAP program completion, and tangible benefits such as shirts, caps, and pens with program logos to program participants in good standing. The incentives for drug treatment significantly changed with the passage of the Violent Crime Control and Law Enforcement Act of 1994. This law allowed eligible inmates who successfully complete the BOP's residential drug treatment program to earn as much as a 1-year reduction from their statutory release dates.[2]

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[2] This early release provision presents issues of disparity for Bureau inmates. The disparity arises when, for example, two inmates convicted of the same offense receive different prison terms because the inmate who has been diagnosed with a substance abuse problem receives a one-year reduction on his/her sentence and the inmate without a substance abuse problem serves the entire sentence. In effect, many perceive this one-year reduction as a reward for drug-abusing behavior.

Sample

The preliminary results contained in this report relate to inmate subjects who were released no later than December 31, 1995, and who were released to the community for 6 months or more. Most of these inmates were within approximately one year of release from BOP custody when they completed the program.[3] This report includes data only for the first 6 months of release for each inmate; the final report will cover a 3-year post-release period for all individuals. The sample contained in this report includes 1,866 individuals — 1,524 men and 342 women — for whom comprehensive data were available.

Treatment Subjects

Treatment subjects were sampled from 20 different institutions with a residential drug treatment program. This represents approximately two-thirds of the institutions that currently operate residential treatment programs. These institutions represent all security levels, except maximum security, and serve both male and female populations.

The four types of residential DAP participants[4] as they were categorized in the analyses are: 1) inmates in residential drug treatment who completed that treatment, 2) inmates who dropped out, 3) inmates discharged from treatment for disciplinary reasons, and 4) inmates who, for a variety of other reasons, did not complete the program. This "incomplete" category, in general, comprises inmates unable to complete the residential, unit-based program because they were transferred to another institution or to a halfway house (CCC), had their sentences shortened toward the end of their incarceration, or spent an extended amount of time on writ or medical furlough. Table 1 provides a breakdown of inmate subjects by gender, treatment and comparison group assignments, and individual categories within the treatment group.

Of the 719 male subjects who entered unit-based residential treatment, 73 percent completed the treatment program, 5 percent voluntarily dropped out of the program, 8 percent were removed for disciplinary reasons, and 14 percent constituted the "incomplete" subject type, as described above.

Of the 180 female subjects who entered unit-based residential treatment program, 54 percent completed the treatment program, 9 percent voluntarily dropped out of the program, 13 percent were removed for disciplinary reasons, and 24 percent were of the "incomplete" category.

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[3] Typically, inmates enter a residential drug abuse treatment program 36 to 24 months before release from BOP custody. This allows inmates to complete treatment and transition into community-based treatment with minimal interruption to their treatment program.

[4] For purposes of this discussion, residential drug abuse program (DAP) refers only to Stage One — the unit-based stage of the program.

Anecdotal information collected during the study suggests that the lower percentage of treatment "completers" among women than among men may be related to policy differences between treatment sites and differential enforcement of program rules.

Comparison Subjects

Male and female comparison subjects were drawn from more than 30 institutions, some that offered residential drug abuse treatment programs and some that did not. The comparison subjects consisted of individuals who had histories of previous drug use and, therefore, would have met the criteria for admission to the residential drug treatment programs. There were 805 male and 162 female comparison subjects.

Table Ex1. Type of Subject by Gender

	MALE		**FEMALE**	
TYPE OF SUBJECT	***NUMBER***	***PERCENT***	***NUMBER***	***PERCENT***
Treatment	719	47.2%	180	52.6%
12-month Program Graduate	122	8.0%	41	12.0%
9-month Program Graduate	401	26.3%	56	16.3%
Drop-out	37	2.5%	16	4.7%
Disciplinary discharge	55	3.6%	24	7.0%
Other reason - incomplete	104	6.8%	43	12.6%
Comparison	805	52.8%	162	47.4%
TOTAL	1,524	100.0%	342	100.0%

Outcome Measures

Criminal recidivism and post-release drug use were the primary outcomes of interest in this evaluation. The other outcomes examined were unsuccessful halfway house completion and percent of post-release time the subject was employed full-time. Criminal recidivism was defined two ways: 1) an arrest for a new offense, or 2) an arrest for a new offense *or* supervision revocation. Revocation was defined as occurring only when the revocation was solely a technical violation of one or more conditions of supervision (e.g., detected drug use, failure to report to probation officer).

Because much of the outcome information was obtained from interviews with U.S. probation officers, the analyses for three of the outcome measures (arrest for new offense or revocation, drug use, and employment) were conducted only for individuals released to supervision. The analysis for arrest for new offense used both supervised and unsupervised subjects because arrest information could be collected on unsupervised subjects from the FBI's National Crime Information Center (NCIC). The analysis concerning unsuccessful halfway house completion was limited to those individuals who received halfway house placements.

Drug use as a post-release outcome refers to the *first* occurrence of drug or alcohol use. This information consisted of four different categories of a violation of a supervision condition as reported by U.S. probation officers: a positive urinalysis (u/a), refusal to submit to a urinalysis, admission of drug use to the probation officer, or a positive breathalyser test.[5]

Employment information was also obtained through interviews with U.S. probation officers. This information was limited to employment occurring prior to any post-release arrest or revocation. This outcome was defined as the percentage of post-release period that the subject was employed full-time.

Halfway house placement failure, for those individuals who received such a placement before release from custody, was reported by halfway house staff. Approximately two-thirds of the subjects received a halfway house placement. Failure to complete a halfway house placement is the result of a disciplinary infraction, whether for a violation of halfway house rules or for criminal activity.

Before examining the effects of treatment, it is important to look at the base rate of failure for each outcome measure for both treatment and control inmates. This base rate of failure is presented by gender in Table 2, and tells us, for example, that the base failure rate for arrest on a new offense for all subjects (both those who received treatment and those who did not receive treatment) is 14 percent for men and 6 percent for women. Overall, these results indicate that for each outcome measure, the percentage with a successful outcome is lower for men with the exception of employment. Forty-four percent of the male subjects were employed full-time during the entire 6-month post-release period as compared to 28 percent of the women.[6]

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[5] A violation of a condition of supervision does not always result in a revocation.

[6] However, looking instead at inmate subjects who were employed either full- or part-time during some or all of the post-release period, this trend changes, with 79 percent of women working either full- or part-time and 82 percent of men working either full- or part-time.

Table Ex2. Outcome Measure by Gender 6 Months Post Release		
	Male	Female
ARREST FOR NEW OFFENSE	14%	6%
ARREST FOR NEW OFFENSE OR REVOCATION	21%	11%
DRUG USE	31%	20%
UNSUCCESSFUL HALFWAY HOUSE COMPLETION	23%	15%
EMPLOYED FULL-TIME ENTIRE POST-RELEASE PERIOD	44%	28%

Analyses

The analyses of the effects of unit-based drug treatment on the various outcome measures controlled for a wide variety of background factors known to be related to recidivism and treatment outcomes, including a number of factors related to drug-using populations that have not been examined in previous evaluation studies. These background measures included drug use frequency for each of the various drug types, drug and alcohol dependency, drug treatment history, mental health treatment history, psychiatric diagnoses of depression and antisocial personality, criminal history, age, race, ethnic status, employment history, motivation for change, level of supervision (e.g., halfway house placements before and after release from custody, release to supervision, frequency of urine testing), and post-release living situation.

Three different methods of analyses were used to assess treatment effectiveness. Two of these methods represent different approaches to control for self-selection into treatment, i.e., selection bias. These methods represent alternative procedures to ensure that effects of treatment are not confounded with effects of volunteering for treatment. One method compares all individuals who had treatment available to those who did not have treatment available. The second method directly controls for selection bias. The third, the traditional method, is similar to that used in some previous evaluations but it does not control for selection bias.

The assessment of treatment effects generally showed consistency in results for the various outcome measures when comparing the differing methods of analyses. The results reported below focus upon those using the strategy directly controlling for selection bias because this strategy was felt to assess most reliably the effects of drug treatment.

All analyses, except those for employment outcomes, were done 1) for males and females combined and 2) for males only. Separate analyses of outcome measures other than employment were not possible given the smaller number of women in the sample and the lower failure rate of women (see Table 2).

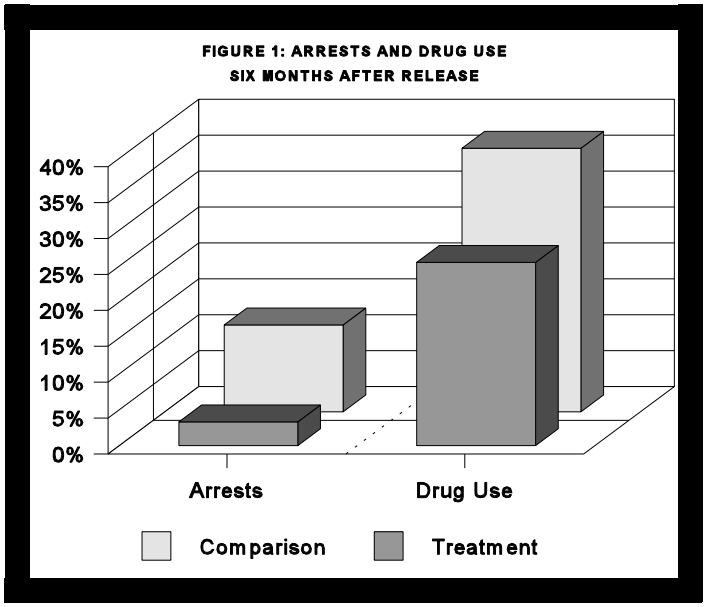
Findings — Residential Drug Abuse Treatment

The effects of unit-based residential treatment on post-release outcomes described below are the differences in outcomes between treatment and comparison groups after controlling for various background factors and for self-selection into treatment.

Recidivism

Arrest for New Offense — Individuals who had received unit-based residential treatment had a lower probability of being arrested in the 6-month follow-up period than did comparison subjects. The probability of arrest for individuals who entered and completed treatment was 3.3 percent as compared to a probability of approximately 12.1 percent for untreated subjects (see Figure 1). In other words, among inmates who completed residential drug abuse treatment, only 3.3 percent were likely to be re-arrested within the first 6 months in the community compared to 12.1 percent for non-treatment inmates; those who received treatment were 73 percent less likely to be re-arrested than those who had not received treatment.

Arrest for New Offense Or Supervision Revocation — When outcome was defined as arrest for new offense or supervision revocation, residential drug treatment effects also were found. Questions arise when combining arrest and supervision revocation in the same outcome measure. Therefore, this analysis should be considered preliminary, with future reports examining the relationship, similarities, and differences in the two measures of recidivism.



Drug Use

The results for drug use, like those for arrests for a new offense, show that individuals who participated in a residential drug abuse treatment program were less likely to have evidence of post-release drug use than were comparison subjects. Among inmates who completed residential drug abuse treatment, 20.5

percent were likely to use drugs in the first 6 months following treatment completion compared to 36.7 percent among untreated inmates; that is, those inmates who completed residential drug abuse treatment were 44 percent less likely to use drugs in the first 6 months following release than those who did not receive treatment (see Figure 1).

CCC Failures

Approximately two-thirds of the individuals received a halfway house placement (CCC) before their release from BOP custody. Results indicate that treatment completion had no effect on whether inmates successfully completed their halfway house stay.

Post-Release Employment

Individuals who participated in residential drug abuse treatment during their incarceration were no more likely to be employed full-time for a greater percentage of the 6-month post-release period than were individuals who did not participate in treatment. This finding was true for both men and women.

Inter-Institutional Differences

The preliminary data show that when controlling for differences in the composition of inmates in the 20 different programs that were evaluated, there were few differences between programs in effectiveness as measured by the five outcome measures. With larger sample sizes and other information on program quality, inter-institutional differences can be examined more thoroughly in the future.

Summary

The preliminary results of this initial look at residential drug abuse treatment programs suggest important and exciting possibilities for the treatment of drug offenders. Despite what is thought to be a difficult period of adjustment — the first 6 months following release from custody — inmates who entered, received, and completed residential drug abuse treatment were 73 percent less likely to be re-arrested than inmates who did not receive such treatment. This 73-percent reduction in arrest rates, coupled with the 44-percent reduction in drug use for treated subjects, strongly suggests that the Bureau of Prisons' residential drug abuse treatment programs make a significant difference in the lives of inmates following their release from custody and return to the community.

This evaluation has been methodologically rigorous and has revealed significant positive effects on arrest and drug use in post-release outcomes for a 6-month follow-up period. Because studies have shown that recidivism rates are highest within the first year and, while lower after that, are still high for another year or two, these results must be interpreted with caution. Future analyses

will evaluate whether these effects are sustained over a longer follow-up period (3 years after release).

CHAPTER 1: INTRODUCTION

In the late 1980's, the Federal Bureau of Prisons (BOP) implemented a residential drug abuse treatment program (DAP) designed to assist inmates struggling with drug problems. The treatment strategy addressed inmate drug abuse by attempting to identify, confront, and alter the attitudes, values, and thinking patterns that led to criminal and drug-using behavior. From that initial effort, the program has grown to include an essential transitional component that keeps inmates engaged in treatment as they return to their home communities.

To assess the effectiveness of its DAP program, the BOP in conjunction with the National Institute on Drug Abuse (NIDA) initiated a drug treatment evaluation project, which has become known as the TRIAD[7] drug treatment evaluation project. The evaluation comprises a multi-site study that compares inmates involved in the DAP (*i.e.*, those who received in-prison residential drug and alcohol abuse treatment) to inmates who did not receive treatment. While initial plans called for an experimental design, we were not able to implement the necessary random assignment procedures. Therefore, various approaches were used to control for any selection bias resulting from such factors as non-random assignment.

This interim report focuses upon assessing the effectiveness of the BOP's in-prison DAP, while at the same time controlling for a variety of factors related to recidivism and treatment outcomes. The preliminary results contained in this report relate to inmates who were released no later than December 31, 1995, and who were in the community for 6 months or more. This report includes data for the first 6 months of release for each inmate; the final report will cover a 3-year post-release period for all research subjects, including those released after December 31, 1995. The sample contained in this report includes 1,866 individuals — 1,524 men and 342 women — for whom comprehensive data were available. In general, most results are reported for men and women combined.

We sampled treatment subjects from 20 different institutions, which is approximately two-thirds of the institutions that currently operate residential treatment programs. These prisons included all security levels except maximum security, and they served both male and female populations. The residential programs included two components of treatment — an in-prison component and a transitional services component (as part of community placement and supervision). The treatment programs consisted of two levels of intensity — 500-hour, 9-month programs and 1,000-hour, 12-month programs.

Male and female comparison subjects were drawn from more than 30 institutions, some of which had residential drug abuse treatment programs and some of which did not. The comparison subjects consisted of individuals who had histories of previous drug use and, therefore, would have met the criteria for admission to the residential drug treatment programs.

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[7]TRIAD is the acronym for "Treating Inmates' Addiction to Drugs."

Results from this 6-month post-release study demonstrate positive effects on three of the five outcome measures: arrests, arrests and revocations, and drug use. That is, the evaluation shows that the approximately 600 offenders who completed the drug abuse treatment program and had been released to the community for a minimum of 6 months were less likely to be rearrested for a new offense, to be rearrested *or* revoked, or to test positive for drug use than was a similar group of inmates who did not complete the drug abuse treatment program.

The probability of rearrest was 3.0 percent for treatment completers as compared to a probability of 12.7 percent for untreated subjects. The probability of rearrest *or* revocation was 4.7 percent for treatment completers as compared to a probability of 17.4 percent for untreated subjects. The probability of drug use was 20.5 percent for treatment completers as compared to a probability of 36.7 percent for untreated subjects. The findings are noteworthy because the first 6 months of an offender's release back to the community are particularly difficult and often critical to a successful reintegration into society.

The fourth outcome measure, post-release employment, revealed no positive effects for those completing treatment. When considering a fifth measure of effectiveness — successful completion of halfway house placements[8] — results were somewhat ambiguous.

The final report will provide results for a larger sample size — particularly for women — and will contain outcome information on all subjects for a 3-year post-release period. We expect a sample size of approximately 2,900 individuals for the final report — 2,300 men and 600 women. In addition, future research efforts will identify the role of post-release treatment in successful outcomes and will attempt to increase our understanding of the role that background and psychological factors play in determining post-release outcomes. Future analyses will consider whether any of these effects are sustained over a longer follow-up period.

Background

A large proportion of State and Federal inmates have histories of substance abuse. A self-report assessment of Federal inmates in 1989 indicated that between 30 and 44 percent had substance abuse histories (Whittenberger, 1990). A survey of State inmates in 1991 indicated that more than 60 percent had used at least one illegal drug regularly (Beck *et al.*, 1993). Considerable evidence suggests that criminal behavior is amplified during periods of moderate and heavy drug use (Anglin and Speckart, 1986; Anglin and Speckart, 1988; Ball *et al.*, 1981; Nurco *et al.*, 1985; Nurco *et al.*, 1988; Speckart and Anglin, 1985; Speckart and Anglin, 1986).

The BOP has provided drug abuse treatment in various forms for decades. Although the number of drug treatment units in Federal institutions grew to a high of 33 in 1978, the number of

[8] In the BOP, referred to as a Community Corrections Center (CCC) placement.

programs began to decline in the early-to-middle 1980's due to changes in the social and political climate (Wallace *et al.*, 1991). With the passage of the Anti-Drug Abuse Acts of 1986 and 1988[9] and an increased emphasis on and resources for drug abuse treatment, the BOP redesigned its drug treatment programs after careful review of drug treatment programs nationwide.

The BOP's renewed interest in prison-based drug treatment programs began in 1988 when then-director J. Michael Quinlan organized a national drug conference. Immediately following this conference, the BOP Executive Staff approved the establishment of five institution-based residential drug abuse treatment programs. Similar programs were established throughout the Federal prison system and, by December 1996, 34 residential programs were operational.

At about the same time, in 1989, the National Institute on Drug Abuse expressed interest in a comprehensive evaluation of prison-based drug treatment programs, and the BOP submitted a research proposal for evaluating drug treatment programs in the Federal system. That proposal resulted in the signing of an interagency agreement between the BOP and NIDA in 1990 for a multi-site evaluation of the BOP's residential drug abuse treatment program, which eventually emerged as the TRIAD project.

The objectives of the TRIAD evaluation project specified in the original evaluation proposal submitted to NIDA in 1990 were:

- To conduct a process evaluation. This aspect of the evaluation would address the following issues:

    1) the nature of the services provided;
    2) the characteristics of the service recipients;
    3) program staffing patterns; and,
    4) implementation of residential drug treatment programs within a correctional environment.

- To conduct an outcome evaluation. The most important objective was to assess the extent to which in-prison residential treatment, reinforced by community-based aftercare services, could reduce drug use and criminal behavior after release from prison. Specific questions were:

    1) Who are the program participants? Does the program serve the most serious offenders?
    2) What are the different types of substance abusing offenders?
    3) What types of incarcerated offenders are more likely to volunteer for in-prison drug treatment programs?

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[9] The Anti-Drug Abuse Act of 1986 laid the groundwork for the drug treatment programs, and the Anti-Drug Abuse Act of 1988 contained provisions for the funding of these programs.

4) Are the 12-month (1,000-hour) drug treatment programs more effective than the 9-month (500-hour) programs?
5) What role do services provided after release from prison play in preventing relapse to drug use or criminal behavior?
6) Are there specific types of drug-abusing offenders who benefit more from participation in the in-prison residential drug treatment?
7) What are the relative effects of pre-treatment characteristics, the treatment program, and the post-release environment on the various outcomes?

Organization of the Report

This report is organized into eight chapters. Chapter 1 provides a brief introduction and background to the long-term evaluation, as well as to this interim report. The chapter concludes with synopses of the report's remaining chapters.

Chapter 2 is divided into two sections. The first section discusses the nature of evaluation research in an applied setting, with a focus upon what we view as the most significant methodological problem — selection bias. Our goal is to represent the difficulties of applied correctional research, to describe the organizational pressures that determine which inmates receive treatment, to depict these influences in an understandable model of selection pressures, and to offer potential solutions to these problems, both analytical and methodological. We discuss the two different processes we used in our analyses to address the problem of selection bias.

In the second section of this chapter, we use our model of selection pressures to critique the research design, analyses, and interpretation of results contained in the most commonly cited, related studies. Overall, our review suggests that methodological problems associated with evaluating residential drug treatment programs create important obstacles to interpreting the results of this research. We believe that, for the most part, the research we reviewed suffers from inferential problems associated with disentangling treatment effects from selection bias effects. We argue that it would be prudent to temper strong conclusions about successful treatment outcomes — which are often portrayed in the literature — with a bit of skepticism, born from a closer look at the methodological problems.

Chapter 3 summarizes the evolution of the Bureau's drug treatment programs from the beginning of the TRIAD drug treatment evaluation project to the completion of the in-prison data collection phase of the study. This summary details the nature and intensity of the services received and the various pathways into treatment. That is, research subjects may have received drug treatment services while in prison, while housed in a halfway house (if applicable), while under post-release supervision (if applicable), or during some combination of the three. Chapter 3 also describes the various components of the BOP's in-prison drug treatment programs, as well as the treatment services available following release from an institution. As is true for many major research efforts,

changes in research design and data collection procedures occurred throughout the project in order to adapt to changes in program implementation.

Chapter 4 describes the research design. After a summary presentation of the basic research design, we provide information on the procedures used for selecting research subjects, the resulting sample, the data collection instruments, and the data collection procedures. We describe the differences between the selection of treatment and comparison subjects, as well as the methods used to collect information at the various stages of a subject's criminal justice status — at any given time, he or she is either in prison, in a halfway house, terminated from BOP custody without supervision, or terminated from BOP custody with supervision by a probation officer. Chapter 4 concludes with listings and descriptions (when necessary) of all the variables used in the analyses.

Chapter 5 contains results of our subject attrition analyses. Some individuals identified as research subjects failed to enter the sample pool due to logistical data collection issues. Other subjects were unavailable for analyses because they refused to participate in the project. Although we controlled for these factors in our analysis of outcomes, we felt it important to describe the subject attrition process and to compare — on the variables available for both groups — those included in the analyses with those lost to attrition.

Chapter 6 contains a description of the subject sample. This chapter provides univariate statistics for each of the six subject groups used in our analytic strategies,[10] and provides the reader with a basic understanding of the background characteristics of the subject groups and the treatment services received. These six subject groups are:

inmates who completed a residential drug abuse treatment program;
inmates who dropped out of a program;
inmates discharged for disciplinary reasons;
inmates who did not complete a program through no fault of their own;
inmates from drug treatment sites who did not volunteer for treatment; and,
inmates who were housed at institutions at which there were no drug abuse treatment programs offered.

Chapter 6 should prove particularly relevant to service providers who want to know more about the general nature of the drug abusing population being served by BOP drug abuse treatment programs.

Chapter 7, which describes the analysis and results, begins with a description of our outcome measures. This is followed by a description of our three analytic strategies and our method of

[10] It is important to note that these subject groupings are not equivalent to the subject groupings used in the analyses of results or to control for selection bias. The groupings used for data analysis are discussed in Chapter 4.

testing, hierarchically, the effects of adding blocks of variables. Two of our three analytic strategies represent methods of addressing the problem of selection bias, and one strategy represents an attempt to replicate the strategy frequently used by other researchers. We continue with a presentation of the consideration of missing data. Because listwise deletion — the deletion of a subject with a missing value on one or more data elements — can result in a biased sample available for analyses, we either imputed the missing values or included the "missingness" category in the effects vectors.

Results are first presented for three post-release outcomes: arrests only, arrests or revocations, and drug use. We present the results for each of the three analytic strategies. We then present, by analytic strategy, results for the two final outcomes: Community Corrections Center (*i.e.*, halfway house) placement failure and percent of time employed full-time after release. We conclude this chapter with an effort to detect differences among the treatment programs for the five outcomes previously discussed.

Chapter 8 provides a summary of the results. This discussion focuses upon the preliminary conclusions we can make regarding the effectiveness of the Bureau's drug abuse treatment programs. We seek to identify consistencies in results across the various outcomes and, more specifically, consistencies across the different analytic strategies for a particular outcome. Finally, this chapter identifies the limitations of our conclusions, the issues to be addressed when data are available for the entire pool of subjects, and the issues important to future analyses.

CHAPTER 2: A REVIEW OF RECENT STUDIES OF HIGH INTENSITY ADULT CORRECTIONAL DRUG TREATMENT PROGRAMS — THE PROBLEM OF SELECTION BIAS AND POSSIBLE SOLUTIONS

We focus our literature review on recent research studies that are most commonly cited. These studies all conclude that prison-based drug treatment is effective. Before we examine these studies in detail, we discuss the nature of evaluation research in an applied setting and what we view as the most significant methodological problem — selection bias. Our goal is to represent the difficulties of applied correctional research, to describe the organizational pressures that determine which inmates receive treatment, to depict these influences in an understandable model of selection pressures,[11] and to offer potential solutions — both analytical and methodological — to these problems.

In the second section of this chapter, we use our model of selection pressures to critique the research design, analyses, and interpretations of results contained in the most commonly cited studies. Overall, our review suggests that methodological problems associated with evaluating residential drug treatment programs create important obstacles in interpreting the results of this research. We believe that, for the most part, the research we reviewed suffers from inferential problems associated with disentangling treatment effects from selection bias effects. We argue that it would be prudent to temper strong conclusions about successful treatment outcomes — which are often portrayed in the literature — with a bit of skepticism, born from a closer look at the methodological problems. We also describe different solutions for overcoming the problem of selection bias.

Selection Bias and the Evaluation of Prison Drug Treatment Programs

There is no question that conducting evaluations in an applied setting is very difficult. Correctional systems are coercive by their very nature, and even when treatment is endorsed and carried out by well-trained, motivated providers, there is typically a tension between the necessities of custody practices and the goals of a therapeutic setting. Custody practices are necessarily rigid and uniform, while treatment delivery must be personalized and flexible.

The ideal model for any assessment is a clinical trial in which we can control the timing, dose (amount of exposure to), and administration of treatment. Using random assignment allows us to discount client characteristics when drawing inferences about the effects of treatment. Unfortunately, there are very few situations in which it is practical to carry out a well- controlled, random assignment design of drug treatment. In most correctional settings, control over who gets treatment and when they get it rests with the treatment providers or some administrative

[11]Throughout this Chapter we refer to the various selection pressures as a means of describing the various elements of selection bias.

authority. Often there are policies that determine eligibility as well. Under these conditions, the best we can achieve is a quasi-experimental design but even these will vary in their rigor. Our emphasis is on the difficulty of in doing either random assignment or quasi-experimental designs in a correctional setting.

We raise these cautions because the internal and external validity of a study is compromised by the vagaries of correctional environments and possible differences in the characteristics of the clients involved in these studies. Rather than running away from these problems, we want to address them and offer some solutions that we used in the current study.

Fletcher and Tims (1992) have outlined the kinds of threats to internal and external validity that can occur in evaluation studies performed in a correctional setting. Their critique is thorough, but it does not give any color or texture to the scope of problems. In this chapter, we try to characterize the nature of some of the problems that occur when a variety of administrative decisions and local practices can contaminate the research design.

Rather than repeat the Fletcher and Tims critique, we focus on what we believe is the most troublesome methodological problem in an applied setting, in general, and in the correctional drug treatment literature in particular: understanding and controlling for selection bias. In a simple two-group design, experimental versus control, we want to be able to assume that whatever effect we observe is attributable to the treatment and not to differences in the characteristics of the subjects in the two groups. Selection bias results from processes that change the composition of the two groups in such a way that we are unable to make a clear inference as to whether the effects we observe are due to the treatment or to the different group compositions.[12]

Adopting a skeptical perspective, we could conclude that the selection process prevents us from drawing any conclusions about treatment effectiveness regardless of whether the original design used randomization to assign offenders to treatment groups. From this perspective, program terminations, both voluntary and involuntary, cause the treatment group to "boil down" to only those participants who are ready and capable of succeeding when released to the community. Thus, the "effect" of treatment may be nothing more than the process of "weeding out" those more likely to fail from those more likely to succeed, and treatment has no additional value to those who remain in treatment.

A more sanguine view is that the selection process results in a motivated group of program participants whose treatment results in even greater success than would be the case had no treatment occurred. The problem becomes choosing a research design that can distinguish between outcomes that are due solely to the selection process and those that are due to both this selection process *and* treatment. Furthermore, the research design must be able to differentiate the effects attributable to the selection process from those attributable to treatment.

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[12] For a technical discussion of sample selection bias, *see* Berk, 1983.

A simple conceptual device for understanding this problem is to treat it as an additive process. We assume a baseline group of untreated comparison clients similar in background to our treatment clients. For conceptual purposes, we can envision treatment subjects who "fall out" of treatment and those who remain. We assume that those clients who remain, on average, would be more successful than the comparison subjects even without treatment because they are a more select, motivated subgroup. But, we also assume treatment has benefits, naturally, and that it "pushes" the success of these motivated individuals higher than it would have been without treatment. The inferential problem comes in identifying the "push" from motivation from the "push" from treatment. In some cases, these causes may be so entangled that the separate influences are extremely difficult to reconstruct.

Although our discussion focuses on selection processes that bias results in favor of finding a treatment effect, it is possible that selection processes can affect group composition in a manner that biases results *against* finding a treatment effect. For example, there might be an incentive structure that would encourage higher risk defendants, rather than lower risk defendants, to enter treatment. Another possibility is that treatment selection is tightly controlled by providers who reserve treatment beds for the most difficult cases.

A Model of Sample Selection Process

To understand the complexity of the problem, we have attempted to represent in Figure 2 the most important selection processes that can occur in the research design when evaluating drug treatment in a correctional setting. In this context, we use the word "selection" to describe the processes that differentiate who enters treatment, as well as the processes that determine who exits treatment prematurely. This latter process is also called "attrition." Figure 2 indicates the kind of selection pressures (filters) that operate within an environment in which treatment is available and an additional selection process that occurs when researchers try to follow-up on inmates who have been released to the community. There are four prominent in-custody selection filters: self-selection, administrative — or clinical — selection, treatment selection, and transitional treatment selection. The last selection pressure occurs when there are biasing processes that determine which clients are lost to follow-up.

The first process, self-selection, is based on either internal motivational states or external incentives that dispose some people to volunteer for treatment.[13] The second process,

[13] One of the reviewers of our report asked us to address the issue of voluntary participation in these in-custody criminal justice programs. All of the programs reviewed in this section, as well as the drug treatment program within the Bureau of Prisons, were composed of voluntary participants. We are unaware of the extent to which criminal justice-based drug treatment programs are voluntary, mandatory, or "coerced." Although there is some literature on whether mandatory or coerced treatment can be successful, this is an aside for the present purposes. Even if drug treatment were mandatory, the attrition process would still affect the internal and external validity of the evaluation. Furthermore, the mandatory nature of drug

administrative selection, reflects the clinical judgment exercised by treatment providers and other administrators who determine whether someone is chosen for a program.[14] The third process, treatment selection, weeds out clients who cannot meet the program demands. As illustrated in our review of the research on prison therapeutic communities, treatment selection can result in high numbers of inmates failing the treatment.

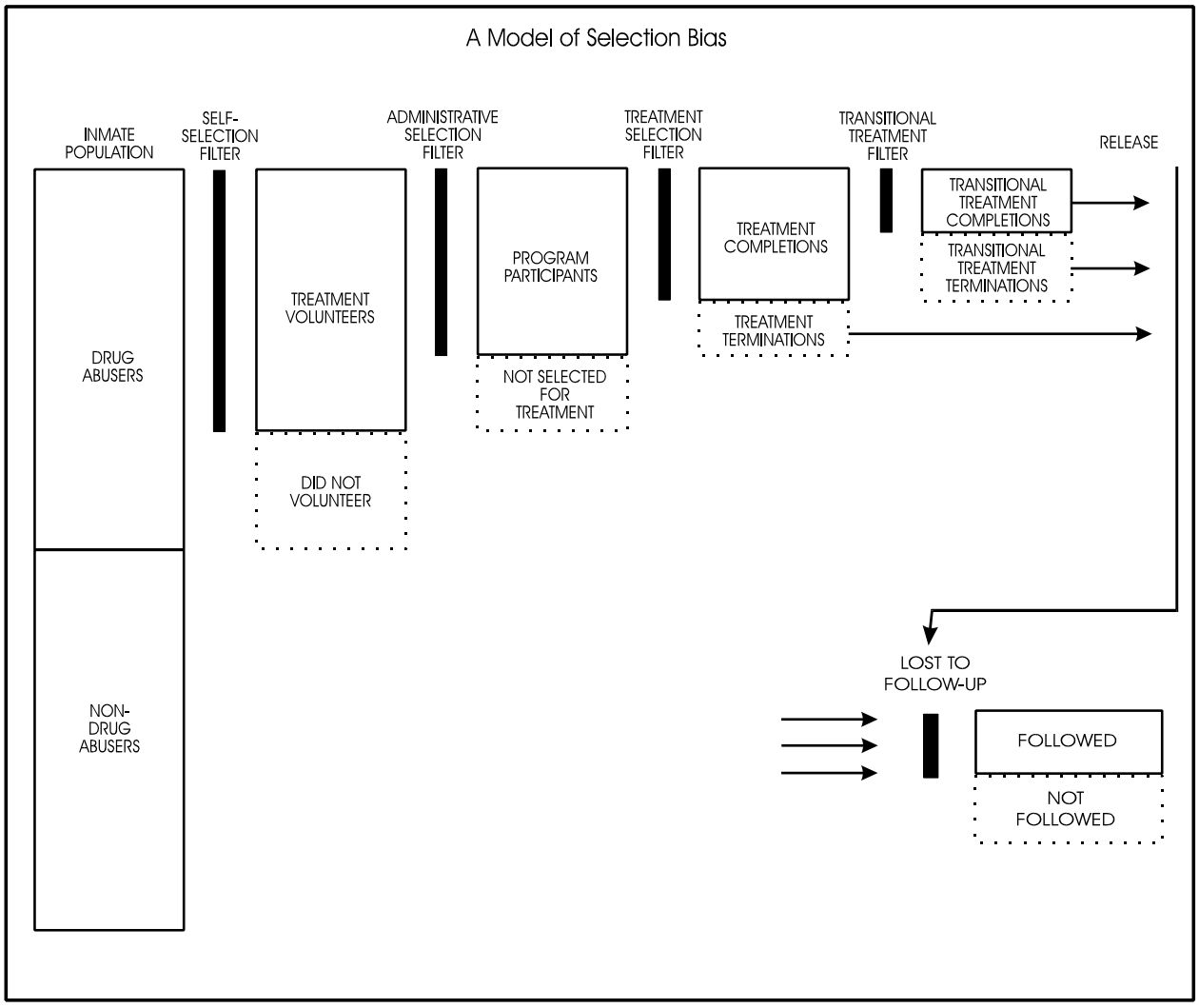

Figure 2 — A Model of Selection Bias

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treatment may mean that the selection process is removed from the client and handed to an administrator.

[14] These selection pressures can come from external sources, such as judges who strongly recommend candidates for treatment, or from internal sources, such as the pressures to fill treatment beds in a crowded prison system.

Another form of selection consists of weeding out participants during the transitional care phase of a multi-phase treatment approach. Participants can be terminated by staff or they can withdraw themselves. It appears from our study and some of the others that selection in this phase is usually not very significant. However, for situations in which the conditions of supervision for treatment subjects were very different from those for control subjects, this additional level of selection might need to be considered. Finally, there may be conditions that affect which study participants are lost to follow-up. For example, if follow-up interviews are voluntary, there may be self-selection bias introduced by those characteristics that are correlated to the individual's willingness or unwillingness to be interviewed.

The problem of selection bias becomes readily apparent from Figure 2 when one focuses on the end of the selection process and sees who remains in treatment. If the study design assesses only those offenders who have made it through every selection filter, it is very difficult to construct a legitimate control group composed of only those non-treated offenders who also would have made it through the same selection process had they had the opportunity to do so.

What may not be readily apparent from Figure 2 is the way selection pressures can also affect the comparison group. Let us assume we begin a study with a pool of drug dependent clients. From this pool a sample of clients is selected into treatment. As we have already noted, the selection pressures may operate in one of two ways. Clients in treatment may have characteristics that dispose them to more successful treatment outcomes (case 1) or they may have characteristics that dispose them to more unsuccessful outcomes (case 2). In both cases, if we have to draw our comparison sample from the individuals remaining after others have been selected for treatment, we may bias our design in a subtle way.

In case 1, the residual group of untreated clients, on average, may be less disposed toward successful post-release outcomes. In that case, we have "creamed" the treatment clients and the residual pool is composed of the "sour" remnants. A sample drawn from the residual pool will likely have less successful outcomes than will a randomly drawn sample of drug dependent clients composed of both motivated and unmotivated individuals. In case 2, the residual group of clients may be more disposed toward successful post-release outcomes. A sample drawn from this residual pool will be more likely to have successful outcomes than will a randomly drawn sample of drug dependent clients. Thus, in case 1, the residual comparison group will introduce bias in favor of finding a positive effect, while in case 2 it will introduce bias against finding such an effect.

Methodological Solutions to Selection Bias

There are several ways to attempt to handle the problem of selection bias in the absence of random assignment. None may be completely satisfactory. The first is to assess *all* client characteristics that can be used to adjust the treatment outcomes. Thus, if there are differences between two groups due to selection pressures, we can control for these difference in a multivariate analysis and adjust our outcomes accordingly. This approach will fail if there are

important unmeasured variables that distinguish the treated from untreated samples and those differences affect recidivism. This approach also requires a thorough theoretical understanding of the selection processes. We can speculate on some of the processes that may be affecting who is selected (or self selected) to go into treatment.

Client motivation or commitment may determine who volunteers for treatment. Perhaps a board or group of administrators chooses clients based upon the seriousness of the subjects' drug dependencies and the extent to which the selecting personnel believe clients will benefit from treatment. Attrition may also have its unique determinants. Perhaps some clients are unreceptive to specific treatment approaches. Perhaps those inmates most entrenched in a criminal lifestyle or most embedded in an inmate subculture are the most likely to withdraw from treatment. All of these processes can affect outcomes. Many researchers in this domain have attempted to control for client characteristics by measuring such variables as age, race, sex, criminal history, and drug dependency and then use these variables in a multivariate analysis. However, unless these variables control for the processes that affect both selection into treatment and recidivism, this technique will fail to control for selection bias. Our argument is that considerable thought should go into understanding and measuring selection pressures so that we can observe and control for these processes.

A complementary approach to handling selection bias is to choose a comparison pool of clients from sites in which no treatment is available or in which treatment is withheld. Based on the work of Bloom (1984), Rhodes (personal communication, 1997) has shown how this approach can be used to make inferences about treatment under conditions in which some clients accept treatment and some decline. Figure 2 graphically shows the different flows of clients under these circumstances. With just a few notations we can represent outcomes for the groups offered treatment and for the groups not offered treatment.

We adopt the following notation:

δ: *the effect of treatment at sites where treatment is offered.*

$\mathbf{F_{treated\ population}}$: *the average proportion of clients recidivating at sites where treatment is offered.*

$\mathbf{F_{untreated\ population}}$: *the average proportion of clients recidivating at sites where treatment is* ***not*** *offered.*

$\mathbf{F_{accept}}$: *the proportion of clients who would recidivate if treatment were offered and they accepted treatment.*

$\mathbf{F_{decline}}$: *the proportion of clients who would recidivate if treatment were offered and they declined treatment.*

$\mathbf{P_{accept}}$: *the proportion of clients who accept treatment if treatment were offered.*

$\mathbf{P_{decline} = (1\text{-}P_{accept})}$: *the proportion of clients who decline if treatment were offered.*

Using this notation alone, we can represent the total average outcomes of clients offered treatment, including those who accept and those who decline, as :

$$F_{\text{treated population}} = P_{\text{accept}}(F_{\text{accept}} - \delta) + (1 - P_{\text{accept}})F_{\text{decline}}$$

We can also show that the average effect for clients not offered treatment is :

$$F_{\text{untreated population}} = P_{\text{accept}}F_{\text{accept}} + (1 - P_{\text{accept}})F_{\text{decline}}$$

With a little algebra, the treatment effect is :

$$\delta = (F_{\text{untreated population}} - F_{\text{treated population}})/P_{\text{accept}}$$

In other words, if we know the total effect for those offered treatment and the total effect where no treatment is available, we can find the treatment effect if we know the population proportion that accepted treatment. Although we have represented this approach as if we are comparing two sites, the technique generalizes to a multi-site evaluation. As an example, consider three sites. In the first prison, no treatment is available. In the second prison, treatment is available and one-third of the inmates accept it. In the third prison, treatment is offered and two-thirds of the inmates accept treatment. Assuming the inmate populations are the same in all three prisons and using similar notation as above, in the prison with no treatment, the average outcome is:

$$T_1 = 1/3F_1 + 1/3F_2 + 1/3F_3$$

In the prison with one-third of the inmates treated, the average outcome is:

$$T_2 = 1/3F_1 + 1/3F_2 + 1/3(F_3 + \delta_3)$$

In the prison where two-thirds of the inmates are treated, the average outcome is:

$$T_3 = 1/3F_1 + 1/3(F_2 + \delta_2) + 1/3(F_3 + \delta_3)$$

By substitution, we can solve for the δ's, and this allows us to evaluate whether the treatment effect varies across the different sites. This approach generalizes to any number of sites. In the

current study, we used this approach in a regression context. We also adopted a third approach because we were unsure of the homogeneity of our population across the different treatment and non-treatment sites and we wanted a more general model that would allow us to build interaction effects into the model. This is the technique we describe next.

To adjust for selection directly, we developed a model that represents the selection process. This model is used in conjunction with a model that represents the effect of treatment. By modeling both processes simultaneously, this procedure allows us to estimate the treatment effect conditional upon the processes that cause an offender to be selected into treatment. This technique is described thoroughly in Appendix C.

Because selection bias is such a difficult problem, we decided to address the problem in two different ways in our analyses of the Bureau of Prisons' drug treatment programs. We measured a number of background characteristics we believed were related to post-release recidivism and drug relapse. However, because we were still unsure of the exact nature of all the selection pressures operating in our study, we adopted a procedure analogous to the method we described above. Our method compared clients from sites where treatment was offered to clients from sites where no treatment was offered. When analyzing the post-release dependent variables using this approach, we combined the outcomes of all treatment participants regardless of whether they completed treatment, withdrew, or were terminated for disciplinary reasons. These data were combined with data from the comparison subjects from DAP sites. Then we contrasted all of these clients — together — with a sample of inmates from sites where no treatment was available.

We also used the model, described in Appendix C, that incorporates some information about the selection process and uses that information to control for selection effects while simultaneously testing for treatment effects. Selection bias adjustment was made to the survival function associated with the time until an offender was arrested and the time until the offender had an officially recorded drug relapse. By modeling selection bias explicitly, we were able to test whether selection bias increased or decreased the survival time. If it increased the survival time, this was evidence that there were pressures that selected lower-risk defendants into treatment. If the selection bias parameter was negative, this suggested that there were pressures that selected higher-risk defendants into treatment, which would in turn lower their survival times. If the selection bias parameter was not significant, we could conclude there were no such selection bias pressures operating.[15]

It is not unusual to find, in previous research, that program completers are more successful than are controls, who in turn are more successful than are program terminators. If it was possible to classify correctly control group offenders' outcomes into hypothetical "completions" and

[15] The derivation and computation of these parametric survival models with correction for selection bias are not available in typical statistical packages. We engaged an econometrician consultant, William Rhodes of Abt Associates, to help us derive the appropriate models and estimate them using GAUSS.

hypothetical "terminations," and if we were to assume that treatment is effective, we would expect to observe that those who completed treatment had more successful outcomes than those in the control group who hypothetically completed the treatment. Further, we would expect to observe that those who did not complete treatment had equivalent outcomes to their counterparts in the control group who would hypothetically be expected not to complete the program. However, in the absence of our ability to classify hypothetical "completers" and "terminators" in the control group, the combined outcomes of program completers and terminators should be significantly better than those of the control group.

Some treatment proponents might argue that even if treatment is, in fact, nothing more than a weeding out process, this is still a useful result of the treatment process because it identifies the individuals who are more likely to succeed. The problem with this logic is that the same goal might be accomplished by simply improving our risk-classification devices in the absence of treatment. Furthermore, it is important to know whether it is treatment per se, risk selection, or both, that accounts for better outcomes in the treatment group. We cannot hope to improve treatment or understand how it works if all we accomplish is the risk selection of inmates.

Additional Selection Concerns

Figure 2 also can be used to conceptualize the selection bias issue by depicting the problem of choosing an appropriate control group and the level of generalizability inherent in the research design. It is clear from Figure 2 that treatment terminations, whether in-prison or community-based, cannot be ignored if we are to make any sense of a program's effects. Furthermore, it is clear that by choosing a comparison group of volunteers, our level of generalization is restricted to treatment for "motivated" treatment participants.

Another problem becomes apparent from Figure 2. Consider a design in which researchers choose a control group (composed of drug abusers) that is a combination of volunteers and non-volunteers. Unless one models the selection process and incorporates it into the analysis, the outcome differences between a self-selected or administratively selected group and an "unselected" group may be attributable entirely to the level of motivation of the volunteers and have nothing to do with the treatment provided.

Our procedures ensured that we collected follow-up data on all inmates who began treatment and were selected into our "convenience" samples. Thus, regardless of whether an inmate completed or was terminated from the program, data were collected on the individual's post-release outcomes. We have organized inmates in our treatment samples into 9-month completers, 12-month completers, disciplinary terminations, program withdrawals, and treatment non-completers. These last two groups of inmates completed some portion of their treatment but had to withdraw for reasons beyond their control. For example, a significant number of inmates in these groups were released earlier than expected. We also collected data from comparison subjects in both DAP and non-DAP sites. These data collection procedures are described in great detail in subsequent chapters.

To recapitulate, depending on the analysis, we treated comparison subjects from DAP sites in different ways. In the first analysis, to replicate previous research, we contrasted both (1) each of the treatment groups (those who completed and those who did not complete for a variety of reasons) and (2) the DAP comparison subjects from the treatment sites, to the non-DAP control subjects from sites at which no treatment was available. If highly motivated inmates were entering treatment at DAP sites, the pool of drug dependent comparison subjects who participated in our study would have been composed of less motivated, perhaps more risky, clients. Under these circumstances, the DAP comparison subjects may have had lower success than did the non-DAP control subjects. Clearly, quite the opposite would happen if the residual pool of DAP comparison subjects had been composed of more motivated inmates. Both of these hypothetical outcomes rest on the assumption that the DAP comparison group is composed of inmates representative of all the groups we have described and that they are represented in the same proportions. Thus, the DAP comparison group is hypothetically composed of completers, disciplinary terminations, program withdrawals, treatment noncompleters, and the residual comparison subjects.

In the second analysis, we combined data from the treatment groups with data from comparison subjects from DAP sites. We reasoned that the DAP comparison clients were inmates who had treatment available but chose not to participate. We would expect that these same types of inmates would be represented among our non-treatment site comparison subjects. Thus, research subjects in the non-treatment sites should consist of all levels of inmates who would have volunteered for treatment, as well as inmates who would have declined treatment. Thus, the appropriate test of treatment in our design is the combined test of (1) all inmates who were selected for treatment, as well as (2) the inmates who did not volunteer for treatment but who had treatment available (were housed at a DAP site), contrasted with the control subjects who did not have treatment available (were not housed at a DAP site).

Finally, in a third approach, we explicitly model the selection process, using non-treated subjects from both DAP and non-DAP sites as "controls." The difference between the two non-treated groups is that comparison subjects from DAP sites were subject to selection bias while control subjects from non-DAP sites were not. By explicitly modeling the selection process, we could statistically capitalize on all comparison subjects to increase the power of our treatment versus non-treatment contrast.

In the following sections we critically review the most commonly cited research on in-custody therapeutic communities using our conceptual model of selection bias as a heuristic device.

A Critical Review of Prison Drug Treatment Research

Our review of the literature focuses on five programs that have received considerable attention in recent reviews of the literature on prison residential drug treatment studies. These programs include Stay 'N Out, Cornerstone, Key/Crest, New Vision at Kyle Unit, and the Amity Right Turn Project.

We examine the published and unpublished reports on these programs in some detail. Our general conclusion is that all of these studies suffer from the inferential problems associated with disentangling treatment effects from selection bias effects. Although we are somewhat critical of the research in this domain, we realize how difficult it is to conduct a program evaluation and how easily controls, intended to introduce rigor into the evaluation, are easily compromised. Our critique attempts to assist future program evaluators in this research area and help them avoid some of the same mistakes that both we and other evaluators have made.

Stay 'N Out Program

Wexler and colleagues have published a series of articles that report on the effectiveness of the Stay 'N Out drug abuse treatment program used by the Department of Corrections in New York State (Wexler and Chin, 1981; Wexler, Falkin, and Lipton, 1988; Wexler, Falkin, and Lipton, 1990; Wexler, Falkin, Lipton, and Rosenblum, 1992; Wexler and Williams, 1986). We focus primarily on the recidivism outcome results reported in Wexler, Falkin, and Lipton (1990) as adapted and slightly modified for a National Institute on Drug Abuse Research monograph (Wexler, Falkin, Lipton, and Rosenblum, 1992).

Their evaluation of the Stay 'N Out therapeutic community (TC) contrasted male inmates who participated in that program with inmates in two other drug treatment programs (milieu and counseling treatment) and a control group of inmates who had volunteered for the therapeutic community but were never admitted to the program because of time constraints. The volunteer control group was used to minimize selection bias issues.

Female TC participants were contrasted with those in a drug counseling treatment group and those in a control group composed of women who volunteered for the TC program but changed their minds prior to admission into the program. Unlike the male control group, the female control group could easily have been composed of unmotivated women who would be the least likely to succeed following release and thus bias any contrast between program and non-program participants.

Wexler, Falkin, and Lipton (1990) reported their results first by way of a series of group contrasts among the mean differences in the outcome variables without controlling for background differences, and then by using a multivariate analysis that controlled for background variables. Although Wexler *et al.* argued that their study provided "convincing evidence that prison-based TC treatment can produce significant reductions in recidivism," (p. 89) we found several shortcomings in the study's analysis and methodology.

Female inmates were excluded from the multivariate analysis because, according to the authors, there were too few to analyze. The outcome variables reported by Wexler *et al.* included the percent of inmates arrested after release to parole supervision, the number of months before such

arrests occurred, and the percent having favorable parole outcomes. This last measure was based on whether an inmate completed parole without a revocation, arrest, or rule infraction.

Wexler *et al.* reported that their multivariate analysis of percentage arrested and parole outcome produced no significant results. There was no effect tied to background characteristics or time-in-treatment. Wexler *et al.* did find significant predictors in their multivariate analysis of time-to-arrest, and they reported those results. We are puzzled by the fact that age and criminal history, which were influential predictors of time-to-arrest, were not significant predictors of percentage arrested or parole outcome. These variables typically are the most influential predictors of any measure of post-release criminal recidivism (Harer, 1994). This is a minor point relative to their interpretation of the multivariate analysis involving time-to-arrest.

In addition to the background characteristics of age and criminal history, Wexler *et al.* included the following variables in their regression analysis: the duration of parole supervision, a dummy code for each type of treatment, time-in-treatment for each of the treatments, the duration of parole supervision, the amount of time an inmate spent in prison after completing treatment but before release, and the square of the amount of time an inmate spent in the therapeutic community.

In their analysis of time-to-arrest, Wexler *et al.* interpreted the linear and quadratic regression coefficients for time-in-treatment. However, they failed to interpret the program participation variables. Although only the dummy variable for the TC treatment was significant, all of the treatment dummy variables demonstrated that regardless of the type of drug treatment given to inmates, having any drug treatment shortened the period between release and arrest relative to the control group. Thus, the treatment dummy codes demonstrated that inmates in treatment were arrested sooner than were inmates in the control group. In addition to the fact that TC inmates failed sooner than did control group participants, the relationship between TC treatment and failure is quadratic. That is, time-to-arrest increased with the amount of treatment up to a point, then declined thereafter. Wexler *et al.* emphasized this finding while disregarding the dummy-coded treatment effects.

The other major finding emphasized in this study, as well as in secondary sources that refer to this study (*see*, for example, Lipton, 1995), was that when the treatment effect was examined without accounting for the other background variables, male inmates were less likely to be arrested if they participated in the TC drug treatment. For female participants, none of the group contrasts reached conventional statistical significance. The percentage of male inmates arrested after release from prison varied by treatment group. Among TC inmates, 26.9 percent were arrested after release. For milieu and counseling inmates, 34.6 and 39.8 percent, respectively, were arrested. Among no-treatment controls, 40.9 percent were arrested after release. In light of the fact that the multivariate analysis of this outcome measure failed to reach statistical significance, we argue that these results probably were attributable to differences in background characteristics of the groups and not to a treatment effect. But there are other reasons why these group differences are possibly not meaningful.

The different groups had different risk periods, with the TC group having the shortest average risk period (34.7 months). The other groups each received, on average, about 41 months of parole supervision. Thus, each subject in the TC group, on average, had 6 fewer months of parole supervision and, therefore, much less time in which to be arrested. Another difficulty with this analysis of percent arrested by treatment group concerns the extent to which Wexler *et al.* should have adjusted their findings for people who were censored. If inmates were technically violated, rather than arrested, this would have removed them from the risk set. Thus, fewer arrests could mean greater parole violations. Without an explicit explanation of the censoring process, we cannot rule out this possibility.

In general, when one is analyzing the time to an event — whether relapse, arrest, or conviction — it is much more appropriate to use event history techniques that allow one to treat different risk periods by censoring observations and removing them from the risk set. This is a problem not only with this set of analyses, but with most other studies in this research domain as well.

Cornerstone Program

Field has published several studies evaluating the Cornerstone Program, a residential program for alcohol- and drug-dependent inmates within the Oregon correctional system. A key component of the Cornerstone Program as described by Field (1985) is that inmates who are admitted to the program must be willing to commit to at least 6 months of follow-up treatment in the community. Another program admission criterion requires that the inmates be granted minimum-security status by the prison superintendent. At first glance, this would seem to be a very narrow selection criterion that would exclude all but the lowest-risk candidates for drug treatment and would have profound implications for possible selection bias effects. However, Field (1985) described the treatment clients as having, on average, about 12 prior arrests, 6 prior convictions, and 6 years of adult incarceration. Also, these clients were described as having chronic substance abuse histories.

In addition to the follow-up treatment in the community, Cornerstone graduates "have a job, a place to live, and a drug-free support network before discharge" (Field, 1985, p. 52). Thus, the community aftercare component of this program went far beyond focusing on drug relapse.

To compare program graduates, Field retrospectively chose three comparison groups. Group I was composed of Cornerstone dropouts, Group II was composed of Oregon parolees with some history of drug abuse, and Group III came from a follow-up study in Michigan that Field chose because the study followed a "similar population over a similar time frame" (Field, 1985, p. 52). There was a uniform 3-year follow-up period, and Field assessed recidivism in two ways. Recidivism was defined as a return to prison within 3 years and, separately, as a conviction within 3 years. Among Cornerstone graduates, 29.2 percent returned to prison within 3 years. Among the comparison groups, 74.1 percent of the dropouts were recommitted, 37.1 percent of the group composed of Oregon parolees with a history of substance abuse were recommitted, and about 43 percent of the Michigan release cohort were recommitted. Those reconvicted within 3 years consisted of 45.8 percent of the Cornerstone graduates, 85.2 percent of the Cornerstone

dropouts, and 74.7 Oregon of the parolees with a substance abuse problem. No reconviction data were available for the Michigan cohort.

There are three major problems with interpreting the results of this study. The first is that we have no basis for comparing Cornerstone graduates with the three comparison groups on any variables related to recidivism, such as age, criminal history, degree of substance abuse, and family social support. Thus, we have no guarantee that the groups were equivalent with respect to their risk of recidivism. Secondly, there are two significant program components to Cornerstone — the first is institution based and the second is community based. Even if this program is influential in reducing relapse and criminal recidivism, we cannot disentangle which program component was the more important one. Finally, as the recidivism data showed for the program dropouts, and as Field noted, program participants simply may have been highly motivated inmates who would have succeeded with or without Cornerstone.

Apparently, the dropout rate at Cornerstone was extremely high. Field (1992) enumerated the dropout rate in a recidivism study of 220 inmates who had been admitted to Cornerstone over a 2-year period. Of those 220 admissions, 65 withdrew after spending one to two days in the program, 58 withdrew after spending between 2 and 6 months in the program, 43 withdrew after spending at least 6 months in the program, and 43 graduated. Thus, there was a far greater number of dropouts than program graduates.

Field used these differential dropout rates to make a point about the duration of treatment. Field (1992) reported on the criminal recidivism of these groups, showing that the longer an inmate was in the program, the less likely he or she would be arrested, convicted, or recommitted to prison following release from prison. Although Field acknowledged that the length of a subject's treatment may have acted merely as a proxy for his or her level of motivation, he argued that pre-treatment incarceration data demonstrated that all four groups were equivalent in their pre-program arrest, conviction, and commitment rates.[16] In other words, by controlling for pre-program levels of criminal history, Field was satisfied that the dropout pattern represented treatment effects and not motivation or other selection effects.[17] Even though Field demonstrated equivalence among the treatment groups (categorized by duration of treatment) with respect to prior criminal history, we know there are a host of other variables that also could affect the group outcomes in the absence of a treatment effect, none of which Field incorporated into his analysis. Furthermore, self-selection probably represents, among other things, the level of motivation and

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[16] Please note that pre-program data were not available in the 1985 study comparing program graduates to the three comparison groups.

[17] As M. Douglas Anglin, one of our reviewers pointed out, motivation is not constant over time. Rather, it is episodic. Anglin argued that treatment outcome is determined by a host of factors, including motivation, treatment retention, and type of services offered. These combine in some complex way to influence outcomes. Nevertheless, it is still the case that the resultant effects cannot be easily disentangled.

commitment one has to maintaining a drug-free lifestyle. Commitment to change may be quite unrelated to one's criminal history; in fact, it may even be inversely related.

As we argue below, program dropouts contaminate the interpretation of treatment effects in more ways than one. Especially for programs in which the dropout rate is extremely high, there arises the possibility that a program is simply selecting out high-risk-of-failure candidates rather than changing or rehabilitating low risk candidates. Another way of viewing this potential selection process is to approach it with a risk-assessment analysis. We consider two possible hypotheses. The first is that dropouts are more likely to have background characteristics that predict criminal recidivism. The second is that they are equivalent to "stayers" on objective measures of risk; however, by observing treatment subjects closely or by testing their motivation in a controlled, closely monitored environment, staff can further "weed out" higher risk inmates.

Key-Crest Program

The Key-Crest Program is a drug treatment intervention occurring in three phases. The Key component is a prison TC for inmates in the Delaware corrections system. Crest, the second component, involves inmates released to a community work-release center where they maintain jobs in the community but live in a facility where they continue their drug treatment in a modified TC. In the final component, offenders are released to the community, either under parole or some other form of supervision. In this stage, drug treatment consists of outpatient counseling and group therapy.

Four groups were evaluated. The first was composed of 43 inmates — selected by correctional counselors — who volunteered to participate in the prison-based TC. Because the Crest program had not yet been implemented, these inmates were the only Key program participants who did not subsequently participate in the Crest stage. The second group consisted of Key-Crest inmates who participated in both stages. Virtually all Key graduates were allowed to participate in Crest after it was implemented. The third and fourth groups were composed of inmates who had drug abuse problems, had not participated in Key, and were given the opportunity to participate in the Crest work-release program. On a random basis, half of these volunteers (the Crest-only group) were provided the Crest program, while the other half (the comparison group) participated in work-release in the absence of residential drug treatment. Thus, the comparison group for these analyses was inmates who had drug abuse problems, had volunteered for Crest, and had not received in-prison TC drug treatment but had received AIDS/HIV prevention education.

There were two selection bias processes operating in the Key-Crest design. The first selection process involved selection into the Key and Key-Crest groups. For one, it appears that the selection involved staff evaluation of candidates for the program. The second selection process occurred as a result of the way baseline data were gathered. These data were gathered just prior to inmate releases from prison. Baseline data were collected on Key graduates, but not on Key terminations. Thus, only Key graduates were followed in the longitudinal design. Data were gathered on Crest and comparison subjects at baseline, in the absence of any knowledge about

potential future attrition in these two groups. Thus, both the Key and Key-Crest groups were composed of inmates who were motivated enough to graduate from the Key component of this program.

Even though Key-Crest participants had the opportunity to drop out of the program while they were in the Crest stage, this group already was composed of a very select group of motivated individuals. As noted in Deleon, Inciardi, and Martin (1995), the Crest-only group was composed of some clients who "displayed negative attitudes toward the treatment program, which generally led to their quitting or being discharged from the Crest program" (p.88). However, all inmates in the Crest-only groups were still followed even though some had dropped out of the program (Inciardi, 1997, personal communication).

The Key-Crest program is being evaluated by Inciardi and his colleagues (Martin, Inciardi, and Saum, 1995; Martin, Butzin, and Inciardi, 1995; Inciardi, Martin, Butzin, Hooper, and Harrison, 1997). Martin, Butzin, and Inciardi (1995) reported data based on interviews conducted 6 months after the inmates were released from prison. Most inmates who had participated in the Crest stage were probably still under supervision at the time of this 6-month interview. Thus, the results at this stage should be interpreted with a great deal of caution. Based on inmate self-reports, the data showed that 97 percent of the Key-Crest group and 84 percent of the Crest-only group said they had not been arrested within 6 months of release from prison. Among the Key-only participants, 74 percent reported they had not been arrested, while 60 percent of the comparison group claimed no arrests. The proportions reporting drug use were similar. When these proportions were adjusted for background characteristics, including time-in-treatment, the same ordinal relationship was obtained. Key-Crest participants were the least likely to self-report arrest and drug use, followed by Crest-only, Key-only, and comparison subjects.

An 18-month follow-up of the program (Inciardi, Martin, Butzin, Hooper, and Harrison, 1997) showed that 77 percent of Key-Crest participants reported being arrest-free at 18 months, while 57 percent of Crest-only, 43 percent of Key-only, and 46 percent of the comparison group reported being arrest-free. Drug use was measured by combining results of self-reports and urinalysis tests. The drug-free pattern corresponded to the arrest-free pattern. However, there is no indication that there was any attempt to check the veracity of the self-reported arrests.

Although several papers written by Inciardi and his colleagues have emphasized that offenders should be receiving aftercare while they are under supervision, at the time of their study there was no formal aftercare (Inciardi, 1997, personal communication). Apparently, this study has no selection bias and no attrition operating in the Crest-only and comparison groups, although the authors have never reported the extent to which inmates withdrew or were terminated from the Crest program. Therefore, the reductions in self-reported arrest and actual drug relapse may be entirely attributable to the effects of transitional treatment. However, the Key-only and Key-Crest groups are composed of offenders who were either selected into treatment or who selected themselves out of treatment. Reductions in self-reported arrest and actual drug relapse in those groups are still potentially contaminated.

New Vision In-Prison Therapeutic Community, Kyle Unit

The New Vision In-Prison Therapeutic Community in Kyle, Texas, is only one component of a comprehensive Texas criminal justice initiative to treat criminal drug abusers. The Kyle unit is being evaluated by a team of researchers affiliated with Texas Christian University. There have been several reports of the evaluation conducted by Simpson and his colleagues (Simpson, Knight, Chatham, Camacho, and Cloud, 1994; Knight, Simpson, Chatham, Camacho, and Cloud, 1995; Knight, Simpson, Chatham, and Camacho, 1997). Outcomes are available for inmates who had been released for 6 months.

The program's evaluation compared a control group to a treatment group composed of inmates who participated in a 9-month prison-based TC, followed by 3 months of community-based residential treatment, followed by a year of outpatient treatment. Program graduates agreed to provide urine samples for drug testing on a monthly basis.

The selection process for participants in the drug treatment program began with a drug-use screening mechanism given to all inmates who entered Texas Department of Corrections facilities. A treatment referral committee reviewed the inmates' records, which included self-reported drug use. Inmates who had less than 9 months remaining on their sentences or who had committed an aggravated offense were excluded from further referral. Inmates who qualified for treatment had their cases forwarded to the Texas Parole Board for the final decision on placement in a drug program. Both comparison and treatment subjects in this study completed the initial referral process. However, the Parole Board rejected a certain number of inmates for treatment while still granting parole to these inmates. The reasons for these decisions were not specified by the authors. Thus, we have an initial selection process that differentiates treatment and comparison subjects. As it turned out, based on a composite risk assessment, treatment subjects were at higher risk for recidivism than were comparison subjects. Nevertheless, Parole Board members used their "clinical judgment" to further refine the selection process based on some unknown set of "clinical" criteria.

Also, treatment subjects were sent to halfway houses. There was no indication that comparison subjects were assigned to halfway houses after release from prison; nor was there any measurement of their level of release supervision (including whether they were tested for drug use). As the authors indicated, in addition to drug treatment, halfway houses fulfill other social service needs and provide assistance in locating employment. Thus, potential differences between the treatment and comparison groups could be attributable to in-prison treatment, halfway-house drug treatment, halfway-house transitional assistance, the drug testing and close supervision of parolees in the treatment plan, or any combination of these factors. Although there does appear to be a selection process operating in the Kyle Unit evaluation, Simpson and his colleagues have described that process more thoroughly than has any other study we reviewed.

A possible, but significant, measurement problem with this study is that the risk sets for the treatment and comparison groups were quite different. Outcome assessment occurred at 6 months

and will occur at 12 months after release from prison. However, for treatment subjects, 6 months after release from prison was only 3 months after release from the halfway house. That is, the 6-month risk set for treatment subjects included 3 months of halfway house placement and 3 months of parole supervision, while the risk set for comparison subjects included 6 months of parole supervision. In their future analysis, the risk set for the treatment group will consist of 3 months of halfway house and 9 months of parole supervision, contrasted with 12 months of supervision for the comparison group. Treatment outcomes will be severely biased in the direction of a more positive treatment effect, because halfway house supervision decreases the probability of arrest relative to parole supervision. Thus, differences between treatment and comparison groups may merely reflect differences in the level of supervision and thus level of arrest risk for the two groups, rather than any effect of treatment.

The attrition process for this evaluation was described comprehensively and provides a good indication of how difficult it is to conduct follow-up interviews for this population. Of 482 treatment referrals, 386 (80 percent) graduated; 29 inmates (6 percent) were transferred for medical reasons, outstanding warrants, or inappropriate classification of drug problems); and 67 (14 percent) were terminated for program non-compliance. Unfortunately, no attempt was made to follow-up on the program terminations. Also, there was attrition among those who completed the program and those who constituted the control group, because inmates were not available at the time the 6-month follow-up data was collected. By that time, only 222 of the original 386 treatment graduates could be interviewed, and 75 of 121 control group inmates released to parole could be interviewed. Attrition was due to offenders who moved out of the area accessible to interviewers, who were recommitted to prison, who could not be located, or who refused to be interviewed. It is not clear why inmates who were recommitted to prison were not interviewed and did not enter into the outcome results. However, there was an equal percentage of recommitment for the treatment and comparison groups — about 10 percent. Not only was the attrition rate extremely high, there was no attempt to collect follow-up data on the program failures; thus, the results could be severely biased.

It is interesting to note that — at least in a set of univariate comparisons — program terminations and graduates were similar in background characteristics. Program graduates were equivalent to program terminations in terms of age, education, marital status, type of commitment offense, and recidivism risk score. Whites were more likely to be removed from the program than were African Americans. It would be useful to know whether graduates and dropouts were comparable in a multivariate analysis. One of the limitations of this kind of research is the failure to learn what distinguishes program graduates from program failures. The more we can understand about this process, the better we might be in selecting participants for the program in the first place and the more we will understand the selection process. Further, it will aid us in tailoring programs to meet the individual and group needs of the participants.

Knight *et al.* also reported 6-month post-release outcomes without controlling for the many background characteristics they measured. Official Texas arrest records indicated that 7 percent of the treatment group members had been arrested, compared to 16 percent of the comparison

group members. Treatment clients self-reported that they had engaged in illegal activities during an average of 11 days in the 6 months since their release from prison, while comparison inmates reported an average of 28 days. In reporting these comparisons, Knight *et al.* acknowledged how dissimilar the risk sets were for these two groups. The drug relapse data they reported were problematic for this same reason.

The dissimilarity in risk sets was acknowledged by Knight *et al.* in 1996, although no adjustments were made to the data. Knight *et al.* reported on considerable background data, including information on sociodemographic characteristics, criminal background, drug-use history, HIV/AIDS risk behaviors, ratings of social and psychological functioning, ratings of treatment experience, clinical assessments of attention-deficit disorders, hopelessness, depression, and symptom reports. These data should have been analyzed with multivariate techniques.

If we ignore the many methodological problems with this study and assume that at the end of the 12-month post-release arrest period the treatment group had a lower drug relapse and lower criminal recidivism rate, the strongest conclusion we can make is that while offenders are *in treatment*, they are less likely to recidivate and return to drugs. To assess what happens to these offenders *after* treatment, Simpson and his colleagues must follow the treatment and control groups for a period after the outpatient counseling has ended.

Amity Right Turn Project

The Amity Right Turn program combines prison- and community-based therapeutic communities for inmates who volunteer for treatment. This program is funded by the California Department of Corrections in the R. J. Donovan medium security Correctional Facility in San Diego. The program is being evaluated by Wexler and his colleagues. Wexler, DeLeon, Thomas, Kressel, and Peters (1997) have written an initial report on their evaluation of the program using reincarceration of subjects in the California prison system as their primary outcome. Reincarceration included a commitment for either a new offense or a technical violation of parole.

The researchers divide the subjects into five groups, with inmates who either had volunteered to be treated, had a drug problem, or were within 9 and 14 months of their parole release composing a waiting list of eligible participants. From this pool, inmates were randomly selected to participate in the prison TC.

There were a total of 715 research subjects. Inmates who were eligible but could not be treated prior to their release composed the control group (n=290). The remaining four groups consisted of the inmates who had been randomly selected for treatment in the prison TC. The composition of the four study groups depended upon whether they volunteered for post-release community-based treatment and whether they completed the prison or community-based program. Thus, the first study group was composed of inmates who volunteered for the prison program but who were terminated (prison treatment dropouts, n=95). The second study group consisted of those inmates

who completed the prison drug program but did not volunteer for the community-based program (prison treatment completions, n=193). The third study group included inmates who volunteered and completed prison drug treatment and who volunteered and were terminated from the community-based program (prison treatment completions/community-based dropouts, n=45). The fourth study group was composed of inmates who volunteered and completed the prison and the community-based program (prison completions/community-based completions, n=92).

Wexler *et al.* reported that the no-treatment control group had significantly higher reincarceration proportions at both 12 and 24 months after release from prison than did all of the other study groups combined. The 12-month comparison showed that the control group had 49.7 percent recidivism and that the combined study groups had 33.9 percent recidivism. At 24 months, these percentages were 59 and 42.6, respectively. When the combined result is separated into the control and four study groups, the five groups had the following reincarceration percentages at 12 months: control group, 49.7; prison treatment dropouts, 45; prison treatment completions, 40; prison treatment completions/community-based dropouts, 40; and prison treatment completions/community-based completions 6.5.[18]

Wexler *et al.* also reported the number of days until reincarceration; however, for some reason these data were only compiled on 256 releasees for the 12-month follow-up and on 166 releasees for the 24-month follow-up period. Generally, the time-to-recidivism data mirrored the 12- and 24-month reincarceration data. A logistic regression of background factors, in conjunction with the treatment effect, indicated that reincarceration was 42 percent less likely for the combined treatment groups than for the control group. The background factors included age, ethnicity, criminal history, IQ, childhood problems, anti-social DSM-III-R diagnosis, distress, and social achievement. Unfortunately, there was no multivariate analysis that combined all of the background factors with dummy-coded representations of the different study groups. This may have given some indication that the combined effect was primarily attributable to the inmates who completed both the prison and community-based programs.

Wexler *et al.* acknowledge that their results were confounded by the fact that, during the post-release period, inmates who were receiving treatment in the community-based TC were at much lower risk than were other releasees simply by their residence in the TC. This would also affect the 24-month outcomes. If the risk periods were defined as beginning the day after release from the community-based facility or the day after release from prison for clients who did not participate in the community-based facility, the "risk environment" would have been more comparable for the different groups involved in the evaluation. It is clear from the analysis of the individual study groups that the dramatic differences between the combined study group and the control group were attributable to, primarily, the prison treatment completion/community-based completion group. Although no analysis was presented, there were much more modest differences

[18] In their report, Wexler *et al.* did not provide the actual percentages of inmates who were reincarcerated for the prison dropout, prison completion, and prison completion/community-based dropout groups. We had to estimate these percentages.

between the control group and the three study groups composed of inmates who spent little or no time in the community-based aftercare facility.

There is another limitation to this study as well. In order to control for selection bias, the researchers used treatment volunteers exclusively. This not only limits their generalizations to volunteers (as it does in most of these studies), but it also gives us no indication how treatment results compare to outcomes of drug dependent prisoners who are unwilling to volunteer for treatment. Secondly, as the authors acknowledged, while they were able to control for selection bias at the prison treatment phase, they were unable to control for selection bias at the community-based treatment phase. The clearest conclusion that can currently be drawn from this study is that the longer an inmate volunteers and stays in treatment, the less likely is his or her reincarceration. Whether prison drug treatment was effective was ambiguous in this study, and whether community-based drug treatment was effective after release was largely untested.

Summary of Research Literature

From our close reading of these studies, we have found fundamental problems in the designs, analyses, and interpretations of results. However, the researchers who have conducted these studies have referred to each other's work as mounting evidence that in-prison drug treatment, especially in combination with post-release community-based treatment, can produce dramatic results. Furthermore, secondary references to these studies (*see*, especially, Lipton, 1995) minimize or fail to mention the methodological problems inherent in these studies and, instead, continue to report what appears to be a consistent set of results across different settings.

The clearest finding comes from the program being evaluated by Inciardi and his colleagues in the state of Delaware. By virtue of random assignment and a comprehensive follow-up of those who dropped out of the transitional care component of the program, we can have confidence in the finding that offenders receiving transitional care in the absence of in-prison treatment are less likely to recidivate and relapse to drug use. Replication of this finding in other settings by other researchers could be very compelling. Further analyses by Wexler and his colleagues of the Amity Right Turn Project may also indicate that community based aftercare is an important drug treatment dimension; a redefinition of their risk period may lead to that conclusion. However, the community-based study groups were composed of volunteer inmates, and even redefining the risk periods cannot circumvent the bias introduced by this self-selection.

Although all of the other results are suggestive of effective treatment, this may merely reflect the culmination of a selection process that demonstrates that drug treatment — whether in prison or in the community — is a winnowing process. By the end of that process, only those most likely to succeed remain in treatment.

In summary, although we found the evidence on drug treatment effectiveness to be less than compelling, after reviewing the recent literature on in-prison therapeutic communities and

conceptually examining the processes that lead to subject selection and attrition, we developed a research design that we felt would address and rectify the major methodological problems. We acknowledge that it is extremely difficult to conduct random-assignment research designs in an applied setting. In the absence of random assignment, statistical techniques — such as those we adopted — are technically difficult, depend upon a great many assumptions, and may not always solve the problem. Our complete design is presented in Chapter 4.

CHAPTER 3: DESCRIPTION OF DRUG TREATMENT PROGRAMS AND SERVICES

This chapter describes the nature of the BOP's drug treatment programs and the changes that occurred since the inception of the TRIAD evaluation project. In addition, we provide a brief description of the post-release treatment services available for inmates released with conditions of supervision.

Paths to Treatment Service

Inmates interested in drug treatment receive services through various means. Treatment services for BOP inmates are available at three different stages: while incarcerated, during a halfway house placement, and while under supervision by a Probation officer. Services can be offered during the latter two stages only if the individual received a halfway house placement or was released with a condition of supervision. Treatment services for study subjects ranged from none to services at all three stages. The treatment available while incarcerated consisted of the residential DAP in combination with non-residential outpatient services and self-help groups. Treatment provided during a halfway house stay is referred to as "transitional services" and consists of outpatient counseling services. Transitional services were required for all DAP graduates.

Treatment provided while an individual was under supervision is referred to as post-release treatment. This treatment consists of a wide range of services, including both outpatient and residential/inpatient services, with an emphasis on outpatient services. It also includes participation in self-help groups.

Program Development by Location

The residential Drug Abuse Treatment Programs offered treatment for alcohol and other drug problems, and were implemented in two distinct categories: pilot programs and comprehensive programs. Later, all programs were referred to as residential Drug Abuse Treatment Programs. Pilot programs offered 1,000 hours of treatment over a 12-month period, with a staff-to-inmate ratio of 1:12. Comprehensive Drug Abuse Treatment Programs provided 500 hours of treatment over a 9-month period, with a staff-to-inmate ratio of 1:24.

The first eight programs were approved for activation in FY's 1989 and 1990.[19] During FY's 1989 and 1990, the BOP implemented its first three residential Drug Abuse Treatment Pilot

[19] The Federal fiscal years run from October 1 through September 30.

Programs at FMC Lexington, FCI Butner, and FCI Tallahassee.[20] By the end of FY 1996, 39 programs had been approved for implementation. Figure 3 shows when each DAP was approved for activation and indicates which of the programs were included in this study and which were eliminated as research sites during the study.[21]

Admission Criteria

All admissions into the BOP's residential Drug Abuse Treatment Programs were voluntary. Initially, residential programs required inmates to have (1) a drug problem and to have completed the BOP's Drug Abuse Education Course; (2) no outstanding legal concerns to interfere with Community Corrections Center (CCC) placement; (3) no serious medical or mental health problems; (4) no violent behavior within the last 12 months; and (5) between 24 and 36 months remaining on their sentences. By the time the first policy was issued, however, a number of these criteria had changed.

Inmates could apply for program admission at any time, with priority given to those inmates with less time remaining on their sentences. All program participants had to have at least 15 months remaining until their release dates (18 months for pilot program participants). An inmate was accepted into a program if:

- the inmate had a history of moderate to severe drug abuse, as reflected in the psychological assessment score on the Inventory of Substance Use Patterns (ISUP) administered by Psychology Services (or as reflected in the presentence investigation report);
- the inmate had no history of violence or assaultive behavior during the current incarceration;
- the inmate was fluent in the English language;
- the inmate had no serious medical, psychiatric, or psychological problems that would interfere with full program participation;
- the inmate was not a State boarder;

[20]FMC refers to a Federal Medical Center and FCI to a Federal Correctional Institution (*see* Glossary of Terms).

[21] A comprehensive history of DAP development between 1988 and 1995 is contained in an unpublished report — "BOP Residential Drug Treatment Program Development: 1988 to 1995 " — available upon request. A narrative description summarizing the selection of the research sites is contained in Chapter 4 under the section entitled "A Chronological History of the Selection of Subjects."

Figure 3
Chronological History of DAP's and DAP Research Sites

DAP Research Site	FY Approved for Activation	Date Selected as Research Site	Date Discontinued as Research Site
Butner FCI	1990	03/90	06/96
Fairton FCI	1990	03/90	06/96
Lexington FCI*	1989	03/90	02/94
Oxford FCI	1990	03/90	06/96
Rochester FCI	1990	03/90	08/91
Seagoville FCI	1990	03/90	06/96[22]
Sheridan FCI	1990	03/90	06/96
Tallahassee FCI	1990	03/90	06/96
Danbury FCI	1991	03/93	06/96
Dublin FCI*	1991	08/93	06/96
El Reno FCI	1991	not selected	not selected
Englewood FCI	1991	not selected	not selected
Leavenworth USP	1991	not selected	not selected
Marianna FCI	1991	02/92	06/96
Phoenix FCI	1991	03/93	08/93
Bastrop FCI	1992	not selected	not selected
La Tuna FCI	1992	03/93	02/93
Bryan FPC*	1992	not selected	not selected
Atlanta USP	1992	not selected	not selected
Lompoc USP	1992	not selected	not selected
Lompoc FCI	1992	03/93	06/96
Allenwood FPC	1992	not selected	not selected
McKean FCI	1992	03/93	06/96
Alderson FPC*	1992	03/93	06/96
Morgantown FCI	1992	03/93	06/96
Yankton FPC	1992	03/93	06/96
Terminal Island FCI	1992	03/93	06/96
Terre Haute USP	1992	not selected	not selected
Three Rivers FCI	1992	03/93	06/96
Talladega FCI	1992	not selected	not selected
Fort Worth FCI	1994	not selected	not selected
Dublin FPC*	1995	not selected	not selected
Sheridan FPC	1995	not selected	not selected
Fort Dix FCI	1995	not selected	not selected
Cumberland FPC	1996	not selected	not selected
Talladega FPC	1996	not selected	not selected
Texarkana FPC	1996	not selected	not selected
Florence FCI	1996	not selected	not selected
Milan FCI	1996	not selected	not selected

Notes: sites having female inmates are denoted by an asterisk (*). Also, Tallahassee and Danbury converted to all-female institutions in 1995, and Lexington converted to all-male in 1994.

[22] As will be noted in Chapter 4, this site was dropped between 1991 and 1993.

- the inmate did not have a State or Immigration and Naturalization Service (INS) detainer or pending charges, and the inmate qualified for Community Corrections Center placement where transitional services would be provided;
- the inmate was willing to sign an agreement to participate in the Residential Drug Abuse Treatment Program; and
- the inmate successfully completed the Drug Abuse Education Program (described below).[23]

After several programs had admitted cohorts of inmates to drug treatment, the admission criteria were reviewed and modified in a number of ways. Inmates with detainers, State boarders, and inmates ineligible for Community Corrections Center placement became eligible to participate in residential Drug Abuse Treatment Programs, as did inmates who spoke Spanish (as more bilingual staff became available).

In October 1993, new BOP policy dictated a further modification in the admission criteria. Individuals now had to meet Diagnostic and Statistical Manual of Mental Disorder (DSM-III-R — American Psychiatric Association, 1987) criteria for substance abuse or dependence.

After the passage of the 1994 Violent Crime Control and Law Enforcement Act (VCCLEA), drug program policy required a number of changes as the VCCLEA made demands on the BOP's residential Drug Abuse Treatment Programs. The VCCLEA provided the Bureau with an incentive for inmate participation: the BOP Director was allowed to provide up to a 1-year sentence reduction for non-violent inmates who successfully completed a residential Drug Abuse Treatment Program. In addition, the VCCLEA required that by the end of FY 1997 the Bureau provide residential drug abuse treatment for all inmates who were "eligible."

In May 1995, the BOP revised its policy in accordance with the VCCLEA. Additional admission criteria required inmates to have a *verifiable, documented*[24] drug abuse problem. This criterion was established to prevent inmates who did not have drug problems from volunteering for drug treatment solely to obtain early release from prison. In addition, while inmates were always taken into the program with priority placement given to those with the least amount of time to serve, the time frame was generally limited to 36 months before release to account for a potential 1-year reduction in custody.

The policy statement issued in May 1995 also implemented more specific criteria for program expulsion. While in the residential Drug Abuse Treatment Program, an inmate could be expelled if he or she was found to have used or possessed alcohol or drugs, exhibited violence or threatened violence against staff or another inmate, committed a serious rule infraction, or exhibited

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[23] Bureau of Prisons, Operations Memorandum 132-90 (5330), September 20, 1990. Inmate Drug Abuse Program.

[24] Self-reported drug use does not qualify as a verifiable, documented problem.

disruptive behavior related to the program. Much of the greater specificity in discharge criteria — especially those related to disruptive behavior in the program — was the direct result of VCCLEA. Drug treatment administrators believed it necessary to define clearly expulsion criteria because program expulsion was accompanied by loss of eligibility for an early release.

It should be noted that some institution residential Drug Abuse Treatment Program administrators did not always apply the admission criteria as dictated by policy. Clinical judgment used as one of the selection filters into treatment is a process that must be acknowledged by researchers. This process, which we referred to in the literature review as the administrative selection process, can affect the profile of individuals being admitted to the treatment program. The question is, "to what extent do clinicians reject individuals who meet the admission criteria?" Although there were no systematic data available to shed definitive light on this process, some information was available from the field notes of researchers located at six of the initial research sites. While there were some programs with twice as many applicants as admitted individuals, the primary reasons for rejection were because staff had made referrals for inmates who did not volunteer or because the inmates did not meet the admission criteria. However, there was evidence that staff occasionally would reject an applicant due to a lack of motivation, because the inmate was disliked by staff, or because the individual was considered a management problem.

Incentives for Program Participation

At the earliest implementation, there were no incentives for DAP participation. However, initially low numbers of DAP volunteers despite increased funding and scrutiny by external agencies and Congress led the BOP Executive Staff to approve residential Drug Abuse Treatment Program incentives in October 1991.

Financial achievement awards[25] were approved as a means of overcoming the "disincentive" of pay losses incurred by inmates who, by participating in treatment, were no longer able to work full-time. Achievement awards were dispensed quarterly and were based on program performance — no unexcused absences from program activities, a 95-percent promptness rate for all scheduled program activities, no guilty findings for disciplinary infractions, and successful completion of all program assignments (including readings, homework, and self-evaluations).

The second incentive approved by the Executive Staff was consideration for a full 6 months in a Community Corrections Center for all successful residential Drug Abuse Treatment Program graduates.

[25] We note that financial incentives can also be viewed as having imposed a contingency management situation into the treatment process. The effect of this particular aspect of the treatment process cannot be disentangled from other aspects of the treatment process.

The third incentive involved tangible and intangible benefits granted to treatment participants by local institution staff. Wardens received the latitude to offer such items as shirts, caps, and pens with program logos to program participants in good standing. Other local incentives included the assignment of participants to preferred living quarters and to units with washer/dryer access, special recreation privileges, and special dining privileges.

The incentives for drug treatment changed with the passage of the Violent Crime Control and Law Enforcement Act. This law allowed eligible inmates who successfully completed the Bureau's residential Drug Abuse Treatment Program to earn as much as a 1-year reduction from their statutory release dates (the qualification for early release was limited to inmates who had not committed a "crime of violence").[26] Successful completion of drug treatment was defined as completion of all phases of the drug treatment program — the residential program, the institutional aftercare program (when applicable), and the transitional services component received while housed in a Community Corrections facility (described below).

A final change in DAP incentives came in 1995, with the discontinuation of tangible incentives for residential Drug Abuse Treatment Program participants. This was modified as part of an overall BOP policy to reduce the quantity of inmates' personal belongings.

Program Design and Content

In-Prison Residential Drug Abuse Treatment Program (DAP)

All residential DAP's are unit-based; that is, all residential Drug Abuse Treatment Program participants live together — separate from the general population — for the purpose of building a treatment community. Each unit has a capacity of approximately 100 inmates, based on a staff-to-inmate ratio of 1:12 or 1:24. Ordinarily, treatment is conducted on the unit for a half-day in two 2-hour sessions. During the other half day, inmates participate in typical institution activities (*e.g.*, work or school). During these times, as well as during meals, treatment participants interact with general population inmates.

Program specifications originally were geared toward the 9-month residential Drug Abuse Treatment Programs, with the 12-month programs following the same guidelines but adding increased flexibility in terms of hours not devoted to required program content. At the outset, the 9-month programs were to include 40 hours of comprehensive assessment and treatment-plan

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[26] This early release provision presents issues of disparity for Bureau inmates. The disparity arises when, for example, two inmates convicted of the same offense receive different prison terms because the inmate who has been diagnosed with a substance abuse problem receives a 1-year reduction in his or her sentence and the inmate without a substance abuse problem serves the entire sentence. In effect, many perceive this 1-year reduction as a reward for drug-abusing behavior.

development, 280 hours of group/individual counseling, 100 hours of wellness lifestyle training, and 40 hours of study devoted to transitional-living issues. The individual/group therapy focused primarily on behavioral-skill building, cognitive-skills development, family issues, vocational/educational issues, criminal-thinking confrontation, pro-social values development, and relapse prevention. The program also provided support groups and elective self-help groups.

Individualized treatment plans were required, based on assessments of the subjects' needs. Full-team reviews were scheduled every 90 days, with a treatment plan review every 30 days.[27] Urinalyses were to be conducted more frequently than was the case with the general population.

In July 1991, residential Drug Abuse Treatment Program content became standardized. All residential Drug Abuse Treatment Program coordinators were brought together, and they agreed that residential Drug Abuse Treatment Programs would include specific core components, including screening and assessment, treatment orientation, criminal-thinking confrontation, cognitive skill building, relapse prevention, interpersonal skill building, wellness, and transitional programming.

Together, these components accounted for 350 hours of programming, both in didactic and process groups. The remaining program hours were to be divided at the discretion of the individual coordinators.

In FY 1993, a workgroup chaired by the BOP national clinical coordinator developed the residential "Drug Abuse Treatment Program Handbook," standardizing 450 hours of the required 500 hours of treatment. These manuals were distributed during staff training conducted in the summer of 1994. As a result of this handbook — and modifications required by VCCLEA — the BOP policy, issued in May 1995, required post-testing of each (REPHRASE) module covered in the handbook.

Finally, due to the changing admission criteria, and because not all inmates were released to Community Corrections Centers or from custody shortly after completing the program,[28] an institutional transition program was established in 1992. The program originally required 25 hours of "refresher" treatment in the last 4 months prior to an inmate's release from the institution. However, in 1995 that policy changed and required each successful residential Drug Abuse

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[27] Full-team reviews include all members of the unit team (unit manager, case manager, and case counselor), as well as representatives from Education and Psychology Services. During these meetings, the following items are discussed: custody and security classification, work assignment and performance, leisure time activities, overall institutional adjustment, education and other program activities, plans for release, and Financial Responsibility Program involvement.

[28] Although priority was placed on admitting individuals near release from custody, individuals with time left to serve after program completion were initially admitted in order to fill the DAP treatment beds.

Treatment Program graduate to receive no less than one hour of individual or group counseling per month for the first 12 months out of the residential unit or until transfer to a Community Corrections Center or release, whichever came first.

Non-Residential Drug Abuse Treatment Services/Self-Help Groups

Although a few non-residential programs existed from the start, these programs were not defined clearly in drug treatment program policies. By June 1992, non-residential programs were better defined and it became mandatory to make these programs available in every BOP institution. This level of programming now provides individual and group counseling to inmates with substance abuse histories. Non-residential programs provide alcohol and other drug abuse treatment services to inmates who are not eligible or not interested in residential Drug Abuse Treatment Programs or who may have overriding mental health problems that preclude the inmate's full residential Drug Abuse Treatment Program participation. Non-residential drug abuse treatment also provides inmates with institutional transitional services. Self-help groups are available in all types of drug abuse treatment in the BOP, but they are most often associated with non-residential drug abuse treatment. However, self-help groups alone do not constitute non-residential drug abuse treatment as defined in BOP policy.

Drug Education Course

Drug Abuse Education is the only drug abuse program service that is mandated by BOP policy. Inmates are required to participate in this program if they meet any of the following criteria:

- there is evidence in the presentence investigation report (PSI) that alcohol or other drug use contributed to the commission of the offense for which the inmate is currently incarcerated;
- alcohol or other drug use was a reason for a violation of supervised release — including parole — or BOP community status (CCC placement) for which the inmate is currently incarcerated; or
- the inmate was recommended by the sentencing judge for drug programming during the current incarceration.

Participants in the 40-hour drug abuse education course receive information about alcohol and drugs, as well as the physical, social, and psychological impact of these substances. Participants must complete an assessment of their lives, including an accounting of the costs that their drug use has had on their health, on the lives of their families, and on the community.

Inmates required to take the Drug Abuse Education course who refuse, or who fail to complete the course successfully are remanded to the lowest pay-grade for the remainder of their incarceration and are ineligible for community programs. It should be noted, however, that inmates may also volunteer for this course.

Community Corrections Centers

Ordinarily, inmates are transferred to a Community Correction Center (*i.e.*, a "CCC," or halfway house) prior to their release to the community or release to supervision. CCC placements provide inmates with structured environments in which to find a job, reunite with their families, and receive vocational and behavioral counseling.

Approximately 9 months before an inmate's probable release date, BOP staff determine an inmate's eligibility for CCC placement. A recommendation for CCC placement is based on the inmate's needs for services, the consideration of public safety, and the proper management of the BOP inmate population as a whole. An inmate may be referred to a CCC for as many as 180 days, but the average length of stay for all inmates is approximately 4 ½ months.

An inmate will most likely be determined *ineligible* for a CCC placement if he or she meets any of the following conditions:

- is a deportable alien;
- is serving a sentence of less than 6 months;
- has pending charges or detainers;
- requires psychological or psychiatric treatment or inpatient care;
- refuses to participate in the Inmate Financial Responsibility Program;[29]
- is deemed an aggressive sex offender; or
- poses a significant threat to the community.

Home confinement is another community option available to the BOP. In cooperation with the Federal Corrections and Supervision Division (probation services) of the Administrative Office of the U.S. Courts (AO), some inmates may be allowed to be placed at home while remaining under BOP custody. Home confinement provides inmates with increasing responsibility while remaining under supervision. Inmates on home confinement status are allowed to work, but are required to stay at home during non-work hours of the day. Where available, electronic monitoring equipment is used to ensure compliance with these conditions. The length of home confinement placement is limited to the last 10 percent of an inmate's sentence or 6 months — whichever is less. Individuals receiving a CCC placement may spend some of their time in home confinement.

When an individual is arrested for a new offense or is found guilty of a serious disciplinary infraction, he or she may be sanctioned and transferred to a local jail or to a Federal correctional facility and thus not successfully complete his or her CCC placement.

[29] This program involves a system of deductions from an inmate's pay in order to meet the requirements of court-ordered fines (e.g., child support, restitution).

Transitional Services During Halfway House Placement

At the outset of program planning in 1989, transitional services were to consist of two phases. The first phase, pre-release services, would include 6 months in a CCC, with specialized programming provided either by a contractor or directly by BOP staff. The second phase — aftercare services — would consist of 6 months during which community services would be coordinated jointly by the BOP and the requisite U.S. Probation or Parole office, or provided directly by CCC staff if community resources were unavailable.

This initial plan was not implemented. Rather, in working closely with the AO's Federal Corrections and Supervision Division, in July 1992, a Memorandum of Understanding (MOU) was signed between the BOP and the AO concerning the provision of transitional care. The agreement specified that for individuals with CCC placements, a planning conference involving CCC contract staff, a community-based drug counselor, and the inmate would be held within the first week of the inmate's arrival at the CCC. Because most Probation offices had community-based treatment contracts for offenders under Federal supervision, the MOU allowed the BOP to use the same contractors — in other words, to "piggyback" on the probation services agreements. This "piggyback" effort ensured that inmates would continue to receive treatment services from the same providers as they moved from BOP custody to Probation supervision.

Transitional services generally include community-based treatment with philosophies similar to those of institution-based treatments.[30] Initially, the intensity of transitional services was to be standardized, with each individual receiving 4 hours of services per week during his or her stay at the Community Corrections Center. However, soon after implementation, the community-based treatment provider began to direct the individual's course of treatment and, typically, now Transitional Services inmates receive, on average, 2 hours of services each week.

At the outset, transitional services were granted only to graduates of the of the DAP. In early 1993, the range of inmates who could receive transitional services during CCC placement was expanded to include any inmate in a CCC who was identified as needing drug treatment, even if he or she had not participated in an in-prison residential drug treatment program. This expansion resulted, in part, from recruitment problems in the early residential Drug Abuse Treatment Programs, which left funding available for an expanded community-based treatment population.

Although some transitional services participants who had not been DAP participants received these services voluntarily, most did not. Most of these latter transitional services participants became involved as a result either of community corrections staff recommending treatment as a condition of the CCC placement or of two new community corrections programs initiatives implemented in 1994. The first initiative provided for the creation of Comprehensive Sanction Centers (CSC's), which were CCC's designed to offer more gradual and structured release

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[30] The Transitional Services treatment would thus build upon the core components of the residential in-prison treatment program and provide continuity of care.

experiences to individuals who might not be appropriate for the traditional CCC experience. All CSC residents were required to be screened for drug treatment needs and then referred, if appropriate, to Transitional Services. CSC's had been fully implemented at 12 different sites in the country by January 1994, and they housed 4 percent of all the Transitional Services participants.

The second initiative was called the Enhanced Transitional Services (ETS) project. Within each of the six BOP regions, at least one CCC facility (not already designated as a CSC) was selected as an ETS site. ETS sites were similar to CCC's but contained special provisions for transitional services. ETS participants were required to participate in community-based treatment *(i.e.*, transitional services) if they were identified by a BOP community corrections manager as having a substance abuse problem and were determined by a community-based treatment provider to need treatment. Four ETS programs were implemented between January and March 1994, and another four began between April and August 1994. In the beginning of 1994, 26 percent of all the Transitional Services participants were in ETS programs.

As of the end of 1996, with the implementation of the VCCLEA initiative, 62 percent of BOP inmates receiving transitional services during their CCC placement were residential Drug Abuse Treatment Program graduates.

Supervised Release

Approximately 80 percent of the TRIAD research subjects were released from BOP custody with provisions for supervision by a U.S. Probation officer. The system of selecting individuals who were to receive treatment services as part of post-release supervision varied among each of the 94 judicial districts. Individuals with an identified history of drug abuse may have been required to receive treatment services while under supervision. Individuals under the supervision of a U.S. Probation officer may have undergone urinalysis tests and have had drug treatment services provided under the "Contract Services Program Plan" when the services were required as a condition of supervision.[31] However, urine testing was required of most individuals flagged as having a drug problem.

Urine testing involves a combination of regularly scheduled collections (fixed-interval testing) and unscheduled collections (random testing). Many, but not all, of the Probation offices follow a phase program for urine testing, with the following three phases:

- Phase I — This phase involves six urinalysis (UA) collections monthly with at least two unscheduled collections. During this phase, the treatment contractor, when requested, should provide four 30-minute counseling (or alternative treatment) sessions each month. This phase usually lasts 6 months.

[31] In the event that the necessary services are not offered by one of the contract agencies, these services may be provided by a non-contract agency or the Probation officer.

- Phase II — When an individual completes 6 months of Phase I satisfactorily, he or she moves on to Phase II, where the urine collections are reduced to four per month, with at least two of these being unscheduled. During this phase, if treatment services are required, there are three counseling sessions of at least 30 minutes each month. This phase generally lasts 3 months.
- Phase III — This phase reduces the monthly urine collections to two unscheduled urine tests. Counseling sessions are reduced to two sessions of at least 30 minutes each month. Usually, the type of treatment provided by a contractor consists of either individual or group counseling. However, when necessary, intensive outpatient counseling, detoxification services, and residential services are provided.

Individuals who have positive urinalyses or violate other conditions of supervision, including being arrested for a new offense, may be revoked and transferred to a Federal prison or other correctional facility. Revocations are made at the discretion of the Probation officer and the judicial official presiding over a revocation hearing. Thus, in some districts an individual will be revoked for one positive urinalysis whereas in other districts an individual may have several positive urinalyses before being revoked. There are however, a few acts, such as possession of a firearm and possession of a controlled substance, that call for mandatory revocation.

Summary

In summary, it is clear that throughout the TRIAD drug treatment evaluation effort various components of the BOP's drug treatment programs underwent changes. The number of programs grew from 8 in fiscal years 1989 and 1990 to 39 in fiscal year 1996. The incentives for program participation changed from financial incentives to offset the loss of pay resulting from program participation to a 1-year reduction in sentence for successful program completion. Admission criteria became more stringent, eventually requiring an official DSM-III-R diagnosis of drug abuse or dependence that was verifiable and documented. Program content became more standardized over time.

Transitional services provided during a CCC placement were initially limited to individuals who had completed the in-prison residential drug treatment program. Later these services became available to other drug-abusing individuals. Furthermore, several new community corrections initiatives mandated such services for individuals with histories of drug use.

Some of the program changes did not affect the research design for the TRIAD evaluation. However, the rapid growth in programs did notably affect the research design. Chapter 4 provides a description of the research design and how it was affected by the program changes.

CHAPTER 4: RESEARCH DESIGN

The primary purpose of the TRIAD project was to conduct a multi-site evaluation of the effectiveness of residential drug abuse treatment, including in-prison treatment and its extension to post-release treatment. We used a quasi-experimental design in which inmates with histories of drug use volunteered to participate in treatment designed around the therapeutic community concept. These subjects composed the treatment groups. Comparison subjects were drawn from research volunteers at DAP and non-DAP sites.

The study was prospective in that inmates were identified at the beginning of their drug treatment, or, if they were comparison subjects, at a point within a year prior to their releases. Once an inmate was identified as a treatment subject, he or she was included in the study regardless of his or her ultimate disposition. Thus, inmates who dropped out of the treatment program, were terminated, or failed in a halfway house were still included in the treatment groups.

The longitudinal design calls for measurement of background and intervening variables. For those in treatment, some of these variables were measured prior to and after treatment. In the final report, post-release outcomes will include results of measures carried out, for each inmate, during a period of 3 years following his or her release from custody. In this interim report, however, we cite results for those treatment and comparison inmates who had been released to the community for at least 6 months.

Sample Selection Process

At the outset of this project, treatment volunteers were intended to be assigned randomly to either a treatment or comparison group, thus circumventing problems with selection bias. Once the treatment programs began, however, we realized the infeasibility of such an implementation. First, there were insufficient numbers of treatment volunteers, which resulted in *all* volunteers being given treatment slots. This situation wouldn't work for us because random assignment requires that there be more volunteers than there are available treatment slots. In addition, treatment staff exerted tremendous pressure to control the treatment assignment process, making it impossible for researchers to exercise that same control. Thus, the TRIAD project had to adopt a quasi-experimental design to address the issue of selection bias.

As we discussed in the section on "Selection Bias and the Evaluation of Prison Drug Treatment Programs" (*see* Chapter 2), we used two approaches to minimize selection bias and to test the effect of treatment tainted by selection pressures. Our first approach combined all treatment outcomes, regardless of whether inmates completed treatment, and is referred to as the Bloom approach. Our second approach, which was implemented by William Rhodes, an econometrician at Abt Associates, modeled selection bias and tested for treatment effects following statistical procedures outlined by Heckman (1979) and Maddala (1983).

In order to test explicitly for selection bias effects, some comparison subjects were selected from sites in which treatment was available. Theoretically, if selection pressures compel more motivated volunteers to participate in drug treatment programs, this would diminish the number of motivated clients remaining in the comparison pool from DAP sites. Under this assumption, the comparison subjects drawn from DAP sites should have been less motivated than were treatment participants, and perhaps they would have had characteristics associated with a higher risk of recidivism.

It also is possible that selection pressures, such as external incentives, compelled less motivated inmates to participate in drug treatment programs. Under this assumption, comparison subjects drawn from DAP sites should have been more motivated than were their treatment counterparts and may have had background characteristics associated with a lower risk of recidivism.

It is important to note that comparison subjects drawn from DAP sites (*i.e.*, sites with residential treatment programs) have some probability of volunteering for treatment even if that probability is extremely low. However, control subjects drawn from sites where no treatment was available, non-DAP sites, have a zero probability of volunteering for treatment.

William Rhodes, in his presentation of the model (*see* Appendix C) refers to the following types of subjects: DAP treatment subjects (those who volunteered for and enrolled in treatment); DAP comparison subjects (those who were offered treatment but declined); and non-DAP control subjects (those whom were never incarcerated in a facility that sponsored treatment programs). Throughout our report, we refer to these groups as DAP treatment groups, DAP comparisons, and non-DAP controls, respectively. Dr. Rhodes used information we provided about the probability that an inmate will receive treatment to estimate a latent variable representing the factors that determine whether an inmate will volunteer for treatment. He then incorporated this information into a model that measures treatment effects in the presence of selection bias pressures. Thus, he was able to use information simultaneously from the DAP treatment groups, DAP comparison group, and non-DAP control group in measuring treatment effects.

Our first approach to control for selection bias — the Bloom model —did not test for selection bias explicitly. In using this approach, for which we combined all treatment groups, a question arose as to how to treat the DAP comparison subjects when we contrasted the combined treatment outcomes with the non-DAP control subjects. As we argued in the section on "Selection Bias and the Evaluation of Prison Drug Treatment Programs" in Chapter 2, DAP comparison subjects should have been combined with DAP treatment subjects and this combination should have been contrasted with the non-DAP control group. Our reasoning was that the DAP comparison group was composed of inmates who — theoretically — would have declined treatment if it were offered and that these inmates should have been represented proportionally in our non-DAP control group.

Because our samples were convenience samples and were not drawn with an explicit plan to reproduce proportionality, we estimated the extent to which DAP comparisons were under- or

over-represented relative to their hypothetical proportions in the non-DAP control group. Our best estimate suggested that we over-sampled DAP comparisons. Furthermore, other information indicated that the probability of volunteering varied over time and by site. Thus, in our analysis, we weighted the DAP comparison sample to approximate proportionality with the volunteering rate for both the period during which the subject was selected and the site at which the subject was housed.

If treatment was having an effect, one would expect that the *combined DAP group* average outcomes would have been significantly better than the non-DAP control group average outcomes. Most studies in this domain have looked at the average outcomes of inmates who have completed drug treatment. The authors of this paper have argued that this approach does not allow us to disentangle the effects of a selection process from the effects of treatment.[32]

Research Subjects

This report on 6-month post-release outcomes describes results concerning only those subjects who were released from BOP custody as of December 31, 1995. This includes approximately two-thirds of the total number of research subjects in the overall study, as the remaining one-third had release dates after December 31, 1995.[33]

The report is based on outcomes for 1,866 individuals (899 treatment subjects, plus 530 comparison subjects at DAP sites and 437 control subjects at non-DAP sites) to whom, at the very least, one of two interviews was administered.[34] Results concerning research subjects, both treatment as well as DAP comparison and non-DAP control, for whom these interview data were not collected are not included in this report. The background information from these interviews was crucial to the analysis of outcomes. A detailed assessment of whether the individuals included in the report are different from those not included in the report, as well as an assessment of other possible biases resulting from subject attrition, is contained in a subsequent chapter entitled "Subject Attrition" (*see* Chapter 5).

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[32] For demonstration purposes, this paper also will present analyses that depict group outcomes for those who completed programs separately from those who withdrew or were terminated. This will allow us to compare our results to those of past studies.

[33] There are approximately 1,000 additional research subjects for whom we have comprehensive data but who are not included in this preliminary report because their release dates are after December 31, 1995. These subjects will be included in future reports. Please note, however, that some of these subjects will not be available for follow-up because they have INS or State detainers.

[34]This interview — the Intake 1 interview — collects a wide range of background information on the subjects.

Individuals who had Immigration and Naturalization Service (INS) detainers or State detainers who had not been released from custody are excluded from this report. There were 209 such subjects for whom interview data were collected. Of these, 110 subjects were INS detainees (89 men and 21 women). Although some INS detainees are released to the streets, it is difficult to assess consistently whether these subjects were deported or not. Most of these 110 INS-related subjects will be followed in the future, as they are expected to be released before the end of the 3-year follow up period. The other 96 (86 men and 10 women) went directly from BOP custody to another form of incarceration. It must be noted that admission criteria had specified that INS detainees and State detainees were not to be admitted to DAP's.

The following section describes the history of site selection and the logistical problems encountered in the study.

A Chronological History of the TRIAD Subject Selection Process

Treatment Subjects

Eight sites were originally selected for the study — three 12-month programs (at FCI's Butner and Tallahassee and FMC Lexington) and five 9-month programs (at FCI's Fairton, Oxford, Seagoville, and Sheridan, plus FMC Rochester). Data collection for the three 12-month programs began with cohorts admitted after August 1, 1991, and for four 9-month programs with cohorts admitted after October 1, 1991.

FMC Rochester was dropped as a research site in the summer of 1991 even before data collection started, and this was done because the program model least resembled the others. After a site visit to FCI Seagoville in November 1991, the decision was made to drop this site as well. That program had accepted many non-English speaking inmates and had developed two separate programs, one in English and one in Spanish, and many of the Spanish-speaking inmates had detainers. Because other programs were scheduled to be implemented in early 1992, another program was to be selected as a replacement.

FCI Marianna was chosen in February 1992 because its first admission cohort, compared to the cohorts at the other new sites, had a greater percentage of inmates who were within several years of release. Programs admitting inmates with higher averages of time before release would only serve to delay follow-up data collection.

By the early fall of 1992, preliminary estimates of the numbers of research subjects to be available for follow-up within several years fell short of expectations. This paucity resulted from a decreasing percentage of new admissions who were within 2 to 3 years of release dates and to the fact that two of the research sites had temporary delays in new admissions resulting from an insufficient number of available drug treatment staff. Approximately half of the admissions between September 1991 and March 1992 at the seven research sites were within 3 years of their

release dates at time of admission. This percentage decreased to approximately 27 percent for admissions between October 1992 and March 1993. At this time NIDA requested a revised research plan to accommodate this unexpected development.[35]

The revised plan increased the number of treatment subjects near release by calling for the selection of additional research sites. As of March 1993, there were 30 BOP residential drug treatment programs nationwide, including the 7 original research sites. Residential Drug Abuse Treatment Programs at 11 sites were eliminated from consideration because they (1) were at maximum-security institutions with very low percentages of inmates near release, or (2) served Cuban inmates who were INS detainees, (3) were not fully operational, or (4) were of a 4-month duration. The remaining 12 programs not already in the study were selected as research sites (these included the programs at FCI's Danbury, LaTuna, Lompoc, McKean, Morgantown, Phoenix, Seagoville,[36] Terminal Island, and Three Rivers; FPC's Yankton and Alderson; and FMC Rochester). Three of these sites were minimum-security sites (FCI Morganton and FPC's Alderson and Morgantown), unlike the original study sites. Two of the 12 newly added research sites housed female inmates (FPC Alderson and FCI Danbury), while only one of the original seven study sites housed women.

To ensure a sufficiently large sample available for follow-up in the not-too-distant future, data collection was limited to those individuals expected to be released from BOP custody by the end of FY 1996. Data collection at 11 of the 12 additional sites began in April 1993. The twelfth site (FCI Danbury) was not expected to be operational until January 1994, due to its transition from a male-only to a female-only facility. Following a prison disturbance in the summer of 1993 at FCI Phoenix — a site housing male inmates — this site was dropped and replaced with FCI Dublin (a female institution in California). Previously, FCI Dublin had not been selected because it was not fully operational.

After a review of notes about trips to various sites and of quality control reports in February 1993, the decision was made to drop FCI La Tuna as a treatment research site. Much of the DAP program at FCI LaTuna was conducted in Spanish (meaning that a high percentage of program participants were not English-speaking) and many of the participants had INS detainers. In February 1994, FMC Lexington was dropped as a research site because it was beginning the process of converting to a male-only facility.

Residential drug treatment subjects were followed after release from custody irrespective of program status upon discharge. Individuals not completing the program received the following discharge classifications: disciplinary discharges, dropouts, and incompletes (due to transfers,

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[35]Although priority was given to individuals close to release, the number of treatment volunteers close to release was initially too small to fill all available treatment slots.

[36] Please note that FCI Seagoville had a sufficient number of English-speaking inmates, unlike the circumstances at the time this site was dropped as a research site in 1991.

releases to halfway houses, or releases from BOP custody). Identification of treatment subjects from program admissions ended in the summer of 1995.

Non-Treatment Subjects

As noted above, the research design was intended to be experimental in nature. Inmates who had volunteered for treatment would be randomly assigned by research staff to either the 12-month intensive residential program or to the "control group." This control group would be composed of various comparison groups, and these subjects could choose to volunteer for a 9-month, moderate-intensity residential program, making them essentially comparison subjects exposed to lower levels of treatment. Inmates who did not opt for the 9-month program would compose a second comparison group that received no residential treatment but could have received treatment of a very low intensity. The low-intensity treatment consisted of in-prison outpatient counseling services or treatment services while in a CCC placement. All these groups (*i.e.*, the two control groups and the above-mentioned treatment group) together would provide one primary set of comparisons, that between subjects randomly assigned to a 12-month residential program and those who volunteered for this treatment but instead received lower-intensity or no treatment.

Inaccurate case flow estimates proved to be the most important reason for not implementing a randomized design within the BOP. The status of the BOP's drug abuse treatment programs in the summer of 1991 indicated that we would not have an excess of volunteers. This was due both to the BOP's rapid expansion of drug treatment programs and to the fact that program expansion was not limited to one geographical region. This program expansion thus was able to provide treatment to most individuals who desired it, which prevented the creation of waiting lists to be used for random assignment procedures**.** In fact, keeping bed capacity filled required the admission of inmates who, contrary to the initial admission criteria, had more than 3 years left to serve before being released. In addition, research sites scattered nationwide created significant logistical problems for implementing a randomized design.

Therefore, two non-treated groups were selected. The first group consisted of individuals at a DAP site who did not volunteer for treatment (*i.e.*, DAP comparison subjects), and the second group consisted of individuals who did not have the opportunity to volunteer for DAP because they were housed in institutions that did not offer DAP (*i.e.*, non-DAP control subjects).

We recognized that the simple fact of being housed at an institution without a DAP did not provide sufficient rationale to conclude that such individuals did not have the opportunity to volunteer for treatment, because BOP policy did not preclude anyone from transferring to a DAP site and then volunteering for treatment. Therefore, we could not yet establish definitively that treatment was not available to individuals housed at non-DAP institutions. However, an assessment of transfer rates provided evidence that treatment rarely occurred for individuals from the non-DAP institutions. In December 1994, an analysis was undertaken to assess the extent to which those receiving DAP residential treatment had been transferred from an institution without a DAP. Of all the DAP participants to that date — not just the research subjects — only 4.6

percent had transferred within 90 days prior to being admitted to a DAP. Anecdotal information showed that at a few select DAP sites there was some effort to recruit individuals from other institutions. However, the extent of this recruitment was minimal, as substantiated by this analysis of transfers. We thus felt satisfied in concluding that individuals from non-DAP sites did not have treatment available.

Toward the end of the process of identifying the non-DAP controls, passage of the 1994 Violent Crime Control and Law Enforcement Act (VCCLEA) created an opportunity for inmates to receive a one-year sentence reduction with successful completion of a drug treatment program. Thus, VCCLEA increased the likelihood that inmates from non-DAP sites would request transfers in order to participate in treatment. Although non-DAP controls were selected after passage of VCCLEA, those selected were too near to release to qualify for its early release provision. For women, it was very difficult to identify non-DAP control subjects, as there were relatively few female-only prisons, and most of them, over time, had implemented a residential DAP.

The first group of non-treated subjects comprised we selected was composed of DAP comparisons. All non-treated subjects — both DAP comparison subjects and non-DAP control subjects — were individuals who, according to their self-reporting, were regular users of drugs. Regular users were defined as those subjects ever having used an illicit drug at least once per week for at least one month or ever having used alcohol daily for at least one month. This would approximate meeting the minimal criteria for admission to a DAP. Other admission criteria could not easily be assessed through readily available data sources. In addition, some of these other admission criteria — such as not having a detainer — were not followed consistently.[37] The screening for drug use was accomplished through the administration of a questionnaire, the History of Drug Use (HDU) survey, developed for this purpose.

The first attempt to identify non-treated individuals who would have been eligible for treatment focused upon the DAP comparisons and used matching procedures. After identifying individuals eligible for drug treatment according to the HDU survey, a sample was to be selected through prospective matching to the cohort of residential drug treatment research subjects. The matching criteria were to include sentence length, age, race, individual security level,[38] and the severity of drug use. This matching process proved ineffective in identifying subjects in time to plan a data collection trip before individuals were released to halfway houses or released from BOP custody. Mainly for this reason, the matching procedure was abandoned after only one set of selections, which consisted of 124 subjects.

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[37] This will be noted later in discussing the subjects not available for follow-up data collection.

[38] This serves as a proxy for criminal justice history because security level is determined by information about the current offense(s) and the history of previous offenses.

Subsequent selection of non-treated subjects, both DAP comparison and non-DAP controls, followed the same procedure. We identified individuals within 6 to 15 months of release who had not volunteered for DAP and for whom it was too late to volunteer. We attempted to administer the HDU to all of these individuals. Any individual who self-reported regular drug use became a potential non-treated comparison subject or a non-DAP control subject and was approached to participate in research.[39]

The institution from which an individual was selected did not serve as the sole determining factor in whether the subject was classified as a DAP comparison or non-DAP control subject. Individuals identified at DAP sites might have arrived just prior to release (*i.e.*, they were transferred to these sites because they were the institutions closest to their release destinations) and thus did not have time to volunteer. On the other hand, individuals selected at non-DAP institutions might have been there only a few months prior to release but had spent most of their previous few years at DAP institutions. Thus, classifying the type of comparison subject was accomplished through looking at each subject's admission and release history and determining whether the individual had been at a DAP institution at a time when a program was available and with sufficient time left to serve to volunteer and complete the DAP.

Data Collection Instruments

The data collection instruments were selected by replicating measures used in previous and current drug treatment evaluations — choosing measures that in previous recidivism and treatment evaluation research had been shown to be related to either treatment outcomes or recidivism, and selecting measures that test some of the theoretical assumptions underlying the drug treatment programs. Many of these measures were used as statistical controls to ensure that possible differences in group composition did not account for differences in the outcome variables.

The measures collected for the study can be summarized as follows:

- Pre-incarceration background data — family background, employment and educational history, drug and alcohol use and treatment history, mental health treatment history, illegal activities, and incarceration and arrest histories.
- Psychological/cognitive measures — motivation and expectations about treatment, Change Assessment Scale (Prochaska and DiClemente, 1986) (a survey of motivation for change), DSM-III-R diagnoses of depression and antisocial personality, Attributional Style Questionnaire (ASQ), Drug-Taking Confidence Questionnaire (DTCQ) (Annis and Martin, 1985a), Inventory of Drug-Taking Situations IDTS) (Annis and Martin, 1985b), Ways of Coping Checklist (Lazarus and Folkman, 1984), and Hope Scale (Snyder, et al., 1991).

[39] Details concerning subject attrition are presented in Chapter 5.

- Treatment structure and process — Drug Program Description Checklist (a staff survey), observations of group sessions and staff meetings (at a limited number of research sites), inmate perceptions of staff empathy and program environment, and length and type of services received.
- Proximal outcomes — institutional adjustment using indicators such as disciplinary actions and positive urine results, changes in pre- and post-treatment measures on Change Assessment Scale, Ways of Coping Checklist, and Drug-Taking Confidence Questionnaire.
- Post-release environment — indicators of poverty and employment rates from census data.

Data Collection Procedures

In-Prison Data

Inmates participating in DAP's were approached by researchers, who explained the project and administered surveys and interviews to those inmates who signed the requisite informed consent statement.[40] The set of pre-treatment surveys was administered within 6 weeks before or after admission to the DAP. The post-treatment surveys were administered within 4 weeks before or after program completion or termination. In addition to the surveys, two personal interviews were administered. While the two interviews — Intake1, with background information, and Intake2, with diagnoses of antisocial personality and depression — had no specified time frame for administration, they generally were administered within several weeks of the pre-treatment surveys. The surveys and interviews generally were administered within the same week for non-treatment subjects, with administration occurring as soon as possible after identification of the subject in order to ensure that the subject would still be in prison (these subjects were selected close to their release dates). At times, this was infeasible due to the large number of research sites and the limited number of researchers.

To encourage inmate participation in the evaluation project, the BOP Executive Staff issued a memo in March 1992 informing wardens that inmates participating in the TRIAD evaluation project were not to lose their performance pay[41] or UNICOR pay while participating in surveys and interviews.

Data on services received were obtained from both treatment staff and automated databases. Staff perceptions about the programs were obtained from three annual staff surveys — 1993 through

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[40] Refusal rates are reported in Chapter 5.

[41] Performance pay refers to the minimal salaries inmates receive for work performed on assigned work details. UNICOR refers to Federal Prison Industries which provides work details at Federal prisons. .

1995 — administered to all DAP staff at the research sites. Supplementary background data and information on the subjects' current incarcerations were extracted from the automated SENTRY database.[42]

Community Corrections Center (CCC) Data

Information on employment and educational activities, urinalysis testing and results, and participation in self-help groups during CCC placement was obtained from surveys mailed to the contract CCC staff. The information on transitional services received was obtained from the transitional services managers and automated databases. Other information about the length of the CCC placement, disciplinary infractions, and successful completion was obtained from the BOP's automated SENTRY database.

Post-Release Data

For those subjects released to supervision, information was obtained through phone calls with Probation officers at three points in time after release: 6 months, 18 months, and 3 years (or completion of supervision at any point). The Probation officers provided information on employment, educational activities, violations of conditions of supervision, urine testing frequencies and results, the numbers and types of supervisory contacts, arrests and incarceration, treatments received, and living situations.

Arrest data were obtained from National Crime Information Center (NCIC) databases for non-supervised subjects. NCIC is the FBI's computerized record system that holds arrest and conviction information about Federal — and most State — crimes. These data were obtained also for the time between end-of-supervision and 6 months after release for those subjects who completed supervision in advance of their 6-month follow-up dates.

Description of Measures

Measurement indicators in this report reflect those items known to be associated with treatment outcome or recidivism and items we consider to be important control variables that have not been examined in previous studies. A subset of background and treatment measures from among those collected are included in this report.[43] The following identifies and defines, where necessary, the measures selected for use in our analyses.

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[42] SENTRY, the BOP's automated database, provides comprehensive information on currently and formerly incarcerated inmates.

[43] The subset of items included in this report exclude the use of several surveys. These surveys will be used in the future to address additional research questions. These research questions are discussed at the conclusion of this report.

Background Characteristics

The BOP's automated SENTRY database provides information on several background characteristics, including sex, race, ethnicity, prior commitments, criminal justice status at time of incarceration, history of violence, sentence length, and age upon release from incarceration.

The self-report data obtained from two interviews administered to research subjects provide other pre-incarceration information. This information includes employment status during the month before incarceration, educational level, drug use, drug and alcohol treatment history, and mental health treatment history.

Psychological/Attitudinal Measures

The Diagnostic Interview Schedule (DIS) interview using the DSM-III-R criteria (American Psychiatric Association, 1987) provides the diagnoses of depression and antisocial personality. Although the DIS interview for obtaining measures of drug and alcohol dependence was administered to 706 treatment subjects, these measures had to be imputed for the remaining subjects, and this was done in several steps.

First, using the subsample of 706 inmates interviewed with the DIS interview schedule, other variables from the drug and alcohol sections of the Intake1 interview known to be correlated with dependence were selected to serve as proxies of drug and alcohol dependence. Second, logistic regression was used to regress the log odds of dependence on these other drug-related variables for the drug dependence measure and on a set of alcohol-related variables for the alcohol-dependence measure.[44] As a result of these logistic regressions, two equations for the estimated log odds of drug and alcohol dependence were generated. These equations were applied to the larger data set, which included data on inmates for whom the original DSM-III-R diagnosis was unavailable. This allowed an estimated log odds of drug and alcohol dependence to be generated for all subjects in the data set, with a few exceptions for cases with missing data.

Because our interest was more in the accuracy of prediction and less in *theory*, the predictive efficacy of the models was relatively more important than was the fit of the models. Nonetheless, the fit of both models was quite acceptable. For both alcohol and drug dependence, the Hosmer-Lemeshow measure of goodness of fit did not suggest any problems with the models. Looking at indicators of predictive accuracy, the concordance value was 93.6 percent for alcohol dependence and 92.2 percent for drug dependence. Both levels of concordance were very high.

The attitudinal measure — the Change Assessment Scale — replicates Prochaska's 32-item survey (Prochaska and DiClemente, 1986). This scale was used to measure the individual's level of recognition of a problem and motivation to do something to change the problem. It was

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[44] Logistic regression is an appropriate analytic method when the dependent variable is binary (Menard, 1995).

selected for this analysis due to its possible association with volunteerism and its previously demonstrated relationship to treatment retention. Furthermore, it represented the dynamic factors investigators are beginning to use to understand better the treatment process.

Confirmatory factor analyses were done to verify the four factors identified by Prochaska. These four factors — each composed of eight items — include:

- Precontemplation — when the individual is unaware of his or her problem.
- Contemplation — when the individual is aware that a problem exists and is contemplating taking some action.
- Action — when the individual has not only considered taking action, but is taking steps to remedy the problem.
- Maintenance — when an individual who has taken action works to maintain the gains attained during the action phase and thus prevent relapse.

These factors were verified through confirmatory factor analytic procedures conducted both with the exclusion of cases with missing items and with the inclusion of all cases, using mean score substitution for missing items. Values on all four factor scales are needed to obtain accurately the "stage of change" for an individual. Cluster analytic procedures were used to classify individuals into their appropriate stages of change based upon their profiles of scores across all four factors. Standardized scores with a mean of 50 and standard deviation of 10 for the four scales (with missing items estimated using mean scores) were cluster-analyzed using Ward's minimum-variance method (Ward, 1963). The six-cluster solution that was most interpretable closely resembled the six clusters identified by Tsoh (1995).

The six clusters, which bear some resemblance to the four factors listed above, can be briefly characterized as follows:[45]

- Uninvolved — the individual does not endorse any of the four scales and can best be described as both denying having a problem and not attempting to change his or her behavior to address the problem.
- Precontemplation — the individual does not recognize the existence of a problem.
- Reluctant — the individual recognizes the problem and is considering taking some action but does not take any action.
- Contemplation — the individual is considering changing and recognizes a problem but has not yet actively addressed the problem.
- Preparation — the individual has made a decision to start changing and has actively started to make changes but has not yet recognized the possibility of relapsing.
- Action — the individual is actively engaged in changing his or her behavior and has started working toward maintaining the change and avoiding relapse.

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[45] While the cluster titles resemble the titles of the factors, they are not synonymous. Each cluster is represented by a unique profile of scores across the four factors.

Treatment Received and Post-Release Supervision

For those receiving in-prison DAP treatment, the time in treatment is recorded as the combined amount of time across all episodes of DAP enrollment, and the type of discharge is recorded as discharge from the last episode. Very few individuals had enrolled in DAP more than once. Additional in-prison treatment, such as enrollment in an outpatient treatment program or in a self-help group, also was recorded.[46]

In general, in-prison drug treatment is offered only to individuals nearing release; treatment providers feel that treatment effects will be eroded by a lengthy exposure to prison culture after treatment. Because a significant number of individuals were not released from prison immediately following treatment, a measure of the time between program completion and release was calculated. This measure assesses whether the effects of treatment diminish with a lengthier stay in prison after treatment. The time between a program's initial startup and the admission of an individual to that program provides an indicator of program stability. Many research subjects were in the first or second cohort of admissions to their programs.

Involvement in transitional services during halfway house placement was recorded for all research subjects — both those who received DAP treatment and those who did not. "Post-release treatment" status depended on whether an individual received treatment required by the Probation officer (contract services) or sought treatment at his or her own initiative. Information on self-help group involvement was recorded as well.

Differing levels of supervision affected outcomes, as some supervised individuals were monitored more closely than were others. The differing types of supervision mentioned in this report break down into the following categories: those who received a halfway house placement, those who were supervised by a Probation officer after release, those who received urinalysis testing while under supervision, and those who were placed in a halfway house by a Probation officer during supervised release.

Post-Release Behaviors

The post-release behaviors and conditions of living included in our analyses consisted of (1) living situation (*e.g.,* was individual living with a spouse), (2) employment status, and (3) adherence to conditions of supervision. Those who violated supervision conditions were divided into two categories: those whose violations related to drug or alcohol use, and those committing other types of violations. The use of some of these behaviors in outcome analyses varied with the type

[46] Individuals who dropped out of a program or were disciplinarily discharged could later reapply for admission to DAP. In addition, some individuals who successfully completed DAP applied for readmission at the same site or another site to which they were transferred and thus completed DAP twice. Almost five percent of the DAP treatment subjects had more than one episode of treatment.

of outcome being examined. For example, employment was one of our outcome measures but it was used as an explanatory variable when examining drug use or arrests as an outcome measure.

Probability-of-Volunteering Coefficient

A probability-of-volunteering coefficient was developed to assist in controlling for selection bias.[47] This coefficient was calculated for each research subject who was at a DAP institution, regardless of whether he or she actually entered treatment.

The first step in the process was to calculate a probability-of-volunteering coefficient for each *institution*. Because of the differing security levels of the institutions in which our research subjects were housed, we presumed that the rate of volunteering would differ among institutions. Creation of this institution coefficient required obtaining estimates of the percentage of the population eligible for treatment and the percentage actually volunteering. The percent of the population eligible serves as the denominator of the coefficient, and the percentage actually volunteering serves as the numerator.

The percentage eligible was estimated using the 1991 Inmate Survey Data. This survey contained questions on drug use analogous to those contained in the History of Drug Use (HDU) questionnaire used to screen DAP comparison and non-DAP control subjects. Some of the research DAP sites were not included in the survey (or had changed security level or gender of population housed), so for some of these sites we used data from the 1996 administrations of the History of Drug Use (HDU) survey, with a downward adjustment of 12.5 percent.[48] For several additional sites where neither the 1991 Inmate Survey Data nor 1996 HDU data were available, the percent eligible was estimated using the average for the other sites of a similar security level. When the 1997 Inmate Survey data become available, the calculation of this coefficient will be reviewed by comparing these results to the 1991 results used in calculations for this report.

Given the expectation that the probability of volunteering had changed over time with the passage of the 1994 Violent Crime Control and Law Enforcement Act, which allows for early release upon successful completion of a residential DAP, estimates were created for eight points in time. The estimates were made for the beginning of each quarter for FY's 1994 and 1995. Calculations for earlier times were not made, due to incomplete and unreliable information concerning volunteering rates.

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[47] The discussion of the Heckman and Maddala approach to modeling outcomes in Appendix C explains the role of this variable in controlling for bias.

[48] This represents the average difference in percent eligible for treatment between 1991 and 1996 as indicated by the results of surveys administered at two sites: FCI's Marianna and Fairton.

Because all individuals identified as DAP comparison or non-DAP control subjects were within 15 months of release (including halfway house placement time), both the denominator and numerator of each institution-level coefficient were limited to estimates based on individuals within 15 months of release. The percent eligible for treatment described above was thus multiplied by the population within 15 months of release housed at the particular institution. The numerator — the number actually volunteering — for each site was obtained from automated databases and included all individuals who (1) were housed at that site, (2) were within 15 months of release, and (3) were either on the DAP waiting list, actively participating in the DAP, or DAP discharges.

The institution coefficients were used to calculate individual-level coefficients in the following manner: the history of an individual's institutional transfers was examined to flag those inmates who spent time at a DAP site and who had enough time to participate in the DAP at such a site. To be more specific, an individual's stay at a DAP institution had to have been for at least 30 days and had to have occurred at a time in his or her incarceration for which there was sufficient time before release to volunteer and complete treatment.

The coefficient for each individual — p — was the weighted average of the probability-of-volunteering coefficient for each stay that the inmate had at a DAP site where he or she could have entered DAP treatment (*i.e.*, meeting the specifications just mentioned). For example, the coefficient for each institution at the particular time the individual was housed there was assigned for each month, and then divided by the total number of months spent at any DAP site.[49]

Summary

The original experimental research design for the TRIAD drug treatment evaluation project was modified several times to accommodate the initial absence of waiting lists for admission to a DAP and to accommodate changes in time-left-to-serve for the drug treatment population. The lack of waiting lists during initial program implementation made the original plans for random assignment infeasible. Therefore, we resorted to a research design that was quasi-experimental. In addition, the admission of inmates to treatment who were not near release required the expansion of treatment research sites from 8 to 20. This change was needed to obtain sufficiently large sample sizes.

Because drug treatment was not available at all sites from which we selected research subjects, we were able to identify two types of comparison groups: one having had DAP treatment available —

[49] For stays at an institution prior to January 1994, the coefficient value of January 1994 was assigned. It is presumed that the coefficients are most likely to have been stable prior to that point in time — that is, before rumors of the possibility of the incarceration-reducing provisions of the Violent Crime Control and Law Enforcement Act of 1994. The waiting lists for the DAP's provide good indicators of when these rumors began to affect participation rates. The size of the waiting lists began to see a dramatic rise in November and December 1994.

the DAP comparisons — and the other not having this treatment available — the non-DAP controls. As discussed in detail in Chapter 2 and in this chapter, these two different non-treated subjects groups allowed us to address selection bias issues using two different analytic methods.

CHAPTER 5: SUBJECT ATTRITION

Introduction

In this chapter, we examine the potential bias that could occur as a result of either an inmate's omission from the data collection process (those we "missed") or an inmate's refusal to participate in the research (called "refusals"). Inmates were missed — *i.e.*, omitted from the data collection process — due to logistical issues related to institutional transfers and releases.

By contrasting those inmates who did participate with those who did not (the missed and the refusals), we hope to understand the nature of any bias that may result from non-participation. This chapter examines three aspects of research participation that could result in a biased inmate sample:

- Comparison/control subjects' willingness to complete the History of Drug Use Questionnaire.[50]
- Treatment and comparison/control subjects' willingness to be research subjects.
- Treatment and comparison/control subjects' willingness to complete an Intake1[51] interview.

We did not expect bias resulting from an inmate's omission from the data collection process, because we believed that any such omission resulted from project data collection logistics rather than some systematic mechanism. Nonetheless, we felt that an understanding of this process, as well as of the refusal process, would increase our knowledge about the evaluation process and about impediments to implementing multi-site evaluations.

Research Variables

The contrasts to be examined in assessing bias resulting from subject attrition were quantified in a series of dichotomous variables. Therefore, logistic regression procedures, traditionally used for analyzing binary dependent variables, were used for these analyses (Menard, 1995). Prior to

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[50]For the sake of simplicity, in this Chapter we use the term "comparison subject" to refer to the DAP comparison subjects and the term "control subject" to refer to non-DAP control subjects.

[51] Refusals for Intake1 were examined because this was the interview essential for the analyses, as discussed in "Research Subjects" (*see* Chapter 4). Intake1 contained most of the background information used in the analyses. In contrast, the Intake2 interview contained only two variables— the DSM-III-R diagnoses of antisocial personality and depression — and was administered after the research subject had agreed to the Intake1 interview.

conducting logistic regression analyses, Chi-square tests were performed on a group of variables (drawn from the automated SENTRY database) that may have influenced an inmate's likelihood of participating in the project. For example, did an inmate's race, age, or ethnicity influence his or her likelihood of participating in the research project? If any of these variables (described below) were found to be significant in the Chi-square test ($p <= .25$), they were included in the logistic regression equations. A coefficient in the logistic regression equation was considered to be significant if the probability for that coefficient was less than or equal to .05. Results for the regression are contained in Appendix A, and a codebook of the variables used in the analyses is contained in Appendix B.

Comparison/Control Subjects — History of Drug Use (HDU) Administration

Inmates who had not participated in residential treatment while incarcerated and were between 8 and 11 months from release (in 1996, the criterion for inclusion was changed to inmates between 7 and 13 months from release) were identified as potential comparison or control subjects. Comparison/control subjects were drawn from institutions at which treatment was available (DAP sites) and from institutions that did not offer treatment (non-DAP sites). An inmate was identified as a DAP subject in this analysis if he or she was housed at a DAP site at the time he or she became a research subject.[52] This analysis was conducted on only those persons identified as potential comparison/control subjects from 1994 through 1996.[53] In addition, for a short period of time, the process of selecting comparison/control subjects involved a matching process (*see* "A Chronological History of the TRIAD Subject Selection Process," Chapter 4).

HDU questionnaires were given to potential comparison/control subjects only to determine their eligibility for inclusion in the research effort. (Note that potential *treatment* subjects were identified as such by virtue of their participation in the DAP and, therefore, did not complete the HDU questionnaire.) By refusing to complete the HDU questionnaire, comparison/control subjects essentially refused participation in the research project.

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[52] This is not to be confused with the DAP comparison/non-DAP control distinction used to differentiate the non-treatment subjects into those who had treatment available and those who did not. For comparison and control subject groupings in our analyses of outcomes, individuals were classified according to whether or not they had *ever* been housed at a DAP site when a treatment program was operational. In assessing subject attrition, we sought to understand the effects of an inmate's *current* institution (*i.e.*, the institution where the inmate was housed when approached for participation in the research project) upon his or her likelihood of participation in the research.

[53] Prior to 1994, the generation of the list of individuals selected for the History of Drug Use questionnaire administration was not automated.

A total of 4,121 male and 1,283 female inmates were identified as potential comparison/control subjects to be screened using the HDU questionnaire. Of these inmates, 3,727 men and 1,113 women were approached to complete the HDU. The remainder either did not appear at their appointments (n=90); were not in the institution (*e.g.*, were on writ) (n=9); were in special housing (n=15); were not fluent in English (n=438);[54] or were not available (n=12) to complete the survey.

HDU Refusals

When approached to complete an HDU, the inmate was notified that participation was voluntary and confidential. Of the 3,727 men identified as potential research subjects, 926 (25 percent) refused to complete an HDU. Significant differences (Chi-square) between inmates willing to take the HDU and those refusing it were found for the following variables: institution, race, ethnicity, institution security level, offense severity, prior commitments, and history of violence. These variables, along with age, were included in the logistic regression models.

Because security level and institution were linear combinations of each other and could not be entered simultaneously into a logistic regression, two models were run. The first model contained the significant variables, with the exception of institution. The second model contained institution variables, and the security-level variable was dropped.

The first model (security level) showed a better fit (Hosmer-Lemeshow Goodness of Fit = 7.65, p=0.4687) when compared with the second model (institution) (Hosmer-Lemeshow Goodness of Fit = 10.03, p=0.2632). Therefore, the results from the first model are reported below.

The logistic regression model (*see* Table A1) shows that six variables had statistically significant effects on refusal rates for male inmates: age, ethnicity, institution security level, offense severity, race, and history of violence. Inmates housed at low-security facilities were 20 percent less likely to refuse an HDU than were inmates housed in minimum-security facilities. Those inmates who had histories of serious violence were 35 percent more likely to refuse than were inmates who did not have histories of violence. Additionally, persons whose offenses were moderate or great were more likely to refuse than were those whose offenses were low/moderate (however, no effect was seen for high-severity offenses). Race and ethnicity, too, played roles in the likelihood of refusing. Black male inmates were 23 percent less likely to refuse than were white male inmates, and Hispanic male inmates were 21 percent less likely to refuse than were non-Hispanics. As the age of the inmate increased, so did the likelihood of refusing.

Of the 1,113 women approached to complete an HDU, 137 (12 percent) refused. Significant differences (as measured by Chi-square) between women who agreed and women who refused to

[54] Most of these individuals were of Hispanic origin and could not read or write English, although some could *speak* English.

complete the HDU were found for the following variables: institution, race, institution security level, offense severity, and history of violence. These variables, as well as age, were included in the logistic regression equation.

As with the model for male HDU refusals, two logistic regression models were run. The first model contained security level, with institution dropped. The second model dropped security level and added institution.

The first model (security level) showed a better fit (Hosmer-Lemeshow Goodness of Fit = 4.96, p=0.7615) than did the second model (Hosmer-Lemeshow Goodness of Fit = 10.91, p=0.2068). Therefore, the results from the first model are reported below.

Four variables have statistically significant effects on HDU refusal rates among women: race, offense severity, history of violence, and age. Black female inmates were 64 percent more likely to refuse an HDU than were white female inmates. Inmates with histories of minor violence — compared to those with no such histories — and those who committed an offense of moderate or high severity — compared with low/moderate offense severity — were almost twice as likely to refuse to complete the questionnaire. No effect was found for offenses of great severity or for histories of serious violence. The older the inmate, the more likely she was to refuse (*see* Table A2).

Comparing the results between men and women, it is evident that although many of the same variables were significant, the "direction" of the relationship was not always the same. Where black men were less likely than white men to refuse completing the HDU survey, black women were more likely than white women to refuse completing this survey. Furthermore, male inmates with a history of *serious* violence were more likely to refuse, whereas female inmates with a history of *minor* violence were more likely to refuse.

Attrition of Identified Research Subjects

Once the comparison subjects who had self-reported drug use histories and treatment subjects who entered a DAP were identified for data collection, subject attrition resulted either from subjects not being approached for data collection or from subjects refusing to participate. Table 1 summarizes this attrition process, and the process is examined in detail at the conclusion of this chapter.

Once both treatment and comparison subjects had been identified, researchers visited their sites to conduct surveys; however, not all subjects identified initially were able to participate in the research effort. Between the time an individual was identified as a research subject and the time a researcher was scheduled for the return trip to that institution to collect data, some research subjects were no longer housed at the institution due to such events as transfers to other institutions, absences due to writs, and releases to CCC's or from BOP custody. Other inmates

had to be excluded from the research pool because they were under special housing restrictions;[55] others were excluded because they had illnesses. A logistic regression was performed to analyze possible differences between those persons who were not available for research participation (*i.e.*, "missed") and those who were.

Subjects Who Were Missed as Research Subjects

A total of 2,459 male inmates were identified as research subjects. Of those, 378 (15 percent) were missed. The following variables had significant differences for those inmates who were missed as research subjects compared with those subjects who were included in the subject pool (Chi-square): status as comparison vs. treatment subject, being housed at a DAP vs. non-DAP site, ethnicity, institution, and race.

These variables (excluding specific institution) along with age were included in the regression analysis. Site was not included in the regression model because of zero cells. The proportion of male inmates across the 34 sites who were missed ranges from 0 to 52 percent.

The logistic regression (*see* Table A3) showed significant effects for status as comparison vs. treatment subject, and for DAP vs. non-DAP site. Male subjects identified at non-DAP sites were nine percent less likely to be missed than were inmates housed at DAP sites. Comparison subjects were 344 percent more likely to be missed than were treatment subjects.

Of the 571 female inmates who were approached to participate in the research project, 22 percent (n=127) were missed. Chi-square tests showed significant differences on the following variables between those persons approached for research and those who were missed: status as comparison vs. treatment subject, institution, institution security level, being housed at a DAP vs. non-DAP site, offense severity, and prior commitments. These variables (along with age), excluding the variables DAP vs. non-DAP site and institution, were included in the logistic regression equation. The variable DAP vs. non-DAP site could not be included because no one at a non-DAP institution was missed (23 percent [n=127] of the women at DAP sites were missed). Institution could not be included in the regression because of zero cells. The proportion of female inmates missed ranged from 0 to 31 percent, across seven sites.

Two variables were significant in the regression (*see* Table A4): status as comparison vs. treatment subject, and security level. Comparison subjects were 214 percent more likely to be missed for research than were treatment subjects, and inmates housed at low-security institutions were 67 percent more likely to be missed than were those housed at minimum-security institutions.

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[55] Detention in a special housing unit segregated from the general population occurs for administrative reasons and as a sanction for disciplinary infractions.

Comparison subjects were more likely to be missed, for both men and women. This can be attributed primarily to the logistics of planning data collection trips. The first priority during data collection trips was placed upon cohorts newly admitted to DAP's and upon graduating cohorts. DAP participant data collection required adherence to specified time frames for administering the surveys and interviews at the beginning and end of treatment for the data serving as pre- and post-treatment measures. Identification of comparison subjects occurred close to those subjects' release dates (approximately one year from release), and at the time of selection it was unknown whether these individuals would receive CCC placements. Therefore, it was more likely that these subjects would be missed; by the time a trip to that site occurred for DAP subject data collection, some of the individuals selected as comparison subjects would likely already have been released to CCC's.

Research Refusals

Once inmates were identified as research subjects — either as DAP participants or as comparison subjects through the HDU — and were available to participate, they were asked to complete two interviews and various surveys. Inmates were reminded that participation in the research was voluntary, and they signed informed consent forms.

Of the 2,081 male inmates who were approached to participate in the research, 223 (11 percent) refused. A logistic regression was run to examine differences between those subjects who participated and those who refused.

The following variables were significant by Chi-square and were included in the regression model: status as comparison vs. treatment subject, being housed at a DAP vs. non-DAP site, ethnicity, prior commitments, race, and history of violence. Institution also was significant, but was not included because of too many zero cells**.** Refusals for research ranged from 0 to 44 percent across the 34 sites.

The only significant variable in the regression equation (*see* Table A5) for men refusing research was the variable denoting type of research subject. Comparison subjects were 160 percent more likely to refuse to complete the research surveys as were treatment subjects.

A total of 444 female inmates were approached to participate in the research. Of those, only 27 (6 percent) refused to complete any of the research forms. Because the number of women who refused was small, no regression equation was performed.

Intake1 Missing and Refusals

Inmates who agreed to participate in research were asked to complete two interviews. As mentioned in the previous chapter, "Research Subjects" (*see* Chapter 4), only those individuals

who had completed the Intake1 interview were included in the analyses. Therefore, analyses were conducted to examine characteristics of those persons who missed the Intake1 interview and those who refused to complete it.[56]

A segment of comparison inmates (75 men and 7 women) was interviewed at halfway houses rather than at institutions. They have been added to the comparison sample for the Intake1 interview analyses. A halfway house category was added to the following variables: being housed at a DAP vs. non-DAP site, institution, and security level.

Out of the 1,933 men to be interviewed, 149 (8 percent) were missed. Chi-square significance was found for the following variables: being housed at a DAP vs. non-DAP site, status as a comparison vs. treatment subject, institution, and security level. Institution could not be included in the regression because of too many zero cells. The range of those who missed the Intake1 interview was 0 to 29 percent, across 35 sites.

A regression equation was attempted with the remaining significant variables. However, due to quasi-complete separation in the sample points and linear combinations of variables, a regression equation could not be estimated. Because no halfway house subjects were missed, deleting them from the regression equation allowed for an analysis of persons missed for the remaining sites.

Significant values were found for the DAP vs. non-DAP site and comparison vs. treatment subject variables. These variables, along with age, were included in the regression equation. Age was not related significantly to the likelihood of being missed. Subjects at non-DAP sites were 84 percent less likely to be missed than were subjects at DAP sites. Additionally, comparison subjects were 50 percent less likely to be missed than were treatment subjects (*see* Table A6).

We can conjecture that the lower rate of missed Intake1 interviews among comparison subjects — in particular those from non-DAP sites — can be attributed to the logistical procedures involved in data collection. During the in-prison data collection phase of the evaluation project, research staff were located at as many as six different DAP research sites.[57] At these sites, the logistics of data collection did not require all data to be collected during a single week. However, at many DAP sites and all non-DAP sites, data collection required a special trip by a field researcher. Therefore, comparison subject data, most notably at non-DAP sites, tended to be collected during a week-long data collection trip to the site. This resulted in a decreased likelihood of missing the Intake1 interview in the case that the individual had been transferred or released.

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[56] Please note that although some subjects initially had agreed to participate in the evaluation project, some later refused participation in one or more survey or interview.

[57] The specific sites varied at different times of the project.

Seven percent (n=29) of 424 female inmates were missed when the Intake1 interviews were being administered. Due to the small number missed, regression equations were not performed.

Refusal Rates for Intake1 Interview

Refusal rates for the Intake1 interview were low for both men (2 percent, n=40) and women (less than 1 percent, n=3), so regression equations were not performed.

Summary of Results for Subject Attrition

It is apparent from Table 1 that the subject attrition problem was most pronounced at the point when the pool of potential comparison subjects was being identified. Not only was the refusal rate highest at this point, but this was the only point at which characteristics of individuals were predictive of refusal. Among men, there were significant effects for race, ethnicity, offense severity, age, and history of violence. Among women, there were significant effects for the same variables except ethnicity, although the "direction" of the relationship was not always the same. It must be noted that all the factors found to be predictive of HDU refusal rates were used as control variables in the analyses of results.

At all other times during the process of data collection, it is clear that subject attrition was attributable solely to administrative causes. For example, the greater rate of missing data collection for comparison subjects was due to the fact that it was logistically more difficult to approach all subjects identified because there was a much shorter time frame within which to coordinate data collection trips for comparison subjects.[58]

[58] We are collecting arrest outcome information for subjects who refused to be interviewed and for subjects who were "missed" due to administrative reasons. Future analyses will examine whether the arrest rate of these individuals differ from those included in our analyses, controlling for the background characteristics available from automated data files.

CHAPTER 6: DESCRIPTION OF SAMPLE

This chapter profiles the research subject sample, the various DAP treatment subject groups, the DAP comparison group, and the non-DAP control group, for whom — at the very least — the Intake1 interview containing extensive background information was completed. Taken together, these subjects were included in the analysis of the various outcome measures, and we provide a profile of the subject sample using the variables included in the outcome analyses. This description provides a simple understanding of how the in-prison and post-release services received differ among the various subject groups, and it serves as a basic description of both a male and female incarcerated population with a history of drug use. We provide separate tables of descriptive statistics for men and women because, as mentioned in Chapter 7, men and women were in separate treatment programs and, when possible, we analyzed men and women separately.[59]

DAP Treatment Groups

This section briefly describes the four types of residential Drug Abuse Treatment Program (DAP) participants as they were categorized in the analyses. The four groups include: (1) inmates in residential drug treatment who completed that treatment (DAP-complete subjects), (2) inmates who dropped out (DAP-dropout subjects), (3) inmates discharged for disciplinary reasons (DAP-discharge subjects), and (4) inmates who, for a variety of other reasons, did not complete the program (DAP-incomplete subjects). This "incomplete" category, in general, comprises inmates who were transferred to another institution or to a Community Corrections Center (CCC) before they could complete the full 9 or 12 months of treatment, those who had their sentences shortened toward the end of their incarceration and were released from BOP custody before they were able to complete the treatment program, and those who spent an extended amount of time on writ or medical furlough and thus were unable to complete treatment before release.

Of the 1,524 male subjects in this analysis, 719 (47 percent) entered residential treatment. The other 53 percent were comparison subjects who never entered residential treatment. Of the 719 who entered treatment, 73 percent completed the treatment program, 5 percent voluntarily dropped out of the program, 8 percent were removed for disciplinary reasons, and 14 percent constituted the "incomplete" subject category.

Of the 342 female subjects in this analysis, 180 (53 percent) entered residential treatment. The other 47 percent were comparison subjects who never entered residential treatment. Of the 180 who entered treatment, 54 percent completed the treatment program, 9 percent voluntarily dropped out of the program, 13 percent were removed for disciplinary reasons, and 24 percent fell into the incomplete category. The fact that there is a lower percentage of treatment

[59] Separate multivariate outcome analyses for men and women were precluded due to small sample sizes for women. However, as we mention in Chapter 8, we plan to conduct separate analyses for all outcomes when data become available for the entire subject sample.

"completers" for women than for men may be related to policy differences between treatment sites and differential enforcement of program rules.

Sample Demographics

This section describes the 1,524 male inmates and 342 female subjects who were interviewed as part of the drug treatment evaluation project and were included in the analysis. The samples were divided into those inmates who received treatment in the DAP and those who did not receive treatment while incarcerated (*i.e.*, comparison subjects). Further divisions were made among both the treatment and comparison groups. The comparison sample was divided into those inmates who were ever housed at a site that offered treatment and who were there long enough to participate in the program (DAP comparison subjects) and those who did not have the opportunity to participate in the program (non-DAP control subjects).The treatment group was divided into the four categories mentioned in the preceding section: those who completed the treatment program (DAP-complete subjects), those who dropped out (DAP-dropout subjects), those who were discharged for disciplinary reasons (DAP-discharge subjects), and those who did not finish the program for other reasons (DAP-incomplete subjects).

Race/Ethnicity

The male sample's racial composition was 59 percent white, 38 percent black, and 3 percent other races (*see* Table 2). Blacks composed more of the non-DAP control group (46 percent) than they did the other groups. There was less difference in the racial makeup among the female subject groups: 52 percent white and 47 percent black (*see* Table 3). There were only five female inmates of other races in the sample.[60] Treatment dropouts — DAP-dropout subjects — had the lowest number of whites (38 percent) and DAP comparisons had the highest (56 percent).

The majority of subjects in both the male and female samples were United States citizens — 94 percent and 93 percent, respectively. Nine percent of the men and eight percent of the women were of Hispanic origin.

Age

Tables 2 and 3 show the ages for inmates at the time of their release from BOP custody. Among women, more than half of the sample were 34 years old or younger at the time of their release. Men, on the other hand, tended to be older, with 57 percent being at least 35 years old at the time of their release. Differences within sample categories also can be seen. For example, among both the male and female treatment subjects, those who were discharged for disciplinary reasons (DAP-discharges) or had withdrawn (DAP-incomplete subjects) tended to be younger than were the subjects in the remaining categories.

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[60]We note that the information on women, in this and subsequent tables, must be interpreted with caution due to small sizes for several of the subject groups.

Education

Sixty-nine percent of the male sample and 78 percent of the female sample had completed at most 12 years of education, or had achieved a General Education Degree (GED). The remainder of the sample (31 percent of the men and 22 percent of the women) reported completing more than 12 years of education, with the years of education for these subjects ranging from 13 to 20 years (data were missing for 10 men and 5 women). Female subjects in the non-DAP control group were much more likely to have reported having completed more than 12 years of education (41 percent) than were those in the other sample groups (*see* Tables 2 and 3).

Frequency of Drug Use During Heaviest Use Period

Tables 4 and 5 present information on pre-incarceration self-report drug use patterns for the 1,524 male and 342 female research subjects. Subjects reported the frequency of drug use during their heaviest use period prior to the incarceration during which they were interviewed. The following "screening" question was asked for each of a number of drugs:

> Have you ever taken one of these drugs more than five times in your life (to feel good, to get high, for other mental effects, or longer than was prescribed)?

When inmates responded "yes" to this screening question, they were then asked a series of other questions regarding drug use.

> Think about the period of time when you were using each drug the most (heaviest). At that time, about how often did you use each drug? Would you say that it was...
>
> - daily or almost every day.
> - 3 or 4 days a week.
> - 1 or 2 days a week.
> - 1 to 3 days a month.
> - less than one day a month.

When inmates responded "no" to the screening question for a particular drug, no further questions were asked about that drug.

In order to make the tables more concise, response categories were condensed into "Not used," "Used less than daily," and "Used daily." The category "Not used" is presented for individuals who were not asked this frequency question because they had used the drug five times or fewer in their lives or had never used the drug. The "Missing" values are the sums of those subjects for

whom we have no data for that particular drug. For analysis purposes, all values of the response set were used in the models, and missing data were imputed.[61]

Tables 4 and 5 indicate that marijuana was the drug used most frequently by research subjects. Among men, 18 percent reported that they did not use marijuana (*i.e.*, used it five times or fewer, or never, during their lives), 25 percent reported use that was less than daily, and 57 percent reported daily use during their heaviest use periods. Among women, 27 percent reported that they did not use marijuana, 26 percent reported less than daily use, and 46 percent reported daily use.

Cocaine (excluding crack) was the next most frequently used drug, with 36 percent of men reporting that they did not use cocaine, 31 percent reporting less than daily use, and 33 percent reporting daily use. Among women, 34 percent reported no use, 24 percent reported less than daily use, and 42 percent reported daily use during their heaviest use periods.

The drug with the lowest use level was opiates, excluding heroin.[62] Among men, 81 percent reported no use, 11 percent reported less than daily use, and 8 percent reported daily use. Among women, 82 percent reported no use, 8 percent reported less than daily use, and 10 percent reported that they used opiates daily during their heaviest use period.

Table 4 indicates that 77 percent of male subjects reported that they did not use crack cocaine, 9 percent used it less frequently than daily, and 14 percent used it daily at some point. Among women, 61 percent reported no use of crack cocaine, 10 percent reported less than daily use, and 29 percent reported daily use. These seemingly low percentages may be partly due to the fact that crack cocaine made its way into the drug market in the 1980's, and thus some of our subjects were already incarcerated when this drug came into wider use.

Drug and Alcohol Dependency

Table 6 depicts the means and standard deviations for imputed values of drug and alcohol dependency for male inmates.[63] Table 7 depicts the same for female subjects. For these variables, we also depict the odds ratios. The odds ratios are computed by raising the natural log base e to the log odds values represented in the two tables. For men, the odds of being drug dependent were highest for DAP-discharges. The odds were 8.47, compared to an average odds ratio of 3.69. Also among the men, those least likely to be drug dependent were inmates who dropped out of treatment (odds ratio of 0.98). This pattern was similar for the women, who were much more

[61] The imputation procedure is discussed in Chapter 7.

[62] Examples of opiates other than heroin include opium, morphine, demerol, dilaudid, talwin, percodan, codeine, and non-prescribed methadone.

[63] A description of the logistic regression procedure used to impute these values is contained in Chapter 4 under “Psychological/Attitudinal Measures.”

likely to be drug dependent if they were DAP-discharge subjects (odds ratio of 20.04) but least likely to be drug dependent if they dropped out of the treatment program (odds ratio of 2.71). In addition, women were *most* likely to be drug dependent if they completed the drug program (odds ratio of 19.76).

Compared to that of drug dependence, there was less variability among the group means for alcohol dependence. Among the men, non-DAP subjects were most likely to be alcohol dependent, with an odds ratio of 1.5. Among the women, DAP-discharges were the most likely to be alcohol dependent. Their odds ratio was 3.41, compared to an average odds ratio of 0.56.

The most consistent pattern emerging from Tables 6 and 7 is that both men and women were most likely to be drug dependent if they were DAP-discharges. Women discharged for disciplinary reasons were more likely to be alcohol dependent, but this relationship is not demonstrated for the men.

In-Prison Outpatient Treatment and Self-Help Group Participation

In the analysis, several variables were used to represent subjects' participation in outpatient drug and alcohol treatment or self-help groups while in prison. The focus of our evaluation is on the effect of residential 9- and 12-month drug treatment programs. However, some inmates participated in "outpatient," or non-residential, drug treatment consisting of several hours of group sessions per week. Also available to most inmates were self-help groups similar to Alcoholics, Cocaine, and Narcotics Anonymous, often led by community members or other inmates. Inmates participating in outpatient or self-help groups may or may not have also participated in residential drug treatment. All drug treatment participation was voluntary.

Tables 8 and 9 present the percentages of both male and female subjects, respectively, who received in-prison, outpatient drug and alcohol treatment and the percentage of subjects who participated in the in-prison self-help programs.[64]

For men, the "DAP completion" group had the greatest proportion of subjects involved in outpatient treatment (10 percent). The same group had the highest percent of participation in self-help programs (6 percent). Women in the non-DAP control subject category had the highest participation rate in outpatient programs (14 percent), while the DAP comparison subjects had the highest participation rate in self-help programs (9 percent).

[64]The percentage of individuals receiving outpatient services may be under-reported due to inconsistencies in recording this information on SENTRY.

Alcohol and Drug Treatment History

Tables 10 and 11 present self-report data on the percent of male and female subjects who had previously received treatment for drug or alcohol use. Subjects were asked, "Excluding now, did you ever get any treatment that was primarily for the use of or addiction to drugs/alcohol?" Self-help groups such as Alcoholics Anonymous and Narcotics Anonymous were not considered to be treatment.

The tables show that men and women were equally likely to have received drug treatment, at proportions of 31 percent and 35 percent, respectively. Men and women also were equally likely to have received alcohol treatment — at 5 percent and 4 percent, respectively. Women in the non-DAP control and treatment dropout (DAP-dropout) groups were less likely than were other women to have received previous drug treatment. None of the treatment subjects who did not complete a DAP reported having past alcohol treatment. Other than these differences, subjects in the various treatment groups and DAP comparison and non-DAP control subject groups were equally likely to have received previous drug and alcohol treatment.

Criminal/Incarceration History

Tables 12 and 13 present four variables detailing subjects' criminal and incarceration histories: whether they had any prior commitments, how recently they had committed an act of violence, their lengths of incarceration, and whether they were in the criminal justice system (*e.g.*, on probation or on parole) when arrested. Only the last variable — referring to their being in criminal justice system when arrested — is based on self-report data; the other three variables listed in Tables 12 and 13 come from official records.

A subject is considered to have had a prior commitment if he or she previously had been sentenced to confinement in any type of criminal justice facility for any period of time. Men were much more likely (at a proportion of 71 percent) than were women (45 percent) to have had prior commitments. The percent of subjects who had prior commitments varied across treatment and DAP comparison/non-DAP control groups. Among men, DAP-discharges were the most likely to have had prior commitments (at 80 percent), and those who were unable to complete the DAP were least likely to have had prior commitments (at 65 percent). Among women, DAP comparisons were the least likely to have had prior commitments (at 37 percent), which is below the average for women (45 percent). Women who were unable to complete DAP or were discharged for disciplinary reasons from a DAP were more likely than were other women to have had prior commitments (58 percent and 62 percent, respectively).

The "recency of violence" variable denoted whether the subject had a history of committing violent acts and, if so, whether the most recent violent act had been committed less than or more than 5 years before the subject's initial designation to a BOP institution for the current offense. This measure included both minor and serious acts of violence, but excluded violence associated with the current offense.

More men had histories of violence than did women. Eighty-six percent of the women had no history of violence, compared with 54 percent of the men. Violence among men was more often committed 5 or more years prior to the current incarceration (at 32 percent), as opposed to such an act being committed more recently (14 percent). Acts of violence committed by women were equally likely to have been committed more than 5 years before the current incarceration as they were to have been committed *less* than 5 years before the current incarceration (7 percent for each category).

Men who dropped out or were discharged from DAP for disciplinary reasons were more likely to have had histories of violence than were men in the other DAP treatment groups. Male subjects who did not complete a DAP were the least likely to have had histories of violence. Women in the DAP treatment groups were slightly more likely than were those in either the DAP comparison and non-DAP control groups to have had no history of violence, with the exception of women who received disciplinary discharges from a DAP.

Tables 12 and 13 also show the length of subjects' incarceration for the current Federal offense. This variable was computed by "subtracting" the date of admission to a Federal prison from the date of release from Federal custody. Any time subjects spent in custody prior to sentencing was excluded.

Most subjects spent between 1 and 5 years in custody for their current offenses. Women spent less time in custody than did men; 34 percent of women and 19 percent of men spent less than one year in custody. Length of incarceration varied across the subject groupings, and this pattern of variation was the same for men and women. Both male and female non-DAP control and DAP incomplete subjects spent less time in custody than was the overall average by sex, while DAP graduates spent more time in custody than was the average. Only 9 percent of DAP graduates spent less than one year in custody.

The last variable displayed in Tables 12 and 13 reveals whether the subject said he or she was in the criminal justice system when arrested for the current offense. Inmates were asked, "At the time of your arrest, what was your legal status?" Subjects were considered to be in the criminal justice system at the time of their arrests if they were on any type of judicially imposed confinement or supervision for a previous offense. Escapees and absconders were counted as being in the criminal justice system.

The percentage of men in the criminal justice system at the time of their arrests was similar to the percentage of women, at 42 percent and 37 percent, respectively. Among men, the likelihood of having been in the system when arrested was greatest for DAP-discharges (at 53 percent) and non-DAP controls (at 48 percent). Men who completed or dropped out of a DAP were least likely to have been in the system when arrested (at 36 percent each). Among women, non-DAP controls and those failing to complete a DAP were most likely to have been in the system when arrested (at 52 percent and 45 percent, respectively). Female DAP comparisons were the least likely to have been in the system when arrested (at 29 percent).

Past Mental Health Treatment/Psychiatric Diagnoses

Tables 14 and 15 contain information relating to the mental health of male and female subjects. Two sets of variables are displayed in these tables: psychiatric diagnoses and histories of previous mental health treatment.

With respect to the psychiatric diagnoses of antisocial personality and depression, men and women had similar percentages of subjects with no such diagnosis (at 54 percent and 49 percent, respectively). However, of those subjects with psychiatric diagnoses, men were more often diagnosed as having antisocial personality (37 percent of the men and 30 percent of the women), whereas women were more often diagnosed with depression (34 percent of the women and 15 percent of the men). Among both men and women, the DAP-discharges were the most commonly diagnosed with antisocial personality (at 45 percent and 47 percent for men and women, respectively) and were the least likely of any group to have no psychiatric diagnosis (40 percent and 24 percent, respectively). Women who were disciplinarily discharged were the most likely to be diagnosed with both antisocial personality and depression (29 percent). Overall, a greater percentage of women (13 percent) were diagnosed with both antisocial personality and depression than were men (7 percent).

Subjects were asked, "Did you ever get treatment for your emotions, nerves, or your mental health...?" Any mental health treatment or counseling received prior to the current incarceration was included.

Men were less likely than were women to report that they had received treatment for their emotions, nerves, or mental health (at 18 percent vs. 40 percent). Male DAP treatment subjects, as well as DAP comparison and non-DAP control subjects, were equally likely to have received mental health treatment. Among women, DAP graduates were the least likely group to have received mental health counseling in the past (at 31 percent), while those failing to complete a DAP were the most likely to have received such services (at 53 percent). Excluding DAP graduates, the other female subject groups were equally likely as each other to have received mental health treatment.

Employment

Tables 16 and 17 present self-report data on male and female subject employment histories. Subjects were asked, "Did you ever support yourself *mainly* from illegal activity for at least one year?" Men and women were equally likely to have supported themselves mainly with illegal activity, at proportions of 39 and 41 percent, respectively. Male and female subjects who were unable to complete the DAP were among the least likely to have supported themselves illegally (at 24 percent and 31 percent, respectively). In addition, women who graduated from the DAP and men who were discharged from the DAP for disciplinary reasons were also among the least likely to have supported themselves illegally (at 26 percent and 27 percent, respectively).

Tables 16 and 17 also present data on subjects' employment status in the month prior to their current incarcerations. Subjects were asked a series of questions designed to ascertain how they were supporting themselves during this month. Subjects who had worked at all during the month were coded as "full- or part-time." Subjects who were unemployed for legitimate reasons such as retirement or disability were coded as "not in labor force." Homemakers also were coded as "not in labor force." Subjects who were unemployed and seeking work during this month were coded as "looking for work." The remaining subjects were given a code denoting "miscellaneous" for their employment statuses. This last category included subjects who were unemployed because their income was derived from illegal activities, subjects who had never worked, and subjects who were unemployed for other reasons.

Male subjects were more likely than were female subjects to have been employed during the month before their incarcerations, at proportions of 55 percent and 39 percent, respectively. Among men, those discharged from the DAP for disciplinary reasons were least likely to have been employed (at 46 percent) and those who were unable to complete the DAP were most likely to have been employed (at 67 percent). The percent of men who were not in the labor force was similar for treatment and DAP comparison/non-DAP control subjects (at about 5 percent). Likewise, the percent of male subjects who were actively looking for work in the month before their incarceration was similar across treatment and DAP comparison/non-DAP control categories (at about 9 percent).

Among women, non-DAP controls were most likely to have been employed (at 49 percent) and those unable to complete the DAP were least likely to have been employed (at 31 percent). The percent of women who were not in the labor force was approximately equal between the DAP treatment and the DAP comparison and non-DAP control groups (at roughly 7 percent), as was the percent of all women who were looking for work (at about 10 percent).

Motivation for Change

Tables 18 and 19 present information on the distribution of the research subject groups among the six "motivation for change" clusters. The greatest percentage of male subjects, 28 percent, were categorized into the contemplation cluster, representing individuals who recognized that they had a problem and were considering changing but had not yet actively started to do so. The least populated category for men was the "uninvolved" category — that is, those who did not endorse any of the four change assessment scales. The "reluctant," "precontemplation," and uninvolved clusters — representing those least motivated for change — were mostly populated from the DAP comparison and non-DAP control groups. With the exception of the DAP disciplinary discharge group, the DAP treatment groups had higher percentages of subjects populating the "preparation" cluster (at approximately 25 percent) than did the DAP comparison and non-DAP control groups (at 14 percent and 16 percent, respectively).

Table 19 shows that, among women, the greatest percentage of the subjects fell within the action cluster (31 percent). Similar to men, the least populated cluster was the uninvolved cluster. Unlike

the men, women non-DAP control subjects were more likely to fall in the preparation cluster than were treatment subjects, at 32 percent vs. 10 to 24 percent.

Other Variables

A variable, COHTIME, was constructed to measure the amount of time (in months) that had elapsed between when a program started and when an inmate participated in the program. This variable was intended to be a proxy for the maturity of a program at the time an inmate participated in that program.

For men, the average time elapsed for treatment participants was 23.5 months. Among men who completed treatment, the mean value of COHTIME was 22.9 months. For men who withdrew from treatment, the mean was 22.8 months. The average for disciplinary terminations among men was 24.6 months, and the average time elapsed for men who did not complete treatment was 26.1 months.

For women, the average for COHTIME was 24.9 months. Among women completing treatment, the average time elapsed was 21.7 months. For women who withdrew from DAP, the average was 27.4 months. For disciplinary terminations, the average was 36.2 months, and the average time elapsed for women of the DAP-incomplete subject type was 25.1 months.

Another variable was computed to measured the number of months between discharge or termination from a DAP and release from custody (TIMETORL). The idea behind measuring this time span was to capture whether inmates would forget what they had learned in treatment or whether they would lose some of their motivation to stay drug-free with increases in the time between program completion and release from custody.

For men, the overall average for everyone who entered treatment was 9.8 months between treatment discharge (be it completion or termination) and release. This variable was skewed to the right, as one might expect, with the longest period between completion of treatment and release from custody being 38.5 months. Among male treatment completers, the average was 10.4 months. For men who withdrew, the average was 13.4 months. For men who were terminated for disciplinary reasons, the average was 11.1 months. The average for male DAP-incomplete subjects was 5 months.

For women, the average for TIMETORL was 8.2 months. Their distribution was also right-skewed. For women who completed treatment, the average time between completion and release was 9.2 months. For withdrawals, the average time was 9.9 months. For women who were disciplinary terminations, the average time was 11.0 months. Finally, for female DAP-incomplete subjects, the average TIMETORL was 3.9 months.

The shortest average periods between program termination and release were found among the male and female DAP-incomplete subjects. This probably is due to the definition of DAP-

incomplete subjects: subjects who could not complete their programs because they were released to a halfway house or from BOP custody, or they had to be transferred to another facility sooner than expected.

CCC Placements

This section presents information regarding subjects who received Community Corrections Center (CCC) placements and who received transitional services during these placements.[65] Data generally were obtained through SENTRY, but other elements were obtained from a questionnaire completed by CCC staff upon each subject's release from the facility.

The CCC questionnaire posed questions regarding the subject's CCC completion status, employment during placement, participation in any educational programs, participation in transitional services or other drug treatment, the number of urinalysis tests administered, the number of positive urinalysis tests, and — for positive tests — the dates of the tests and the types of drugs for which the tests were positive.

Tables 20 and 21 demonstrate that of the 1,866 subjects in this analysis (1,524 men and 342 women), 68 percent of the men and 67 percent of the women received CCC placements. The breakdown by subject type shows that, for both men and women, those who ended their DAP participation because they dropped out of the program (DAP-dropout subjects) or received a disciplinary discharge (DAP-discharge subjects) were less likely to have received a CCC placement. Twenty-three percent of the men and 15 percent of the women failed to complete their CCC placements successfully. For the men, DAP-discharges showed the greatest likelihood of failure, while for the women, those whose treatment was incomplete were most likely to fail at the CCC. The most common reason for failing a CCC placement for both men and women was drug or alcohol use or possession (71 percent of the men who failed did so for this reason, as did 68 percent of the women).[66]

It should be noted that in Tables 20 and 21, one reason for CCC failure is listed as "Accountability," and subjects who failed for this reason usually were cited as having unexcused absences from the CCC. Another category, "Violation of CCC rules," comprises subjects who were cited for committing any of a variety of transgressions. These transgressions include gambling, acting disruptively, refusing an order, failing to find a job, engaging in sexual acts, being in an unauthorized area, driving without permission, and leaving work without authorization. Also, in Tables 20 and 21, the category, "Other" encompasses the following reasons for failure: possession of a weapon in the treatment program, commission of new criminal activity or arrest, violation of the transitional services program rules, and commission of one or more other objectionable acts.

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[65] A description of CCC placements and transitional services is found in Chapter 3.

[66] This represents the sum of the first four reasons listed as reasons for failing CCC placement.

Table 22 shows that of the men who received CCC placements, 61 percent participated in transitional services. As expected, subjects who completed the DAP and those who terminated the program as "incomplete" (usually meaning they were transferred or released before they had time to finish the program) were more likely to receive transitional services. Sixty-six percent of the men who participated in transitional services successfully completed the program. DAP-disciplinary discharges showed the highest percentage of members having failed transitional services (at 57 percent). The reasons for failing transitional services were very similar to the reasons for failing CCC placements (*see* discussion above). The reason for this similarity is that, according to the rules of the transitional services program, a failure of transitional services results in a failure of the CCC placement. Similarly, whenever one failed the CCC placement, one was removed from transitional services.

Table 23 shows that of the women who received CCC placements, 60 percent participated in transitional services. Again, those of the "DAP complete" and "DAP incomplete" groups showed the highest percentages of having received transitional services. Seventy-two percent of the female subjects who received transitional services successfully completed the transitional services program. As with the men, the reasons for failing transitional services closely mirrored the reasons for failing CCC placements.

Note that in Tables 22 and 23, which deal with transitional services release status, there is a category called "Admin/Neutral." This code was used somewhat differently from region to region. Sometimes it was used simply to reflect that subjects were continuing transitional services upon release to supervision. Other cases reflect that BOP custody ended before sufficient services were rendered to consider treatment as "completed." In general, this category seems to have been used when a subject was participating in, but did not complete, transitional services treatment due to some circumstance beyond his or her control.

Tables 24 and 25 demonstrate that most of the subjects in a CCC (95 percent of men and 93 percent of women) were tested for drug or alcohol use during their CCC placements. Sixteen percent of the men who were tested and nine percent of the women who were tested had a positive test during their stays at CCC's. The tables also show the drugs for which subjects tested positive.

Information regarding the subjects' placements into home confinement was collected from SENTRY. Thirty-five percent of the men and 50 percent of the women received periods of home confinement. The average length of time in home confinement for both men and women was 8 weeks.

Post-Release Behavior

Several items included in the analysis pertain to subject behavior after release from the incarceration during which they were identified as research subjects. This section presents descriptive statistics on the following post-release variables: supervision, new offenses and

revocations, violations of conditions of supervision, CCC placements while under supervision, drug and alcohol treatment, self-help group participation, employment status, cohabitation status, and drug use while under supervision.

Most of the data for this section were gathered from interviews with U.S. Probation officers for those subjects who were released to supervision (85 percent of the male subjects and 82 percent of the female subjects — *see* Tables 26 and 27). For subjects who were not released to supervision, new offense data were gathered from the National Crime Information Center (NCIC) database.

Post-Release Offenses

When both supervised and unsupervised subjects are viewed together, we see that 14 percent of the men were arrested for a new offense in the first 6 months after release from Federal prison (*see* Table 26). The subject types showing the highest percentages of new offenses were DAP-dropouts (at 19 percent) and DAP-discharges (at 18 percent). The offenses for which the male subjects were arrested were categorized into drug-related, violent, robbery, property, forgery and fraud, traffic, and other. "Other" offenses included arson, kidnaping, property damage, family offenses, gambling offenses, liquor offenses, trespassing, obstruction of police, public peace offenses, weapon offenses, and extortion. Of the men who were arrested, more were arrested for "other" offenses (at 21 percent) than for any other category of offense, followed by property offenses (at 18 percent) and drug-related and traffic offenses (at 17 percent each).

Among supervised and unsupervised women, 6 percent were arrested for a new offense in the first 6 months after release (*see* Table 27). Of the women who were arrested, more were arrested for property offenses (29 percent) than for any other offense category. There were insufficient numbers of women with new offenses to discuss meaningfully the differences among those offenses across subject types.

Post-release arrests were examined for subjects released to supervision. Arrests for these individuals were categorized into arrests for a new offense and arrests for revocation (*i.e.*, for violations of conditions of supervision). Eighty-five percent of male subjects were released to supervision, and, of those, 14 percent were arrested for a new offense during the first 6 months of their releases, 7 percent were revoked from supervision, and 78 percent had neither a new arrest nor a revocation (*see* Table 28). Men who completed the treatment program showed the highest likelihood of having no new offense or revocation. The breakdown of offense categories for those supervised male subjects who had either a new offense or a revocation showed that the most commonly committed offenses were revocations (35 percent), "other" offenses (16 percent), and traffic offenses (13 percent).

Eighty-two percent of female subjects were released to supervision. Of those, 6 percent were arrested for a new offense, 5 percent had their supervision revoked, and 89 percent had neither an arrest for a new offense nor a revocation (*see* Table 29). Of those who had any arrest or revocation, more committed offenses for which they were revoked (47 percent) than committed a

new offense. There were insufficient numbers of supervised women with new offenses to discuss meaningfully the differences among subject types in this regard.

Post-Release Drug Use

Tables 30 and 31 present information on urinalysis (UA) testing and drug use for subjects on post-release supervision. The tables show the monthly rate of urinalysis testing, the percent of subjects who used drugs, the breakdown of how such use was detected, and the drugs for which urinalysis tests were positive. The data for these tables come from information supplied by Probation officers. Additionally, Tables 30 and 31 show that the mean monthly number of drug tests was between two and three across all subject types and across gender.

The "Drug Use" sections of the tables derive their percentages from the number of subjects who had a positive urinalysis, refused a urinalysis test or missed such a test, admitted using either illicit drugs or alcohol to their Probation officers, or had a positive alcohol breathalyser reading. The male and female subjects differed quite a bit in their overall likelihood of using drugs while on supervision, but for both sexes the most common method of detection was the urinalysis test. The drug detected most often for both men and women was cocaine.

As seen in Table 30, 31 percent of the men used drugs while under supervision, and the male DAP-discharges were most likely to have used drugs (at 36 percent). The percent of male DAP-dropouts who used drugs was lower than that of the other subject types, and by a fairly wide margin (16 percent for the DAP-dropouts, as opposed to the next-lowest group — DAP-completers — whose proportion of drug users was 28 percent).

Table 31 shows that 20 percent of the women used drugs while on supervision. Those of the DAP-incomplete subject type were the most likely to have used drugs (at 23 percent), while DAP-dropouts were least likely to have done so (at 17 percent).

For the men, cocaine and marijuana were the drugs most commonly detected by urinalysis — at 48 and 24 percent respectively — with opiates ranking third (at 10 percent). For the women, the drugs most commonly detected by urinalysis were, as was the case for men, cocaine, marijuana, and opiates — in that order. Sixty-nine percent of the tests were positive for cocaine. Barbiturates and benzodiazepine were the only other drugs for which women had a positive urinalysis result (3 percent each).

Violations of Supervision and Post-Release CCC Placements

Tables 32 and 33 show the percentages of supervised subjects who committed violations of supervision — excluding drug use violations — as well as the percentages of supervised subjects who received CCC placements during their supervision periods. (Note that these CCC placements are not to be confused with BOP CCC placements that occur before the inmate is released from BOP custody.)

In Tables 32 and 33, violations of supervision include failure to report to a Probation officer, failure to report for treatment services, association with anyone involved in criminal activity, abscondence, failure to turn in or falsification of monthly reports, failure to inform Probation officer of police contact or arrest, and involvement in new criminal activity. The male subjects who dropped out of treatment were the least likely to have violated a condition of supervision (at 10 percent), and those who did not complete treatment the most likely (at 22 percent). The non-DAP control subjects were those most likely among female subjects to have committed a violation of supervision (at 15 percent).

Supervised subjects were placed in CCC's — by Probation officers or the court — for three reasons: to undergo more intensive supervision, to spend time there as a consequence of drug use violations, or to serve the placement as an alternative to a new prison term. Looking at the post-release CCC information in Table 32, it is clear that the highest percentage of the men to have received post-release CCC placements (at 7 percent) were the DAP-discharges. There are insufficient numbers of women with post-release CCC placements to discuss meaningfully the differences among subject types.

Drug and Alcohol Treatment and Self-Help Participation

Tables 34 and 35 present information on supervised subjects' participation in post-release drug and alcohol treatment and participation in self-help groups. Because this information was drawn from interviews with U.S. Probation officers, data are available for the 85 percent of male subjects and the 82 percent of female subjects who were under supervision at the time of their release from BOP custody.

A distinction is drawn between contract and non-contract services. The basic distinction between these services is that contract services are provided by an agency that has a contract with the judicial district, whereas non-contract services are provided either by an agency without such a contract or by the Probation officer. In addition, some contract services may be mandated as a condition of post-release supervision by the sentencing court and some are funded through Federal judicial districts. Non-contract services also may be mandated and funded through the Federal judicial districts; however, non-contract services can be voluntary and can be funded by outside sources such as the Veterans Association, American Indian Reservations, State or local government, and so on. In some cases, the releasee pays for services if he or she is able. Some subjects received both contract and non-contract services.

Forty-five percent of the male subjects participated in contract drug or alcohol treatment after release from prison. Three percent of the men participated in non-contract services, and another 3 percent participated in both. The other 49 percent of the male subjects did not participate in post-release drug or alcohol treatment. Looking across subject types, those subjects who terminated their DAP participation by dropping out of the program showed the highest percentage not receiving post-release treatment (at 65 percent). Those who completed treatment and those who did not complete treatment for reasons beyond their control showed the lowest proportions of subjects not receiving post-release treatment (at 41 percent).

Twenty-two percent of the male subjects participated in self-help groups such as Alcoholics Anonymous, Narcotics Anonymous, and the like. Participation in these groups can be mandated or voluntary. Again, DAP-dropouts showed the lowest percentage of participation in self-help groups (at 16 percent), while the treatment completers showed the highest percentage (at 26 percent). However, unlike the case for post-release drug or alcohol treatment, with self-help groups for men, the DAP-incomplete subjects did not show as high a percentage of participation as did the treatment group.

Fifty-one percent of the female subjects participated in contract drug or alcohol treatment after their releases from prison. Three percent of the women participated in non-contract services, and another 3 percent participated in both. Forty-three percent of the female subjects did not participate in post-release drug or alcohol treatment. Looking across subject types, non-DAP control subjects and subjects who terminated their DAP participation by dropping out (at 52 percent and 50 percent, respectively) showed the highest percentage of members not receiving post-release treatment. Those who completed treatment showed the highest percentage of members receiving any post-release treatment (at 64 percent). Twenty-nine percent of the female subjects participated in self-help groups such as Alcoholics Anonymous, Narcotics Anonymous, and the like. The comparison DAP and control non-DAP subjects showed the lowest percentages of participation in self-help groups (at approximately 20 percent each), while the DAP-incomplete subjects and those subjects terminated from the program for disciplinary reasons show the highest percentages (at 46 percent and 43 percent, respectively).

Employment and Cohabitation Status for Supervised Subjects

Tables 36 and 37 present information on supervised subjects' employment and cohabitation statuses. Because this information was drawn from interviews with Probation officers, data are available only for the 85 percent of male subjects and 82 percent of female subjects who were under supervision at the time of their releases from Federal prison.

Forty-four percent of the male subjects who were being supervised were employed full-time for the entire supervision time period (usually 6 months), and another 29 percent were employed full-time for a portion of that time period.[67] Nine percent of supervised male subjects were employed part-time, either for the entire time period or for at least some portion of that time. The 5 percent of subjects who fell into the "ineligible" category were either involved in a school program, retired or disabled, or detained in custody and had very little time available "on the streets" to be employed. The 13 percent of individuals who were unemployed fell into one or more of the following categories: looking for but not finding work, not working due to domestic or child care responsibilities, not looking for work, not working because involved in drug treatment, temporarily unemployed for medical reasons, or on abscond status (with the Probation officer unable to verify employment).

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[67] The period of time was less than 6 months if the subject's supervision was terminated, if the subject was arrested, or if the subject spent any time incarcerated on a detainer for a previous offense.

The male subjects with the highest employment rates were the DAP-complete subjects (49 percent were employed full time for the entire time period) and DAP-incomplete subjects (47 percent full time for the entire time period). The highest unemployment percentages were 26 percent (for DAP-dropouts) and 23 percent (for DAP-disciplinary discharge subjects).

Twenty-eight percent of the female subjects who were being supervised were employed full-time for the entire supervision time period (usually 6 months), and another 35 percent were employed full time for a portion of that time period. Sixteen percent of supervised female subjects were employed part time, either for the entire time period or for at least some portion of that time. The 8 percent of subjects who fell into the "ineligible" category were either involved in a school program, retired or disabled, or detained in custody and had very little time available on the streets to be employed. The 13 percent of individuals who were unemployed fell into one or more of the same categories listed above for male unemployed subjects.

The female subjects with the highest employment rates were those who completed treatment (34 percent were employed full time for the entire supervision time period). The highest unemployment percentages were for non-DAP control subjects (at 23 percent) and for those subjects who were terminated from the program for disciplinary reasons (at 21 percent).

Twenty percent of the male subjects who were supervised lived with a spouse, 15 percent with a common-law spouse, and 65 percent with neither. Those subjects who fell into the "neither" category were either living alone or with one or more of the following: children, parents or guardians, relatives, friends (non-relatives), or unspecified others. DAP-discharges were the least likely to be living with a spouse or common-law spouse while under supervision (at 76 percent).

Twelve percent of the female subjects who were supervised lived with a spouse, 9 percent with a common-law spouse, and 80 percent with neither. DAP-discharges and DAP-completers were the least likely to be living with a spouse or common-law spouse while under supervision (at 87 percent and 86 percent, respectively).

Summary

The description of our research subject characteristics indicates that there were both similarities and differences between male and female subjects. The differences within each gender were most pronounced between the combination of (1) DAP-discharges and (2) DAP-dropouts and DAP graduates. In general, there were few differences between the DAP comparison group and the non-DAP control group.

CHAPTER 7: ANALYSIS AND RESULTS

This chapter describes the approaches taken to model the five selected outcomes and presents the results of these models. The five outcome measures are arrests, arrests or revocations, drug use, Community Corrections Center (CCC) failure, and post-release employment. The chapter begins with a description of the five outcome measures. The next section provides a description of our three different analytic strategies and our method of hierarchically testing the effects of adding blocks of variables. This is followed by a section presenting the procedures we used to handle missing data. Appendix B contains a comprehensive codebook of the variables used in the different analyses.

Subsequent sections present what we believe to be the most significant results of our study, *i.e.*, results for three of the outcome measures: arrests only, arrests or revocations, and drug use. Following these results, we present results for the two other outcomes. The final section focuses on possible institutional differences in program effectiveness. These results are based on analyses exclusively of inmates who entered treatment.

Outcome Measures

Our outcome measures in this report are: recidivism (measured two ways), drug use, Community Corrections Center placement failure, and employment. In order to have one outcome measure for all research subjects, the measure of recidivism is divided into two categories — arrest for a new offense and arrest for a new offense or revocation. The primary source of information for post-release outcomes was telephone interviews with Probation officers. However, almost 20 percent of the research subjects were not released to supervision. We did not want to exclude these individuals from our analyses because we could still gather their arrest information from the NCIC database[68] and — for some — from our BOP databases. However, NCIC data do not consistently identify revocations and do not record information on minor misdemeanors, while this information is consistently recorded by Probation officers. Therefore, our measure of recidivism for all subjects was an arrest for a new offense and our measure of recidivism for supervised subjects was either an arrest for a new offense or a revocation.

Telephone interviews with Probation officers provided outcome information for supervised subjects for the following outcomes measures: arrest for a new offense, arrest for a revocation, post-release employment, and drug use. The outcome information for new arrests was obtained from the automated NCIC database for subjects not released to supervision, for subjects ending supervision before 6 months, and for subjects released to supervision without information from a Probation officer. The third data source — the automated BOP databases — was used to obtain the outcome information on CCC placements.

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[68] Post-release data from a Probation officer were not obtained for 18 individuals released to supervision. For these subjects, arrest data were obtained from the NCIC database.

Excluding the outcome measure of CCC placement failure, data collection for post-release outcomes were scheduled for three points in time following a subject's release: at 6 months, 18 months, and 3 years. If an individual terminated supervision within 3 years of release, the information from Probation officers was collected through the end of supervision. This report contains information through the first data collection point, *i.e.*, through 6 months after release.

Arrest data obtained from Probation officers contained all arrests during supervision, regardless of whether the individual was convicted or incarcerated. The measure of arrest for this report was defined as the first occurrence of an arrest during the first 6 months of release from custody. We analyzed the single most serious offense for individuals with multiple charges at time of first such arrest.[69] In cases where individuals had multiple offenses they were classified as being revoked only when there were no arrests for a new offense**.**

We verified the consistency of information on arrests for a new offense by comparing Probation officer arrest information to NCIC arrest information. This comparison was done for 50 randomly selected subjects with arrest information obtained from a Probation officer. We found only one subject for whom NCIC data showed an arrest not reported by a Probation officer,[70] which gives us reason to believe in the comparability of data from the two sources (*i.e.*, Probation officers and NCIC).

Drug use information was obtained from the Probation officers' records of violations of conditions of supervision for our research subjects. We limit this outcome measure to individuals who were tested for drug use. There were 142 individuals released to supervision who were not included in the analyses of drug use — 134 individuals who had no urinalysis testing and 8 who had missing information on urinalysis testing.[71] Four different violation categories were used as indicators of drug use: a positive urinalysis test for any illegal drug, a refusal to submit to a urine test, a positive breathalyser test, and an admission of drug use to the Probation officer. Although we would ideally like to model the *number* of drug use occurrences, we limit our analyses to the first occurrence. This was necessary because in some districts an individual is revoked after the first positive urinalysis while in other districts individuals are revoked only after repeated

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[69] NCIC codes are ordered by severity of offense, so we used their hierarchy when determining which offense was to be considered the most severe.

[70] The Probation officer was recontacted to verify that we had correctly recorded the arrest information.

[71] Four of the individuals who did not receive urinanalysis testing during supervision were detected as having used alcohol (by means of a positive breathalyzer or admission of alcohol use to the Probation officer). However, information on alcohol-only users is insufficient for inclusion in drug use models because the majority of detected drug use is for that of an *illicit* drug (*i.e.*, there is no regular screening for use of alcohol). Future analyses will assess why some individuals did not receive urinalysis testing.

positives. When an individual was arrested during the 6-month post-release period, we counted the first evidence of drug use only if the detection occurred before the arrest.[72]

Post-release employment was measured as the percentage of time an individual was employed full time during supervision. When an individual was arrested before the end of the 6-month period, the time period considered was limited to the time between the date of release from prison or CCC (*i.e.*, the date of release from BOP custody) and the date of arrest.[73] Probation officers provided information about the starting and ending dates — as well as the number of hours worked per week — for each job the subject held during supervision. This information was used to calculate the total number of days employed full time.

Analytic Strategies

For each outcome variable, we first analyzed the data in a fashion similar to that of past practices. We refer to this as the traditional approach. In the traditional approach, we decompose treatment groups into the following subgroups: inmates who completed a 12-month drug abuse program (GRAD12MO), those who completed a 9-month program (GRAD9MO), inmates who dropped out of a program (WITHDRTX), inmates discharged for a disciplinary reason (DISCIPTX), inmates who — through no fault of their own — did not complete a program (INCOMPTX), and inmates from drug treatment sites who did not volunteer for treatment (COMPDAP). These groups were compared to inmates who were sampled from institutions at which there was no drug abuse program offered — *i.e.*, our non-DAP control subjects.

A second analytic approach was to collapse treatment groups (including inmates who completed treatment and those who did not), combine them with the DAP comparison group, and compare this combined group's outcomes to those of non-DAP control subjects. As mentioned earlier, we refer to this as a Bloom model. For reasons discussed in our conceptualization of the selection bias problem, we believe that this gives a fairer test of the effect of drug treatment than does our traditional model. The Bloom approach compares the *weighted* combined outcomes for the DAP treatment and comparison groups to the outcomes for the non-DAP control group. The weights were the inverse of the probability of selection for the DAP comparison subjects, one for the DAP treatment subjects, and zero for the non-DAP control subjects.[74]

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[72]We considered arrest to be our primary indicator of failure and thus censored all other outcome events after the first arrest.

[73]We also excluded time spent in jail due to a detainer from consideration as time available for employment.

[74]As mentioned in Chapter 4, we weighted the DAP comparison sample to make its size proportional to the volunteering rate both for the period during which the subject was selected and for the site at which the subject was housed.

Finally, we report on an analysis that explicitly models the effects of selection bias and tests for the impact of treatment under conditions of selection bias. As mentioned earlier, we refer to this model as the Heckman model. The statistical models we used varied with the specific outcome being measured and the approach we took (*see* Appendix C).

Following these analyses, we present inter-institutional comparisons of the outcome measures for the treatment subjects. Because one of this study's strengths is that it is a multi-site evaluation, it is important to analyze any potential differences in the drug treatment programs. Such differences are relevant for two reasons. First, if there are program differences, we could use this information as a first step in trying to understand what it is about drug treatment programs that makes them effective. Second, program differences might mask treatment outcomes; for example, if some programs were very effective and others extremely ineffective, we might find no overall treatment effect.

In the inter-site analyses, we looked at two variables that helped us understand the nature of the institutions' varying degrees of "success." One variable measured the amount of time between discharge from a DAP and release from prison. If there was treatment decay for inmates who spent a lot of time in prison after completing a DAP, this variable would measure such a phenomenon. A second variable measured the amount of time between the start of a program and the time an individual entered the program; the longer a program had been established, the larger the value of this variable. This variable helped us find the correlation between the "maturity" of a program and its "success."

Because there were a large number of variables that we could have incorporated into our models, we adopted a hierarchical procedure for adding "blocks" of variables. We began with a simple "base" model that included variables representing treatment effects, as well as such demographic characteristics as age, sex, race, and "background" variables (which we knew from previous research to have been related to criminal recidivism). The background variables included measures of criminal history and employment prior to incarceration. In addition, the base model contained information on drug use and drug dependency. The drug measures were important in understanding the extent to which treatment was related to extent of previous drug use.

All other measures were organized into blocks of related variables. The variables included in each block are listed below. Note that the variables varied depending on the outcome measures and that the last two blocks were not applicable to the models of CCC failure.

- Additional background variables — previous drug treatment, previous alcohol treatment, previous mental health treatment, and diagnoses of antisocial personality and depression.

- Change Assessment variables — six clusters, representing the various profiles on the four change assessment subscales (*see* Chapter 4).

- Additional treatment variables — participation in in-prison self-help groups, in-prison outpatient treatment, CCC transitional services, post-release contract and other treatment services, and post-release self-help groups.

- Supervision variables — CCC placement (including successful and unsuccessful completion), "released to supervision," monthly rate of urinalysis testing during supervision,[75] and CCC placement during supervision.

- Post-release behaviors — cohabitation with a spouse, violation of a condition of supervision (for infraction other than drug use), drug use after release, and hours worked per week during post-release period.

The hierarchical procedure first tested the difference between the base model and each of several other models (which were built by adding one block of variables at a time to the base model). Because the base model was nested within the "built" models when adding blocks of variables, we were able to analyze the effects of the blocks. For those analyses in which we used logistic regression (including the discrete-time hazard models), we compared the differences in minus twice the log of the likelihood ratio test statistic. This statistic is distributed as a Chi-square variable with degrees of freedom equivalent to the additional number of parameters being estimated.

In the case of the ordinary least squares regressions, the difference in R-square was tested with an F ratio. After all of the blocks were tested separately, we added all blocks of variables that passed the first significance test ($p<.05$). We then computed a Chi-square or F-test, comparing these additional blocks' combinations to the base model. If the combined set was significantly different from the base model, this became our final model. We also included effects vectors representing the time periods during which the inmates were released. This release cohort variable was intended to proxy any effects on outcomes that may have been attributable to changes in the release environment over time. For example, economic conditions can vary over time and this can have an effect on criminal recidivism and employment.

The hierarchical testing of blocks of variables was completed using the traditional approach. The blocks of variables identified as significant were then used in the other two analytic strategies — the Bloom and Heckman approaches — that address selection bias issues. We recognized that different blocks of variables may have been significant when using other approaches, but because the focus was upon treatment effects, we opted to present models controlling for the same sets of variables. In the models examining possible site differences in treatment effectiveness the model specification was the same as that used in the traditional models.

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[75] Differences between subject groups in monthly rate of urinalysis testing do not necessarily indicate that some groups were "watched" more than others. This is only one indicator of post-release level of supervision. Future analyses will include measures of the frequency of contact with Probation officers.

In discussing the results, we reported coefficients significant at .05 or less. We used a two-tailed test for all coefficients except those representing effects for DAP in-prison treatment. For these treatment effects we used a one-tailed test because we hypothesized that individuals who received in-prison drug treatment would have more favorable outcomes than would those who did not receive drug treatment. Furthermore, we hypothesized that those who completed in-prison treatment would have more favorable outcomes than those who received but did not complete in-prison treatment.

Ideally, we would have modeled male and female outcomes separately because men and women were in separate treatment programs. However, a thorough representation of male and female differences would have required the inclusion of a large number of interaction terms in the analyses, and the smaller female sample size and the lower female failure rate reduced the power for all analyses of outcome measures excepting employment. Unlike our other outcome measures, employment outcomes did not consist of dichotomies of success and failure, but rather were represented by continuous variables. We therefore were able to model employment separately for males and females. For all other outcome measures we presented results for the combined male and female sample, testing for the significance of gender. However, we conducted additional analyses with male subjects only and report the differences in significant coefficients when comparing the results for the combined model of both men and women to the model with men only.[76]

For the event history and logistic regression analyses we report fit statistics: the concordance rate, the Hosmer-Lemeshow goodness-of-fit index, and Somer's D.

Although we report the results for all significant coefficients, we focus this chapter, as well as the conclusions that follow, on the effects of the in-prison DAP. This is true especially for comparisons among the three modeling approaches. Our comparisons focus upon identifying the consistency of the effects of DAP treatment across the models.

Missing Data

We eliminated six subjects from our analyses because these six answered only a few questions during the Intake1 interview. Because a large number of our variables are based upon Intake1, these subjects would have had considerable missing data, and it would have been impractical to estimate the missing values. Except for these six individuals, most cases had very few missing values.

[76] Tables for male subjects only will be made available upon request. We do not report differences in effects when variables are near significance in one model ($p<.10$) but significant in the other model ($p<.05$). Such differences do not reflect substantial changes in the size of coefficients and their standard errors.

We decided to handle missing data by either adding a category for "missingness" or imputing values, rather than accepting the default method of listwise deletion for the analyses. Without using these methods, listwise deletion would have resulted in too many cases being omitted from the analyses. We felt that the biases resulting from a listwise deletion of a large number of cases would be greater than those resulting from imputation. This is true especially given the much smaller number of cases with imputed values than those with a "missingness" code. In addition, we did verify whether the use of imputed values modified the results. Base and final models with imputed values were compared to analogous models deleting those cases with imputed values, and this was done for both the traditional and Bloom modeling approaches for all outcomes. The treatment effects were not significantly different and thus were not reported in tables. However, we reported the differences in variables significant in the model with imputed values and those significant in the model where cases with missing data were omitted from the analysis.[77]

For categorical variables in which there was a substantial amount of missing data, we treated "missingness" as another category. This allowed us to test explicitly the relationship among missingness, other categories of the variable, and the outcome variables. The most notable variables with large numbers of missing values were the diagnoses of depression and antisocial personality and the motivation for change cluster scores. We collected this information through an interview and survey that was separate from Intake1 (our primary interview) and, due to logistical problems, it was not always possible to administer this additional interview and survey. Other variables with a code of "missingness" in the effects vectors for categorical variables were employment status in the month before incarceration, criminal justice status at time of incarceration, type of post-release treatment, and participation in self-help group after release.

For the variables with a small amount of missing data, we estimated categorical and ordinal values with a hotdeck procedure and continuous values with BMDP's maximum-likelihood missing value procedure. The following were the categorical and ordinal variables and the number of cases for which values were estimated:

- Frequency of drug use for various drugs — cocaine (n=32), crack (n=4), hallucinogens (n=5), heroin (n=12), marijuana (n=5), opiates (n=4), barbiturates (n=5), and stimulants (n=4);
- Previously supported self mainly through illegal sources (n=16);
- Level of education (n=14);
- Previous mental health treatment (n=10);
- Previous drug treatment (n=10);
- Received UA testing during post-release supervision (n=10);
- Received transitional services in a halfway house (n=11);
- With whom living after release (n=10); and
- Number of hours worked in post-release job (applies only to those employed) (n=22).

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[77] Tables of results where cases with missing data were deleted are available upon request. As with our gender comparisons, we did not report differences in effects when variables were near significance in one model ($p<.10$) but significant in another model ($p<.05$).

Three continuous variables were estimated using the maximum-likelihood regression procedure in BMDP. These variables were the estimated log odds of drug dependency (n=17), estimated log odds of alcohol dependency (n=52), and monthly rate of urinalysis testing (n=32).[78]

Traditional Models of Arrest and Drug Use

Event history techniques were used to perform the analyses of arrests, arrests and revocations, and drug use. In an event history analysis, the hazard rate is modeled. The hazard rate is the risk of having an event at a given point in time, t, given that the event did not occur before that point.

Databases appropriate for use in discrete hazard rate logistic regression models were constructed following the principles described by Allison (1984) and Yamaguchi (1991). In our presentation of results, we discussed the odds ratio because this was the easiest parameter to interpret in logistic regression. The odds ratio is simply the percentage of difference between the odds of failure to non-failure, given a one-unit increase in the independent variable. An odds ratio of less than one indicates that the odds of failure decrease when the value of the independent variable increases. On the other hand, an odds ratio greater than one indicates that the odds of failure increase when the value of the independent variable increases.

The time unit used in the discrete event history models is "month." Actually, each month is set artificially in the analyses as 30 days. The variables D_T2, D_T3, D_T4, D_T5, and D_T6 represented the second through sixth months of release, and the coefficients for these variables represented the likelihood of failure in each month as compared to the first month of release.

Most of the independent variables used in the event history models were time-independent. That is, the values for the variables were fixed at the beginning of the observation period and did not change over the post-release observation period. However, several of the independent variables were time-dependent. That is, the values for these variables could have changed from one month to the next during the period of post-release observation.

"Average number of hours employed per week" is an example of a time-dependent independent variable that is used in this analysis. For each month that a research subject remained in the risk set (at risk of failure, defined as arrest, arrest or revocation, or detected drug use), the average number of hours employed per week during that month (EMP_HRS) was computed. The other independent variables whose values could have changed on a monthly basis were (1) a dummy variable (HHSE_STR) indicating whether the research subject had been placed in a halfway house during the month in question, (2) a dummy variable (SUP_REL) indicating whether the research subject was under supervised release during the month in question, and (3) a dummy variable (DIRTY) indicating whether the research subject tested positive for or admitted to drug or alcohol use (or had refused to submit to the test) during the month in question.

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[78]Although substitution for missing values tends to bias parameter estimates toward zero, we did not consider this problematic. Such substitution did not occur frequently.

For arrests, we presented two sets of models. The first contained results for both supervised and unsupervised subjects. The second contained results for supervised subjects only. In the models for supervised and unsupervised subjects together, we were obliged to omit all of the post-release supervision and behavior variables because this information was not available for unsupervised subjects. However, we included a variable indicating whether or not the individual was supervised.

The results of the likelihood-ratio tests are presented in Tables 38, 39, 40, and 41 for arrest as failure for all subjects, arrest as failure for supervised subjects, arrest or revocation as failure, and drug use as failure, respectively. As can be seen in Table 38, adding blocks of variables to control for additional background factors, change assessment, and additional treatment did not significantly improve the base model for arrests when all subjects were considered. On the other hand, adding variables for level of supervision did significantly improve the fit of the model for all subjects.[79] In the model of arrests, when supervised subjects only were considered, we also found that the blocks for supervision and post-release variables were significant (*see* Table 39).

The results in Table 40 demonstrate a similar pattern for models of failure defined as either an arrest or a revocation. For this type of failure, the additional treatment variables significantly improved the fit of the base model. The pattern for male subjects was the same as the pattern noted for all subjects.

The results presented in Table 41 show that the additional background variables and the change assessment variables once again had no effect upon post-release failure, with failure defined in this case as drug use. Again, we see that adding separately the additional treatment variables, the supervision variables, and the post-release variables increased the fit of the model. Consistent with the results presented in Tables 39 and 40, the models for male subjects behaved in a similar fashion to the models for all subjects.

For all three measures of failure for supervised subjects, we see that for men the same set of additional blocks of variables significantly improved the fit of the model, as was the case for models of all subjects. Below are the results for the final models with comparisons to the results for the base models for the three types of failures, using the traditional approach.

Arrest as Failure

Arrest for a new offense was our only outcome measure that included all the research subjects. The information for the other outcome variables was available only from Probation officers and was thus obtained only for the subjects released to supervision (approximately 80 percent of the subjects). While arrest data were obtained from Probation officers for those released to

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[79] We found no additional block of variables significant in the model for men only. We note, however, that our model of arrests for all subjects has fewer variables in this block than our model for supervised subjects. This was because most of the post-release supervision variable information is available only for supervised subjects.

supervision, such data were obtained from the National Crime Information Center automated databases for individuals who were not supervised by a Probation officer and for those who ended supervision in less than 6 months.

The results for the base and final models of a new arrest as an indication of release failure are presented in Tables 42 and 43. Table 42 presents the results for all subjects and Table 43 the results for supervised subjects only.

As can be seen in both tables, the only group that differs from the referent group — that is, subjects who were never at a DAP site and never had the opportunity to receive DAP treatment — was the 9-month program treatment completers (GRAD9MO). This was true in both the base and final models. Completing the 9-month program significantly reduced the odds of being arrested during the first 6 months following release from prison. In the base models of all subjects and of supervised subjects only, 9-month program completers had odds of arrest that were around 40 percent lower than were the odds of arrest for non-DAP control subjects. In the final model with all subjects, the 9-month program completers had odds of arrest that were 41 percent lower than those for non-DAP control subjects, as compared with odds that were 34 percent lower in the model for supervised subjects only.

Even though 12-month completers (GRAD12MO) had lower odds of being arrested than did non-DAP control group members, the difference was not statistically significant in either model. This nonsignificant result was partly the product of a smaller effect than is noted for 9-month completers, and it was affected by the larger standard error associated with the estimate of the 12-month effect.

Although the comparisons are not reported in Table 42 or 43, it is possible to compare the coefficients for the 9-month and 12-month subjects. This is done with a standard t-test. We performed appropriate t-tests to compare the coefficients for both the base and the final models. In neither case were the differences between the 9-month and 12-month subjects significantly different.[80]

The arrest model for all subjects shows that several other base model variables were significantly related to arrest, although none of the drug use variables were related to arrest (*see* Table 42). Age at release (AGERLSE) had the expected effect: older inmates were less likely to be arrested than were all inmates on average. The odds of women (EFEM) being arrested were 28 percent lower than those for all subjects on average. Subjects with prior commitments (EPRIORCM) were more likely to be arrested than were subjects with a history of recent violence (ERECVIOL).[81]

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[80] Details for this and all other t-test comparisons between 9-month and 12-month program graduates are available upon request.

[81] Violence has two dimensions: severity and recency. Previous research by Harer (1994) and others has found that recency of violence is a more important predictor of recidivism than is

There was a significant effect for one of the supervision variables in the final model. Subjects who failed their CCC placements (ECCCFAIL) had 39 percent higher odds of being arrested.[82]

The time coefficients showed a pattern that is not surprising. In comparison to month one, the odds of arrest increased significantly each month beginning in the fourth month (D_T4). However, it appears that the odds of arrest start to decline in month six (D_T6).[83]

Most of the same base model variables significant in the model with all subjects were significant in the model for supervised subjects only (*see* Table 43). Two variables, significant in the model with all subjects, were not significant in the model with supervised subjects only: recent history of violence (ERECVIOL) and CCC placement failure (ECCCFAIL). On the other hand, we found two variables significant only in the model with supervised subjects. Individuals who served longer amounts of time (TIMESRVD) and individuals with higher log odds of alcohol dependency (DEPLOGTA) had higher odds of arrest than subjects on average.

In the model with supervised subjects only, several supervision and post-release behavior variables were significant. Subjects with higher rates of urinalysis tests (UARATE) had lower odds of being arrested. Of the post-release variables, living with a spouse (ESPOUSE) and having more hours of employment during the month of observation (EMP_HRS) both lowered the odds of being arrested. Having evidence of drug use during a month (DIRTY) significantly increased the odds of being arrested.

The various fit statistics at the bottom of Table 42 indicate that in the models for all subjects, both the base and final models had adequate fit statistics. However, we did not find that the final model had a higher concordance rate, Somer's D, or goodness-of-fit index. This is not surprising for the model with all subjects because the final model includes few variables in addition to the base model variables. On the other hand, the fit statistics for supervised subjects only (*see* Table 43) showed the final model to have a higher concordance rate and a higher Somer's D than did the

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seriousness. As such, we chose our violence measure in the base model to reflect recency rather than severity. We also re-estimated the model with seriousness of violence included instead of recency and found that severity of violence is not significantly related to arrest. Because we cannot include recency and seriousness of violence simultaneously due to collinearity between the measures created by those with no history of violence, we include recency of violence in our models for both theoretical and empirical reasons.

[82] Although most individuals who failed their CCC placements did so due to drug use, a few individuals failed their CCC placements because of arrests for new offenses. This arrest is not to be confused with a post-release arrest occurring after release from BOP custody.

[83] The coefficient indicating whether an individual released to supervision (SUP_REL) was not significant. Nonetheless, we ran the same model with supervised subjects only and we found no difference in significant coefficients.

base model. However, the goodness-of-fit index indicates that the final model had a poor fit, whereas the base model had a good fit.

Comparisons between both models, for all subjects and for supervised subjects only, for males and females combined vs. males only, showed very few differences. In both models for men — unlike the models for men and women combined — those with more frequent past use of crack (CRK_FRQ) had decreased odds of failure. Among supervised and unsupervised subjects, the coefficient for failing a CCC placement (ECCCFAIL) was significant in the arrest model for men and women combined but not in the arrest model for men only.

The model for supervised and unsupervised subjects where we deleted cases with missing data was very similar to the model where we did not delete these cases. There were no differences in significant coefficients. A similar comparison for the supervised subjects indicated that, with one exception, there were no differences in significant coefficients. Not being in the labor force just before the most recent incarceration (ELEGITUN) was significant and associated with lower odds of rearrest in the model for supervised subjects where missing data cases were deleted but not in the model with estimated values for missing data.

In summary, the traditional model of failure defined as a new arrest showed that individuals who completed the 9-month DAP had lower odds of failure. The lower odds of failure for 12-month DAP completers was not statistically significant. The likelihood of being arrested was significantly higher in months four through six after release, although the odds ratio decreased between the fifth and sixth months.

Arrest or Revocation as Failure

Unlike the models for failure defined as a new arrest, those for failure defined as either a new arrest or revocation were limited to those subjects under supervision, which constituted more than 80 percent of the subjects.[84]

The results for either a revocation or an arrest counting as a failure are presented in Table 44. In the base model of this type of failure, we see that 9-month DAP-complete subjects (GRAD9MO) had significantly lower odds of failure than did the non-DAP control subjects. However, when the effects of the additional variables considered in the final model were included, the effect of 9-month treatment was no longer significant. None of the other coefficients for a DAP subject group were statistically significant in either the base or final models.

Even though neither treatment group was significantly different from the non-DAP controls, it is possible that the two treatment groups were different from each other. We tested whether this was the case with the t-test comparison method. In neither the full nor the reduced model were the differences in coefficients between the 9- and 12-month groups statistically significant.

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[84]We note that, as will be discussed in Chapter 8, arrests and revocations combined may be considered to be very different phenomena from that of arrests.

For the final model, with failure as measured by an arrest or revocation, many of the same base variables significant in the model of arrests considered alone were significant here as well. Older subjects (AGERLSE) and female subjects (EFEM) had significantly lower odds of experiencing a failure. Having a prior commitment (EPRIORCM) or a longer period of time served (TIMESRVD) increased the odds of failure. A base variable not significant in the model of arrest but significant in the model of arrest or revocation was the effect of having a history of working before coming to prison (EWORKJOB). The effect, though, was not what one would expect. Having a history of work *increased* the odds of failing upon release. In addition, both those who were not in the work force (ELEGITUN) and those who were unemployed (EUNEMP) before their most recent incarceration periods had lower odds of failure upon release.

Compared to all respondents on average, those subjects who received both contract and non-contract treatment (EBOTH) had lower odds of failure. On the other hand, those subjects about whom the information on whether they received post-release treatment was missing (ETXMISS) had significantly higher odds of failure. Subjects with higher rates of monthly urinalysis testing (UARATE) had lower odds of failure, whereas those who had failed their CCC placements (ECCCFAIL) had higher odds of failure.

For arrests or revocations, as with new arrests only, both living with one's spouse (ESPOUSE) and having a higher number of hours worked full time per month (EMP_HRS) were associated with lower odds of failure. The variable DIRTY — denoting evidence of drug use — was significant and indicated that individuals with one or more positive tests were 225 percent more likely to fail. A violation of a condition of supervision for reasons other than drug use (SUPVVIOL), not significant in the arrests model, was significant in the model of arrests and revocations. Having violated a condition of supervision (SUPVVIOL) was associated with increased odds of failure.

Release cohort10 (ECOHO10) members were more likely to fail when failure was defined as having a new arrest or revocation. The time coefficients (D_T3 ... D_T6) showed a pattern that is not surprising. In comparison to month one, the odds of failure increased each month beginning in the third month.

The fit statistics suggest that there was an adequate fit of the models to the data, and this was especially true for the final model. The improvement in concordance and Somer's D between the base model and final model was fairly large. The concordance value increased from 67.7 percent in the base model to 85.6 percent in the final model and Somer's D increased from .378 to .723.

We compared the results for men only to those of men and women combined. Unlike the results for arrests, we found several differences in the results when failure was defined as arrests *and* revocations. A number of coefficients significant in the models for men and women combined were no longer significant in the models for men only: working before incarceration (EWORKJOB), not being in the labor force before incarceration (ELEGITUN), two post-release treatment variables — having had both contract and non-contract services, and having no post-release treatment information (EBOTH and ETXMISS), failing a CCC placement (ECCCFAIL),

and violating a condition of supervision for reasons other than drug use (SUPVVIOL). Several variables not significant in the model with both genders were significant in the male-only model. Individuals with more frequent past crack use (CRK_FRQ) had lower odds of failure, and those with higher odds of alcohol dependency (DEPLOGTA) had higher odds of failure in the male model.

The results for the final model where we delete cases with missing data were very similar to the analogous model with imputed values for missing data. Only two variables not significant in the model with imputed values were significant in the model with cases deleted. More frequent past use of crack (CRK_FRQ), as well as having received contract services only while under supervision (ECTRONLY), were associated with lower odds of failure in the missing data model.

In summary, unlike the results for failure defined as a new arrest only, the results for failure defined as either a new arrest or revocation showed no direct effects for treatment. The pattern of increasing failure rates throughout the 6-month period was similar to that found for new arrests only, with the exception that we did not see a significant decrease in the failure rate in the 6-month period.

First Detected Drug Use as Failure

The results for failure defined as drug use, like those for arrests and revocation, were limited to those subjects who were released to supervision and for whom data were obtained from a Probation officer (n=1,436). This represents approximately 80 percent of the subjects. While we use the term drug use to refer to this outcome, note that drug use includes a positive urinalysis test for any illegal drug, refusing to submit to a test, a positive breathalyser, or admitting drug use (including alcohol use) to the Probation officer.

The results for first drug use are presented in Table 45. As can be seen in the results, only the 12-month DAP completion subjects (GRAD12MO) differed significantly from the non-DAP control group in the base model. In the final model, though, the comparisons of both 9-month (GRAD9MO) and 12-month DAP completers (GRAD12MO) to non-DAP control subjects were significant. Twelve-month DAP completers had odds of drug use that were 49.7 percent lower than were the odds for non-DAP control subjects. The odds of drug use for the 9-month group were 32.0 percent lower than were those for the non-DAP controls. Surprisingly, we found that the comparison subjects from DAP sites (COMPDAP) had odds of failure that were 26.4 percent lower than were the odds of failure for the non-DAP controls. In addition, individuals who did not complete treatment through no fault of their own (INCOMPTX) had odds of failure that were 43.3 percent lower than were the odds of failure for the non-DAP controls.

Once again, we tested whether there were significant differences between the two treatment completer groups themselves. There were no significant differences in either the base or final model.

In the final model we noted that several of the background variables were related to the odds of drug use. Being female (EFEM) and having a prior commitment (EPRIORCM) had the expected effects. Women were less likely than were men to have used drugs in the first 6 months, and subjects with prior commitments had higher odds of drug use. Also, race and ethnicity assumed importance in these models. Compared to all subjects on average, blacks (EBLACK) had 30 percent higher odds of failure and Hispanics (EHISP) had 33 percent higher odds of failure. The pattern of effects for the drug use variables showed that although the variable measuring the odds of drug dependency (DEPLOGTD) was statistically significant in the base model, it was not significant in the final model. Instead, we found the variable indicating frequency of past marijuana use (POT_FRQ) to be significant in the final model. More frequent marijuana use in the past increased the odds of failure.[85]

Two of the treatment variables had significant effects. The most noteworthy is the effect of receiving both contract treatment services and other post-release treatment (EBOTH). Compared to all other subjects, these subjects were more than 100 percent more likely to fail (*i.e.*, use drugs after release). This is a counterintuitive finding, suggesting that there may have been problems with the model specification. Potential problems with model specification included the potential that some of the post-release treatment variables may have measured risk of failure and that we may not have been capturing the time ordering of treatment.[86] The other treatment variable with a significant effect was ENRGENY, which represented having received outpatient treatment while in prison. These individuals were 28 percent less likely to fail.

Similar to our findings for the model of arrests and revocations, subjects who failed their CCC placements (ECCCFAIL) had increased odds of drug use. The effects of the post-release variables were generally consistent with patterns noted for the models of arrests and arrests or revocations (*see* Table 45).

The time coefficients showed a different pattern from that found in either the models of arrests or the models of arrests and revocation. Rather than the likelihood of failing increasing each month and beginning to decline in the sixth month, we see a pattern of decline each month, with the coefficients being significant only for months four and six. In month four, individuals were 34 percent less likely to fail, but in month six, they were 64 percent less likely to fail.

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[85] The variable POT_FRQ was significant at the .10 level in the base model.

[86] Anecdotal information from the research staff interviewing Probation officers indicated that some individuals receiving both services received the non-contract services only after having had a positive urinalysis. Thus, such individuals began supervision receiving contract services only but were referred to receive additional non-contract services *after* the positive urinalysis. This means that receipt of non-contract services may have represented an effect rather than a cause of detected drug use.

The fit statistics suggested that there may have been a problem of fit to the data in the final model. The concordance and Somer's D measures for the final model were fine, but the Hosmer-Lemeshow statistic suggested that the final model did not fit the data very well.

The final model of drug use for men only was very similar to the model for men and women combined. The only coefficient that differed was that for release month four (D_T4). This coefficient was not significant in the male-only model. However, it is noteworthy that in the final model for men, unlike the model for men and women combined, the Hosmer-Lemeshow good-of-fit statistic showed that the model fit the data well (p=.7006).

Comparing the final model with imputed values for missing data to a model with listwise deletion of cases with missing data showed few differences. Only in the model with deleted cases did we find that subjects with histories of recent violence (ERECVIOL) had increased odds of failure. On the other hand, we found that the coefficient representing individuals who entered but did not complete treatment (INCOMPTX) was significant only in the model with imputed values for missing data.

To summarize, the results for failure defined as drug use showed that there were treatment effects. We found positive effects for both the 9-month and 12-month DAP completers, and we found positive effects for those who had treatment available but chose not to participate in treatment (COMPDAP). The pattern of failure by month differed from that found in the previous two models. Rather than seeing the risk of failure increasing after the first month, we saw a pattern of decreasing failure. The time coefficients were significant only for the fourth and sixth months.

Bloom Models of Arrests and Drug Use

This section describes the results for models of failure that compared all research subjects in DAP sites to all research subjects in non-DAP sites, *i.e.*, we took the Bloom approach. As noted previously, these comparisons allowed us to avoid the problems of selection bias caused by the multiple stages in which subjects volunteer themselves into or out of treatment.

Arrest as Failure

As can be seen in the results presented in Tables 46 and 47, the dummy variables comparing the different groups to the non-DAP control subjects were replaced with the variable SUBJECT, which took on a value of one for research subjects who were ever at a DAP site. For the reasons provided above, this variable did not take into consideration whether the research subject actually received treatment.

Examination of the results in Tables 46 and 47 shows that there was no effect for treatment demonstrated in either the base or the final model when considering either all subjects or supervised subjects only.

While the other control variables significant in the traditional model were also significant in the Bloom model, we found additional variables significant in the Bloom model. When considering all subjects or supervised subjects only, we found those with higher education levels (GRADEA) and those who were neither black nor white (RACEOTH) had increased odds of failure, but those with more frequent previous marijuana use (POT_FRQ) and those not having had a CCC placement (ECCCNO) had lower odds of failure.

In addition, in the model with both supervised and unsupervised subjects, more frequent previous use of hallucinogens (HAL_FRQ) was associated with lower odds of arrest. Individuals who had been under criminal justice supervision at the time of arrest (ECJSUPVNY), those with more frequent past use of crack (CRK_FRQ), those with higher log odds of alcohol dependence (DEPLOGTA), and those released to supervision (SUP_REL), had increased odds of failure. Also, the odds of failure increased during the second month of release (D_T2).

Several different variables were found significant in the model for supervised subjects only. Individuals who were black (BLACK), or were not in the labor force before incarceration (ELEGITUN), or who had a greater number of non-drug related violations of their conditions of supervision (SUPVVIOL) had lower odds of failure.

As with the traditional model, the fit statistics for the final models were somewhat suspect. The concordance and Somer's D statistics appeared to be reasonable, but were somewhat inconsistent with the Hosmer-Lemeshow statistic; this indicates that there may have been a problem of fit of this model to the data. We found a higher concordance rate and a higher value for the Somer's D statistics for the model with supervised subjects only than for the model with supervised and unsupervised subjects.

Among both supervised and unsupervised subjects, the model for men only — contrasted with the model for men and women combined — showed differences for two of the drug use variables. In the model for men, frequent past use of crack (CRK_FRQ) was no longer significant, but frequent past use of stimulants (STIM_FRQ) was significant. Among men, subjects with more frequent past use of stimulants (STIM_FRQ) had higher odds of arrest.

A similar contrast for the supervised subjects also showed differences for two drug use variables. In the model for men only, subjects with more frequent previous use of crack and stimulants (CRK_FRQ, STIM_FRQ) had higher odds of failure. Both of these coefficients were not significant in the model for men and women combined.

Models where cases with missing data were deleted did not differ from the models with imputed values for missing data.

Arrest or Revocation as Failure

The results presented in Table 48 demonstrate that there was no treatment effect when failure was defined as either an arrest or a revocation in either the base or final model using the Bloom

approach. This fairly closely replicated the finding for treatment discovered in the traditional model as well, although in the traditional model of treatment, 9-month DAP-complete subjects were significantly different from non-DAP control subjects in the base model specification. In the final traditional model, though, there was no treatment effect, as was true for the results reported in Table 48.

The effects of the other independent variables again show differences when comparing results using the Bloom approach to that using the traditional approach. All the variables, except release cohort 10 (ECOHO10), violations of the conditions of supervision (SUPVVIOL), and the third release month (D_T3), which were significant in the traditional model, were also significant in the Bloom approach. However, the Bloom approach resulted in additional significant variables. Individuals with higher levels of education (GRADEA), more frequent past use of stimulants (STIM_FRQ), and higher log odds of alcohol dependence (DEPLOGTA) had increased odds of failure, whereas individuals having received transitional services (ETSYES) and individuals not having received a CCC placement (ECCCNO) had decreased odds of failure.

The fit statistics of the Bloom model were similar to those of the traditional model with the exception of the Hosmer-Lemeshow goodness-of-fit statistics, which did not show a good fit of the model to the data.

A model for men only did not converge. Nonetheless, we ran several models, deleting one of the post-release variables at a time, and found that there was no effect for treatment as represented by the variable SUBJECT.

When we ran the final model deleting cases with missing values, we found no differences in the results from those obtained through models with imputed values for missing data.

First Detected Drug Use As Failure

The results presented in Table 49 demonstrated a statistically significant effect of treatment in reducing the odds of having a failure (when defined as drug use). This finding did not appear until the additional blocks of variables were entered into the final model. According to the results of the final model, the group of individuals who spent time at DAP sites had odds of drug use that were 37 percent lower than did subjects who were never at a DAP site. Given the nature of this test, this was a conservative estimate of the direct effectiveness of treatment in lowering the odds of drug use.

Similar to the findings for arrests and arrests or revocation, the independent variables were sensitive to the specification of treatment (compare Tables 45 and 49). Being either black (EBLACK) or Hispanic (EHISP), while not significant in the Bloom model, were significant in the traditional model of drug use. In contrast, four variables not significant in the traditional model were found significant in the Bloom model. Individuals of another race (EOTHRACE), individuals with histories of supporting themselves primarily through illegal means (ESUPILL), individuals with higher levels of education (GRADEA), and individuals with histories of more

frequent crack use (CRK_FRQ) had significantly higher odds of drug use in the Bloom model but not in the traditional model.

Several treatment variables and many of the supervision variables not significant in the traditional model were significant in the Bloom models. Individuals who were involved in self-help groups after release (EAAYES), those placed in a CCC during supervision (HHSE_STR), and those with higher rates of urinalysis testing (UARATE) had higher rates of failure in the Bloom model. In addition, individuals who did not receive CCC placements (ECCCNO) had lower odds of drug use.

Living with one's spouse after release (ESPOUSE) was not significant in the Bloom model, as were the time variables D_T4 and D_T6. In contrast, several of the release cohort variables were significant. Those subjects in release cohorts 1 through 6 and release cohort 12 (ECOHO1_6 and ECOHO12) had increased odds of failure, whereas those of release cohort 13 (ECOHO13) had lower odds of failure.

As was the case with the traditional model of drug use, the fit statistics for the Bloom model of drug use suggested that there may have been a problem of fit to the data in the final model. The concordance and Somer's D measures for the final model were fine, but the Hosmer-Lemeshow statistic strongly suggested that the final model did not fit the data very well.

Once again we compared the results for men to those for men and women combined. Base variables not significant in the traditional model but significant in the Bloom model included recency of violence (ERECVIOL), being Hispanic (EHISP), and the log odds of alcohol dependency (DEPLOGTA). These variables were associated with higher odds of failure. On the other hand, variables *not* significant in the Bloom model but significant in the traditional model included past use of crack (CRK_FRQ), having received both contract and non-contract services (EBOTH), having no information on post-release treatment (ETXMISS), and having been involved in a self-help group during supervision (EAAYES).

The results from our models where we deleted cases with missing data differed somewhat from those where we imputed missing values. Several coefficients of variables significant in the model with all cases were no longer significant in the model with listwise deletion of those cases with missing data. These variables included: having had a CCC placement before release (ECCCNO) or during supervision (HHSE_STR), having been involved in a self-help group during supervision (EAAYES), and having had both contract and non-contract services during supervision (EBOTH). The reverse occurred for those living with a common law spouse after release (ECOM_LAW). In the model with missing data cases deleted, ECOM_LAW was associated with lower odds of failure.

Models of Arrest and Drug Use Controlling for Selection Bias

To assess the impact that selection processes may have had on the effect of drug treatment, a survival model was developed for our outcomes of arrest, arrest and revocations, and drug use. The analysis was developed and conducted by William Rhodes of Abt Associates, and the explanation of the procedure and the mathematical basis of the Heckman selection bias models can be found in Appendix C. In this section, we present the results and interpret the significant coefficients for the Heckman models.

The arrest results from the survival model refer to the effects of variables on the survival time. Thus, a significant positive coefficient indicated that a variable increased the survival time of an offender, while a significant negative coefficient implied the opposite. In the previous analyses, where we reported results based on a discrete-time hazard model using logistic regression, the hazard was a reciprocal function of survival. Thus, any coefficient that increased survival should have decreased the hazard of an event. Coefficients that were significant and positive in these analyses should have been significant and negative in the hazard rate analyses if there had been symmetry in the results.

Arrest as Failure

Having settled upon a final model specification in our previous analyses, we used the same final specification for the analyses presented in this section. Thus, all of the variables we used in previous results sections (where we depicted the final model) were represented in the analyses we reported in this section. The only difference in the specifications was the way selection bias was modeled. In the first set of analyses — where the treatment groups were decomposed into those who completed treatment, those who did not complete treatment, those who withdrew, and those who were terminated for disciplinary reasons — we also generated a separate dummy coefficient for DAP comparison subjects who were sampled from the treatment sites. For all these contrasts, the control subjects from the non-DAP sites composed the referent group. In the second set of analyses, we collapsed into one group all subject groups we sampled from DAP sites, namely everyone who started treatment — whether they finished or not — and everyone who was sampled as a DAP comparison subject. This combined group was contrasted with control subjects from the non-DAP sites. Our logic was that this would be a conservative test of treatment's effects, presuming that all subgroups at DAP sites should have been represented at approximately the same proportions as they would among the non-DAP sites.

In this last set of analyses, because we had an explicit test of selection bias, we were able to model all treated subjects together and use the comparison subjects from the DAP sites, as well as the non-DAP control subjects. Comparison subjects from the DAP sites had differing degrees of selection bias affecting them, while the probability of selection bias for control subjects from non-DAP sites should have been zero.

Table 50 depicts the survival analysis for all subjects, and Table 51 depicts the survival analysis for supervised subjects only, where the outcome measure is arrest for a new offense. The

parameter estimate COVARIAN at the very end of the tables represents the level of selection bias. That is,

$$\rho = 1 - \frac{2}{1 + e^{Covarian}} \qquad \textbf{(7)}$$

where ρ is the estimated correlation between ϵ_1 and ϵ_2. The variable COMPLETE represented the effects of treatment, controlling for selection bias. In our model, the variable COMPLETE was the conditional probability of completing treatment, given that someone had begun treatment. Thus, inmates who withdrew or were terminated had lower values for this variable.

The effect of treatment represented by the coefficient COMPLETE was positive and significant in the model for all subjects, as well as in the model for supervised subjects only. Thus, inmates who completed treatment had longer survival times until arrest than did untreated comparison subjects. In other words, the time until their arrests was significantly longer if they had completed drug treatment.

One of the last variables in the tables, COVARIAN, represented the level of selection bias. The coefficient was negative and significant, indicating that inmates who were more likely to fail were selected into the treatment groups. Rather than "creaming" the best candidates for drug treatment, the selection processes captured inmates who were less likely to succeed than were the comparison subjects from the DAP sites.[87] That is, after controlling for other variables that affected recidivism, those who entered treatment seem to have been — based on unmeasured variables — higher risks than were those who did not enter treatment. Thus, the selection bias occurring in this study operated to mute the effects of treatment. By measuring and controlling for this bias, we were able to detect a rather strong treatment effect.

In the model for supervised and unsupervised subjects, the control variables found to be significant were the same as those found significant in the traditional model. A comparison of significant coefficients between the traditional and Heckman models for supervised subjects showed few differences between analogous coefficients in the two models. In the Heckman model, the coefficients for the amount of time served (TIMESRVD), the log odds of alcohol

[87]There was some indication that a "creaming" effect did not occur, most notably among men, from our examination of the motivation to change data. Thirty-eight percent of the men (in contrast to 24 percent of the women) fell into the three least motivated clusters: precontemplation, uninvolved, and reluctant. The admission of "unmotivated" individuals into DAP may have been a function of the perceived and real benefits of the treatment programs.

dependency (DEPLOGTA), and for a drug-related violation of supervision conditions (DRUGVIOL) were no longer significant.[88]

We could have calculated the average or median changes in survival time that result from treatment, but because we were using just the left tail of this assumed log-normal distribution to estimate the mean and median of the entire distribution, such an estimate could have been very misleading.[89] Instead, we estimated the change in the conditional probability of being arrested within the first 6 months, depending on whether a subject was treated or untreated. We evaluated these probabilities at the means of all variables other than the treatment variable. This gave us an estimate of the size of the effect, controlling for all the other variables in the model. When these probabilities were computed for supervised subjects only, where "treatment" was interpreted as the conditional probability of completing treatment, untreated subjects had a probability of .127 of being arrested in the first 6 months, while treated subjects had a probability of .030. The analogous probabilities for supervised and unsupervised subjects were .121 for untreated subjects and .033 for treated subjects.

Arrests and Revocations as Failure

The findings for failure, with failure defined as either an arrest for a new offense or a revocation, using the Heckman approach were similar to those found using the two other approaches (*see* Table 52). When controlling for self-selection, we found an effect for DAP treatment. As with the models for failure defined as a new arrest only, the Heckman model for arrests and revocations found an effect for the level of selection bias (COVARIAN). The negative coefficient for COVARIAN once again indicated that inmates who were more likely to fail were selected into the treatment groups.

The coefficient for COMPLETE, the conditional probability of entering and completing treatment, was positive, as was the case for arrests, indicating that individuals who entered and completed treatment had longer survival times. Most of the coefficients found significant using the Heckman approach were the same as those found significant in the traditional model.

The only coefficient found to be significant in the Heckman model but not in the traditional model was the coefficient for individuals receiving contract services only (ECTRONLY), with these individuals having longer survival times. Several coefficients significant in the traditional model were not significant in the Heckman model. Three coefficients for employment status before the most recent incarceration — employed (EWORKJOB), not in the labor force (ELEGITUN), and

[88] We note that the differences in the two models for the coefficients representing drug use may have been due to the differences in the variables, where one was a time-dependent covariate and the other was the number of drug violations occurring during the post-release time period.

[89] All observations were in the extreme portion — the first 6 months — of the distribution.

unemployed (EUNEMP) — as well as the coefficient for the amount of time served (TIMESRVD) were not significant in the Heckman model.

When we estimated the size of the effect controlling for all the other variables in the model, we found that untreated subjects had a conditional probability of .174 of being arrested or revoked in the first 6 months, while treated subjects had a probability of .047.

First Detected Drug Use as Failure

Table 53 presents the survival model results for first drug use, controlling for selection bias. This model included only those subjects who had urine testing during their post-release period (1,436 of the 1,860 subjects). Once again, the variable COMPLETE, which represented the conditional probability of entering and completing treatment, was significant. The positive coefficient for COMPLETE indicated that individuals who completed treatment had a longer time until first drug use than did non-treated subjects.

As in the models using failure defined as arrest for a new offense and arrests or revocations, the variable COVARIAN — which represents the level of selection bias — was negative. Although COVARIAN was not statistically significant (p=.0936), our experience with selection bias models was that the power for tests of selection bias were very low. While the parameter estimate was not statistically significant, the implied correlation of -.26 seemed substantively significant, and we chose to reject the null hypotheses of no selection bias.[90] This means that individuals who completed treatment were less likely to have a longer survival time than were individuals who did not enter treatment or did not complete treatment. Once again, as with the arrest model, without controlling for selection bias, the effects of treatment would have been attenuated.

The background characteristics found to be significant in the Heckman model differed somewhat from those found significant in the traditional model. There was a significant effect for blacks (EBLACK) in the traditional model but not the Heckman model. On the other hand, two variables not significant in the traditional model were significant in the Heckman model. In the Heckman model, individuals who were involved in self-help groups after release (EAAYES) had shorter survival times and individuals who did not receive a CCC placement (ECCCNO) had longer survival times. This result was unexpected, but it is consistent with the results for the Bloom model and will require further investigation.

Similar to the process used in the analysis of the probability of arrest within the first 6 months, we computed the conditional probability of first drug use within the first 6 months. Untreated subjects had a probability of .367 of using drugs in the first 6 months, while treatment completers had a probability of .205 of using drugs in the first 6 months.

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[90] The correlation of the error term (correlation = 1-2/(1+exp(COVARIAN)) was -.261 for drug use, as compared to -.331 and -.369 for rearrest and rearrest or revocation, respectively. Thus, for both arrests and drug use, the correlation indicated that the highest-risk subjects were selected into treatment.

CCC Failure

The effect of in-prison DAP treatment upon Community Corrections Center outcomes was limited to the research subjects who received CCC placements — approximately two-thirds of the sample. A logistic regression was run on a variable indicating whether an inmate failed to successfully complete his or her CCC placement. (During this placement, an individual was ordinarily housed in a contract halfway house but may also have spent the latter part of this placement on home confinement.)

Logistic regression was used because this analytic method is appropriate when the dependent variable is binary (Menard, 1995). We present the results using the odds ratios, as was the case in our previous logistic regression models of arrest, revocation, and drug use.

Traditional Model

Table 54 presented the results of the Chi-square difference between the base models and the models with additional blocks of variables for all subjects and for males only. There were two blocks of variables — the additional background factors and the change assessment variables — that significantly improved the fit of the model for all subjects and for males only (*see* Table 54).

Table 55 showed the analysis for all subjects who had CCC placements. The final model included only those additional blocks of variables that decreased the log likelihood Chi-square so that the difference between the base model and the model with additional variables produced a Chi-square that was statistically significant.

Looking at the variables that represented treatment groups, we saw that in the base model the groups that differed significantly from the referent group — *i.e.*, non-DAP control subjects — were the 12-month and 9-month DAP-complete groups (GRAD12MO and GRAD9MO). In the final model, we saw that both the 12-month and 9-month DAP-complete group (GRAD12MO and GRAD9MO) coefficients remained significant. In both cases, completion of the program was associated with a lower probability of failure in the CCC. The odds ratio associated with the 12-month program was .495, and the odds ratio associated with the 9-month program was .676. This indicated that inmates who had completed the 12-month program were 50 percent less likely to fail in the CCC than were control subjects from non-DAP sites, while graduates of the 9-month program were 32 percent less likely to fail in the CCC. Although not reported in Table 55, we performed t-tests to compare the two coefficients. This test indicated that the coefficients for the 12-month and 9-month program graduates did not differ significantly from each other.

As can be seen in Table 55, women (EFEM) had lower CCC placement failure rates than did subjects on average. The odds of failure for women were 24 percent lower than for all subjects on average. Among the background variables that we expected to be related to post-release recidivism, prior commitments (EPRIORCM) was significant for this analysis. Inmates who had a prior commitment were 32 percent more likely to fail in the CCC than all subjects, on average. In

addition, inmates who were not in the labor force at the time of incarceration — meaning that the offender was in school, was a homemaker, or was disabled (ELEGITUN) — and inmates who used stimulants more frequently in the past (STIM_FRQ) had lower odds of CCC placement failure than did all subjects, on average.

Three of the six clusters from the motivation for change scores were significant. (Effects vectors were used to represent groupings.) ECLUST1 inmates are those in the "contemplation" stage and can be described as individuals who recognize that they have problems and are considering changing. These individuals were 35 percent more likely to fail the CCC placement. ECLUST2 inmates were characterized as being in the "preparation" stage of motivation for change. This stage consists of individuals who had actively started to change their behaviors. These inmates were almost 39 percent less likely to fail than were inmates on average. Finally, inmates in ECLUST4 represented those in the action phase of motivation for change. This stage involves not only changing one's intemperate behaviors, but also avoiding relapse. These inmates were 51.8 percent more likely to fail in the CCC than were inmates on average. Of the three significant motivation for change coefficients, this last effect does not make sense. One would expect that of all the stages of motivation for change, the action stage should have been associated with the lowest likelihood of failure. We found quite the opposite.

The fit statistics presented at the bottom of Table 55 indicated that both the base and final models had adequate fits and that the measures of fit for both models were quite similar. The concordance measure, which assessed how well the predicted probabilities generated by the model matched the actual outcomes, was 69.5 percent in the base model and 71.7 percent in the final model. The Hosmer-Lemeshow statistics indicated a good fit of the model to the data.

The logistic regression results for CCC failure for men differed somewhat from those for male and female subjects together. The variable GRAD12MO, representing the 12-month DAP-complete group, was significant in the final model for both men and women, but was not significant in the final model for men exclusively. The variable DISCIPTX, representing inmates terminated from treatment for disciplinary reasons, was significant in the final model for men but was not significant in the final model for men and women combined. The odds ratio showed that inmates in this group were 199 percent more likely to fail their CCC placement than were inmates in the referent group (non-DAP controls). Lastly, the coefficient for previous stimulant use (STIM_FRQ) was no longer significant in the model for men only.

The results for listwise deletion of cases with missing data differed somewhat from results for models where we imputed values for missing data. The coefficient for age at release (AGERLSE) was significant in the model with listwise deletion but not in the model with imputed values for missing data. On the other hand, the model with missing data cases deleted showed that the coefficients for two of the motivation for change profiles — ECLUST1 and ECLUST2 — were no longer significant.

Bloom Model

For the next analysis, the Bloom approach was used; that is, all DAP site groups, both treated and not treated, were collapsed into one category (SUBJECT) and the referent group was composed of the non-DAP control inmates. Comparing this logistic regression analysis to Table 55 — which decomposed the treatment effects into program completions, withdrawals, and disciplinary terminations — we found that the models were very similar in most of the statistically significant background coefficients (*see* Table 56). However, after collapsing all of the treatment groups into one group, there was no longer a treatment effect on the probability of CCC failure.

While the previous model indicated that inmates completing either the 9-month or 12-month program were less likely to fail in the CCC, the Bloom model indicated no effect of treatment. This suggested that either the prior effect may have been attributable to the selection bias that operated in the treatment groups or that selection bias masked the effects of treatment.

Several variables not significant in the traditional model were significant in the Bloom model for CCC placement failure. In the Bloom model, subjects who reported ever having supported themselves illegally for at least one year (ESUPILL) were 28 percent more likely to fail than were all subjects, on average. In addition, subjects who had higher levels of past cocaine use (COC_FRQ) had lower odds of failure, whereas those who had higher log odds of illicit drug dependence (DEPLOGTD) had higher odds of CCC placement failure. The frequency of past stimulant use (STIM_FRQ), significant in the traditional model, was not significant in the Bloom model.

As in some of our previous models, the fit statistics indicated that there may have been a problem of fit to the data in the final model. The Hosmer-Lemeshow goodness-of-fit statistic suggested that the final model did not fit the data very well. However, the other fit statistics indicated that the fit was adequate.

The Bloom model for men only did not differ from the Bloom model for men and women combined. However, the model with listwise deletion of cases with missing data differed somewhat from the model with imputed values for missing data. The coefficients for the amount of time served (TIMESRVD) and past use of opiates (OPIA_FRQ), previously insignificant, were significant in the model with deleted cases. Subjects with longer amounts of time served (TIMESRVD) and those with more frequent past use of opiates (OPIA_FRQ) had lower odds of CCC placement failure only in the Bloom model.

Heckman Model

The results using the Heckman approach, which controls for selection bias, are presented in Table 57. Similar to the findings using the Bloom method of addressing selection bias, we found no effect for DAP treatment. All the coefficients significant in the traditional model were also significant in the Heckman model. There was only one coefficient not significant in either the traditional or Bloom model but significant in the Heckman model: higher frequency of past heroin

use (HER_FRQ) was associated with a higher probability of CCC placement failure in the Heckman model.[91]

In summary, the results for CCC placement were not consistent when comparing the traditional approach to the two approaches addressing selection bias. The traditional approach showed that both the 9-month and 12-month program graduates were less likely to fail their CCC placements, while both the Bloom approach and the Heckman approach found no significant effect for DAP treatment. We must point out, however, that the coefficient for the 12-month program graduates was not significant in the male-only traditional model.

Percent of Time Employed Full Time

Employment as an outcome variable was measured as percent of time employed full time for the 6-month post-release reporting period or until arrest (for those arrested). The time detainees spent incarcerated for a previous offense (*i.e.*, an offense committed before release from BOP custody) was excluded as time available for employment. These data were available only for inmates who were released to supervision, as opposed to those released directly to the community without supervision and those going straight to detainers.

The analysis excluded 19 men and 6 women whose post-release employment statuses were unknown. In addition, subjects who were unemployed because they were retired, disabled, homemakers, or in school full time were not included in these models, and this group consisted of 63 men (4.9 percent of the men released to supervision) and 21 women (7.5 percent of the women). For men, the final analysis sample size was 1,168, and for women it was 254.

Traditional Model

An ordinary least squares regression was used to analyze the percentage of time inmates were employed full time.[92] Analyses were conducted separately for men and women. The results of F tests of significance for the addition of the five different blocks of variables using the traditional approach were contained in Table 58. The results indicated that for men, there is only one block of variables — the additional background variables — that did not produce a statistically significant increase in R-square. On the other hand, for the women there were two blocks of variables that did not result in a significant increase in R-square: the change assessment variables and the post-release supervision variables.

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[91] We note, however, that the coefficients for HER_FRQ was marginally significant (p=.10) in the traditional model.

[92] Our dependent variable — percent of time employed full time — has a distribution that is both limited on the left and truncated on the right. Although it would have been more appropriate to have used a truncated Tobit analysis to analyze these data, ordinary least squares procedures typically were robust enough to use on this type of data.

Table 59 depicted the result of the analyses for men and Table 60 represented the result of the analyses for women. The fit of both final models was adequate, and the adjusted R-square for both the men's and women's final models was .19.

Among men, only one of the treatment groups, DISCIPTX, in the base model was significantly related to the employment outcome, but the level of significance was marginal (.10 level of significance). Men discharged for disciplinary reasons (DISCIPTX) spent a smaller percentage of their post-release periods employed full time. Inmates who served more time (TIMESRVD) worked less in their first 6 months after release. Inmates who had used illegal activities to make money prior to their incarceration (ESUPILL) worked less after their release. Inmates who had jobs prior to their current commitments (EWORKJOB) worked during a higher percentage of their post-release periods. Also, inmates with higher levels of education (GRADEA) worked a larger percentage of the time during their post-release period.

Looking at the drug variables, inmates who claimed they had used hallucinogens (HAL_FRQ) frequently in the past worked less in the post-release period than did inmates who had lower past frequency of this kind of drug use. Furthermore, inmates who had higher log odds of drug dependence (DEPLOGTD) worked a greater percentage of time during their post-release periods.

Inmates who received contract treatment services under supervision (ECTRONLY) were employed more than were inmates who did not receive these services. Furthermore, inmates who either failed their Community Corrections Center placement (ECCCFAIL) or who had not been placed in a CCC (ECCCNO) worked less than did all inmates, on average.

Finally, those who had a higher number of supervision violations (SUPVVIOL), excluding violations for drug use, worked less time after release than did inmates on average. Inmates from cohorts released during the first six quarters of release dates (ECOHO1_6) worked full time less than did all subjects, on average.[93]

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[93] The effects vector for cohort was included to control for any differences in the national socioeconomic environments facing subjects at release. Because the coefficients for most of the different cohorts did not differ significantly from the overall unadjusted mean, we felt fairly confident that there was very little cohort effect, at least as measured in this indirect manner. The finding that one cohort, of the many different cohorts, differed significantly from the mean may have indicated a real effect. Then again, it may have reflected the greater chance of Type I error (rejecting the null hypothesis with sample data when the null hypothesis was actually true in the population) when examining the multiple coefficients generated with the effects coding scheme. Because we did not have any theoretical reason to expect that any cohort differed from the others, we did not feel comfortable in placing much importance on this coefficient.

There were no significant differences between the models where we deleted cases with missing data and the models where we used all cases and imputed missing data.

The data for women were similar in that there were no treatment effects on employment in the final model (*see* Table 60). Hispanic women (EHISP) spent a smaller portion of their post-release time employed full time. Two of the work history variables (EWORKJOB and ELEGITUN) were significant. Women who had worked prior to their commitments or had not been in the labor force worked a greater percentage of time after their releases. Women with frequent past use of cocaine (COC_FRQ) had longer periods of full-time post-release employment. For whatever reason, women for whom post-release information was unknown (EJOB_UNK) worked a smaller percentage of their post-release time. Drug dependence (DEPLOGTD), no previous drug treatment (EPSTDGTX), and no previous mental health treatment (EPSTMHTX) were all associated with lower employment levels. Women who received drug treatment transitional services in a halfway house (ETSYES) worked more after their release. Women who participated in Alcoholics or Narcotics Anonymous (EAAYES) were more likely to work after release.

The only coefficient that differed, when comparing models for women with and without deletion of cases with missing data, was educational level (GRADEA). A higher level of education was associated with longer periods of employment in the model where we deleted cases with missing data.

Bloom Model

Using the Bloom approach, we found that for both men and women the results were similar to those obtained using the traditional model. As was the case in the traditional model, we did not find treatment effects on post-release employment (*see* Tables 61 and 62). We did find, however, differences in the control variables that were significant in the Bloom model.

Among men, six variables not significant in the traditional model were significant in the Bloom model. Variables that were associated with longer periods of post-release full-time employment were (1) ECLUST6 (which represented the individual who did not endorse any of the motivation for change scales), (2) having received transitional services during a CCC placement (ETSYES), and (3) having received a halfway house placement during supervision (HHSE_STR). The variables associated with shorter periods of employment were (1) having a history of past violence (EPASTVIOL) and (2) more frequent use of cocaine and barbiturates in the past (COC_FRQ, BARB_FRQ). Frequent use of hallucinogens in the past (HAL_FRQ), which was significant in the traditional model, was not significant in the Bloom model.

Among women, the variables significant in the Bloom model but not significant in the traditional model were (1) educational level (GRADEA), (2) past use of marijuana (POT_FRQ), (3) having a diagnosis of antisocial personality (EDIAGASP), and (4) having drug-related violations of conditions of supervision (DRUGVIOL). All, with the exception of antisocial personality, were associated with longer periods of post-release employment. Only one variable (EAAYES, *i.e.*, post-release involvement in a self-help group) that was significant in the traditional model was not significant in the Bloom model.

Bloom models where we excluded cases with missing data showed few differences from models where we imputed values for missing data. Among men, the models excluding missing data showed that frequency of past cocaine use (COC_FRQ) was no longer significant and that release cohort 9 (ECOHO9) was associated with shorter periods of employment. Among women, we surprisingly found the coefficient for 12-month DAP graduates (GRAD12MO) to have been significant and negative when cases with missing data were deleted. Women who graduated from a 12-month program were shown to have shorter periods of post-release employment.

Heckman Model

Our model of employment for men in which we controlled for selection bias showed that there was no significant DAP treatment effect and that there was no evidence of selection bias (*see* Table 63). We also found differences between the traditional and Heckman approaches in the control variables that were significant. More frequent past use of hallucinogens (HAL_FRQ), significant in the traditional model, was not significant in the Heckman model. Four variables not significant in the traditional model were significant in the Heckman model. Age at release from prison (AGERLSE), not having been in the labor force just prior to incarceration (ELEGITUN), and having missing information on post-release treatment (ETXMISS) were associated with working full time a smaller percentage of the time after release. Having received transitional services during a halfway house placement (ETSYES, the fourth of the variables mentioned above) was associated with working full time a greater percentage of the time after release.

The Heckman model for women was similar to the model for men in that it found neither a treatment effect nor evidence of self-selection (*see* Table 64). A few of the control variables that were significant in the Heckman model were not significant in the traditional model. In the Heckman model, women who were involved in prison-based self-help groups (ENRSUPY) spent a smaller percentage of time after release in full-time employment. In addition, in the Heckman model, more frequent past use of hallucinogens and marijuana (HAL_FRQ, POT_FRQ) were associated with longer amounts of time employed full time. Lastly, involvement in a self-help group after release (EAAYES), significant in the traditional model, was not significant in the Heckman model.

In summary, we found no effects for DAP treatment on full-time employment after release from BOP custody. This was true for both men and women when using either the traditional, Bloom, or Heckman approach. We did find some positive effects for other treatment received. Women who received transitional services and men who received post-release contract services spent a greater percentage of their post-release periods employed full time. With the exception of the variable capturing a history of previous employment, which is positively associated with post-release employment, the variables significantly related to post-release employment differ between men and women. This suggests that the pathway to post-release employment is different across gender.

Inter-Institutional Comparisons of Outcome Variables

This last section describes the results for models in which subjects who entered treatment were considered. Two models were presented for each of the outcome variables. The purpose of the first model was to compare treatment subject group variation among institutions in terms of their odds of failure — when defined as arrests, arrests or revocations, drug use, and CCC placement failure — and the percent of time their subjects spent employed full time.[94] This model also included a measure of the amount of time served after discharge from the program, as well as a measure of program maturity. The various treatment groups were categorized in a manner similar to that used in the previous analyses. The groups were categorized into DAP-complete, DAP-dropout, DAP-disciplinary discharge, and DAP-incomplete, with the DAP-incomplete group serving as the referent group. However, because these were effects vectors rather than dummy codes, the interpretation differs. In this analysis, a significant coefficient for either of the treatment groups signified that the group differed from all subjects, on average.[95]

The final model supplemented the first by including two different sets of variables than were used in the previous analyses. The first set of variables consisted of the individual-level control variables included in all the base models.[96] The second set of variables consisted of the additional background, change assessment, treatment, supervision, and post-release behavior blocks of variables found to be needed in the final model in the previous analyses for the particular outcome variable being considered. Because the block of variables found to improve significantly the fit of the model as compared to the base model varied by outcome, this variation was incorporated into the inter-institutional comparisons.

The final model tested whether any institutional differences discovered in the first models held up after controlling for the individual characteristics of the subjects in treatment and the block of variables representing the relevant additional block(s) of variables. It should be noted that individual selection bias issues were not problematic in the models considered here because the analyses were limited to those subjects who entered treatment. Selection bias would have been problematic only in models that compared people who entered treatment to people who did not.

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[94] Comparing outcomes across differing programs is problematic because there were differing criteria for acceptance into the programs. Field notes from researchers suggest that there was little variation in program acceptance but that there was anecdotal evidence of variation in program retention. In future analyses we will attempt to incorporate information on program retention rates.

[95] In previous analyses, the referent group was created using dummy codes and thus the interpretation of coefficients had to do with a particular group differing from the referent group rather than from all subjects on average.

[96] These models omitted gender as a base variable because it was redundant with the effects vectors for each site.

Arrests and Drug Use

For each of the three types of failure — new arrests only, arrest or revocation, and drug use — the final model was a significant improvement in fit over the model with only the site effects included. This can be easily computed from the -2 Log Likelihood statistics presented in the tables. For example, in Table 66 we see that -2 Log Likelihood for the sites-only model was 730.505 with 24 degrees of freedom. By comparison, -2 Log Likelihood for the full model was 584.937 with 66 degrees of freedom. Even with a difference of 42 degrees of freedom in comparing the respective Log Likelihoods, the difference of 145.568 was clearly significant at $p < .05$.

Arrest as Failure

This discussion about the models of failure for all treatment subjects focuses on the effect each institution had in raising or lowering the log odds of failure for all subjects who entered treatment at that institution. As such, we did not focus upon interpreting the effects of the different treatment groups (COMPLETE, DISCIP, and WITHDRAW), but instead we highlighted the effects of the institutional effects vectors. The coefficients for the institutional effects vectors demonstrated the impact that the *institutions* had in raising (positive coefficient) or lowering (negative coefficient) the odds ratios for their respective treatment subjects in comparison to all subjects from DAP sites when taken together. Specifically, the coefficients show how much the subjects from that institution differed in log odds from the overall, unadjusted log odds for all treatment subjects.

The sites-only model of arrests omitted cases from five sites. Four of these sites — Danbury and Dublin (female sites), as well as LaTuna and Three Rivers (male sites) — were deleted because there were no failures there, thus creating zero cell counts. Cases from Alderson (EALDSITM) were deleted because of the covariate pattern that led to excessively large parameter estimates.[97]

Regarding the sites-only model of arrest as failure for both supervised and unsupervised subjects, the results in Table 65 demonstrated that no institution had a significant coefficient at the .05 level. Individuals who spent more time in prison after leaving the treatment program (TIMETORL) were less likely to have been arrested, although the odds ratio was only 4 percent lower than it was for all treatment subjects on average. In the base model of arrest as failure for supervised subjects only, we found that one site's subjects — the site of Oxford (EOXFSITM) — had significantly higher odds of rearrest (*see* Table 66). We found the same effect for time in prison after treatment (TIMETORL).

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[97]We note that in the model with both supervised and unsupervised subjects, the sample size for the sites with zero cell counts was small. There were 30 cases from Danbury, 19 from Dublin, 14 from LaTuna, and 27 from Three Rivers. Also, we note that of the 139 cases from Alderson (a minimum-security female site), there were only five individuals who were arrested for new offenses, thus indicating a low failure rate for this site.

The results (reported in Table 65) for the final model indicated again that there were no significant coefficients for any sites. In addition, when controlling for the individual characteristics of the treatment subjects at each site, the coefficient for the amount of time spent in prison after discharge from DAP (TIMETORL) was not significant. The results for supervised subjects only in Table 66 once again showed no significant coefficient for any site or for time spent in prison after discharge from DAP.

The fit statistics indicated that the final models fit the data in a superior fashion than they did to the base model, with the exception of the model for supervised subjects, where the Hosmer-Lemeshow goodness-of-fit statistic was better for the base model.

An analogous model for men only showed an identical pattern of site effects for supervised subjects only.

Arrest or Revocation as Failure

In the sites-only model of failure with failure defined as either an arrest or a revocation, the results in Table 67 showed that two sites had significant coefficients: EMNASITM (Marianna) and ESHESITM (Sheridan). Inmates from Marianna had odds of failure that were 102 percent higher than subjects on average, and inmates from Sheridan had odds of failure that were 128 percent higher. Once again, a greater amount of time spent in prison after discharge from the DAP (TIMETORL) was associated with a lower odds of failure. This finding is contrary to what program providers had anticipated.

Controlling for the individual-level differences in subjects at the different treatment sites, we once again saw that the sites-only model results were misleading. The subjects at neither of the two sites significant in the sites-only model were significant in the final model. On the other hand, the results for the final model in Table 67 suggested that treatment subjects from Phoenix (EPHXSITM) and Rochester (ERCHSITM) had higher odds of failure. Once again, TIMETORL, the time spent in prison after involvement in DAP, was not significant in the final model.

The fit statistics did not indicate any problems of fit to the data for either the base or final models. Once again, the fit statistics provide further indication that the final model was superior in performance to the base model.

As was the case with failure defined as a new arrest, the models for men and women were similar to those for men only. However, in this case the model for men only showed only one of the sites found significant in the combined model to remain significant in the model for men only (Phoenix — EPHXSITM).

First Detected Drug Use as Failure

Once again we were faced with limitations in the analysis due to sites having zero cell counts or excessively large parameter estimates. A total of 14 cases were omitted from the analysis.

There were no drug use failures at Dublin (EDUBSITM). For Danbury (EDANSITM), with a sample size of eight, the parameter estimate was excessively large. The results for the sites-only model in Table 68 suggested that treatment subjects at LaTuna (ELATSITM), Marianna (EMNASITM), and Oxford (EOXFSITM) had odds ratios for drug use that were higher than was the norm for all treatment subjects. In addition, we found that a longer amount of time served after discharge from treatment (TIMETORL) was associated with lower odds of failure. As in the previous model for arrests or revocations, when we controlled for the individual-level characteristics and the post-release treatment and supervision received, there was a very different pattern of effects for the sites. The results for the final model in Table 68, though, showed that the effects for ELATSITM, EMNASITM, and EOXFSITM were no longer significant.[98] On the other hand, there was now a significant coefficient for EMCKSITM (McKean). Individuals from EMCKSITM had odds that were 95 percent lower than those for all treatment subjects on average. We do note, however, that the coefficient for EMCKSITM was near significance in the sites-only model (p=.07). Once again, the amount of time spent in prison after DAP involvement was not significant in the final model even though it was significant in the sites-only model.

The concordance rate and Somer's D measures of fit suggested that the final model provided a better fit than did the sites-only model. The concordance rate using the sites-only model was 66 percent, whereas it was 83 percent in the final model. The Hosmer-Lemeshow statistic indicated an adequate fit for both models.

As was the case with failure defined as a new arrest, the site effects in the drug use models for male and female treatment subjects combined were similar to those for men only.

CCC Outcomes

This section reports the results for models of CCC failure in which only subjects who entered treatment at one of the DAP sites and who received a CCC placement were considered. Two models were presented in Table 69; the first model compared only how treatment subjects varied across institutions in terms of their odds of failure.[99] The second model supplemented the first by including the sets of variables used in the traditional and Bloom full models — the variables found to produce a significant difference in the fit of the model over the base model. The final model was a significant improvement in fit over the model with site effects only.

In the site model, only two institutions had significant coefficients. Individuals from Lexington (ELEXSITM) — a female site — had lower odds of failing their CCC placements, but those from LaTuna (ELATSITM) — a male site — had higher odds of failing. The effects for these sites

[98] We note, however, that the coefficients for LaTuna and Marianna were near significance (p<= .10).

[99] This analysis excludes four female subjects from Danbury. All four of these subjects completed their CCC placements, thus creating a zero cell.

remained when we controlled for all the variables included in the full model. The log odds ratio associated with ELEXSITM indicated that women from this site were 93 percent less likely to fail their CCC placements. The odds ratio for ELATSITM, on the other hand, indicated that these male inmates had an odds ratio of failing that was 780 percent higher than was the case for all subjects on average.

The equivalent model of male treatment subjects indicated that while the coefficient for LaTuna (ELATSITM) remained significant, there were two additional sites with significant coefficients — Butner (EBUTSITM) and Fairton (EFAITSITM). Individuals from Butner (EBUTSITM) were 58 percent less likely to fail and individuals from Fairton (EFAISITM) were 50 percent less likely to fail their CCC placement than were all treatment subjects on average.

Employment Outcomes

The following section presents the inter-institutional results for post-release employment as the outcome of interest. As with the other employment models, these are presented separately for men and women.

An F test for the significance in the R-square difference between the base and final model for both men and women indicated that the R-square was significantly higher in the final model. The sites-only model for men in Table 70 showed several sites to be significant at the .05 level. For EMRGSITM (Morgantown's) and EYANSITM (Yankton's) male DAP treatment subjects, there was a positive relationship with percent of time employed full time. On the other hand, the relationship was negative for EFAISITM (men at Fairton) and EOXFSITM (men at Oxford).

When controlling for other variables in the full model, none of these sites remained significant. Rather, we saw a positive coefficient for EMNASITM (males at Marianna). In the sites-only model, COHTIME — the time served in prison after completion or discharge from the DAP — was significant at the .01 level. The longer a male inmate stayed in prison after treatment, the less time he was likely to be employed full time after release. However, COHTIME was not significant in the final model. In the model excluding cases with missing data, the coefficient for Marianna was not significant.[100]

A similar analysis for women — reported in Table 71 — uncovered no site effects in the sites-only and full models. This is not surprising given there were no treatment effects on post-release employment for women in either the traditional or Bloom models.

[100] We did not discuss models where we deleted cases with missing data for the other outcome measures presented in this section on inter-institutional comparisons. Earlier in this chapter, we noted that discussion of such models would occur only when there were differences between these models and the models with estimated values for missing data.

Summary of Results

The following provides a summary of the effects of treatment for each outcome measure, and also it highlights other factors consistently related to these outcomes.[101] While the background factors related to the outcomes varied across the models, we noted that there was great consistency across all three models for those factors known to be important predictors of the outcome being considered. Most of the differences, not surprisingly, occurred in the Bloom model, where we had to weight comparison cases. There were some differences when we compared models of men and women combined to those of men only and when we compared models with imputed values for missing data to models with listwise deletion of cases with one of more missing values. It is noteworthy, however, that these differences generally did not arise for either the in-prison treatment indicators or factors known to be related to the outcome measures.

Arrests

The traditional approach showed a positive effect for 9-month program graduates but not for 12-month program graduates when modeling arrests for either all subjects or supervised subjects only. We found inconsistent results using the two different approaches that addressed selection bias issues. We found no DAP treatment effect using the Bloom approach, but we did find an effect for DAP treatment completers when we used the Heckman approach, which directly controls for selection bias. In addition, the Heckman approach found evidence for selection bias: individuals who completed treatment were at a higher risk of arrest.

Women had lower failure rates across all three modeling approaches. Two background variables were consistently related to post-release arrests: age at release and prior commitments. Older individuals had reduced odds of failure, whereas those with prior commitment had higher odds of failure. Among subjects released to supervision, we found several post-release factors consistently related to arrest. Higher rates of urinalysis testing, a greater number of hours worked after release, and living with a spouse were associated with reduced odds of arrest.[102]

Arrests and Revocations

When we defined failure as an arrest for a new offense or revocation, we found positive effects in only one of our analytic strategies, the Heckman approach. This approach also found a significant

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[101] In our summary, we considered a variable consistently significant across all three modeling approaches when a variable was significant at $p<.05$ in all three analytic strategies or when a variable was significant at $p<.05$ in two analytic strategies and marginally significant at $p<.10$ in the third analytic strategy.

[102] Because the Heckman models are not event history models with time-dependent covariates, the variable representing post-release employment is the percentage of the post-release period employed full time.

coefficient for the selection bias factor: individuals who entered and completed treatment were at higher risk for failure. The three background factors consistently related to our outcome measure of arrest for a new offense — gender, age at release, and prior commitments — were associated also with our outcome of arrest for a new offense or revocation. Several additional treatment variables also were found significant across all three analytic strategies. Individuals who received both contract and non-contract treatment after release had lower odds of failure whereas those with post-release treatment information missing had higher odds of failure. Concerning the supervision variables, individuals who failed their CCC placements had higher odds of failure whereas individuals with higher urinalysis testing rates had lower rates of failure. Not surprisingly, individuals with drug-related supervision violations had higher odds of failure. Lastly, individuals who were employed a greater percentage of time after release were less likely to have an arrest or revocation.

Drug Use

Using all three modeling approaches, we found treatment effects for failure when it was defined as drug use. Having received DAP treatment lowered the odds of post-release drug use. In the traditional model we found positive effects of DAP treatment for both the 12-month and 9-month program graduates. We also found positive effects for DAP treatment in the Bloom and Heckman models.

Several background variables were consistently related to post-release drug use: being female, having prior commitments, and having higher levels of past use of marijuana. The additional treatment variables significant across all three models included in-prison outpatient treatment and a combination of contract and non-contract post-release services. Receiving outpatient treatment decreased the odds of drug use, whereas receiving both contract and non-contract services increased the odds of drug use. In all three models, the supervision variable of CCC placement failure increased the odds of drug use. The significant post-release behaviors included having non-drug related supervision violations and post-release employment. All of these behaviors, except for supervision violations, were related to decreased odds of failure.

CCC Placement Failure

Our results for CCC placement failure were not consistent across the three modeling approaches. We found positive effects for DAP treatment when we used the traditional approach, but not when we used either of the two approaches that addressed selection bias issues. The Heckman model did not uncover any effects for self-selection. Across all three modeling approaches, several background variables were consistently significant. Women had lower odds of CCC placement failure. On the other hand, individuals with prior histories of incarceration had higher odds of CCC placement failure. We also found several of the motivation for change clusters significant. Inmates in the “contemplation” and “action” phases of motivation for change were more likely to fail their CCC placements, whereas those who were in the “preparation” phase of motivation at the time of admission to treatment were less likely to fail.

Employment

Among both men and women, we found no significant effects for DAP on post-release employment and no evidence of selection bias. The other factors associated with more positive post-release employment outcomes differed between men and women.

Among men, we found positive effects for receiving post-release contract services. The background factors consistently related to increased time in full-time employment in all three modeling approaches included a higher education level, being employed just before the most recent incarceration, higher log odds of drug dependency, and a longer time served. The background factor negatively related to post-release employment was having previously supported oneself mainly through illegal sources. Not having received a CCC placement and having failed a CCC placement were consistently negatively related to post-release employment, as was violating a condition of supervision for reasons other than drug use.

In all three models for women, we found that having received transitional services during a halfway house placement was related to longer periods of post-release employment. The background factors negatively related to post-release employment across all three modeling approaches were being Hispanic, having higher log odds of drug dependency, having an unknown employment status before incarceration, and having a history of previous drug or mental health treatment. Another factor positively related to post-release employment included being employed before the most recent incarceration. Unlike our results for men, there were no post-release behaviors related to increased success in post-release employment for women.

Inter-Institutional Comparisons

When controlling for background factors, we found little evidence of differences among DAP treatment subjects in outcomes due to site effects. There were no site effects for arrests for a new offense or for post-release employment among women. For the other outcome measures, site effects were limited to one or two sites and were not consistent across the various outcome measures. We note that our attempt to identify site effects was hindered by small sample size at several sites. These sites often could not be included in the analyses. In addition, several sites had no failures and also could not be included in the analyses.

CHAPTER 8: SUMMARY AND CONCLUSIONS

The results of this preliminary analysis compel us to join the growing chorus of researchers who hold that treatment programs in prison, when properly implemented, can and often do work. Our findings consistently showed that the residential Drug Abuse Treatment Programs (DAP's) in the BOP contributed to a reduced likelihood of post-release failure, when failure was defined as renewed drug use. In addition, our findings showed positive effects of treatment on arrests for a new offense in two of our three modeling strategies — both of which controlled for selection bias. We also found that DAP treatment had a positive effect on arrests and revocations, although this finding was limited to the results obtained by using the Heckman approach, one of the two approaches that controlled for selection bias.

We found marginal support for the proposition that successful completion of a DAP increased the likelihood of completing a CCC placement successfully; the positive effect was found in only one of our three modeling strategies, the traditional approach. We had limited confidence in this finding because it was limited to an analytic strategy that does not address selection bias issues. Lastly, we do not find support in this analysis for the proposition that DAP treatment had a significant effect on post-release employment.

We also found that men were more likely to fail than were subjects on average, when failure was defined as either a new arrest, an arrest or revocation, post-release drug use, or an unsuccessful completion of a CCC placement.

Selection bias is a pernicious problem in conducting evaluation research, one that is often not recognized or adequately considered and addressed in research design. We attempted to deal with selection bias in as many as three different ways. While this has added significantly to the materials presented in this report, it has provided greater confidence in our results and in the conclusions we have drawn because of the increased rigor and scrutiny we brought to bear on our empirical analyses.

The substantive results across the two modeling approaches addressing selection bias issues were consistent for our outcome measure of drug use but not for our outcome measure of arrest for a new offense or of arrests and revocations. Thus, it is clear that we cannot assume that two different methods of addressing selection bias will yield identical results. This finding suggests the need to use various analytic procedures when conducting treatment evaluations in order to increase confidence in our findings. Although the findings were similar when comparing a modeling strategy that does not control for selection bias (*e.g.*, our traditional model) with ones that do incorporate such controls, for one of our outcome measures — arrests — we point out that such consistency was fortuitous. In the traditional model of arrests, we found weak evidence for a treatment effect. It is likely that such an effect was uncovered in this model despite the direction of selection bias because, as was apparent in the Heckman model, the effect was a strong effect. Lastly, we note that the Heckman model was able to provide important information about the nature of self-selection into treatment. We found that our treatment subjects were at a

higher risk of experiencing negative outcomes than were individuals not self-selecting into treatment when our outcome measures were arrest for a new offense, arrest or revocation, and drug use. We did not, however, find evidence of selection bias for our outcome measures of CCC placement failure and post-release employment.

The positive results we found for arrests, arrests and revocations, and drug use using the Heckman approach — our most efficient method of detecting treatment effects — occurred within a multi-site context of 20 programs serving both male and females and operating within different security levels and different geographic regions. Thus, our results have greater generalizability than would a study with treatment subjects from one or two treatment programs.

We organized this discussion section around the effect that selection bias had on the different outcomes of interest: a new arrest, a new arrest or a revocation, evidence of drug use, CCC placement failure, and post-release employment behavior. We also summarized the findings related to the effectiveness of the DAP's as implemented at different BOP facilities.

Selection Bias Reviewed

Traditional Approach

Before discussing the results for the different outcomes, we will briefly review our methodological strategy regarding selection bias. Our first approach related to what we called the traditional models. In the traditional approach, our outcome models compared treatment subjects — those identified as having completed treatment, having voluntarily withdrawn from treatment, having been removed from treatment for disciplinary reasons, or having not completed treatment for other reasons — to non-DAP control subjects. The logic of these models was that comparisons to non-DAP control subjects were most defensible, as non-DAP control subjects never had the opportunity to refuse treatment. DAP comparison subjects, in contrast, had the opportunity to accept treatment but did not do so. Their lack of participation, by definition, made them different from DAP treatment subjects in a known way: they did not participate in programming. However, because we did not know the reasons why DAP comparison subjects did not accept treatment — and we did not think it reasonable to assume that failure to participate in programming was a random decision on the part of individuals — we did not know how subjects who refused DAP treatment differed from DAP treatment subjects. These unknown differences between DAP treatment subjects and DAP comparison subjects represented selection bias. Were we to have compared DAP treatment subjects to DAP non-treatment subjects, we knew that our analyses would have been biased by this unknown process that differentiated those who volunteered for treatment from those who did not.

In the traditional models, we compared DAP treatment subjects to non-DAP subjects because we assumed that at least some of the non-DAP subjects would have volunteered for treatment had they been at a DAP facility and had the opportunity. These non-DAP subjects who would have volunteered, then, would likely have been similar to our DAP treatment subjects on the dimension

of what leads people to volunteer for treatment. However, the problems that remained, and what made this solution less than ideal, were related to the non-DAP controls also having had an unknown percentage of subjects who would not have volunteered for treatment (creating the double quandary of not knowing the numbers *and* not knowing the selection bias process related to volunteering) and an unknown percentage of subjects who would have volunteered but would not have successfully completed the programming. However, we still did not have a pure comparison of "like" people who did and did not receive treatment. Especially problematic in this traditional approach was the failure to account adequately for those who failed DAP treatment.

Despite some similarity of our traditional model to models used in previous studies, we tried to improve upon these previous studies in several ways. First, our traditional approach included a comprehensive set of control variables. Second, we used event history techniques that most adequately controlled for the right censoring of data (Allison, 1984; Blossfeld and Rohwer, 1995).[57] Third, our study was multi-site and this increased the generalizability of our findings.

Bloom Comparisons

We referred to our second approach for dealing with selection bias issues as the Bloom approach. If we assumed that (1) DAP and non-DAP sites had similar types of inmates (we believe a very reasonable assumption) and (2) DAP and non-DAP sites differed only with respect to whether DAP treatment was available (on average, a reasonable assumption), and if we randomly sampled from the population of DAP and non-DAP inmates and measured their post-release behaviors, we expected that, on average, inmates from DAP sites would have better post-release outcomes because many of those inmates received DAP treatment. Selection bias was not a problem in these comparisons because we were not distinguishing between those who volunteered and those not at the DAP sites. The problems associated with those who finished DAP treatment and those who did not were not at issue for the same reason.

However, this approach had its own problems. While we had a "pure" comparison of inmates at DAP sites to inmates at non-DAP sites, we did not have a "pure" comparison of treated subjects to non-treated subjects. The reason for this was obvious; less than 100 percent of the inmates at the DAP sites volunteered for and completed DAP treatment. As such, the comparisons of DAP inmates to non-DAP inmates were contaminated by the non-volunteers and the non-completers. In practical terms, this meant that it was harder to uncover the effects in the models using the Bloom strategy of comparison. On the plus side, however, if we uncovered effects — and we did for some of the outcomes — we could be fairly certain that the effects were due to the presence of treatment at the DAP sites, given our two assumptions noted above. In essence, the second approach, *i.e.*, the Bloom approach, was an unbiased approach (at least in terms of individual selection bias), but an inefficient method for uncovering treatment effects.

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[57]Right censoring occurred for study participants who had not experienced the post-release outcome in question. These subjects remained at risk of experiencing the event, and our event history models took the censoring of the observations at the 6-month point into account.

Modeling Selection Bias

Our third approach attempted to model the selection bias process based upon the econometrics approach of Heckman (1979) and Maddala (1983). While this approach required us to make much stronger assumptions about the nature of the unknown selection bias process, it did provide us with much more powerful tests of the treatment effects than did the Bloom approach, and it allowed us to make better use of the DAP comparison groups. Also, it allowed us to have some understanding of the differences between the treatment and non-treatment groups at the DAP sites. There was, however, one caveat to using the Heckman approach. Both in this particular context and in general, we were unable to affirm with complete certainty that all relevant factors related to selection bias had been identified. In addition, because we were dealing with the extreme left tail of the log-normal distribution for survival, even the conditional probabilities we calculated for the various outcome measures must be interpreted with caution.

The Heckman approach rested upon more statistical assumptions than did our other two approaches. However, the general congruence between the results produced for outcomes when modeled by the first two approaches — the traditional and Bloom approaches — and the Heckman approach, when applied, gave us much more confidence in our results.

Summary Findings

This report focused on assessing whether there was evidence of treatment effectiveness and as such was concerned primarily with the effects of the control variables in masking or uncovering the effects of treatment. Therefore, our discussion of the various outcomes was limited to a discussion of the effects of treatment when other factors were controlled.

New Arrest as Post-Release Outcome

Post-release failure as indicated by a new arrest was modeled using all three approaches. Consistent findings from all three approaches showed a positive effect of in-prison drug abuse treatment in lowering the likelihood of post-release failure. In terms of in-prison residential drug treatment, in the traditional model, 9-month DAP completion subjects were shown to have odds of arrest that were about 41 percent lower than were those of non-DAP controls for all subjects and 34 percent lower for supervised subjects only.

The 12-month completers also had lower odds of arrest than did the non-DAP controls, but not statistically significant lower odds. It is useful to note that because there were fewer 12-month completers, an effect for 12-month completers was more difficult to find. Comparing the odds of arrest for 9- and 12-month DAP completers in the final traditional model revealed no statistically significant difference between the two groups for both the model of all subjects and the model of supervised subjects only.

The Bloom comparisons did not show a statistically significant effect for treatment in the final model. As previously mentioned, this test of a treatment effect was less efficient because we did not have a direct comparison of treated to non-treated subjects. In addition, as noted in Appendix C, the Bloom approach can yield relatively poor estimates when the proportion of non-DAP control cases is small. Our non-DAP control sample was small — it was about 20 percent of the sample. Lastly, it is not surprising that finding a treatment effect using the Bloom approach would be even more difficult given the results in the traditional model, where 9-month graduates but not 12-month graduates had significantly lower odds of failure.

The findings from the Heckman approach showed a strong treatment effect for the new arrests-only outcome. This strong result was due, in part, to the ability of these models to include non-treated subjects at DAP sites as part of the comparison group; the other models could not include the non-treated DAP site subjects as part of the direct comparison group. The model provided a measure of the selection bias effects through the COVARIAN parameter. The negative direction of the coefficient for COVARIAN in the arrest models told us that treatment subjects, in fact, were at a higher risk for failure than were those who did not select treatment, making our findings all the more significant.

New Arrest or Revocation as Post-Release Outcome

Unlike our findings for failure defined as a new arrest only, we found a treatment effect when we defined failure as either an arrest or revocation when using the Heckman analytic strategy. In the traditional models of failure, we saw that what appeared to be a treatment effect in the base model disappeared when we added the further controls in the final model. In addition, the Bloom model was unable to uncover a treatment effect for the new arrest or revocation outcome. We do note, however, that we found some evidence of an indirect treatment effect in the traditional model. This indirect effect would operate through the intervening post-release behavior variables of employment and violation of conditions of supervision.

We cannot definitively say what accounted for the lack of an effect for treatment when arrests and revocations were considered equivalent failures, especially when the models of arrest alone showed that DAP treatment lowered the odds of failure. Further research is clearly needed to disentangle this puzzle, especially given parallel findings reported by Saylor and Gaes (1996) in their analysis of the BOP's Post-Release Employment Project (PREP). In the PREP study, Saylor and Gaes found also a greater ability to model arrests alone than arrests and revocations considered together, suggesting some systematic mechanism at work.

It may well be that when we considered arrests and revocations as equivalent indicators of post-release failure, we actually were measuring two very different phenomena. In general, the behaviors leading to a revocation differed from those leading to arrests, with many revocations resulting from technical violations. There may have been more discretion in whether to revoke an individual for a technical violation (in particular for drug use), and, in addition, revocation might have been associated with greater levels of overall supervision. Revocation may also have reflected, in part, the policy and philosophy of a particular district. As previously noted, the

tolerance for positive urinalysis test results differed by district, and many revocations were a response to drug use.

Furthermore, an arrest for a revocation that results in re-incarceration removed an individual from the risk pool for an arrest for a new offense. It may have been, as we suggested above, that considering arrest and revocation as equivalent failures was akin to comparing apples and oranges. In fact, it may have been necessary to model them as competing risks of failure. We plan to pursue this issue in a future report. We also plan to include level of supervision differences between districts in future models.

First Detected Drug Test as Post-Release Outcome

Post-release failure, defined as the first detected drug use, was modeled following the same strategy used for arrest only, *i.e.*, using all three approaches. All three approaches found that DAP treatment lowered the odds of failure. In fact, the effect of treatment in reducing the odds of being detected for drug use was the most statistically powerful effect found in these analyses. As seen in the traditional final model, completing the 12-month DAP treatment program lowered the odds of detection of drug use by about 49 percent in comparison with detection in non-DAP subjects. Completing the 9-month program lowered the odds by about 32 percent, again in comparison with non-DAP subjects. As noted previously, because DAP treatment subjects were at a higher risk for post-release failure than were non-treatment subjects, the results for the traditional models probably understated the effect of treatment in lowering the odds of this type of post-release failure.

The results for the final drug use model using the Bloom comparison approach were consistent with the traditional models in pointing to an effect of treatment in lowering the odds of post-release failure. Subjects from DAP sites were approximately 19 percent less likely to have failed (when failure was defined as drug use) than were subjects from non-DAP sites. As noted previously, this approach almost certainly underestimated the treatment effect because treatment volunteering and completing rates were not 100 percent or close to 100 percent. As such, taken together with the results from the traditional models, there appeared to be strong reason to claim that treatment was having an effect in lowering the odds of post-release detection of drug use, at least during the initial 6-month follow-up period. This was further confirmed by the consistency of the Heckman models in finding a significant effect for treatment.

CCC Placement Failure

An effect for DAP treatment on CCC placement failure was found only when using the traditional approach. We did not find any positive treatment effects when using either the Bloom or Heckman approaches. In the traditional final model, 12-month DAP-completers were 50 percent less likely to fail in a CCC than were comparison subjects from non-DAP sites, and graduates of the 9-month program were 32 percent less likely to fail CCC placement. The mixed findings of a treatment effect in the traditional models, and a non-finding in both the Bloom and Heckman

models, suggested that there was no treatment effect. The results were consistent for both approaches, which addressed selection bias issues.

Post-Release Employment

The percentage of time during the first 6 months following release spent employed full time was also modeled using the first two approaches, the traditional and the Bloom approaches. While the models demonstrated that several of the variables included in our models as control variables were significantly related to employment success, we were unable to uncover any in-prison treatment effect upon employment. However, we did find that post-release treatment for men and self-help group participation for women were related to post-release employment success. The absence of an in-prison treatment effect was not surprising because the DAP programs did not focus upon vocational rehabilitation.[58] Maintaining a crime-free lifestyle, one of the principal goals of the DAP program, implied being employed in a legitimate occupation. Yet, affecting the ability of an individual to obtain and keep employment — especially when the individual has limited job experience and skills — often requires specialized vocational services and the acquisition of job skills.

Inter-Institution Outcomes

The remaining question considered in this section is whether DAP's work better at some locations than at others. The short answer to the question was that it did not appear that our models adequately discerned institutional differences. The institutional effects were not an adequate proxy for differences in the quality of treatment program and staff. There were only a couple of sites at which we saw subjects doing significantly better or worse on average after entering treatment.

We limited our discussion of institutional differences in treatment effectiveness to the two outcome variables for which we had the most reason to believe that treatment would have an effect: (1) arrests and (2) detection of drug use as indicators of post-release failure. In the final models for these outcomes, there were only a couple of significant departures for groups of subjects treated at a specific institution from the overall odds for all treatment subjects. For the final model of arrests, no institution was significantly different in its risk of failure from that of all treatment subjects. For the final model of first detection of drug use, subjects treated at McKean had lower odds of failure. We note, however, that men-only models show a somewhat different pattern. As yet, we have not been able to differentiate a real effect from an effect resulting from the increased likelihood of a Type I error (*i.e.*, a "false positive").

Unfortunately, the models considered in this analysis did not appear to be very well suited to capturing the differences among institutions in how DAP's were operated and how, consequently,

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[58]The DAP's did incorporate resume writing and job interviewing skills, as well as some discussion of work skills. Nonetheless, this could not be considered to be equivalent to vocational rehabilitation or habilitation.

this affected the post-release outcomes of DAP treatment subjects. For the most part, we could not discriminate between more and less successful DAP's with our current empirical analyses.

One possible explanation for our inability to discriminate appropriately between DAP's may have been due to a confounding between site and type of treatment subject categories. Anecdotal information from program staff indicated that programs differed in their philosophies and in their implementations of discharge policies. Some program directors were known to have been reluctant to discharge any participant unless *required* to do so by non-treatment program policies, whereas others readily discharged individuals who did not participate or did not cooperate with the treatment regimen.

Transitional Services

The primary interest of this preliminary report was in the effectiveness of the DAP's. The results discussed indicated that DAP's, defined as the in-prison residential programs, had positive effects on several outcomes. Nonetheless, some discussion was needed about the BOP's transitional services provided in a halfway house. This was particularly important because the DAP's, in general, included transitional services components. The DAP was viewed as consisting of a continuum of care that included *both* the in-prison residential component and the transitional services received in the halfway house.[59] In that respect, one would not have been able to distinguish whether in-prison treatment followed by treatment in a halfway house setting was better than was in-prison treatment alone, or whether, for that matter, treatment in the halfway house alone was just as effective in-prison treatment alone. Ideally, we would have liked to assign relative weights to each component — the in-prison component and the halfway house component.

The only effect of transitional services was seen when modeling employment outcomes for women. Women who received these services spent a greater percentage of their post-release time employed full-time. This effect occurred even though there was no evidence of any effect of DAP treatment on employment for either men or women.

Nonetheless, it would be premature to draw conclusions about the role of transitional services. The models contained in this report did not account for several possible confounding factors. First, not all individuals receiving transitional services completed their community corrections (CCC) placements. Not completing transitional services was possibly confounded with CCC failure. Comparison subjects and individuals who enrolled in but did not complete a DAP were coded as having received transitional services regardless of whether they completed their placements or failed. CCC placement failures may have received very little service because they failed shortly after release from prison.

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[59] It must be noted that this statement is true only for individuals who complete the in-prison program component *and* receive a halfway house placement.

This confounding was also the case for DAP graduates, but in a different manner. For DAP graduates, failure of transitional services was synonymous with CCC placement failure and vice-versa, according to policies relating to DAP graduates. The results contained in this report showed that CCC placement failure was related to the following outcomes: post-release employment (for males), arrests or revocations, and drug use. Individuals who did not successfully complete this placement were more likely to fail. It is also noteworthy that, although not significant in the traditional model for arrests, CCC failure was significant in the Heckman model, which controlled for self-selection bias.

Conclusions

Positive results reported in this interim report of residential drug abuse treatment join the growing body of research suggesting that treatment programs for offenders work if properly implemented. We found that DAP treatment did seem to "work," at least in terms of lowering the odds of experiencing some critical post-release outcomes: new arrests, arrest or revocation, and detection of drug use. The results presented here, though, did not detect the same impact of DAP treatment on post-release employment success (defined narrowly as percentage of time employed full time) or CCC placement failure. Questions remain as to the suitability of these analysis models.

In summary, it seems that we have made a good case for the proposition that treatment has an effect on important types of post-release behavior in the first 6 months following release. However, the effects for men appear to be less favorable than do the effects for women.

Although an individual's first 6 months are considered crucial to successful reintegration, this time frame represents only a portion of the period crucial to reintegration. Recidivism rates generally are highest within the first year and — while lower after that — are still high for another year or two. Therefore, these results must be interpreted with some caution. Our future analyses will evaluate whether these effects and the differences in outcomes between men and women are sustained over a longer follow-up period.

Future Research Efforts

In the future, we will consider the longitudinal aspects of following subjects for the remainder of the study's 3-year period and address newly arising substantive questions. We have attempted to identify some of the limitations of the conclusions we have drawn, to suggest potential issues to be addressed when data are available for the entire pool of subjects, and identify general issues important to future research. The major areas we covered relate to the treatment component, to measuring program quality, to proximal outcomes, to gender concerns, to disentangling arrest and revocation as outcome measures, and to a general recognition related to understanding the treatment process.

Understanding the Treatment Component as a Continuum

While the focus of this report has been upon the effects of DAP treatment, it is important that treatment be understood as a continuum of programs, not limited solely to services received while subjects are incarcerated. To understand the treatment services, we must examine more broadly the role of treatment across its entire spectrum, including post-release treatment. For example, we know that, overall, almost half of the research subjects under supervision were required by their Probation officers to seek drug or alcohol treatment services. Our findings suggested that the post-release services had an effect on outcomes of interest independent of the in-prison DAP treatment; while there was no evidence of DAP treatment having an effect on post-release employment, post-release services did seem to have an effect. For men, having received contract services was related to a greater percentage of post-release time employed full time. For females, being involved in a self-help group had a positive relationship with full-time employment. These issues require further examination.

We also found effects for post-release treatment in outcomes affected by DAP treatment.[60] Such effects were found in both the traditional and Bloom models for arrests or revocations and for drug use. The results for failure when it was defined as an arrest or revocation indicated that individuals who received both contract and non-contract services were less likely to fail than were subjects on average. For failure when it was defined as drug use, we found an effect for this same post-treatment variable, but the direction of the relationship was the opposite. Individuals receiving both contract and non-contract services were more than 200 percent more likely to fail when failure was defined as drug use.

This finding was surprising.[61] However, as noted earlier in our initial discussion of these findings, there were potential problems with model specification. It may have been that some of the post-release treatment variables were measuring risk of failure. In addition, we recognized that our models did not capture the time ordering of treatment. While contract services were initiated at the beginning of supervision, this was not always the case for non-contract services. These services sometimes began at later points during supervision, most notably after an individual had received a positive urinalysis test result.

In addition to these issues related to the post-release component of the treatment continuum, we need to focus on the nature of within-treatment effects, as well as on the relationship between and among effects. For example, we need to test for interactive effects between treatments. That is, we need to address such questions as: “are the effects of treatment additive in nature?” and “can we assess the contribution of the dosage level of the service?” The latter question is applicable

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[60] Effects for models of new arrests only were not identified because the additional treatment variables were not included in the final models. This block of variables did not make a significant difference in the likelihood ratio test statistic.

[61] It must be noted that the post-release treatment variables were not contained in the Heckman approach models for drug use. This will be considered in future research.

particularly to the post-release treatment services that, unlike transitional services in halfway houses which provide 2 hours of service per week to the vast majority of participants, provide a wide range in frequency and type of services. The variation might relate to the amount of hours of individual and group treatment received on a weekly basis, the duration of these services, the intensity of services received (as reflected by receiving inpatient services or residential treatment), or a combination of these variations. Evaluation research recently has recognized the importance of specifying the strength of a treatment needed to produce an effect. Thus, treatments may need to be provided at some minimal threshold level before any effect will be observed. Conclusions about treatment in general must be understood in the context of understanding this threshold level.

Program Quality

Much remains to be done concerning the assessment of institutional differences. In this report, institution was used as a proxy for possible differences in program quality. We included a measure of program maturity to differentiate a program at the beginning stages of operation from its later, presumably smoother, operation when well established. Since the results reported included information for only two-thirds of the research subjects, many sites lacked a sufficiently large sample size to provide the power necessary to detect differences in effects among sites; only 9 of the 20 research sites had more than 50 subjects included in our analyses. Four sites had fewer than 15 subjects, resulting in very large standard errors for these sites.

Even with the larger sample sizes we expect to have for our future analyses, sample size will remain a problem for some sites. However, indicators of program quality other than program maturity remain to be identified. Whereas the programs had many common components, this was less the case at the outset of program implementation. Furthermore, even with a common set of program modules to help establish the content of the materials provided during the programs, the actual implementation of the programs varied, as did the quality and stability of staffing. Not only did this vary across sites, but also within sites across time. Such variations could not be accounted for in our measure of program maturity, which simply measured length of time a program had been operational.

Data from several annual staff surveys, staffing data, and other qualitative program information will be compiled in an attempt to group programs according to one or more dimensions of "quality." This could improve our ability to associate program characteristics with outcome not only because of the statistical power resulting from the grouping of two or more programs together, but also because there would be some very specific hypotheses to test. For example, we could test the hypothesis that programs staffed by individuals with previous experience in drug treatment counseling and corrections are more effective than are programs staffed with individuals having neither or only one of these experiences.

Proximal Outcome

A dimension of outcome not included in this report, but important in and of itself, concerns proximal outcomes. These proximal outcomes represent the intervening mechanism through which the treatment program affects ultimate outcome (*i.e.*, "distal outcome").

Each program makes assumptions about the cognitive and behavioral deficiencies of the clientele served, and programs are designed to ameliorate these deficiencies. Without addressing these deficiencies, the programs cannot be expected to have any effect on the "distal outcomes," as these deficiencies contribute to these outcomes. It is likely that these proximal outcomes contribute to our understanding of inter-site institutions as well.

Although our findings suggested that drug abuse treatment had a positive effect, our study lacked the programmatic specificity to identify the particular factors that contributed to this successful outcome, and this would require the identification of intervening mechanisms. An assessment of the extent to which the population served had the purported deficiencies and the extent to which these deficiencies were remedied will help us understand *how* the treatment programs work.

Beyond the theoretical grounding, there is a methodological rationale for examining proximal outcomes. The causal link between treatment and outcomes is strengthened when a strong association between treatment and proximal outcomes predicted by theory exists, as well as a strong association between the proximal outcome and the distal outcome (in this case arrests and drug use) (Mohr, 1992). This concept of an intervening mechanism based upon theory will be examined in a future report using pre- and post-treatment measures selected because of their relationships to relapse prevention and to the theoretical underpinnings of the DAP's.

Another rationale for the examination of the proximal outcomes arises from the goal of generalization in any evaluation research. More recent evaluation research recognizes the limited utility of research that solely addresses the question of whether a program works (Chen, 1990). When the response is yes, as appears to be the case here, the successful replication of the program and its improvement depend upon an understanding of the causal mechanisms that lead to this "success."

At the outset of this evaluation project, one of the goals was to assess whether the 1,000-hour, 12-month programs were more effective than were the 500-hour, 9-month programs. The models included in this report do not lead us to the conclusion that the additional costs of providing 12-month programs result in a significantly better outcome. (It must be noted that the BOP eliminated these 12-month programs effective January 1996 for reasons unrelated to the

research.[62]) Nonetheless, without a more precise method of controlling for the quality of programs, the answer to this question should not be based solely on the results of this report.

In addition, we were not able to model the primary outcomes of concern — arrests and drug use — separately for men and women. One must entertain the possibility that, because of differences not only in program implementation but in the population served, the one 12-month program for women in this study (Lexington) cannot validly be grouped together with the two 12-month programs for men. In a future report with a larger sample of females, it is hoped that this question can be addressed more definitively.

Gender

This brings us to the question of gender differences in outcomes. Women had lower failure rates than did men for all outcomes except employment. With the small sample size of women available for this report, it was not possible to model women separately from men, with the exception of our model for employment; therefore, we could not draw specific conclusions about the women. However, it is expected that models for women could be run in future analyses. Of the anticipated additional 462 DAP treatment subjects who are not included in this report (excluding some who have detainers), 20 percent are women (n=125). The anticipated total number of women subjects, both treatment and comparison, is 610.

New Arrest vs. Revocation

Our results indicated that DAP treatment was found to be related to failure, when failure was defined as a new arrest, in two of our modeling strategies — in the traditional and Heckman approaches. However, we found DAP treatment to be related to failure, when failure was defined as either a new arrest or a revocation, in only one modeling approach — the Heckman approach. Future event history analyses will be needed to understand how arrests may differ from revocations; *i.e.*, it will be important to consider the ways in which the behaviors leading to a new arrest differ from those leading to an arrest for revocation. Further, we will need to analyze the data using "competing risk" models — *i.e.*, models that acknowledge that different events can occur within the same risk period and that one event can terminate the risk period for the other event. For example, an arrest for a revocation, when it results in incarceration, terminates the risk for a new arrest. It will be essential to treat the two events — a new arrest versus an arrest for a revocation — differently in the event history analysis.

Future analyses of arrests and revocations will also include information on overall levels of supervision. Individuals who are more closely supervised are perhaps more likely to be detected of supervision violations.

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[62] The decision to eliminate these 12-month, 1,000-hour programs was made in order to increase program capacity. The 1994 Violent Crime Control and Law Enforcement Act contained provisions stipulating that, over time, the BOP needed to treat an increasingly higher percentage of inmates with drug problems.

A Final Note

Sometimes we forget that not only can program evaluation be useful when it examines the underlying causal mechanism of a program's effect(s), thus facilitating its replication, it can be useful also by providing information that may increase the efficiency and effectiveness of existing program operations. For example, a close look at the descriptive statistics suggests that the disciplinary discharges represent individuals with a greater number of problems in several domains. Preliminary analyses not contained in this report indicate that antisocial personality and pre-incarceration employment status are predictive of program retention. Furthermore, antisocial personality is associated with a higher rate of polydrug dependency. Such information could be used by program staff in either improving screening mechanisms or in improving the methods used to address the issues of these specific subtypes of drug-abusing offenders. Specific issues such as these will be the focus of specific special issue reports.

Another commentary on future analyses pertains to increasing the understanding of the relationship among the various outcomes of interest. Models of arrests indicated that having used drugs during supervision was associated with a higher log odds of being arrested. Both arrests and drug use were affected by drug treatment. Disentanglement of the causal nature of these relationships, which were most likely reciprocal in nature, will require utilizing a path analytic method. Path analytic methods will also be necessary to identify the direct and indirect effects of treatment. The results of the regression models we presented were limited to identifying the direct effects of treatment.

Our last commentary pertains to addressing the question of "what works with whom." As we direct our focus to the examination of the effects of factors other than residential treatment on the various outcomes, we will no longer assume a linear additive model. Interaction effects between variables such as treatment and level of drug addiction will be considered. Such interaction effects could identify the variation in treatment effectiveness for individuals with different characteristics.

TABLES

Table 1

Subject Attrition

	Male	Female
Potential Research Subjects	2,459	571
Missed Research	(378)	(127)
Refused Research	(223)	(27)
Total	1,858	417
Number Approached in Prison for Intake	1,858	417
Number Approached in CCC for Intake[63]	75	7
Total Approached for Intake	1,933	424
Missed Intake	(149)	(29)
Refused Intake	(40)	(3)
Total Sample Who Agreed to Intake	1,744	392

Note: A total of 2,136 intake interviews were completed. However, 10 interviews were lost in the mail, 47 interviews were not usable, and 213 detainees were dropped from the subject pool, resulting in 1,866 complete intake interviews.

[63] These individuals were identified by virtue of being transitional services participants. This was done to increase the pool of subjects who received treatment during their halfway house placements but not while in prison.

Table 2

Demographics

Male Subjects

	Comparison		Treatment				Total
	DAP (n=419)	Non-DAP (n=386)	Completed (n=523)	Dropout (n=37)	Discip. Discharge (n=55)	In-complete (n=104)	(n=1,524)
Race							
White	59 %	52 %	64 %	57 %	55 %	59 %	59 %
Black	40 %	46 %	32 %	32 %	38 %	38 %	38 %
Other	2 %	2 %	4 %	11 %	7 %	3 %	3 %
Age at time of release							
19-29 years	23 %	24 %	17 %	19 %	35 %	36 %	23 %
30-34 years	19 %	25 %	20 %	16 %	20 %	20 %	21 %
35-39 years	18 %	19 %	27 %	27 %	20 %	18 %	22 %
40 + years	40 %	32 %	35 %	38 %	25 %	25 %	35 %
	(n=415)	(n=380)	(n=523)	(n=37)	(n=55)	(n=104)	(n=1,514)
Education							
12 years or less[1]	67 %	71 %	67 %	73 %	75 %	74 %	69 %
Greater than 12 years	33 %	29 %	33 %	27 %	25 %	26 %	31 %
Missing=10							

Note: Due to rounding, columns may not sum to 100 percent.

[1] Includes those earning a General Education Degree (GED).

Table 3

Demographics

Female Subjects

	Comparison		Treatment				Total
	DAP (n=111)	Non- DAP (n=51)	Complete (n=97)	Dropout (n=16)	Discip. Discharge (n=24)	In-complete (n=43)	(n=342)
Race[1]							
White	56 %	47 %	54 %	38 %	54 %	44 %	52 %
Black	43 %	51 %	44 %	56 %	46 %	56 %	47 %
Age at time of release							
19-29 years	29 %	27 %	16 %	12 %	42 %	37 %	26 %
30-34 years	29 %	39 %	33 %	31 %	21 %	21 %	30 %
35-39 years	21 %	14 %	26 %	25 %	29 %	26 %	22 %
40+ years	22 %	20 %	25 %	31 %	8 %	16 %	21 %
	(n=110)	(n=51)	(n=96)	(n=16)	(n=23)	(n=42)	(n=338)
Education							
12 years or less[2]	79 %	59 %	88 %	81 %	87 %	71 %	78 %
Greater than 12 years	27 %	41 %	12 %	19 %	13 %	29 %	22 %
Missing=4							

Note: Due to rounding, columns may not sum to 100 percent.

[1] There were 5 women of other races.

[2] Includes those earning a General Education Degree (GED).

Table 4
Frequency of Drug Use During Heaviest Use Period
Male Subjects

	Comparison		Treatment				Total
	DAP (n=430)	Non-DAP (n=375)	Completed (n=523)	Dropout (n=37)	Discip. Discharge (n=55)	In-complete (n=104)	(n=1,524)
Barbiturates							
Not used[1]	74 %	78 %	75 %	81 %	71 %	75 %	76 %
Used less than daily	18 %	14 %	15 %	14 %	16 %	15 %	15 %
Used daily	8 %	8 %	10 %	5 %	13 %	11 %	9 %
Missing=4							
Powder Cocaine							
Not used	35 %	37 %	35 %	53 %	28 %	39 %	36 %
Used less than daily	34 %	31 %	29 %	17 %	30 %	34 %	31 %
Used daily	31 %	32 %	35 %	31 %	43 %	28 %	33 %
Missing=27							
Crack Cocaine							
Not used	80 %	75 %	77 %	84 %	75 %	75 %	77 %
Used less than daily	10 %	11 %	7 %	5 %	11 %	8 %	9 %
Used daily	10 %	14 %	16 %	11 %	15 %	18 %	14 %
Missing=4							
Hallucinogens							
Not used	73 %	74 %	69 %	68 %	62 %	78 %	72 %
Used less than daily	21 %	22 %	24 %	24 %	31 %	15 %	22 %
Used daily	5 %	5 %	7 %	8 %	7 %	7 %	6 %
Missing=5							
Heroin							
Not used	76 %	78 %	79 %	76 %	67 %	78 %	77 %
Used less than daily	5 %	7 %	7 %	5 %	9 %	3 %	6 %
Used daily	20 %	16 %	14 %	19 %	24 %	19 %	17 %
Missing=9							
Marijuana							
Not used	15 %	13 %	22 %	32 %	15 %	32 %	18 %
Used less than daily	26 %	26 %	23 %	19 %	24 %	23 %	24 %
Used daily	59 %	61 %	56 %	49 %	62 %	45 %	57 %
Missing=5							
Opiates							
Not used	80 %	84 %	80 %	92 %	78 %	79 %	81 %
Used less than daily	12 %	10 %	11 %	3 %	13 %	8 %	11 %
Used daily	8 %	6 %	9 %	5 %	9 %	13 %	8 %
Missing=4							
Stimulants							
Not used	65 %	69 %	64 %	73 %	65 %	69 %	66 %
Used less than daily	19 %	18 %	20 %	8 %	18 %	13 %	18 %
Used daily	16 %	13 %	17 %	19 %	16 %	19 %	16 %
Missing=4							

Due to rounding, columns may not sum to 100 percent.

[1] "Not used" is defined as using the drug fewer than five times during one's life.

Table 5
Frequency of Drug Use During Heaviest Use Period
Female Subjects

	Comparison		Treatment				Total
	DAP (n=113)	Non-DAP (n=49)	Completed (n=97)	Dropout (n=16)	Discip. Discharge (n=24)	In-complete (n=43)	(n=342)
Barbiturates							
Not used[1]	83 %	84 %	65 %	81 %	54 %	76 %	75 %
Used less than daily	9 %	4 %	23 %	0 %	24 %	14 %	14 %
Used daily	8 %	12 %	11 %	19 %	21 %	10 %	11 %
Missing=1							
Powder Cocaine							
Not used	35 %	34 %	29 %	56 %	25 %	37 %	34 %
Used less than daily	23 %	34 %	26 %	12 %	25 %	18 %	24 %
Used daily	43 %	32 %	43 %	31 %	50 %	44 %	42 %
Missing=5							
Crack Cocaine							
Not used	65 %	53 %	57 %	69 %	54 %	67 %	61 %
Used less than daily	10 %	18 %	13 %	0 %	4 %	4 %	10 %
Used daily	26 %	29 %	30 %	31 %	42 %	28 %	29 %
Hallucinogens							
Not used	81 %	84 %	75 %	100 %	67 %	86 %	80 %
Used less than daily	15 %	16 %	22 %	0 %	30 %	12 %	18 %
Used daily	5 %	0 %	2 %	0 %	4 %	2 %	3 %
Heroin							
Not used	79 %	84 %	82 %	88 %	71 %	72 %	80 %
Used less than daily	3 %	6 %	2 %	0 %	0 %	0 %	3 %
Used daily	18 %	10 %	16 %	13 %	29 %	28 %	18 %
Missing=3							
Marijuana							
Not used	23 %	24 %	28 %	38 %	21 %	35 %	27 %
Used less than daily	28 %	30 %	22 %	31 %	21 %	33 %	26 %
Used daily	50 %	45 %	49 %	31 %	58 %	33 %	46 %
Opiates							
Not used	83 %	94 %	76 %	88 %	71 %	81 %	82 %
Used less than daily	8 %	4 %	14 %	0 %	4 %	6 %	8 %
Used daily	10 %	2 %	9 %	13 %	25 %	12 %	10 %
Stimulants							
Not used	77 %	69 %	56 %	81 %	63 %	74 %	69 %
Used less than daily	7 %	6 %	16 %	6 %	8 %	13 %	10 %
Used daily	17 %	24 %	28 %	13 %	29 %	14 %	21 %

Note: Due to rounding, columns may not sum to exactly 100 percent.
[1] "Not used" is defined as using the drug fewer than five times during one's life.

Table 6
Estimated Log Odds of Drug and Alcohol Dependence
Male Subjects

	Comparison		Treatment				Total
	DAP (n=430)	Non-DAP (n=375)	Completed (n=523)	Dropout (n=37)	Discip. Discharge (n=55)	In-complete (n=104)	(n=1,524)
Drug Dependence							
Mean Log Odds	1.29	1.13	1.46	-0.02	2.14	1.26	1.31
Standard Dev.	2.92	2.87	3.27	3.85	2.77	3.58	3.11
Odds Ratio	3.63	3.10	4.30	0.98	8.47	3.54	3.69
Missing=17							
Alc. Dependence							
Mean Log Odds	0.09	0.41	0.21	-0.37	-0.01	-0.80	0.14
Standard Dev.	3.25	3.53	3.70	3.87	3.90	3.88	3.57
Odds Ratio	1.09	1.50	1.23	0.69	0.99	0.45	1.15
Missing=54							

Table 7
Estimated Log Odds of Drug and Alcohol Dependence
Female Subjects

	Comparison		Treatment				Total
	DAP (n=113)	Non-DAP (n=49)	Completed (n=97)	Dropout (n=16)	Discip. Discharge (n=24)	In-complete (n=43	(n=342)
Drug Dependence							
Mean Log Odds	1.80	1.74	2.98	1.00	3.00	2.58	2.27
Standard Dev.	2.73	2.64	2.57	3.64	3.51	3.34	2.91
Odds Ratio	6.06	5.70	19.76	2.71	20.04	13.19	9.70
Alc. Dependence							
Mean Log Odds	-1.07	-0.63	-0.17	-2.61	1.23	-0.42	-0.57
Standard Dev.	3.32	3.40	3.79	2.51	3.16	3.92	3.56
Odds Ratio	0.34	0.53	0.84	0.07	3.41	0.66	0.56
Missing=8							

Table 8
Participation in In-Prison Outpatient Drug and Alcohol Treatment and Self-Help Groups
Male Subjects

	Comparison		Treatment				Total
	DAP (n=430)	Non-DAP (n=375)	Completed (n=523)	Dropout (n=37)	Discip. Discharge (n=55)	In-complete (n=104)	(n=1,524)
Outpatient	5 %	4 %	10 %	3 %	5 %	5 %	6 %
Self-Help Groups	0 %	1 %	6 %	0 %	5 %	5 %	3 %

Table 9
Participation in In-Prison Outpatient Drug and Alcohol Treatment and Self-Help Groups
Female Subjects

	Comparison		Treatment				Total
	DAP (n=113)	Non-DAP (n=49)	Completed (n=97)	Dropout (n=16)	Discip. Discharge (n=24)	In-complete (n=43)	(n=342)
Outpatient	7 %	14 %	0 %	0 %	4 %	5 %	5 %
Self-Help Groups	9 %	4 %	0 %	0 %	0 %	0 %	4 %

Note: The percentage of inmates participating in outpatient treatment may be underreported due to inconsistencies in recording this information in SENTRY.

Table 10

Drug and Alcohol Treatment History

Male Subjects

	Comparison		Treatment				Total
	DAP (n=426)	Non-DAP (n=374)	Completed (n=521)	Drop-Out (n=36)	Discip. Discharge (n=55)	In-complete (n=103)	(n=1,515)
Past Drug Treatment	30 %	32 %	31 %	36 %	31 %	36 %	31 %
Past Alcohol Treatment	4 %	4 %	7 %	3 %	4 %	4 %	5 %
Missing = 9							

Note: Self-help groups, such as AA, were not included in treatment history.

Table 11

Drug and Alcohol Treatment History

Female Subjects

	Comparison		Treatment				Total
	DAP (n=113)	Non-DAP (n=49)	Completed (n=97)	Drop-Out (n=16)	Discip. Discharge (n=24)	In-complete (n=43)	(n=342)
Past Drug Treatment*	37 %	27 %	37 %	19 %	42 %	37 %	35 %
Past Alcohol Treatment	3 %	6 %	3 %	6 %	8 %	0 %	4 %
*Missing = 1							

Note: Self-help groups, such as AA, were not included in treatment history.

Table 12

Criminal and Incarceration History

Male Subjects

	Comparison		Treatment				Total
Any Prior Commitments	DAP (n=430)	Non-DAP (n=375)	Completed (n=523)	Drop-Out (n=37)	Discip. Discharge (n=55)	In-complete (n=104)	(n=1,524)
Yes	74 %	71 %	70 %	73 %	80 %	65 %	71 %
Recency of Violence							
None	54 %	53 %	55 %	41 %	40 %	62 %	54 %
<5 years ago	14 %	13 %	12 %	16 %	27 %	16 %	14 %
5+ years ago	32 %	34 %	33 %	43 %	33 %	22 %	32 %
Length of Current Incarceration							
0-12 Months	13 %	36 %	9 %	11 %	15 %	34 %	19 %
13-59 Months	74 %	57 %	80 %	76 %	75 %	58 %	71 %
60-83 Months	8 %	6 %	8 %	5 %	7 %	7 %	7 %
84 + Months	5 %	1 %	4 %	8 %	4 %	2 %	3 %
In CJ System at Arrest[1]							
Yes	44 %	48 %	36 %	36 %	53 %	40 %	42 %

[1] Subjects were defined as being in the criminal justice system at the time of arrest if they were on judicially imposed confinement or supervision.

Table 13

Criminal and Incarceration History

Female Subjects

	Comparison		Treatment				Total
Any Prior Commitments	DAP (n=113)	Non-DAP (n=49)	Completed (n=97)	Drop-Out (n=16)	Discip. Discharge (n=24)	In-complete (n=43)	(n=342)
Yes	37 %	51 %	41 %	50 %	62 %	58 %	45 %
Recency of Violence							
None	86 %	84 %	87 %	100 %	75 %	91 %	86 %
<5 years ago	10 %	10 %	4 %	0 %	8 %	5 %	7 %
5+ years ago	4 %	6 %	9 %	0 %	17 %	5 %	7 %
Length of Current Incarceration							
0-12 Months	34 %	84 %	9 %	6 %	25 %	46 %	34 %
13-59 Months	61 %	16 %	87 %	88 %	75 %	54 %	63 %
60-83 Months	4 %	0 %	3 %	6 %	0 %	0 %	3 %
84+ Months	1 %	0 %	1 %	0 %	0 %	0 %	1 %
In CJ System at Arrest[1]							
Yes	29 %	52 %	36 %	33 %	41 %	45 %	37 %

[1] Subjects were defined as being in the criminal justice system at the time of arrest if they were on judicially imposed confinement or supervision.

Table 14

Psychiatric Diagnoses

Male Subjects

	Comparison		Treatment				Total
Psychiatric Diagnosis[1]	DAP (n=398)	Non-DAP (n=333)	Completed (n=497)	Drop-Out (n=32)	Discip. Discharge (n=47)	In-complete (n=87)	(n=1,394)
None	52 %	51 %	58 %	59 %	40 %	60 %	54 %
Depression	6 %	8 %	10 %	9 %	8 %	6 %	8 %
Antisocial Personality	32 %	33 %	28 %	22 %	45 %	26 %	30 %
Depression and Antisocial Personality	10 %	8 %	5 %	9 %	6 %	8 %	7 %
Missing = 130							
	(n=429)	(n=374)	(n=520)	(n=36)	(n=54)	(n=102)	(n=1,515)
Had Past Mental Health Treatment	20 %	18 %	17 %	22 %	20 %	18 %	18 %
Missing = 9							

[1] Diagnoses were based on the Diagnostic Interview Schedule (DIS) using DSM-III-R criteria.

Table 15

Psychiatric Diagnoses

Female Subjects

	Comparison		Treatment				Total
Psychiatric Diagnosis[1]	DAP (n=106)	Non-DAP (n=45)	Completed (n=76)	Drop-Out (n=13)	Discip. Discharge (n=17)	In-complete (n=33)	(n=290)
None	51 %	47 %	54 %	54 %	24 %	45 %	49 %
Depression	23 %	29 %	16 %	31 %	0 %	21 %	21 %
Antisocial Personality	12 %	16 %	18 %	15 %	47 %	18 %	17 %
Depression and Antisocial Personality	14 %	9 %	12 %	0 %	29 %	15 %	13 %
Missing = 52							
	(n=112)	(n=49)	(n=97)	(n=16)	(n=24)	(n=43)	(n=341)
Had Past Mental Health Treatment	41 %	41 %	31 %	44 %	46 %	53 %	40 %
Missing = 1							

[1] Diagnoses were based on the Diagnostic Interview Schedule (DIS) using DSM-III-R criteria.

Table 16

Employment History

Male Subjects

	Comparison		Treatment				Total
Support Self One Year With Illegal Income	DAP (n=424)	Non-DAP (n=373)	Completed (n=514)	Dropout (n=36)	Discip. Discharge (n=52)	In-complete (n=102)	(n=1,501)
Yes	36 %	42 %	43 %	49 %	27 %	24 %	39 %
Employment in Month Before Incarceration							
Full or Part Time	50 %	60 %	56 %	50 %	46 %	67 %	55 %
Not in Labor Force	5 %	6 %	4 %	3 %	6 %	4 %	4 %
Looking for Work	11 %	6 %	9 %	8 %	13 %	5 %	9 %
Illegal Income, Other Reason Unemployed, or Never Worked	35 %	28 %	31 %	39 %	35 %	25 %	31 %
Missing = 23							

Table 17

Employment History

Female Subjects

	Comparison		Treatment				Total
Support Self One Year With Illegal Income	DAP (n=110)	Non-DAP (n=49)	Completed (n=95)	Dropout (n=16)	Discip. Discharge (n=22)	In-complete (n=42)	(n=334)
Yes	43 %	43 %	26 %	48 %	49 %	31 %	41 %
Employment in Month Before Incarceration							
Full or Part Time	40 %	49 %	40 %	31 %	32 %	31 %	39 %
Not in Labor Force	5 %	12 %	5 %	13 %	5 %	12 %	7 %
Looking for Work	12 %	10 %	6 %	13 %	9 %	12 %	10 %
Illegal Income, Other Reason Unemployed, or Never Worked	43 %	29 %	48 %	44 %	55 %	45 %	43 %
Missing = 8							

Table 18

Motivation for Change

Male Subjects

	Comparison		Treatment				Total
Motivation for Change (Prochaska)	DAP (n=385)	Non-DAP (n=312)	Completed (n=460)	Drop-Out (n=31)	Discip. Discharge (n=47)	In-complete (n=82)	(n=1,317)
Contemplation	25 %	24 %	32 %	29 %	36 %	29 %	28 %
Preparation	14 %	16 %	29 %	26 %	19 %	24 %	21 %
Reluctant	31 %	28 %	12 %	16 %	9 %	13 %	21 %
Action	7 %	10 %	18 %	10 %	19 %	21 %	13 %
Precontemplation	13 %	14 %	8 %	10 %	15 %	7 %	11 %
Uninvolved	10 %	7 %	3 %	10 %	2 %	5 %	6 %

Missing = 207

Table 19

Motivation for Change

Female Subjects

	Comparison		Treatment				Total
Motivation for Change (Prochaska)	DAP (n=95)	Non-DAP (n=44)	Completed (n=67)	Drop-Out (n=14)	Discip. Discharge (n=21)	In-complete (n=40)	(n=281)
Contemplation	24 %	20 %	24 %	21 %	5 %	25 %	22 %
Preparation	21 %	32 %	24 %	14 %	10 %	23 %	22 %
Reluctant	20 %	7 %	7 %	0 %	19 %	3 %	11 %
Action	14 %	23 %	40 %	57 %	62 %	40 %	31 %
Precontemplation	16 %	14 %	4 %	7 %	0 %	8 %	10 %
Uninvolved	5 %	5 %	0 %	0 %	5 %	3 %	3 %

Missing = 61

Table 20

CCC Placements, Failures, and Reasons for Failing

Male Subjects

	Comparison		Treatment				Total
Subjects Who Received CCC Placements	DAP (n=430)	Non-DAP (n=375)	Completed (n=523)	Drop-Out (n=37)	Discip. Discharge (n=55)	In-complete (n=104)	(n=1,524)
Received Placement	61 %	67 %	78 %	51 %	38 %	64 %	68 %
Subjects Who Failed CCC Placement	(n=264)	(n=253)	(n=407)	(n=19)	(n=21)	(n=67)	(n=1,031)
Failed	22 %	26 %	19 %	21 %	48 %	24 %	23 %
Reasons for Failing CCC Placement	(n=48)	(n=57)	(n=71)	(n=4)	(n=10)	(n=14)	(n=204)
Positive drug UA	58 %	53 %	52 %	25 %	60 %	29 %	52 %
Positive alcohol[1]	4 %	7 %	8 %	0 %	20 %	14 %	8 %
Possession of drugs	2 %	11 %	3 %	0 %	10 %	0 %	5 %
Possession of alcohol	4 %	9 %	8 %	0 %	0 %	0 %	6 %
Act of violence	0 %	2 %	4 %	0 %	0 %	0 %	2 %
Accountability[2]	2 %	2 %	3 %	25 %	0 %	7 %	3 %
Violation of CCC rules[3]	15 %	7 %	7 %	0 %	10 %	7 %	9 %
Escape	8 %	5 %	7 %	50 %	0 %	21 %	8 %
Other	6 %	6 %	7 %	0 %	0 %	21 %	7 %
Missing = 28							

[1] As determined by breathalyzer or urinalysis.
[2] Accountability transgressions consist almost solely of unexcused absences from the CCC.
[3] "Violation of CCC rules" comprises subjects who were cited for committing any of a variety of transgressions, including but not limited to gambling, acting disruptively, and refusing an order.

Table 21

CCC Placements, Failures, and Reasons for Failing

Female Subjects

	Comparison		Treatment				Total
	DAP (n=113)	Non-DAP (n=49)	Completed (n=97)	Drop-Out (n=16)	Discip. Discharge (n=24)	In-complete (n=43)	(n=342)
Subjects Who Received CCC Placements							
Received Placement	62 %	61 %	80 %	44 %	50 %	77 %	67 %
Subjects Who Failed CCC Placement							
	(n=70)	(n=30)	(n=8)	(n=7)	(n=12)	(n=33)	(n=230)
Failed	14 %	13 %	12 %	14 %	8 %	27 %	15 %
Reasons for Failing CCC Placement							
	(n=9)	(n=3)	(n=8)	(n=0)	(n=1)	(n=7)	(n=28)
Positive drug UA	44 %	67 %	38 %	-- %	0 %	43 %	43 %
Positive alcohol[1]	22 %	0 %	12 %	-- %	100 %	0 %	14 %
Possession of drugs	11 %	0 %	0 %	-- %	0 %	0 %	4 %
Possession of alcohol	0 %	0 %	12 %	-- %	0 %	14 %	7 %
Act of violence	11 %	0 %	12 %	-- %	0 %	14 %	11 %
Accountability[2]	0 %	0 %	0 %	-- %	0 %	0 %	0 %
Violation of CCC rules[3]	11 %	33 %	0 %	-- %	0 %	0 %	7 %
Escape	0 %	0 %	12 %	-- %	0 %	14 %	7 %
Other	1 %	0 %	12 %	-- %	0 %	14 %	8 %
Missing = 6							

Note: Due to rounding, columns may not sum to 100 percent.

[1] As determined by breathalyzer or urinalysis.
[2] Accountability transgressions consist almost solely of unexcused absences from the CCC.
[3] "Violation of CCC rules" comprises subjects who were cited for committing any of a variety of transgressions, including but not limited to gambling, acting disruptively, and refusing an order.

Table 22

Transitional Services Received and Release Status

Male Subjects

Subjects Who Received Transitional Services

	Comparison		Treatment				Total
	DAP (n=264)	Non-DAP (n=253)	Completed (n=405)	Drop-Out (n=16)	Discip. Discharge (n=20)	In-complete (n=65)	(n=1,023)
Received TS	33 %	40 %	92 %	50 %	35 %	74 %	61 %
Missing = 8							

Transitional Services Release Status

	(n=87)	(n=99)	(n=349)	(n=6)	(n=7)	(n=47)	(n=595)
Completed	58 %	76 %	66 %	67 %	43 %	62 %	66 %
Failed	24 %	15 %	18 %	0 %	57 %	26 %	19 %
Admin/Neutral[1]	18 %	9 %	16 %	33 %	0 %	13 %	15 %
Missing = 34							

Note: 493 male subjects did not receive a CCC placement, and 402 did not participate in transitional services.

[1] "Admin/Neutral" was used somewhat differently from region to region but, in general, this category seems to have been used when a subject was participating in but did not complete transitional services treatment due to some circumstance beyond his control.

Table 23

Transitional Services Received and Release Status

Female Subjects

Subjects Who Received Transitional Services

	Comparison		Treatment				Total
	DAP (n=70)	Non-DAP (n=29)	Completed (n=78)	Drop-Out (n=7)	Discip. Discharge (n=11)	In-complete (n=32)	(n=227)
Received TS	27 %	28 %	96 %	43 %	45 %	84 %	60 %
Missing = 3							

Transitional Services Release Status

	(n=18)	(n=8)	(n=72)	(n=2)	(n=4)	(n=25)	(n=129)
Completed	67 %	62 %	80 %	100 %	100 %	52 %	72 %
Failed	28 %	25 %	10 %	0 %	0 %	32 %	17 %
Admin/Neutral[1]	6 %	12 %	11 %	0 %	0 %	16 %	11 %
Missing = 11							

Note: 112 female subjects did not receive a CCC placement, and 90 did not participate in transitional services.

[1] "Admin/Neutral" was used somewhat differently from region to region but, in general, this category seems to have been used when a subject was participating in but did not complete transitional services treatment due to some circumstance beyond her control.

Table 24

Drug and Alcohol Urinalysis During CCC Placement

Male Subjects

Percent Tested for Drugs and Alcohol

	Comparison		Treatment				Total
	DAP (n=241)	Non-DAP (n=229)	Completed (n=377)	Drop-Out (n=13)	Discip. Discharge (n=16)	In-complete (n=62)	(n=938)
Percent tested	98%	99%	93%	77%	100%	87%	95%
Missing = 93							

Percent of Subjects Testing Positive for Drugs or Alcohol

	(n=235)	(n=226)	(n=349)	(n=10)	(n=16)	(n=54)	(n=890)
Percent testing positive	17%	14%	15%	10%	50%	17%	16%
Missing = 1							

Drugs Positive By Urinalysis or Breathalyzer

Drug Type	(n=31)	(n=39)	(n=50)	(n=1)	(n=8)	(n=8)	(n=137)
Alcohol	13%	26%	20%	0%	25%	38%	21%
Amphetamines	3%	3%	4%	0%	12%	0%	4%
Barbiturates	0%	3%	0%	0%	0%	0%	1%
Benzodiazepines	0%	3%	0%	0%	25%	0%	2%
Cocaine	55%	33%	40%	0%	12%	50%	40%
Heroin	0%	0%	0%	0%	0%	0%	0%
LSD	0%	0%	0%	0%	0%	0%	0%
Marijuana	23%	23%	24%	0%	25%	12%	23%
Methadone	0%	0%	0%	0%	0%	0%	0%
Opiates	6%	10%	12%	100%	0%	0%	9%
PCP	0%	0%	0%	0%	0%	0%	0%
Missing = 4							

Note: 493 male subjects did not receive a CCC placement. More than one positive test was reported for 20 male subjects. The results shown reflect the first positive UA. Due to rounding, columns may not sum to 100 percent.

Table 25

Drug and Alcohol Urinalysis During CCC Placement

Female Subjects

Percent Tested for Drugs and Alcohol

	Comparison		Treatment				Total
	DAP (n=62)	Non-DAP (n=27)	Completed (n=75)	Drop-Out (n=7)	Discip. Discharge (n=8)	In-complete (n=30)	(n=209)
Percent tested	98%	93%	92%	86%	88%	90%	93%
Missing = 21							

Percent of Subjects Testing Positive for Drugs or Alcohol

	(n=61)	(n=25)	(n=69)	(n=6)	(n=7)	(n=27)	(n=195)
Percent testing positive	11%	12%	6%	0%	0%	11%	9%

Drugs Positive By Urinalysis or Breathalyzer

Drug Type	(n=7)	(n=3)	(n=4)	(n=0)	(n=0)	(n=3)	(n=17)
Alcohol	14%	33%	25%	0%	0%	0%	18%
Amphetamines	0%	0%	0%	0%	0%	0%	0%
Barbiturates	0%	0%	0%	0%	0%	0%	0%
Benzodiazepines	0%	0%	25%	0%	0%	33%	12%
Cocaine	57%	0%	50%	0%	0%	67%	47%
Heroin	0%	0%	0%	0%	0%	0%	0%
Marijuana	14%	33%	0%	0%	0%	0%	12%
Methadone	0%	0%	0%	0%	0%	0%	0%
LSD	0%	0%	0%	0%	0%	0%	0%
Opiates	14%	0%	0%	0%	0%	0%	6%
PCP	0%	0%	0%	0%	0%	0%	0%
Prescription[1]	0%	33%	0%	0%	0%	0%	6%

Note: 112 female subjects did not receive CCC placements. More than one positive test was reported for four female subjects. The results shown reflect the first positive UA. Due to rounding, columns may not sum to 100 percent.

[1] The prescription drug may or may not have been prescribed to the inmate; therefore, we do not know whether the positive test was a transgression of the rules.

Table 26

Post-Release Offenses

Male Subjects

Arrested for New Offense							
	Comparison		Treatment				Total
	DAP (n=430)	Non-DAP (n=375)	Completed (n=523)	Dropout (n=37)	Discip. Discharge (n=55)	In-complete (n=104)	(n=1,524)
New offense	14 %	16 %	11 %	19 %	18 %	15 %	14 %
Offense Type							
	(n=61)	(n=60)	(n=56)	(n=7)	(n=10)	(n=16)	(n=210)
Drug-related	10 %	20 %	16 %	29 %	20 %	25 %	17 %
Violent	16 %	15 %	13 %	14 %	40 %	0 %	15 %
Robbery	5 %	7 %	9 %	0 %	0 %	6 %	6 %
Property	15 %	22 %	20 %	14 %	10 %	13 %	18 %
Forgery, fraud	8 %	8 %	2 %	0 %	10 %	6 %	6 %
Traffic	20 %	12 %	20 %	29 %	10 %	19 %	17 %
Other	26 %	17 %	21 %	14 %	10 %	31 %	21 %

Table 27

Post-Release Offenses

Female Subjects

Arrested for New Offense							
	Comparison		Treatment				Total
	DAP (n=113)	Non-DAP (n=49)	Completed (n=97)	Dropout (n=16)	Discip. Discharge (n=24)	In-complete (n=43)	(n=342)
New offense	4 %	8 %	3 %	12 %	12 %	9 %	6 %
Offense Type							
	(n=5)	(n=4)	(n=3)	(n=2)	(n=3)	(n=4)	(n=21)
Drug-related	0 %	25 %	0 %	50 %	0 %	50 %	19 %
Violent	0 %	0 %	33 %	0 %	0 %	0 %	5 %
Robbery	0 %	0 %	0 %	0 %	0 %	0 %	0 %
Property	40 %	50 %	0 %	0 %	33 %	25 %	29 %
Forgery, fraud	0 %	0 %	33 %	50 %	0 %	0 %	10 %
Traffic	20 %	25 %	33 %	0 %	33 %	0 %	19 %
Other	40 %	0 %	0 %	0 %	33 %	25 %	19 %

Table 28

Post-Release Offenses for Supervised Subjects

Male Subjects

	Comparison		Treatment				Total
	DAP (n=430)	Non-DAP (n=375)	Completed (n=523)	Dropout (n=37)	Discip. Discharge (n=55)	In-complete (n=104)	(n=1,524)
Subjects Who Were Supervised Upon Release							
Supervised	89 %	79 %	88 %	84 %	76 %	75 %	85 %
Subjects With New Offense or Revocation							
	(n=382)	(n=296)	(n=459)	(n=31)	(n=42)	(n=78)	(n=1,288)
New offense	14 %	16 %	11 %	19 %	17 %	14 %	14 %
Revocation	8 %	8 %	6 %	6 %	10 %	12 %	7 %
Not arrested	78 %	76 %	84 %	74 %	74 %	74 %	78 %
Offense Type							
	(n=85)	(n=70)	(n=75)	(n=8)	(n=11)	(n=20)	(n=269)
Drug-related	5 %	10 %	8 %	25 %	9 %	20 %	9 %
Violent	11 %	11 %	9 %	13 %	27 %	0 %	10 %
Robbery	2 %	4 %	7 %	0 %	0 %	0 %	4 %
Property	7 %	13 %	9 %	0 %	9 %	5 %	9 %
Forgery, fraud	6 %	6 %	1 %	0 %	0 %	0 %	4 %
Traffic	14 %	9 %	15 %	25 %	9 %	15 %	13 %
Revocation	36 %	33 %	35 %	25 %	36 %	45 %	35 %
Other	19 %	14 %	16 %	12 %	9 %	15 %	16 %

Note: Due to rounding, columns may not sum to 100 percent.

Table 29

Post-Release Offenses for Supervised Subjects

Female Subjects

Subjects Who Were Supervised Upon Release							
	(n=113)	(n=49)	(n=97)	(n=16)	(n=24)	(n=43)	(n=342)
Supervised	87 %	67 %	91 %	75 %	63 %	81 %	82 %
Subjects With New Offense or Revocation							
	(n=98)	(n=33)	(n=88)	(n=12)	(n=15)	(n=35)	(n=281)
New offense	5 %	6 %	3 %	17 %	20 %	6 %	6 %
Revocation	3 %	6 %	9 %	0 %	7 %	3 %	5 %
Not arrested	92 %	88 %	88 %	83 %	73 %	91 %	89 %
Offense Type							
	(n=8)	(n=4)	(n=11)	(n=2)	(n=4)	(n=3)	(n=32)
Drug-related	0 %	0 %	0 %	50 %	0 %	33 %	6 %
Violent	0 %	0 %	9 %	0 %	0 %	0 %	3 %
Robbery	0 %	0 %	0 %	0 %	0 %	0 %	0 %
Property	25 %	25 %	0 %	0 %	25 %	33 %	16 %
Forgery, fraud	0 %	0 %	9 %	50 %	0 %	0 %	6 %
Traffic	13 %	25 %	9 %	0 %	25 %	0 %	13 %
Revocation	38 %	50 %	73 %	0 %	25 %	33 %	47 %
Other	25 %	0 %	0 %	0 %	25 %	0 %	9 %

Note: Due to rounding, columns may not sum to 100 percent.

Table 30

Post-Release Drug and Alcohol Use

Supervised Male Subjects

	Comparison		Treatment				Total
	DAP (n=343)	Non-DAP (n=260)	Completed (n=430)	Dropout (n=26)	Discip. Discharge (n=38)	In-complete (n=68)	(n=1,165)
Monthly Rate of Urinalysis							
Mean	2. 57	2. 29	2. 64	2. 46	2. 26	2. 47	2. 51
Std. dev.	1. 97	1. 80	1. 99	2. 34	2. 25	1. 88	1. 96
Drug/Alcohol Use While Under Supervision							
	(n=382)	(n=296)	(n=459)	(n=31)	(n=42)	(n=78)	(n=1,288)
Used drugs/alcohol	31 %	34 %	28 %	16 %	36 %	33 %	31 %
Method of Detection of Drug/Alcohol Use							
	(n=119)	(n=100)	(n=129)	(n=5)	(n=15)	(n=26)	(n=394)
Positive UA	68 %	78 %	70 %	80 %	80 %	73 %	72 %
UA miss/refusal	21 %	18 %	19 %	20 %	7 %	23 %	19 %
Admit drug use	10 %	2 %	10 %	0 %	7 %	4 %	7 %
Positive breathalyzer	1 %	2 %	1 %	0 %	7 %	0 %	1 %
Positive Urinalysis by Drug Type							
	(n=81)	(n=78)	(n=90)	(n=4)	(n=12)	(n=19)	(n=284)
Alcohol	5 %	3 %	6 %	0 %	8 %	0 %	4 %
Amphetamine	2 %	1 %	2 %	25 %	8 %	0 %	2 %
Barbiturate	1 %	1 %	1 %	0 %	0 %	0 %	1 %
Benzodiazepine	0 %	3 %	0 %	0 %	0 %	0 %	1 %
Cocaine	47 %	50 %	47 %	50 %	25 %	58 %	48 %
Heroin	0 %	4 %	1 %	0 %	0 %	0 %	1 %
Marijuana	27 %	24 %	21 %	25 %	42 %	11 %	24 %
Opiate	10 %	5 %	13 %	0 %	0 %	16 %	10 %
Multiple[1]	7 %	9 %	9 %	0 %	17 %	16 %	9 %

Note: 236 male subjects were not under post-release supervision. Due to rounding, columns may not sum to 100 percent.

[1] 22 male subjects tested positive for two drugs (20 used cocaine in combination with another drug, and 2 used amphetamines in combination with another drug); 3 subjects tested positive for three drugs (cocaine in combination with two other drugs); and one subject tested positive for four drugs (cocaine, heroin, LSD, and alcohol).

Table 31

Post-Release Drug and Alcohol Use

Supervised Female Subjects

	Comparison		Treatment				Total
	DAP (n=86)	Non-DAP (n=26)	Completed (n=80)	Dropout (n=11)	Discip. Discharge (n=14)	In-complete (n=33)	(n=250)
Monthly Rate of Urinalysis Testing							
Mean	2. 50	2. 66	2. 97	2. 65	2. 63	2. 34	2. 66
Std. dev.	1. 93	2. 39	2. 04	2. 13	1. 52	1. 79	1. 98
Drug/Alcohol Use While Under Supervision							
	(n=98)	(n=33)	(n=88)	(n=12)	(n=15)	(n=35)	(n=281)
Used drugs/alcohol	19 %	21 %	19 %	17 %	20 %	23 %	20 %
Method of Detection of Drug/Alcohol Use							
	(n=19)	(n=7)	(n=17)	(n=2)	(n=3)	(n=8)	(n=56)
Positive UA	84 %	43 %	71 %	100 %	100 %	38 %	70 %
UA miss/refusal	11 %	29 %	24 %	0 %	0 %	25 %	18 %
Admit drug use	5 %	29 %	6 %	0 %	0 %	38 %	12 %
Positive breathalyser	0 %	0 %	0 %	0 %	0 %	0 %	0 %
Positive Urinalysis Tests by Drug Type							
	(n=16)	(n=3)	(n=12)	(n=2)	(n=3)	(n=3)	(n=39)
Alcohol	0 %	0 %	0 %	0 %	0 %	0 %	0 %
Amphetamine	0 %	0 %	0 %	0 %	0 %	0 %	0 %
Barbiturate	6 %	0 %	0 %	0 %	0 %	0 %	3 %
Benzodiazepine	6 %	0 %	0 %	0 %	0 %	0 %	3 %
Cocaine	69 %	0 %	83 %	50 %	100 %	67 %	69 %
Heroin	0 %	0 %	0 %	0 %	0 %	0 %	0 %
Marijuana	13 %	100 %	0 %	0 %	0 %	33 %	15 %
Opiate	6 %	0 %	17 %	50 %	0 %	0 %	10 %
Multiple[1]	0 %	0 %	0 %	0 %	0 %	0 %	0 %

Note: 61 female subjects were not under post-release supervision. Due to rounding, columns may not sum to 100 percent.

[1] No female subjects tested positive for a combination of two or more drugs.

Table 32

Violations of Supervision and Post-Release CCC Placement

Supervised Male Subjects

	Comparison		Treatment				Total
	DAP (n=382)	Non-DAP (n=296)	Completed (n=459)	Dropout (n=31)	Discip. Discharge (n=42)	In-complete (n=78)	(n=1,288)
Percent Having a Violation of Supervision, Excluding Drug Use Violations							
Violated	19%	18%	17%	10%	21%	22%	18 %
Post-Release CCC Placement							
Placed in CCC	4%	3%	3%	0%	7%	4%	4 %

Note: 236 male subjects were not under post-release supervision.

Table 33

Violations of Supervision and Post-Release CCC Placement

Supervised Female Subjects

	Comparison		Treatment				Total
	DAP (n=98)	Non-DAP (n=33)	Completed (n=88)	Dropout (n=12)	Discip. Discharge (n=15)	In-complete (n=35)	(n=281)
Percent Having a Violation of Supervision, Excluding Drug Use Violations							
Violated	9 %	15 %	8 %	8 %	13 %	11 %	10 %
Post-Release CCC Placement							
Placed in CCC	1 %	3 %	2 %	0 %	0 %	3 %	2 %

Note: 61 female subjects were not under post-release supervision.

Table 34

Drug and Alcohol Treatment and Self-Help Participation

Supervised Male Subjects

Drug and Alcohol Treatment	Comparison		Treatment				Total
	DAP (n=377)	Non-DAP (n=293)	Completed (n=450)	Dropout (n=31)	Discip. Discharge (n=42)	In-complete (n=76)	(n=1,269)
Contract only	44 %	39 %	51 %	29 %	38 %	46 %	45 %
Non-contract only	2 %	4 %	3 %	6 %	5 %	8 %	3 %
Contract and non-contract	2 %	2 %	5 %	0 %	5 %	5 %	3 %
No treatment	52 %	55 %	41 %	65 %	52 %	41 %	49 %
Missing = 19							
Self-Help Groups (AA/NA)							
	(n=376)	(n=290)	(n=448)	(n=31)	(n=41)	(n=76)	(n=1,262)
Participated	18 %	23 %	26 %	16 %	24 %	18 %	22 %
Missing = 26							

Note: 236 male subjects were not under post-release supervision.

Table 35

Drug and Alcohol Treatment and Self-Help Participation

Supervised Female Subjects

Drug and Alcohol Treatment	Comparison		Treatment				Total
	DAP (n=93)	Non-DAP (n=31)	Completed (n=87)	Dropout (n=12)	Discip. Discharge (n=14)	In-complete (n=35)	(n=272)
Contract only	49 %	39 %	60 %	42 %	50 %	46 %	51 %
Non-contract only	1 %	3 %	3 %	0 %	0 %	11 %	3 %
Contract and non-contract	2 %	6 %	1 %	8 %	7 %	0 %	3 %
No treatment	47 %	52 %	36 %	50 %	43 %	43 %	43 %
Missing = 9							
Self-Help Groups (AA/NA)							
	(n=94)	(n=31)	(n=86)	(n=12)	(n=14)	(n=35)	(n=272)
Participated	21 %	19 %	34 %	25 %	43 %	46 %	29 %
Missing = 9							

Note: 61 female subjects were not under post-release supervision.

Table 36

Employment and Cohabitation Status

Supervised Male Subjects

Employment Status

	Comparison		Treatment				Total
	DAP (n=378)	Non-DAP (n=293)	Completed (n=452)	Dropout (n=31)	Discip. Discharge (n=40)	In-complete (n=75)	(n=1,269)
Full-time/entire period	41 %	41 %	49 %	32 %	33 %	47 %	44 %
Full-time/partial period	29 %	34 %	29 %	26 %	25 %	20 %	29 %
Part-time	10 %	9 %	7 %	13 %	13 %	7 %	9 %
Ineligible[1]	6 %	4 %	5 %	3 %	8 %	8 %	5 %
Unemployed	15 %	12 %	10 %	26 %	23 %	19 %	13 %
Missing = 19							

Cohabitation Status

	(n=382)	(n=295)	(n=455)	(n=31)	(n=42)	(n=76)	(n=1,281)
With spouse	17 %	20 %	22 %	16 %	12 %	26 %	20 %
With common-law spouse or paramour	13 %	14 %	15 %	19 %	12 %	17 %	15 %
Neither	69 %	65 %	63 %	65 %	76 %	57 %	65 %
Missing = 7							

Note: 236 male subjects were not under post-release supervision. Due to rounding, columns may not sum to 100 percent.

[1] Subjects were categorized as ineligible if they were involved in a school program, retired or disabled, or detainees who had very little time available “on the streets” to be employed.

Table 37

Employment and Cohabitation Status

Supervised Female Subjects

Employment Status

	Comparison		Treatment				Total
	DAP (n=97)	Non-DAP (n=31)	Completed (n=87)	Dropout (n=12)	Discip. Discharge (n=14)	In-complete (n=34)	(n=275)
Full-time/entire period	26 %	26 %	34 %	17 %	21 %	26 %	28 %
Full-time/some period	34 %	32 %	36 %	25 %	36 %	44 %	35 %
Part-time	14 %	13 %	18 %	25 %	14 %	18 %	16 %
Ineligible[1]	10 %	6 %	5 %	17 %	7 %	6 %	8 %
Unemployed	15 %	23 %	7 %	17 %	21 %	6 %	13 %
Missing = 6							

Cohabitation Status

	(n=97)	(n=32)	(n=87)	(n=12)	(n=15)	(n=35)	(n=278)
With spouse	12 %	12 %	10 %	17 %	7 %	11 %	12 %
With common-law spouse or paramour	13 %	12 %	3 %	0 %	7 %	9 %	9 %
Neither	74 %	75 %	86 %	83 %	87 %	80 %	80 %
Missing = 3							

Note: 61 female subjects were not under post-release supervision. Due to rounding, columns may not sum to 100 percent.

[1] Subjects were categorized as ineligible if they were involved in a school program, retired or disabled, or detainees who had very little time available "on the streets" to be employed.

Table 38
Traditional Models of Failure as Arrest: Supervised and Unsupervised Subjects

Male and Female Subjects

Model of Arrest with:	L^2	DF	Diff.[†]	DF
Base Variables	2049.9	38	---	---
Base and Additional Background Variables	2041.6	45	8.3	7
Base and Change Assessment Variables	2044.1	44	5.8	6
Base and Additional Treatment Variables	2047.5	41	2.4	3
Base and Supervision Variables	2041.9	41	8.0**	3
Final Model Variables	2029.8	49	20.1**	11

Male Subjects

Model of Arrest with:	L^2	DF	Diff.[†]	DF
Base Variables	1836.4	37	---	---
Base and Additional Background Variables	1826.8	44	9.6	7
Base and Change Assessment Variables	1830.3	43	6.1	6
Base and Additional Treatment Variables	1832.9	40	3.5	3
Base and Supervision Variables	1830.9	40	5.5	3
Final Model Variables	1823.0	48	13.4	11

Notes to Table:

[†]Difference in L^2 between traditional base model and model in question.

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* Significant at $p < .10$
** Significant at $p < .05$
*** Significant at $p < .01$

NOTE: The blocks for additional treatment variables and supervision variables exclude variables not available for unsupervised subjects.

Table 39
Traditional Models of Failure as Arrest: Supervised Subjects Only

Male and Female Subjects

Model of Arrest with:	L^2	DF	Diff.†	DF
Base Variables	1682.2	38	---	---
Base and Additional Background Variables	1672.8	45	9.4	7
Base and Change Assessment Variables	1673.4	44	8.8	6
Base and Additional Treatment Variables	1670.6	47	11.6	9
Base and Supervision Variables	1666.7	43	15.5***	5
Base and Post Release Behavior Variables	1546.4	43	135.8***	5
Final Model Variables	1521.1	55	161.1***	17

Male Subjects

Model of Arrest with:	L^2	DF	Diff.†	DF
Base Variables	1506.4	37	---	---
Base and Additional Background Variables	1494.3	44	12.1	7
Base and Change Assessment Variables	1497.1	43	9.3	6
Base with Additional Treatment Variables	1494.3	46	12.1	9
Base with Supervision Variables	1492.9	42	13.5***	5
Base and Post Release Behavior Variables	1377.2	42	129.2***	5
Final Model Variables	1363.9	54	142.5***	17

Notes to Table:

†Difference in L^2 between traditional base model and model in question.

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* Significant at $p < .10$
** Significant at $p < .05$
*** Significant at $p < .01$

Table 40
Traditional Models of Failure as Arrest or Revocation‡

Male and Female Subjects

Model of Arrest or Revocation with:	L^2	DF	Diff.†	DF
Base Variables	2427.4	38	---	---
Base and Additional Background Variables	2413.6	45	13.8*	7
Base and Change Assessment Variables	2418.6	44	8.8	6
Base with Additional Treatment Variables	2374.5	47	52.9***	9
Base with Supervision Variables	2375.0	42	52.4***	4
Base and Post Release Behavior Variables	2066.1	43	361.3***	5
Final Model Variables	1983.9	64	443.5***	26

Male Subjects

Model of Arrest or Revocation with:	L^2	DF	Diff.†	DF
Base Variables	2143.4	37	---	---
Base and Additional Background Variables	2131.8	44	11.6	7
Base and Change Assessment Variables	2135.1	43	8.3	6
Base with Additional Treatment Variables	2098.1	46	45.3***	9
Base with Supervision Variables	2103.2	41	40.2***	4
Base and Post Release Behavior Variables	1824.7	42	318.7***	5
Final Model Variables	1755.6	63	387.8***	26

Notes to Table:

†Difference in L^2 between traditional base model and model in question.

* Significant at $p < .10$
** Significant at $p < .05$
*** Significant at $p < .01$

‡ Only includes subjects who were under supervision and thus had a probability of revocation greater than zero.

Table 41
Traditional Models of Failure as Evidence of Drug Use[‡]

Male and Female Subjects

Model of Drug Use with:	x^2	DF	Diff.[†]	DF
Base Variables	3012.8	38	---	---
Base and Additional Background Variables	3006.0	45	6.8	7
Base and Change Assessment Variables	3004.8	44	8.0	6
Base and Additional Treatment Variables	2854.0	47	158.8***	9
Base and Supervision Variables	2934.1	42	78.7***	4
Base and Post Release Behavior Variables	2921.4	42	91.4***	4
Final Model Variables	2745.5	63	267.3***	25

Male Subjects

Model of Drug use with:	x^2	DF	Diff.[†]	DF
Base Variables	2585.3	37	---	---
Base and Additional Background Variables	2576.6	44	8.7	7
Base and Change Assessment Variables	2578.0	43	7.3	6
Base with Additional Treatment Variables	2447.5	46	137.8***	9
Base with Supervision Variables	2513.5	41	71.8***	4
Base and Post Release Behavior Variables	2510.1	41	75.2***	4
Final Model Variables	2355.2	62	230.1***	25

Notes to Table:

[†]Difference in x^2 between traditional base model and model in question.

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* Significant at $p < .10$
** Significant at $p < .05$
*** Significant at $p < .01$

[‡] Only includes subjects who were under supervision and who had urine testing, thus had a probability of drug use greater than zero.

Table 42
Arrests, Male and Female Subjects, Supervised and Unsupervised Subjects
Traditional Model

Variable	Base Model b	Base Model se(b)	Base Model OR	Final Model b	Final Model se(b)	Final Model OR
Base Model Variables						
INTERCPT	-3.2054***	0.6236	.	-3.2548***	0.6391	.
COMPDAP	-0.1506	0.1870	0.860	-0.2483	0.1954	0.780
GRAD12MO	-0.2497	0.2949	0.779	-0.4186	0.3187	0.658
GRAD9MO	-0.4604**	0.2089	0.631	-0.5320**	0.2204	0.587
INCOMPTX	-0.0221	0.2723	0.978	-0.2413	0.3025	0.786
DISCIPTX	0.0113	0.3206	1.011	-0.1370	0.3344	0.872
WITHDRTX	0.3772	0.3788	1.458	0.2147	0.3913	1.239
EBLACK	-0.1130	0.1491	0.893	-0.0891	0.1509	0.915
ERACEOTH	0.2778	0.2328	1.320	0.2072	0.2359	1.230
EHISP	-0.2493	0.1528	0.779	-0.2566*	0.1534	0.774
EFEM	-0.3300***	0.1252	0.719	-0.3257***	0.1253	0.722
EPRIORCM	0.4429***	0.1029	1.557	0.4222***	0.1033	1.525
ERECVIOL	0.2695**	0.1153	1.309	0.2722**	0.1167	1.313
EPASTVIO	-0.0328	0.1113	0.968	-0.0712	0.1123	0.931
TIMESRVD	0.0308	0.0369	1.031	0.0271	0.0369	1.028
ECJSPVNY	0.1940	0.2155	1.214	0.2223	0.2195	1.249
ECJSPVNM	-0.2070	0.4083	0.813	-0.2232	0.4138	0.800
AGERLSE	-0.0487***	0.0104	0.952	-0.0455***	0.0104	0.956
ESUPILL	-0.0359	0.0758	0.965	-0.0351	0.0760	0.965
EWORKJOB	0.0014	0.1729	1.001	-0.0104	0.1749	0.990
ELEGITUN	-0.0707	0.3191	0.932	-0.0522	0.3208	0.949
EUNEMP	0.0232	0.2391	1.023	0.0103	0.2405	1.010
EJOB_UNK	-0.1396	0.4954	0.870	-0.1168	0.5010	0.890
GRADEA	0.0207	0.0367	1.021	0.0194	0.0365	1.020
COC_FRQ	0.0013	0.0391	1.001	0.0014	0.0397	1.001
CRK_FRQ	-0.0546	0.0415	0.947	-0.0573	0.0416	0.944
HAL_FRQ	0.0042	0.0585	1.004	-0.0115	0.0593	0.989
HER_FRQ	0.0278	0.0429	1.028	0.0120	0.0433	1.012
OPIA_FRQ	-0.0020	0.0607	0.998	0.0027	0.0609	1.003
POT_FRQ	-0.0593	0.0420	0.942	-0.0574	0.0424	0.944
BARB_FRQ	0.0387	0.0586	1.039	0.0437	0.0591	1.045
STIM_FRQ	-0.0194	0.0466	0.981	-0.0138	0.0466	0.986
DEPLOGTA	0.0188	0.0209	1.019	0.0190	0.0209	1.019
DEPLOGTD	0.0169	0.0355	1.017	0.0091	0.0355	1.009

Table 42 — Continued

Variable	Base Model b	Base Model se(b)	Base Model OR	Final Model b	Final Model se(b)	Final Model OR
Supervision Variables						
ECCCNO				-0.0017	0.1034	0.998
ECCCFAIL				0.2585**	0.1177	1.295
SUP_REL				0.1990	0.2001	1.220
ECOHO1_6				0.2173	0.2221	1.243
ECOHO7				0.4607*	0.2765	1.585
ECOHO8				0.2614	0.2268	1.299
ECOHO9				-0.3405	0.2887	0.711
ECOHO10				0.2854	0.1972	1.330
ECOHO11				-0.3884*	0.2342	0.678
ECOHO12				-0.1007	0.1648	0.904
ECOHO13				-0.0877	0.1752	0.916
D_T2	0.4280	0.2623	1.534	0.4340*	0.2626	1.543
D_T3	0.4751*	0.2624	1.608	0.4826*	0.2627	1.620
D_T4	0.6071**	0.2586	1.835	0.6205**	0.2589	1.860
D_T5	0.8791***	0.2497	2.409	0.8988***	0.2501	2.457
D_T6	0.6786***	0.2614	1.971	0.7050***	0.2618	2.024
-2 Log Likelihood						
Intercept Only		2176.541			2176.541	
Intercept and Covariates		2049.924			2029.842	
Concordant Pairs		69.4%			71.4%	
Somer's D		0.420			0.458	
Hosmer-Lemeshow Goodness of Fit	9.7344 with 8 DF (p=0.2842)			11.431 with 8 DF (p=0.1785)		

* $p < .10$
** $p < .05$
*** $p < .01$

Table 43
Arrests, Male and Female Subjects, Supervised Subjects Only
Traditional Model

Variable	Base Model b	Base Model se(b)	Base Model OR	Final Model b	Final Model se(b)	Final Model OR
Base Model Variables						
INTERCPT	-2.4360***	0.6766	.	-2.5776***	0.7021	.
COMPDAP	-0.1641	0.2068	0.849	-0.2076	0.2185	0.813
GRAD12MO	-0.2520	0.3042	0.777	-0.2137	0.3389	0.808
GRAD9MO	-0.5114**	0.2323	0.600	-0.4099*	0.2503	0.664
INCOMPTX	-0.1910	0.3309	0.826	-0.2926	0.3676	0.746
DISCIPTX	0.0000	0.3693	1.000	-0.2959	0.3936	0.744
WITHDRTX	0.3918	0.4130	1.480	0.3547	0.4568	1.426
EBLACK	-0.1061	0.1591	0.899	-0.1186	0.1658	0.888
ERACEOTH	0.2857	0.2381	1.331	0.1488	0.2565	1.160
EHISP	-0.1302	0.1569	0.878	-0.1625	0.1625	0.850
EFEM	-0.2820**	0.1397	0.754	-0.3192**	0.1447	0.727
EPRIORCM	0.4631***	0.1080	1.589	0.4190***	0.1126	1.520
ERECVIOL	0.1512	0.1311	1.163	0.1359	0.1377	1.146
EPASTVIO	0.0130	0.1244	1.013	-0.0432	0.1313	0.958
TIMESRVD	0.0472	0.0384	1.048	0.1054***	0.0399	1.111
ECJSPVNY	0.0996	0.2262	1.105	0.0385	0.2490	1.039
ECJSPVNM	-0.0585	0.4201	0.943	-0.0415	0.4679	0.959
AGERLSE	-0.0587***	0.0116	0.943	-0.0564***	0.0113	0.945
ESUPILL	-0.0624	0.0845	0.940	-0.1051	0.0885	0.900
EWORKJOB	-0.1144	0.1852	0.892	0.1772	0.1981	1.194
ELEGITUN	-0.0746	0.3407	0.928	-0.5095	0.3514	0.601
EUNEMP	-0.2133	0.2702	0.808	-0.3226	0.2832	0.724
EJOB_UNK	0.3189	0.5154	1.376	0.5281	0.5630	1.696
GRADEA	0.0177	0.0408	1.018	0.0470	0.0424	1.048
COC_FRQ	-0.0096	0.0435	0.990	0.0060	0.0460	1.006
CRK_FRQ	-0.0685	0.0479	0.934	-0.0920*	0.0493	0.912
HAL_FRQ	0.0232	0.0634	1.023	0.0092	0.0662	1.009
HER_FRQ	0.0172	0.0487	1.017	-0.0094	0.0508	0.991
OPIA_FRQ	-0.0357	0.0687	0.965	-0.0441	0.0747	0.957
POT_FRQ	-0.0594	0.0471	0.942	-0.0552	0.0492	0.946
BARB_FRQ	0.0780	0.0632	1.081	0.0776	0.0660	1.081
STIM_FRQ	-0.0407	0.0521	0.960	-0.0130	0.0536	0.987
DEPLOGTA	0.0533**	0.0233	1.055	0.0533**	0.0233	1.055
DEPLOGTD	0.0076	0.0395	1.008	0.0050	0.0400	1.005

Table 43 — Continued

Variable	Base Model b	Base Model se(b)	Base Model OR	Final Model b	Final Model se(b)	Final Model OR
Supervision Variables						
ECCCNO				-0.1379	0.1203	0.871
ECCCFAIL				0.1235	0.1377	1.131
UARATE				-0.1477***	0.0468	0.863
HHSE_STR				-0.7069	0.7396	0.493
Post-Release Variables						
ESPOUSE				-0.5367***	0.1957	0.585
ECOM_LAW				0.1650	0.1701	1.179
SUPVVIOL				-0.0312	0.0632	0.969
DIRTY				0.6347***	0.1834	1.886
EMP_HRS				-0.0438***	0.0049	0.957
ECOHO1_6				0.1515	0.2504	1.164
ECOHO7				0.1973	0.3788	1.218
ECOHO8				0.1925	0.2771	1.212
ECOHO9				-0.4091	0.3515	0.664
ECOHO10				0.4151*	0.2318	1.514
ECOHO11				-0.4751*	0.2606	0.622
ECOHO12				0.1567	0.1854	1.170
ECOHO13				0.1192	0.1910	1.127
D_T2	0.3111	0.2866	1.365	0.3639	0.2914	1.439
D_T3	0.4246	0.2828	1.529	0.4872*	0.2884	1.628
D_T4	0.4438	0.2848	1.559	0.5361*	0.2923	1.709
D_T5	0.8082***	0.2700	2.244	0.9025***	0.2785	2.466
D_T6	0.7039**	0.2782	2.022	0.8213***	0.2871	2.273
-2 Log Likelihood						
Intercept Only		1798.026			1798.026	
Intercept and Covariates		1682.162			1521.068	
Concordant Pairs		70.7%			83.0%	
Somer's D		0.444			0.675	
Hosmer-Lemshow Goodness of Fit	8.4068 with 8 DF (p=0.3948)			16.380 with 8 DF (p=0.0373)		

* p < .10
** p < .05
*** p < .01

Table 44
Arrests or Revocations, Male and Female Subjects
Traditional Model

Variable	Base Model b	Base Model se(b)	Base Model OR	Final Model b	Final Model se(b)	Final Model OR
Base Model Variables						
INTERCPT	-3.0183***	0.5327	.	-3.3434***	0.6664	.
COMPDAP	-0.0746	0.1684	0.928	-0.1634	0.1870	0.849
GRAD12MO	-0.2665	0.2535	0.766	-0.1965	0.3039	0.822
GRAD9MO	-0.3808**	0.1840	0.683	-0.1700	0.2182	0.844
INCOMPTX	-0.0361	0.2573	0.965	-0.0794	0.2993	0.924
DISCIPTX	0.1603	0.3016	1.174	-0.3386	0.3461	0.713
WITHDRTX	0.2856	0.3621	1.331	0.4614	0.4059	1.586
EBLACK	-0.0516	0.1358	0.950	0.0256	0.1510	1.026
ERACEOTH	0.2547	0.2141	1.290	-0.0881	0.2428	0.916
EHISP	-0.1014	0.1280	0.904	-0.0674	0.1374	0.935
EFEM	-0.2332**	0.1057	0.792	-0.3042***	0.1160	0.738
EPRIORCM	0.3437***	0.0867	1.410	0.2801***	0.0950	1.323
ERECVIOL	0.1224	0.1095	1.130	0.1411	0.1211	1.151
EPASTVIO	0.0843	0.0986	1.088	-0.0258	0.1108	0.975
TIMESRVD	-0.0045	0.0328	0.995	0.0814**	0.0351	1.085
ECJSPVNY	0.1178	0.1671	1.125	-0.0182	0.1918	0.982
ECJSPVNM	0.0699	0.3072	1.072	0.1777	0.3560	1.194
AGERLSE	-0.0255***	0.0086	0.975	-0.0275***	0.0090	0.973
ESUPILL	-0.0288	0.0670	0.972	-0.0628	0.0738	0.939
EWORKJOB	-0.1033	0.1484	0.902	0.3503**	0.1675	1.420
ELEGITUN	-0.0915	0.2636	0.913	-0.5850**	0.2851	0.557
EUNEMP	-0.2782	0.2222	0.757	-0.5042**	0.2483	0.604
EJOB_UNK	0.2652	0.4086	1.304	0.4583	0.4784	1.581
GRADEA	0.0124	0.0319	1.012	0.0280	0.0340	1.028
COC_FRQ	0.0008	0.0343	1.001	-0.0047	0.0385	0.995
CRK_FRQ	-0.0066	0.0351	0.993	-0.0602	0.0384	0.942
HAL_FRQ	0.0642	0.0503	1.066	0.0323	0.0552	1.033
HER_FRQ	0.0187	0.0370	1.019	-0.0288	0.0406	0.972
OPIA_FRQ	0.0063	0.0526	1.006	-0.0164	0.0612	0.984
POT_FRQ	-0.0533	0.0370	0.948	-0.0709*	0.0410	0.932
BARB_FRQ	0.0117	0.0515	1.012	0.0134	0.0561	1.013
STIM_FRQ	-0.0610	0.0417	0.941	0.0122	0.0448	1.012
DEPLOGTA	0.0225	0.0186	1.023	0.0260	0.0195	1.026
DEPLOGTD	0.0261	0.0311	1.026	0.0315	0.0339	1.032
Treatment Variables						
ENRGENY				-0.2749	0.1741	0.760
ENRSUPY				-0.1932	0.2781	0.824
ETSYES				-0.0427	0.0886	0.958
ECTRONLY				-0.2767	0.1735	0.758
EOTHONLY				-0.0780	0.2925	0.925
EBOTH				-0.9689***	0.3083	0.379
ETXMISS				1.6965***	0.4793	5.455
EAAYES				0.1294	0.1800	1.138
EAAMISS				-0.1694	0.3012	0.844

Table 44 — Continued

Variable	Base Model b	Base Model se(b)	Base Model OR	Final Model b	Final Model se(b)	Final Model OR
Supervision Variables						
ECCCNO				-0.1841	0.1161	0.832
ECCCFAIL				0.2301**	0.1173	1.259
UARATE				-0.1938***	0.0435	0.824
HHSE_STR				-0.2925	0.4667	0.746
Post-Release Variables						
ESPOUSE				-0.4205**	0.1710	0.657
ECOM_LAW				-0.0984	0.1617	0.906
SUPVVIOL				0.1286***	0.0399	1.137
DIRTY				1.1800***	0.1531	3.254
EMP_HRS				-0.0490***	0.0043	0.952
ECOHO1_6				0.0529	0.2138	1.054
ECOHO7				0.4958*	0.2873	1.642
ECOHO8				-0.0374	0.2440	0.963
ECOHO9				-0.5005*	0.2876	0.606
ECOHO10				0.4079**	0.1957	1.504
ECOHO11				-0.3035	0.2050	0.738
ECOHO12				0.2234	0.1554	1.250
ECOHO13				-0.1573	0.1689	0.854
Time Variables						
D_T2	0.2598	0.2411	1.297	0.3256	0.2541	1.385
D_T3	0.6349***	0.2267	1.887	0.7299***	0.2410	2.075
D_T4	0.6514***	0.2283	1.918	0.7872***	0.2442	2.197
D_T5	0.8282***	0.2242	2.289	0.9670***	0.2412	2.630
D_T6	0.7604***	0.2294	2.139	0.9316***	0.2469	2.539
-2 Log Likelihood						
Intercept Only		2548.969			2548.969	
Intercept and Covariates		2419.674			1989.642	
Concordant Pairs		67.7%			85.6%	
Somer's D		0.378			0.723	
Hosmer-Lemshow Goodness of Fit	9.5458 with 8 DF (p=0.2984)			12.177 with 8 DF (p=0.1435)		

* p < .10
** p < .05
*** p < .01

Table 45
Drug Use, Male and Female Subjects
Traditional Model

Variable	Base Model b	se(b)	OR	Final Model b	se(b)	OR
Base Model Variables						
INTERCPT	-1.8589***	0.4415		-2.2986***	0.5255	.
COMPDAP	-0.1051	0.1432	0.900	-0.3060**	0.1575	0.736
GRAD12MO	-0.4602**	0.2286	0.631	-0.6677***	0.2585	0.513
GRAD9MO	-0.1703	0.1494	0.843	-0.3858**	0.1752	0.680
INCOMPTX	-0.1492	0.2172	0.861	-0.4054*	0.2495	0.667
DISCIPTX	-0.1963	0.2835	0.822	-0.3719	0.3044	0.689
WITHDRTX	-0.4859	0.4090	0.615	-0.5971	0.4295	0.550
EBLACK	0.1767	0.1241	1.193	0.2656**	0.1323	1.304
ERACEOTH	0.3186	0.2075	1.375	0.1969	0.2209	1.218
EHISP	0.2257**	0.1023	1.253	0.2876***	0.1077	1.333
EFEM	-0.2862***	0.0856	0.751	-0.2747***	0.0909	0.760
EPRIORCM	0.1692**	0.0686	1.184	0.1761**	0.0729	1.193
ERECVIOL	0.0893	0.0961	1.093	0.1417	0.1017	1.152
EPASTVIO	0.0727	0.0852	1.075	0.0184	0.0905	1.019
TIMESRVD	-0.0943***	0.0321	0.910	-0.0591*	0.0315	0.943
ECJSPVNY	0.1290	0.1525	1.138	0.0644	0.1580	1.067
ECJSPVNM	0.0208	0.2847	1.021	0.0235	0.2924	1.024
AGERLSE	-0.0049	0.0073	0.995	0.0028	0.0077	1.003
ESUPILL	0.0302	0.0550	1.031	0.0191	0.0586	1.019
EWORKJOB	-0.1143	0.1268	0.892	0.0092	0.1337	1.009
ELEGITUN	0.1532	0.2164	1.166	0.1208	0.2326	1.128
EUNEMP	-0.0998	0.1727	0.905	-0.1925	0.1827	0.825
EJOB_UNK	-0.0442	0.3681	0.957	-0.0403	0.3768	0.960
GRADEA	-0.0033	0.0273	0.997	0.0088	0.0288	1.009
COC_FRQ	-0.0089	0.0288	0.991	-0.0175	0.0305	0.983
CRK_FRQ	0.0649**	0.0270	1.067	0.0453	0.0287	1.046
HAL_FRQ	0.0106	0.0439	1.011	0.0137	0.0456	1.014
HER_FRQ	0.0356	0.0298	1.036	-0.0085	0.0320	0.991
OPIA_FRQ	0.0407	0.0436	1.042	0.0641	0.0466	1.066
POT_FRQ	0.0596*	0.0326	1.061	0.0761**	0.0349	1.079
BARB_FRQ	-0.0601	0.0438	0.942	-0.0710	0.0466	0.931
STIM_FRQ	-0.0205	0.0351	0.980	-0.0059	0.0369	0.994
DEPLOGTA	-0.0167	0.0160	0.983	-0.0246	0.0165	0.976
DEPLOGTD	0.0663**	0.0273	1.069	0.0269	0.0287	1.027
Treatment Variables						
ENRGENY				-0.3344**	0.1339	0.716
ENRSUPY				-0.0374	0.1753	0.963
ETSYES				-0.0021	0.0723	0.998
ECTRONLY				-0.2598*	0.1557	0.771
EOTHONLY				-0.1778	0.2539	0.837
EBOTH				0.7955***	0.2261	2.216
ETXMISS				0.9226*	0.5018	2.516
EAAYES				0.2098	0.1516	1.233
EAAMISS				-0.2454	0.2672	0.782

Table 45 — Continued

Variable	Base Model b	se(b)	OR	Final Model b	se(b)	OR
Supervision Variables						
ECCCNO				-0.1568	0.1026	0.855
ECCCFAIL				0.4266***	0.0952	1.532
UARATE				0.0565*	0.0302	1.058
HHSE_STR				0.2063	0.3687	1.229
Post-Release Variables						
ESPOUSE				-0.2335**	0.1086	0.792
ECOM_LAW				-0.0191	0.1123	0.981
SUPVVIOL				0.2331***	0.0360	1.262
EMP_HRS				-0.0078***	0.0030	0.992
ECOHO1_6				0.0089	0.1841	1.009
ECOHO7				-0.1791	0.2810	0.836
ECOHO8				0.1746	0.2046	1.191
ECOHO9				0.0359	0.2076	1.037
ECOHO10				-0.1355	0.1785	0.873
ECOHO11				0.2614*	0.1565	1.299
ECOHO12				0.0619	0.1307	1.064
ECOHO13				-0.1677	0.1387	0.846
Time Variables						
D_T2	-0.2751*	0.1439	0.759	-0.1778	0.1509	0.837
D_T3	-0.2985**	0.1499	0.742	-0.1047	0.1571	0.901
D_T4	-0.6677***	0.1711	0.513	-0.4169**	0.1779	0.659
D_T5	-0.5514***	0.1693	0.576	-0.2345	0.1763	0.791
D_T6	-1.3578***	0.2317	0.257	-1.0287***	0.2378	0.357
-2 Log Likelihood						
Intercept Only	3274.165			3274.165		
Intercept and Covariates	3012.751			2745.484		
Concordant Pairs	71.8%			79.9%		
Somer's D	0.447			0.605		
Hosmer-Lemeshow Goodness of Fit	8.6707 with 8 DF (p=0.3708)			15.403 with 8 DF (p=0.0518)		

* p < .10
** p < .05
*** p < .01

Table 46
Arrests, Male and Female Subjects, Supervised and Unsupervised Subjects
Bloom Model

Variable	Base Model b	Base Model se(b)	Base Model OR	Final Model b	Final Model se(b)	Final Model OR
Base Model Variables						
INTERCPT	-4.7408***	0.6099	.	-4.9127***	0.6536	.
SUBJECT	0.0086	0.1564	1.009	-0.2111	0.1779	0.810
EBLACK	-0.3514***	0.1356	0.704	-0.2695*	0.1411	0.764
ERACEOTH	0.6252***	0.2069	1.869	0.5156**	0.2181	1.675
EHISP	-0.1022	0.1249	0.903	-0.1271	0.1340	0.881
EFEM	-0.3742***	0.1110	0.688	-0.4427***	0.1225	0.642
EPRIORCM	0.3991***	0.0945	1.491	0.3419***	0.0964	1.408
ERECVIOL	-0.1196	0.1316	0.887	-0.0882	0.1369	0.916
EPASTVIO	0.0774	0.1148	1.080	0.0120	0.1196	1.012
TIMESRVD	0.0337	0.0334	1.034	0.0265	0.0354	1.027
ECJSPVNY	0.5287	0.3293	1.697	0.7695**	0.3380	2.159
ECJSPVNM	-0.5544	0.6476	0.574	-0.7845	0.6618	0.456
AGERLSE	-0.0421***	0.0092	0.959	-0.0410***	0.0097	0.960
EWORKJOB	0.1666	0.1906	1.181	0.1131	0.1935	1.120
ESUPILL	0.0846	0.0691	1.088	0.0741	0.0720	1.077
ELEGITUN	-0.2922	0.3628	0.747	-0.2327	0.3669	0.792
EUNEMP	-0.2704	0.2732	0.763	-0.4049	0.2809	0.667
EJOB_UNK	-0.0203	0.5800	0.980	0.3003	0.5912	1.350
GRADEA	0.1344***	0.0332	1.144	0.1084***	0.0342	1.115
COC_FRQ	-0.0328	0.0379	0.968	0.0025	0.0409	1.003
CRK_FRQ	0.1531***	0.0355	1.165	0.1322***	0.0376	1.141
HAL_FRQ	-0.1645***	0.0638	0.848	-0.1445**	0.0655	0.865
HER_FRQ	-0.0080	0.0405	0.992	-0.0149	0.0423	0.985
OPIA_FRQ	-0.0081	0.0534	0.992	-0.0015	0.0559	0.999
POT_FRQ	-0.1890***	0.0372	0.828	-0.1760***	0.0393	0.839
BARB_FRQ	0.0762	0.0529	1.079	0.0352	0.0568	1.036
STIM_FRQ	0.0393	0.0443	1.040	0.0541	0.0460	1.056
DEPLOGTA	0.0502**	0.0209	1.052	0.0553**	0.0217	1.057
DEPLOGTD	0.0214	0.0336	1.022	-0.0168	0.0353	0.983

Table 46 — Continued

Variable	Base Model b	Base Model se(b)	Base Model OR	Final Model b	Final Model se(b)	Final Model OR
Supervision Variables						
ECCCNO				-0.3314***	0.1024	0.718
ECCCFAIL				0.5299***	0.1174	1.699
SUP_REL				0.5628**	0.2201	1.756
ECOHO1_6				0.9334***	0.1717	2.543
ECOHO7				0.6424**	0.2537	1.901
ECOHO8				0.0678	0.2378	1.070
ECOHO9				-0.8381**	0.3555	0.433
ECOHO10				-0.2990	0.2347	0.742
ECOHO11				0.2998*	0.1705	1.350
ECOHO12				-0.1219	0.1788	0.885
ECOHO13				-0.0297	0.1720	0.971
D_T2	0.6656***	0.2344	1.946	0.6847***	0.2369	1.983
D_T3	-0.0011	0.2741	0.999	0.0912	0.2770	1.096
D_T4	1.3205***	0.2192	3.745	1.4254***	0.2231	4.159
D_T5	0.5317**	0.2522	1.702	0.6738***	0.2558	1.962
D_T6	0.6988***	0.2462	2.011	0.8460***	0.2500	2.330
-2 Log Likelihood						
Intercept Only		2547.506			2547.506	
Intercept and Covariates		2324.885			2250.129	
Concordant Pairs		60.2%			61.1%	
Somer's D		0.238			0.258	
Hosmer-Lemeshow Goodness of Fit	63.538 with 8 DF (p=0.0001)			115.88 with 8 DF (p=0.0001)		

* p < .10
** p < .05
*** p < .01

Table 47
Arrests, Male and Female Subjects, Supervised Subjects Only
Bloom Model

Variable	Base Model b	se(b)	OR	Final Model b	se(b)	OR
Base Model Variables						
INTERCPT	-4.6073***	0.6606	.	-5.0224***	0.7329	.
SUBJECT	0.0740	0.1755	1.077	0.0513	0.2019	1.053
EBLACK	-0.4005***	0.1416	0.670	-0.3572**	0.1619	0.700
ERACEOTH	0.7238***	0.2099	2.062	0.5983**	0.2521	1.819
EHISP	0.0494	0.1296	1.051	-0.1261	0.1608	0.882
EFEM	-0.4048***	0.1200	0.667	-0.5766***	0.1520	0.562
EPRIORCM	0.4203***	0.0975	1.522	0.2698**	0.1068	1.310
ERECVIOL	-0.2627*	0.1522	0.769	-0.1450	0.1653	0.865
EPASTVIO	0.0778	0.1276	1.081	-0.0630	0.1475	0.939
TIMESRVD	0.0286	0.0358	1.029	0.1628***	0.0404	1.177
ECJSPVNY	0.5208	0.3345	1.683	0.5627	0.3605	1.755
ECJSPVNM	-0.3971	0.6560	0.672	-0.5105	0.7019	0.600
AGERLSE	-0.0446***	0.0100	0.956	-0.0486***	0.0106	0.953
ESUPILL	0.0491	0.0745	1.050	-0.1634*	0.0888	0.849
EWORKJOB	0.1035	0.1993	1.109	0.3222	0.2199	1.380
ELEGITUN	-0.3710	0.3935	0.690	-1.0597**	0.4151	0.347
EUNEMP	-0.4716	0.2985	0.624	-0.5314	0.3270	0.588
EJOB_UNK	0.4310	0.5959	1.539	0.8633	0.6566	2.371
GRADEA	0.1508***	0.0362	1.163	0.1618***	0.0414	1.176
COC_FRQ	-0.0235	0.0411	0.977	0.0691	0.0478	1.072
CRK_FRQ	0.1793***	0.0381	1.196	0.0402	0.0475	1.041
HAL_FRQ	-0.2197***	0.0710	0.803	-0.0863	0.0724	0.917
HER_FRQ	-0.0108	0.0442	0.989	-0.0546	0.0522	0.947
OPIA_FRQ	-0.0359	0.0569	0.965	0.0202	0.0675	1.020
POT_FRQ	-0.1825***	0.0402	0.833	-0.1456***	0.0471	0.864
BARB_FRQ	0.1058*	0.0554	1.112	0.0351	0.0657	1.036
STIM_FRQ	0.0066	0.0486	1.007	0.0656	0.0529	1.068
DEPLOGTA	0.0761***	0.0225	1.079	0.0806***	0.0253	1.084
DEPLOGTD	0.0274	0.0357	1.028	-0.0143	0.0413	0.986

Table 47 — Continued

Variable	Base Model b	Base Model se(b)	Base Model OR	Final Model b	Final Model se(b)	Final Model OR
Supervision Variables						
ECCCNO				-0.3479***	0.1241	0.706
ECCCFAIL				0.2631*	0.1514	1.301
UARATE				-0.2892***	0.0473	0.749
SUP_REL				.	.	.
HHSE_STR				-1.0163	0.9531	0.362
ESPOUSE				-0.8565***	0.2421	0.425
ECOM_LAW				0.2866	0.1989	1.332
SUPVVIOL				-0.1684**	0.0703	0.845
DIRTY				1.2820***	0.1756	3.604
EMP_HRS				-0.0562***	0.0048	0.945
ECOHO1_6				0.9160***	0.2056	2.499
ECOHO7				1.0124***	0.3106	2.752
ECOHO8				-0.1738	0.2886	0.840
ECOHO9				-1.8516***	0.5401	0.157
ECOHO10				-0.2706	0.2910	0.763
ECOHO11				0.3417*	0.1964	1.407
ECOHO12				0.3164	0.2024	1.372
ECOHO13				0.3602*	0.1943	1.434
D_T2	0.6092**	0.2455	1.839	0.5372**	0.2645	1.711
D_T3	-0.0916	0.2925	0.912	-0.0117	0.3080	0.988
D_T4	1.3306***	0.2287	3.783	1.5265***	0.2486	4.602
D_T5	0.3838	0.2737	1.468	0.6799**	0.2934	1.974
D_T6	0.7726***	0.2556	2.165	1.0556***	0.2771	2.874
-2 Log Likelihood						
Intercept Only	2246.057			2246.057		
Intercept and Covariates	2008.106			1626.307		
Concordant Pairs	59.1%			76.3%		
Somer's D	0.218			0.557		
Hosmer-Lemshow Goodness of Fit	86.909 with 8 DF (p=0.0001)			93.517 with 8 DF (p=0.0001)		

* p < .10
** p < .05
*** p < .01

Table 48
Arrests or Revocations, Male and Female Subjects
Bloom Model

Variable	Base Model b	Base Model se(b)	Base Model OR	Final Model b	Final Model se(b)	Final Model OR
Base Model Variables						
INTERCPT	-4.3224***	0.5391	.	-5.6530***	0.8100	.
SUBJECT	0.0310	0.1468	1.031	0.0470	0.1783	1.048
EBLACK	-0.2380*	0.1280	0.788	-0.0555	0.1544	0.946
ERACEOTH	0.5307***	0.1990	1.700	0.0747	0.2504	1.078
EHISP	0.0372	0.1130	1.038	-0.0044	0.1433	0.996
EFEM	-0.3839***	0.1041	0.681	-0.5640***	0.1336	0.569
EPRIORCM	0.3801***	0.0847	1.462	0.2277**	0.0970	1.256
ERECVIOL	-0.2462*	0.1299	0.782	-0.0982	0.1474	0.906
EPASTVIO	0.1896*	0.1061	1.209	-0.0086	0.1295	0.991
TIMESRVD	-0.0205	0.0323	0.980	0.1008***	0.0379	1.106
ECJSPVNY	0.4161*	0.2472	1.516	0.4082	0.2877	1.504
ECJSPVNM	-0.2731	0.4813	0.761	-0.3348	0.5558	0.715
AGERLSE	-0.0253***	0.0082	0.975	-0.0315***	0.0091	0.969
ESUPILL	0.0716	0.0639	1.074	-0.0817	0.0782	0.922
EWORKJOB	0.0763	0.1702	1.079	0.5015**	0.2061	1.651
ELEGITUN	-0.2278	0.3099	0.796	-1.1146***	0.3526	0.328
EUNEMP	-0.5025**	0.2520	0.605	-0.7051**	0.2966	0.494
EJOB_UNK	0.3856	0.5220	1.471	0.8129	0.6431	2.254
GRADEA	0.1155***	0.0310	1.122	0.1150***	0.0359	1.122
COC_FRQ	-0.0111	0.0347	0.989	0.0430	0.0428	1.044
CRK_FRQ	0.1638***	0.0325	1.178	0.0232	0.0416	1.023
HAL_FRQ	-0.1220**	0.0573	0.885	-0.0348	0.0612	0.966
HER_FRQ	0.0005	0.0367	1.000	-0.0667	0.0448	0.936
OPIA_FRQ	-0.0295	0.0492	0.971	0.0431	0.0602	1.044
POT_FRQ	-0.1409***	0.0347	0.869	-0.1389***	0.0431	0.870
BARB_FRQ	0.0557	0.0493	1.057	-0.0292	0.0592	0.971
STIM_FRQ	-0.0100	0.0414	0.990	0.0945**	0.0478	1.099
DEPLOGTA	0.0584***	0.0193	1.060	0.0701***	0.0228	1.073
DEPLOGTD	0.0170	0.0309	1.017	-0.0044	0.0378	0.996
Treatment Variables						
ENRGENY				-0.3654	0.2452	0.694
ENRSUPY				-0.4669	0.4668	0.627
ETSYES				-0.2501***	0.0952	0.779
ECTRONLY				-0.0316	0.2053	0.969
EOTHONLY				-0.4145	0.3920	0.661
EBOTH				-1.6182***	0.3980	0.198
ETXMISS				2.1752***	0.5168	8.804
EAAYES				0.3546	0.2184	1.426
EAAMISS				-0.3911	0.3822	0.676

Table 48 — Continued

Variable	Base Model b	Base Model se(b)	Base Model OR	Final Model b	Final Model se(b)	Final Model OR
Supervision Variables						
ECCCNO				-0.5462***	0.1168	0.579
ECCCFAIL				0.5429***	0.1306	1.721
UARATE				-0.2807***	0.0471	0.755
HHSE_STR				-0.1225	0.5193	0.885
Post-Release Variables						
ESPOUSE				-0.6226***	0.2067	0.537
ECOM_LAW				-0.0143	0.1861	0.986
SUPVVIOL				0.0572	0.0448	1.059
DIRTY				1.6011***	0.1589	4.958
EMP_HRS				-0.0586***	0.0044	0.943
ECOHO1_6				0.5712***	0.1948	1.770
ECOHO7				1.3046***	0.2667	3.686
ECOHO8				-0.3408	0.2721	0.711
ECOHO9				-1.6491***	0.4284	0.192
ECOHO10				0.0319	0.2236	1.032
ECOHO11				0.1580	0.1765	0.171
ECOHO12				0.3185*	0.1723	1.375
ECOHO13				0.0655	0.1797	1.068
D_T2	0.4156*	0.2134	1.515	0.3721	0.2405	1.451
D_T3	0.2139	0.2265	1.239	0.3355	0.2517	1.399
D_T4	1.1331***	0.1961	3.105	1.4360***	0.2241	4.204
D_T5	0.3364	0.2299	1.400	0.7456***	0.2576	2.108
D_T6	0.6886***	0.2163	1.991	1.0871***	0.2455	2.966
-2 Log Likelihood						
Intercept Only	2819.505			2819.505		
Intercept and Covariates	2596.770			1963.616		
Concordant Pairs	61.1%			83.0%		
Somer's D	0.246			0.677		
Hosmer-Lemeshow Goodness of Fit	72.063 with 8 DF (p=0.0001)			70.558 with 8 DF (p=0.0001)		

* $p < .10$
** $p < .05$
*** $p < .01$

Table 49
Drug Use, Male and Female Subjects
Bloom Model

Variable	Base Model b	Base Model se(b)	Base Model OR	Final Model b	Final Model se(b)	Final Model OR
Base Model Variables						
INTERCPT	-2.9136***	0.4422	.	-3.4254***	0.5628	.
SUBJECT	-0.0771	0.1251	0.926	-0.4615***	0.1490	0.630
EBLACK	-0.0273	0.1042	0.973	0.0912	0.1140	1.095
ERACEOTH	0.7463***	0.1622	2.109	0.7629***	0.1817	2.144
EHISP	-0.0367	0.1145	0.964	0.1192	0.1212	1.127
EFEM	-0.3207***	0.0867	0.726	-0.3756***	0.0972	0.687
EPRIORCM	0.2377***	0.0670	1.268	0.2603***	0.0732	1.297
ERECVIOL	0.0503	0.1016	1.052	0.1529	0.1083	1.165
EPASTVIO	0.0148	0.0148	1.015	-0.1528	0.0990	0.858
TIMESRVD	-0.1135***	0.0299	0.893	-0.0484	0.0297	0.953
ECJSPVNY	0.0166	0.1746	1.017	-0.0698	0.1855	0.933
ECJSPVNM	0.0840	0.3321	1.088	0.1086	0.3486	1.115
AGERLSE	-0.0031	0.0031	0.997	0.0118	0.0076	1.012
ESUPILL	0.1595***	0.0547	1.173	0.1199**	0.0593	1.127
EWORKJOB	0.0499	0.1395	1.051	0.2870*	0.1498	1.332
ELEGITUN	0.1461	0.2481	1.157	-0.0098	0.2744	0.990
EUNEMP	-0.1770	0.1988	0.838	-0.2820	0.2103	0.754
EJOB_UNK	-0.0362	0.4278	0.964	0.0583	0.4518	1.060
GRADEA	0.0659**	0.0286	1.068	0.0735**	0.0310	1.076
COC_FRQ	-0.0248	0.0295	0.975	0.0735	0.0310	1.001
CRK_FRQ	0.1230***	0.0279	1.131	0.0667**	0.0307	1.069
HAL_FRQ	-0.0849	0.0491	0.919	-0.0500	0.2406	0.976
HER_FRQ	-0.0023	0.0317	0.998	-0.0617*	0.0344	0.940
OPIA_FRQ	-0.1192**	0.0482	0.888	-0.0622	0.0520	0.940
POT_FRQ	0.0656**	0.0332	1.068	0.1131***	0.0365	1.120
BARB_FRQ	-0.0050	0.0442	0.995	-0.0479	0.0484	0.953
STIM_FRQ	-0.0356	0.0380	0.965	-0.0330	0.0407	0.968
DEPLOGTA	-0.0262	0.0174	0.974	-0.0288	0.0181	0.972
DEPLOGTD	0.0856***	0.0291	1.089	0.0290	0.0317	1.029
Treatment Variables						
ENRGENY				-0.3882**	0.1733	0.678
ENRSUPY				0.0957	0.2426	1.100
ETSYES				-0.0327	0.0693	0.968
ECTRONLY				-0.3821**	0.1751	0.682
EOTHONLY				0.0620	0.2800	1.064
EBOTH				0.5580**	0.2651	1.747
ETXMISS				1.1141**	0.5658	3.047
EAAYES				0.3223**	0.1640	1.380
EAAMISS				-0.2797	0.2893	0.756

Table 49 — Continued

Variable	Base Model b	se(b)	OR	Final Model b	se(b)	OR
Supervision Variables						
ECCCNO				-0.2272**	0.0955	0.797
ECCCFAIL				0.4395***	0.0980	1.552
UARATE				0.0754**	0.0302	1.078
HHSE_STR				0.9603***	0.3686	2.613
Post-Release Variables						
ESPOUSE				-0.0916	0.1134	0.912
ECOM_LAW				-0.1608	0.1248	0.851
SUPVVIOL				0.1805***	0.0375	1.198
EMP_HRS				-0.0089***	0.0029	0.991
ECOHO1_6				0.4975***	0.1522	1.645
ECOHO7				-0.3812	0.2933	0.683
ECOHO8				0.2822	0.1976	1.326
ECOHO9				-0.0255	0.2146	0.975
ECOHO10				0.0163	0.1686	1.016
ECOHO11				0.3707**	0.1480	1.449
ECOHO12				-0.1081	0.1490	0.897
ECOHO13				-0.3794**	0.1527	0.684
D_T2	0.0102	0.1376	1.010	0.1634	0.1447	1.178
D_T3	-0.3502**	0.1570	0.705	-0.1023	0.1644	0.903
D_T4	-0.5054***	0.1686	0.603	-0.2004	0.1760	0.818
D_T5	-0.6777***	0.1822	0.508	-0.3442*	0.1891	0.709
D_T6	-0.5389***	0.1782	0.583	-0.1951	0.1853	0.823
-2 Log Likelihood						
Intercept Only	3464.186			3464.186		
Intercept and Covariates	3156.096			2897.069		
Concordant Pairs	68.0%			76.3%		
Somer's D	0.371			0.535		
Hosmer-Lemeshow Goodness of Fit	22.125 with 8 DF (p=0.0047)			23.91 with 8 DF (p=0.0024)		

* p < .10
** p < .05
*** p < .01

Table 50
Arrests, Males and Females, Supervised and Unsupervised Subjects
Complete, Heckman Model

Variable	Final Model b	se(b)
Base Model Variables		
CONSTANT	6.0448***	0.5777
COMPLETE	1.1203***	0.3101
EBLACK	0.1701	0.1524
ERACEOTH	-0.3030	0.2480
EHISP	0.3047**	0.1397
EFEM	0.2728***	0.1128
EPRIORCM	-0.4165***	0.0930
ERECVIOL	-0.2713**	0.1186
EPASTVIO	0.0696	0.1089
TIMESRVD	-0.0152	0.0349
ECJSPVNY	-0.2460	0.2033
ECJSPVNM	0.2921	0.3798
AGERLSE	0.0434***	0.0098
ESUPILL	0.0458	0.0714
EWORKJOB	0.0099	0.1574
ELEGITUN	0.1060	0.2896
EUNEMP	-0.0138	0.2226
EJOB_UNK	0.0719	0.4436
GRADEA	-0.0149	0.0333
COC_FRQ	-0.0043	0.0368
CRK_FRQ	0.0557	0.0389
HAL_FRQ	-0.0118	0.0548
HER_FRQ	-0.0187	0.0407
OPIA_FRQ	0.0279	0.0574
POT_FRQ	0.0525	0.0406
BARB_FRQ	-0.0561	0.0543
STIM_FRQ	0.0102	0.0427
DEPLOGTA	-0.0113	0.0200
DEPLOGTD	0.0066	0.0330
Supervision Variables		
ECCCNO	-0.0450	0.0991
ECCCFAIL	-0.2348**	0.1159
SUP_REL	-0.2123	0.1943
Post-Release Variables		
ECOHO1_6	-0.1232	0.2113
ECOHO7	-0.4653*	0.2731
ECOHO8	-0.2596	0.2225
ECOHO9	0.3394	0.2584
ECOHO10	-0.3102	0.1912

Table 50 - Continued

Variable	Final Model b	se(b)
ECOHO11	0.3093*	0.2054
ECOHO12	0.0210	0.1519
ECOHO13	0.1590	0.1627
Selection Bias Variables		
COVARIAN	-0.6877***	0.2348
SIGMA	1.7163***	0.0973

* $p < .10$
** $p < .05$
*** $p < .01$

Table 51
Arrests, Males and Females, Supervised Subjects Only
Complete, Heckman Model

Variable	Final Model b	se(b)
Base Model Variables		
CONSTANT	5.4181***	0.6098
COMPLETE	1.2406***	0.3377
EBLACK	0.1977	0.1617
ERACEOTH	-0.3340*	0.2565
EHISP	0.2062	0.1478
EFEM	0.2219*	0.1241
EPRIORCM	-0.4426***	0.0988
ERECVIOL	-0.1830	0.1314
EPASTVIO	-0.0052	0.1193
TIMESRVD	-0.0470	0.0363
ECJSPVNY	-0.1706	0.2218
ECJSPVNM	0.2178	0.4133
AGERLSE	0.0485***	0.0107
ESUPILL	0.0915	0.0789
EWORKJOB	0.0016	0.1742
ELEGITUN	0.3090	0.3192
EUNEMP	0.1520	0.2506
EJOB_UNK	-0.3050	0.4861
GRADEA	-0.0213	0.0373
COC_FRQ	0.0072	0.0407
CRK_FRQ	0.0789*	0.0445
HAL_FRQ	-0.0271	0.0610
HER_FRQ	-0.0103	0.0447
OPIA_FRQ	0.0759	0.0652
POT_FRQ	0.0518	0.0448
BARB_FRQ	-0.0918	0.0599
STIM_FRQ	0.0219	0.0471
DEPLOGTA	-0.0353	0.0220
DEPLOGTD	0.0026	0.0363
Supervision Variables		
ECCCNO	0.0612	0.1130
ECCCFAIL	-0.1967	0.1294
UARATE	0.0911*	0.0399
HHSE_STR	0.4974	0.5655
Post-Release Variables		
ESPOUSE	0.5493***	0.1626
ECOM_LAW	-0.1436	0.1488
SUPVVIOL	-0.0923	0.0603
DRUGVIOL	0.0348	0.0416
PRCTFULL	0.0044***	0.0018
ECOHO1_6	-0.1696	0.2236
ECOHO7	-0.1244	0.3306
ECOHO8	-0.2630	0.2581
ECOHO9	0.3789	0.2996
ECOHO10	-0.3923*	0.2147
ECOHO11	0.2177	0.2191

Table 51 — Continued

Variable	Final Model b	se(b)
ECOHO12	-0.0759	0.1676
ECOHO13	0.0822	0.1730
Selection Bias Variables		
COVARIAN	-0.7738***	0.2555
SIGMA	1.6808***	0.1046

* p < .10
** p < .05
*** p < .01

Table 52
Arrests or Revocations
Male and Female Subjects, Complete
Heckman Model

Variable	Final Model b	se(b)
Base Model Variables		
CONSTANT	5.4249***	0.5092
COMPLETE	1.0047***	0.2525
EBLACK	0.1977	0.1617
ERACEOTH	0.1047	0.1228
EHISP	0.1318	0.1062
EFEM	0.1899**	0.0882
EPRIORCM	-0.2830***	0.0708
ERECVIOL	-0.1560	0.0987
EPASTVIO	-0.0439	0.0866
TIMESRVD	-0.0327	0.0267
ECJSPVNY	-0.1098	0.1530
ECJSPVNM	0.0348	0.2843
AGERLSE	0.0221***	0.0072
ESUPILL	0.0890	0.0579
EWORKJOB	-0.1328	0.1300
ELEGITUN	0.3665	0.2315
EUNEMP	0.1937	0.1894
EJOB_UNK	-0.1672	0.3728
GRADEA	-0.0125	0.0263
COC_FRQ	-0.0037	0.0298
CRK_FRQ	0.0789	0.0445
HAL_FRQ	-0.0327	0.0444
HER_FRQ	-0.0079	0.0315
OPIA_FRQ	0.0471	0.0463
POT_FRQ	0.0380	0.0324
BARB_FRQ	-0.0490	0.0434
STIM_FRQ	0.0296	0.0346
DEPLOGTA	-0.0212	0.0159
DEPLOGTD	0.0015	0.0263
Treatment Variables		
ENRGENY	0.1279	0.1241
ENRSUPY	0.0965	0.1960
ETSYES	0.0638	0.0691
ECTRONLY	0.3161**	0.1416
EOTHONLY	0.1034	0.2426
EBOTH	0.4696*	0.2505
ETXMISS	-1.3987***	0.4023

Table 52— Continued

Variable	Final Model b	se(b)
EAAYES	-0.0705	0.1490
EAAMISS	-0.0042	0.2559
Supervision Variables		
ECCCNO	0.1279	0.0937
ECCCFAIL	-0.2213***	0.0939
UARATE	0.1321***	0.0324
HHSE_STR	-0.1062	0.3471
Post-Release Variables		
ESPOUSE	0.4651***	0.1227
ECOM_LAW	-0.0305	0.1168
SUPVVIOL	-0.2102***	0.0387
DRUGVIOL	-0.0705***	0.0266
PRCTFULL	0.0062***	0.0013
ECOHO1_6	-0.0377	0.1643
ECOHO7	-0.4128*	0.2235
ECOHO8	-0.1003	0.1918
ECOHO9	0.4016	0.2159
ECOHO10	-0.3520	0.1566
ECOHO11	0.2177**	0.1525
ECOHO12	-0.0550	0.1223
ECOHO13	0.2441	0.1301
Selection Bias Variables		
COVARIAN	-0.8639***	0.2243
SIGMA	1.3728***	0.0684

* $p < .10$
** $p < .05$
*** $p < .01$

Table 53
Drug Use
Male and Female Subjects Who Had Urine Testing During Follow-Up, Complete
Heckman Model

Variable	Final Model b	se(b)
Base Model Variables		
CONSTANT	6.0021***	0.5711
COMPLETE	0.6829**	0.3157
EBLACK	-0.1604	0.1485
ERACEOTH	-0.3313	0.2500
EHISP	-0.2704**	0.1190
EFEM	0.3390***	0.0989
EPRIORCM	-0.1956***	0.0794
ERECVIOL	-0.2066*	0.1139
EPASTVIO	-0.0236	0.1003
TIMESRVD	0.0682**	0.0343
ECJSPVNY	-0.0310	0.1807
ECJSPVNM	-0.0558	0.3369
AGERLSE	0.0028	0.0085
ESUPILL	-0.0307	0.0644
EWORKJOB	-0.0796	0.1483
ELEGITUN	-0.1024	0.2558
EUNEMP	0.1753	0.2021
EJOB_UNK	0.1391	0.4309
GRADEA	-0.0279	0.0315
COC_FRQ	0.0381	0.0332
CRK_FRQ	-0.0552*	0.0318
HAL_FRQ	-0.0516	0.0501
HER_FRQ	-0.0041	0.0352
OPIA_FRQ	-0.0315	0.0511
POT_FRQ	-0.0769**	0.0380
BARB_FRQ	0.0805	0.0505
STIM_FRQ	0.0135	0.0401
DEPLOGTA	0.0302*	0.0181
DEPLOGTD	-0.0403	0.0311
Treatment Variables		
ENRGENY	0.2905**	0.1420
ENRSUPY	0.0312	0.1886
ETSYES	0.0208	0.0776
ECTRONLY	0.3103	0.1772
EOTHONLY	0.2332	0.2859
EBOTH	-0.8881***	0.2507
ETXMISS	-1.1132	0.5817

Table 53 — Continued

Variable	Final Model b	se(b)
EAAYES	-0.3056	0.1736
EAAMISS	0.4010	0.3082
Supervision Variables		
ECCCNO	0.2735***	0.1138
ECCCFAIL	-0.5684***	0.1095
UARATE	-0.0553*	0.0332
HHSE_STR	-0.2364	0.3927
Post-Release Variables		
ESPOUSE	0.2684**	0.1182
ECOM_LAW	0.0064	0.1233
SUPVVIOL	-0.2421***	0.0400
PRCTFULL	0.0065***	0.0015
ECOHO1_6	0.0412	0.1981
ECOHO7	0.1732	0.2971
ECOHO8	-0.2384	0.2238
ECOHO9	0.0199	0.2305
ECOHO10	0.1693	0.1967
ECOHO11	-0.2956*	0.1674
ECOHO12	-0.1123	0.1401
ECOHO13	0.1800	0.1496
COVARIAN	-0.5354	0.4060
SIGMA	-0.8999***	0.1323

* $p < .10$
** $p < .05$
*** $p < .01$

Table 54
Traditional Models of CCC Outcomes

Male and Female Subjects

Model of CCC Failure with:	x^2	DF	Diff.†	DF
Base Variables	1021.7	33	---	---
Base and Additional Background Variables	1008.6	40	13.1*	7
Base and Change Assessment Variables	1005.1	39	16.6**	6
Base and Additional Treatment Variables	1018.8	36	2.9	3
Full Model Variables	992.0	46	29.7***	13

Male Subjects

Model of CCC Failure with:	x^2	DF	Diff.†	DF
Base Variables	864.4	31	---	---
Base and Additional Background Variables	851.6	38	12.8*	7
Base and Change Assessment Variables	845.9	37	18.5***	6
Base and Additional Treatment Variables	861.6	34	2.8	3
Full Model Variables	833.1	44	31.3***	13

† Difference in x^2 between traditional base model and model in question.

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* Significant at $p < .10$
** Significant at $p < .05$
*** Significant at $p < .01$

Table 55
CCC Failure, Male and Female Subjects
Traditional Model

Variable	Base Model b	Base Model se(b)	Base Model OR	Final Model b	Final Model se(b)	Final Model OR
Base Model Variables						
INTERCPT	-0.2040	0.6392	.	-0.5544	0.6818	.
COMPDAP	-0.1922	0.2060	0.825	-0.2128	0.2095	0.808
GRAD12MO	-0.6231**	0.3201	0.536	-0.7034**	0.3298	0.495
GRAD9MO	-0.3689*	0.2078	0.691	-0.3914*	0.2148	0.676
INCOMPTX	0.0203	0.2922	1.021	-0.0094	0.2981	0.991
DISCIPTX	0.4080	0.4181	1.504	0.4250	0.4262	1.530
WITHDRTX	-0.0489	0.5476	0.952	-0.0790	0.5499	0.924
EBLACK	0.1603	0.2018	1.174	0.1236	0.2055	1.132
ERACEOTH	-0.0463	0.3534	0.955	-0.0253	0.3591	0.975
EHISP	-0.0088	0.1506	0.991	-0.0379	0.1532	0.963
EFEM	-0.2613**	0.1168	0.770	-0.2764**	0.1224	0.759
EPRIORCM	0.2765***	0.0982	1.318	0.2780***	0.1004	1.320
ERECVIOL	-0.0345	0.1492	0.966	-0.0624	0.1534	0.940
EPASTVIO	0.1487	0.1243	1.160	0.1636	0.1274	1.178
TIMESRVD	-0.0409	0.0453	0.960	-0.0358	0.0461	0.965
ECJSPVNY	-0.1286	0.1853	0.879	-0.1561	0.1900	0.856
ECJSPVNM	0.2809	0.3354	1.324	0.3053	0.3443	1.357
AGERLSE	-0.0144	0.0104	0.986	-0.0140	0.0107	0.986
ESUPILL	0.1058	0.0795	1.112	0.1028	0.0810	1.108
EWORKJOB	0.0260	0.1769	1.026	0.0308	0.1796	1.031
ELEGITUN	-0.7181**	0.3579	0.488	-0.7370**	0.3649	0.479
EUNEMP	0.2017	0.2486	1.224	0.2617	0.2524	1.299
EJOB_UNK	0.2379	0.4789	1.269	0.1962	0.4867	1.217
GRADEA	-0.0404	0.0400	0.960	-0.0399	0.0410	0.961
COC_FRQ	-0.0517	0.0411	0.950	-0.0464	0.0419	0.955
CRK_FRQ	0.0218	0.0416	1.022	0.0101	0.0427	1.010
HAL_FRQ	-0.0224	0.0648	0.978	-0.0129	0.0657	0.987
HER_FRQ	0.0793*	0.0436	1.082	0.0806*	0.0446	1.084
OPIA_FRQ	0.0085	0.0617	1.009	-0.0048	0.0629	0.995
POT_FRQ	-0.0011	0.0451	0.999	0.0062	0.0460	1.006
BARB_FRQ	-0.0104	0.0595	0.990	-0.0103	0.0602	0.990
STIM_FRQ	-0.1103**	0.0507	0.896	-0.1145**	0.0515	0.892
DEPLOGTA	0.0279	0.0226	1.028	0.0186	0.0241	1.019
DEPLOGTD	0.0948**	0.0395	1.099	0.0785*	0.0411	1.082

Table 55 — Continued

Variable	Base Model b	Base Model se(b)	Base Model OR	Final Model b	Final Model se(b)	Final Model OR
Other Background Factors						
EPSTDGTX				-0.0120	0.0848	0.988
EPSTETOH				-0.2362	0.2015	0.790
EDIAGNO				0.1149	0.1364	1.122
EDIAGDEP				-0.1871	0.2444	0.829
EDIAGASP				-0.0459	0.1548	0.955
EDIAGBTH				0.4157*	0.2291	1.515
EPSTMHTX				-0.0099	0.1023	0.990
Motivation for Change Variables						
ECLUST1				0.3028**	0.1533	1.354
ECLUST2				-0.3752**	0.1867	0.687
ECLUST3				-0.1463	0.1848	0.864
ECLUST4				0.4177**	0.1959	1.518
ECLUST5				0.2140	0.2221	1.239
ECLUST6				-0.3567	0.3349	0.700
-2 Log Likelihood						
Intercept Only		1294.811			1294.811	
Intercept and Covariates		1194.420			1173.060	
Concordant Pairs		69.5%			71.7%	
Somer's D		0.395			0.438	
Hosmer-Lemeshow Goodness of Fit		5.6426 with 8 DF (p=0.6872)			12.668 with 8 DF (p=0.1238)	

* $p < .10$
** $p < .05$
*** $p < .01$

Table 56
CCC Failure, Male and Female Subjects
Bloom Model

Variable	Base Model b	Base Model se(b)	Base Model OR	Final Model b	Final Model se(b)	Final Model OR
Base Model Variables						
INTERCPT	-1.1610	0.7660		-1.7871**	0.8272	
SUBJECT	-0.1922	0.1826	0.825	-0.2423	0.1872	0.785
EBLACK	0.2926	0.2752	1.340	0.2312	0.2775	1.260
ERACEOTH	-0.2139	0.5131	0.807	-0.1603	0.5164	0.852
EHISP	-0.2164	0.1882	0.805	-0.2679	0.1924	0.765
EFEM	-0.3816***	0.1438	0.683	-0.3003**	0.1505	0.741
EPRIORCM	0.2802***	0.1066	1.323	0.2736***	0.1113	1.315
ERECVIOL	-0.1102	0.1796	0.896	-0.1374	0.1844	0.872
EPASTVIO	0.1879	0.1448	1.207	0.2118	0.1493	1.236
TIMESRVD	-0.0424	0.0485	0.958	-0.0514	0.0504	0.950
ECJSPVNY	-0.2644	0.2379	0.768	-0.2750	0.2438	0.760
ECJSPVNM	0.2650	0.4386	1.303	0.2705	0.4492	1.311
AGERLSE	-0.0213*	0.0114	0.979	-0.0219*	0.0117	0.978
ESUPILL	0.2496***	0.0878	1.283	0.2454***	0.0909	1.278
EWORKJOB	0.2403	0.2298	1.272	0.1540	0.2366	1.167
ELEGITUN	-0.7897*	0.4675	0.454	-0.8068*	0.4759	0.446
EUNEMP	0.4064	0.3140	1.501	0.4356	0.3208	1.546
EJOB_UNK	-0.1406	0.6922	0.869	-0.0379	0.7142	0.963
GRADEA	0.0059	0.0476	1.006	0.0229	0.0496	1.023
COC_FRQ	-0.1351***	0.0475	0.874	-0.1369***	0.0490	0.872
CRK_FRQ	0.1085**	0.0473	1.115	0.0848*	0.0498	1.089
HAL_FRQ	-0.1781**	0.0767	0.837	-0.1377*	0.0786	0.871
HER_FRQ	0.0659	0.0509	1.068	0.0787	0.0528	1.082
OPIA_FRQ	-0.0597	0.0700	0.942	-0.0642	0.0725	0.938
POT_FRQ	0.0432	0.0521	1.044	0.0524	0.0549	1.054
BARB_FRQ	0.0836	0.0627	1.087	0.0826	0.0651	1.086
STIM_FRQ	-0.1031*	0.0593	0.902	-0.0983	0.0608	0.906
DEPLOGTA	0.0204	0.0261	1.021	0.0139	0.0282	1.014
DEPLOGTD	0.1800***	0.0464	1.197	0.1478***	0.0498	1.159

Table 56 — Continued

Variable	Base Model b	Base Model se(b)	Base Model OR	Final Model b	Final Model se(b)	Final Model OR
Other Background Factors						
EPSTDGTX				-0.0219	0.1006	0.978
EPSTETOH				-0.3051	0.2469	0.737
EDIAGNO				0.1951	0.1593	1.215
EDIAGDEP				-0.4460	0.3293	0.640
EDIAGASP				0.0485	0.1815	1.050
EDIAGBTH				0.3327	0.2648	1.395
EPSTMHTX				-0.2323*	0.1223	0.793
Motivation for Change Variables						
ECLUST1				0.5299***	0.1641	1.699
ECLUST2				-0.5362**	0.2299	0.585
ECLUST3				-0.2394	0.1918	0.787
ECLUST4				0.4635**	0.2342	1.590
ECLUST5				0.1286	0.2429	1.137
ECLUST6				-0.3225	0.3314	0.724
-2 Log Likelihood						
Intercept Only		1137.353			1137.353	
Intercept and Covariates		1004.207			973.289	
Concordant Pairs		66.4%			68.0%	
Somer's D		0.331			0.364	
Hosmer-Lemshow Goodness of Fit		18.32 with 8 DF (p=0.0190)			25.631 with 8 DF (p=0.0012)	

* p < .10
** p < .05
*** p < .01

Table 57
CCC Failure, Male and Female Subjects, Complete
Heckman Model

Variable	Final Model b	se(b)
Base Model Variables		
CONSTANT	-0.1970	0.3965
COMPLETE	-0.0796	0.2791
EBLACK	0.0755	0.1214
ERACEOTH	0.0004	0.2120
EHISP	-0.0145	0.0873
EFEM	-0.1557*	0.0674
EPRIORCM	0.1595***	0.0556
ERECVIOL	-0.0377	0.0894
EPASTVIO	0.1004	0.0744
TIMESRVD	-0.0336	0.0260
ECJSPVNY	-0.0938	0.1122
ECJSPVNM	0.1931	0.2043
AGERLSE	-0.0103*	0.0061
ESUPILL	0.0644	0.0466
EWORKJOB	0.0117	0.1036
ELEGITUN	-0.3394*	0.1953
EUNEMP	0.1291	0.1463
EJOB_UNK	0.0543	0.2929
GRADEA	-0.0274	0.0232
COC_FRQ	-0.0260	0.0240
CRK_FRQ	0.0075	0.0246
HAL_FRQ	-0.0100	0.0374
HER_FRQ	0.0527**	0.0260
OPIA_FRQ	-0.0023	0.0361
POT_FRQ	-0.0069	0.0260
BARB_FRQ	-0.0074	0.0343
STIM_FRQ	-0.0597**	0.0288
DEPLOGTA	0.0114	0.0137
DEPLOGTD	0.0421*	0.0230
Other Background Factors		
EPSTDGTX	-0.0077	0.0491
EPSTETOH	-0.1399	0.1123
EDIAGNO	0.0547	0.0774
EDIAGDEP	-0.1520	0.1351
EDIAGASP	-0.0076	0.0886
EDIAGBTH	0.2563*	0.1335
EPSTMHTX	-0.0004	0.0585

Table 57 - Continued

Variable	Final Model b	Final Model se(b)
Motivation for Change Variables		
ECLUST1	0.1615*	0.0899
ECLUST2	-0.2104**	0.1038
ECLUST3	-0.0719	0.1076
ECLUST4	0.2249*	0.1282
ECLUST5	0.1231	0.1285
ECLUST6	-0.1974	0.1932
COVARIAN	-0.0266	0.3147

* $p < .10$
** $p < .05$
*** $p < .01$

Table 58
Traditional Models of Employment Outcomes

Male Subjects

Model of Arrest with:	r^2	DF	Diff. F†	DF
Base Variables	.12	32	---	---
Base and Additional Background Variables	.13	39	1.9	7
Base and Change Assessment Variables	.13	38	2.2**	6
Base and Additional Treatment Variables	.16	41	6.0***	9
Base with Supervision Variables	.19	36	25.1***	4
Base and Post Release Behavior Variables	.16	36	13.6***	4
Full Model Variables	.24	63	5.8***	31

Female Subjects

Model of Arrest with:	r^2	DF	Diff. F†	DF
Base Variables	.19	32	---	---
Base and Additional Background Variables	.24	39	2.0**	7
Base and Change Assessment Variables	.21	38	.9	6
Base and Additional Treatment Variables	.29	41	3.3***	9
Base with Supervision Variables	.21	36	1.2	4
Base and Post Release Behavior Variables	.24	36	3.6***	4
Full Model Variables	.39	60	2.2***	28

Notes to Table:

†Difference in F between traditional base model and model in question.

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* Significant at $p < .10$
** Significant at $p < .05$
*** Significant at $p < .01$

Table 59
Employment, Male Subjects
Traditional Model

Variable	Base Model b	Base Model se(b)	Final Model b	Final Model se(b)
Base Model Variables				
INTERCPT	28.9880***	9.7275	31.1511***	11.7475
COMPDAP	-4.7479	3.2558	-1.4176	3.2076
GRAD12MO	6.8039	4.6655	7.6667	4.9449
GRAD9MO	1.0064	3.3279	-1.2716	3.6508
INCOMPTX	-0.7225	5.4825	3.8089	5.7456
DISCIPTX	-12.1673*	7.1087	-2.9298	6.9049
WITHDRTX	-12.1044	7.8443	-6.0142	7.6433
EBLACK	-2.9191	2.8175	-3.4784	2.7798
ERACEOTH	-5.0171	4.5967	-1.5629	4.5358
EHISP	-2.7351	2.2407	-2.1440	2.1592
EPRIORCM	-2.1987	1.4511	-1.6160	1.3893
ERECVIOL	-2.5504	2.3005	-2.9855	2.2016
EPASTVIO	-0.4744	1.9754	1.4067	1.8920
TIMESRVD	1.7559***	0.6041	1.3406**	0.5844
ECJSPVNY	0.4576	3.6917	-0.1537	3.5564
ECJSPVNM	-0.7546	6.8442	2.6929	6.6083
AGERLSE	0.0117	0.1553	-0.1408	0.1508
ESUPILL	-4.1514***	1.3080	-3.7893***	1.2462
EWORKJOB	8.8585***	2.9656	8.4418***	2.8475
ELEGITUN	-6.9466	6.0237	-7.5520	5.7750
EUNEMP	-4.0649	4.1255	-1.5922	3.9668
EJOB_UNK	0.6873	8.6356	-2.4449	8.2787
GRADEA	1.7266***	0.5989	1.9288***	0.5750
COC_FRQ	-0.6574	0.6667	-0.7420	0.6391
CRK_FRQ	-0.1397	0.7253	0.2243	0.7004
HAL_FRQ	-2.4919**	0.9875	-2.0709**	0.9471
HER_FRQ	-1.9161***	0.7386	-1.2945*	0.7091
OPIA_FRQ	0.0534	1.0900	0.3028	1.0457
POT_FRQ	0.3520	0.7277	-0.0248	0.7025
BARB_FRQ	-0.4755	1.0050	-1.1064	0.9584
STIM_FRQ	1.1121	0.7913	0.7238	0.7586
DEPLOGTA	0.3374	0.3584	0.3072	0.3492
DEPLOGTD	1.4265**	0.6021	1.8886***	0.5852

Table 59 — Continued

Variable	Base Model b	Base Model se(b)	Final Model b	Final Model se(b)
Motivation for Change Variables				
ECLUST1			-4.3391*	2.4184
ECLUST2			2.8107	2.5269
ECLUST3			-0.6487	2.5118
ECLUST4			-0.1627	3.2289
ECLUST5			3.4910	3.3681
ECLUST6			-0.2364	4.1331
Treatment Variables				
ENRGENY			-1.9615	2.3342
ENRSUPY			5.8666	3.5946
ETSYES			1.8097	1.5434
ECTRONLY			10.6332**	5.0352
EOTHONLY			5.1938	6.9086
EBOTH			1.8855	7.0250
ETXMISS			-28.8633	18.1778
EAAYES			2.5506	4.2879
EAAMISS			2.2318	7.9856
Supervision Variables				
ECCCNO			-6.3523***	2.1549
ECCCFAIL			-6.0824***	2.2321
UARATE			0.2027	0.6528
HHSE_STR			7.1196	6.1718
Post-Release Variables				
ESPOUSE			2.7810	1.9862
ECOM_LAW			-1.1263	2.1549
SUPVVIOL			-4.8482***	1.0276
DRUGVIOL			-1.1688*	0.6348
ECOHO1_6			-10.7246***	3.9251
ECOHO7			0.3506	5.6580
ECOHO8			-1.0738	4.5840
ECOHO9			0.4155	4.3406
ECOHO10			-0.8845	3.5866
ECOHO11			1.2534	3.2579
ECOHO12			1.7959	2.6678
ECOHO13			4.5604	2.7820
R-square	0.1238		0.2381	
Adjusted R-square	0.0998		0.1959	

* $p < .10$
** $p < .05$
*** $p < .01$

Table 60
Employment, Female Subjects
Traditional Model

Variable	Base Model b	Base Model se(b)	Final Model b	Final Model se(b)
Base Model Variables				
INTERCPT	-18.1320	25.5520	-34.9104	29.6574
COMPDAP	-1.6120	9.5452	4.4894	9.4709
GRAD12MO	1.4758	11.8266	-17.7553	13.1370
GRAD9MO	5.2333	10.2356	-5.2152	11.1940
INCOMPTX	9.2328	11.2147	-1.6556	11.8636
DISCIPTX	3.2188	15.0151	5.5858	14.5952
WITHDRTX	-9.9643	15.8853	-6.1062	15.6109
EBLACK	-5.7266	8.2061	-6.3921	8.3974
ERACEOTH	6.2170	14.7012	7.2205	15.0828
EHISP	-15.1066**	6.0649	-12.2818**	6.0289
EPRIORCM	1.6899	3.1259	2.6031	3.0649
ERECVIOL	-7.3324	8.2581	-9.9475	8.2552
EPASTVIO	7.6958	8.6831	12.6088	8.6087
TIMESRVD	3.8931	2.3631	0.9551	2.4593
ECJSPVNY	-1.8024	9.2954	-2.9381	9.8612
ECJSPVNM	9.3431	16.9679	10.2751	17.8340
AGERLSE	0.3051	0.4169	0.0393	0.4339
ESUPILL	-0.1664	3.0356	1.7705	3.0027
EWORKJOB	12.8315**	6.4468	18.0217***	6.4765
ELEGITUN	10.8900	10.4673	23.7031**	10.4613
EUNEMP	-1.0021	9.1731	-4.7113	9.3345
EJOB_UNK	-30.7304	17.7794	-52.2313***	18.2290
GRADEA	2.1674	1.4791	2.1355	1.5048
COC_FRQ	2.8502	1.4889	2.8587**	1.4613
CRK_FRQ	0.6521	1.3554	1.6786	1.3846
HAL_FRQ	3.3112	2.5466	3.9732	2.5676
HER_FRQ	-1.0010	1.6868	0.7605	1.7733
OPIA_FRQ	1.9957	2.1947	0.1654	2.1874
POT_FRQ	2.6858	1.5535	2.8965	1.5113
BARB_FRQ	1.4788	2.0555	2.7277	2.0991
STIM_FRQ	0.7587	1.5980	1.7658	1.6052
DEPLOGTA	-0.7855	0.8454	-0.4133	0.8716
DEPLOGTD	-4.8396***	1.5075	-4.6802***	1.4696

Table 60 — Continued

Variable	Base Model b	Base Model se(b)	Final Model b	Final Model se(b)
Other Background Factors				
EPSTDGTX			-8.0701**	3.1985
EPSTETOH			-2.6913	7.8299
EDIADNO			3.5047	4.6410
EDIAGDEP			2.9908	6.5546
EDIAGASP			-7.5541	6.4994
EDIAGBTH			0.3519	7.6459
EPSTMHTX			-9.0855***	3.1244
Treatment Variables				
ENRGENY			2.1950	6.2035
ENRSUPY			-12.1973	7.9493
ETSYES			10.7616***	3.4218
ECTRONLY			-7.7252	9.0857
EOTHONLY			4.1375	15.9861
EBOTH			-23.9320	16.8020
ETXMISS			24.1327	27.5755
EAAYES			29.2638**	11.8031
EAAMISS			-37.6599	22.4596
Post-Release Variables				
ESPOUSE			-5.9017	6.0354
ECOM_LAW			-2.8794	6.2510
SUPVVIOL			-3.0339	3.1748
DRUGVIOL			-3.5556	2.0597
ECOHO1_6			-8.2872	9.1853
ECOHO7			0.3558	21.0413
ECOHO8			9.5539	10.5283
ECOHO9			11.3059	9.0468
ECOHO10			-16.7597	10.1613
ECOHO11			6.1236	8.2996
ECOHO12			-0.7174	7.5298
ECOHO13			-1.1123	7.2006
R-square	0.1931		0.3864	
Adjusted R-square	0.0763		0.1957	

* p < .10
** p < .05
*** p < .01

Table 61
Employment, Male Subjects
Bloom Model

Variable	Base Model b	Base Model se(b)	Final Model b	Final Model se(b)
Base Model Variables				
INTERCPT	18.0146	10.2451	21.0744	12.7373
SUBJECT	-3.5604	2.8745	1.8861	2.9951
EBLACK	3.0948	2.6951	0.0421	2.7033
ERACEOTH	-8.9317**	4.3530	-1.4294	4.4435
EHISP	-4.9315**	2.2753	-2.9729	2.2203
EPRIORCM	-0.4662	1.4890	0.2588	1.4555
ERECVIOL	1.8407	2.5948	2.0221	2.4858
EPASTVIO	-7.6954***	2.1527	-5.6721***	2.0801
TIMESRVD	2.2669***	0.6135	1.4912**	0.6016
ECJSPVNY	1.2142	4.4424	0.0261	4.2645
ECJSPVNM	-3.2037	8.4249	-0.2077	8.0704
AGERLSE	-0.0860	0.1588	-0.2757*	0.1570
ESUPILL	-5.4151***	1.3133	-3.7362***	1.2719
EWORKJOB	11.8585***	3.5387	13.6494***	3.4174
ELEGITUN	-6.9966	7.3456	-5.7371	7.0227
EUNEMP	-6.9066	4.6378	-2.8907	4.5137
EJOB_UNK	2.8166	10.8210	-7.7734	10.3919
GRADEA	2.0291***	0.6811	2.4063***	0.6581
COC_FRQ	-0.6309	0.6916	-1.5554**	0.6713
CRK_FRQ	-1.0787	0.7983	-0.1806	0.7925
HAL_FRQ	0.3992	1.0212	0.3392	0.9999
HER_FRQ	-1.6510**	0.7822	-1.3699*	0.7675
OPIA_FRQ	0.5870	1.0225	0.1373	1.0290
POT_FRQ	1.3623	0.7713	0.3937	0.7539
BARB_FRQ	-3.2637***	0.9803	-2.7819***	0.9526
STIM_FRQ	1.1009	0.8298	0.2699	0.8005
DEPLOGTA	0.3667	0.3969	0.4476	0.3867
DEPLOGTD	1.2818**	0.6544	2.7600***	0.6624

Table 61 — Continued

Variable	Base Model b	Base Model se(b)	Final Model b	Final Model se(b)
Motivation for Change Variables				
ECLUST1			-13.7879***	2.5427
ECLUST2			2.0891	2.9414
ECLUST3			-3.0012	2.3318
ECLUST4			-0.9989	3.7778
ECLUST5			4.8392	3.3196
ECLUST6			11.7391***	3.6201
Treatment Variables				
ENRGENY			-1.8194	2.9068
ENRSUPY			2.6520	5.3936
ETSYES			3.9286***	1.5117
ECTRONLY			14.9758***	5.7312
EOTHONLY			14.3607*	8.2256
EBOTH			4.6274	8.3332
ETXMISS			-49.8283**	20.5906
EAAYES			-0.0096	4.7608
EAAMISS			9.8603	8.9572
Supervision Variables				
ECCCNO			-4.5489**	2.0410
ECCCFAIL			-6.1504***	2.3266
UARATE			0.1417	0.6870
HHSE_STR			20.8516***	6.0948
Post-Release Variables				
ESPOUSE			3.3081	2.2198
ECOM_LAW			-3.8566	2.4917
SUPVVIOL			-3.3162***	1.0871
DRUGVIOL			-1.3010*	0.6834
ECOHO1_6			-6.4278*	3.8913
ECOHO7			2.3430	5.1635
ECOHO8			-3.1430	4.6208
ECOHO9			-4.5410	4.1592
ECOHO10			-3.9675	3.6514
ECOHO11			1.0290	3.0413
ECOHO12			1.4886	2.9491
ECOHO13			4.4582	3.0243
R-square	0.1507		0.2682	
Adjusted R-square	0.1311		0.2310	

* $p < .10$
** $p < .05$
*** $p < .01$

Table 62
Employment, Female Subjects
Bloom Model

Variable	Base Model b	Base Model se(b)	Final Model b	Final Model se(b)
Base Model Variables				
INTERCPT	-9.1822	23.4301	-40.8729	27.1713
SUBJECT	-0.6381	7.5064	2.4555	7.5681
EBLACK	-4.4442	7.2187	-1.9871	7.5018
ERACEOTH	9.5151	12.2625	6.7541	12.9251
EHISP	-8.6656	4.9906	-11.3521**	5.4915
EPRIORCM	-0.4632	3.0889	1.8176	2.9491
ERECVIOL	-1.2212	6.5812	-0.8017	6.8201
EPASTVIO	12.1045	8.8515	7.9503	8.5550
TIMESRVD	3.0405	2.4398	-0.0019	2.4832
ECJSPVNY	1.4795	11.2743	5.2046	11.0227
ECJSPVNM	6.2776	21.2466	-0.5006	20.4219
AGERLSE	-0.2444	0.4089	-0.3086	0.4289
ESUPILL	-1.0723	2.9837	-0.1124	2.9739
EWORKJOB	6.0379	6.3938	16.9472***	6.5986
ELEGITUN	16.9356	10.8024	28.5125***	10.6952
EUNEMP	4.9202	9.3432	-5.2177	9.1034
EJOB_UNK	-45.3106**	18.6578	-64.1265***	18.7402
GRADEA	3.6815***	1.1565	4.2940***	1.3039
COC_FRQ	6.1302***	1.4930	5.0228***	1.4809
CRK_FRQ	-2.9981**	1.2838	-0.2952	1.4137
HAL_FRQ	1.8972	2.6475	2.1343	2.6173
HER_FRQ	-2.8573**	1.3575	0.7411	1.6200
OPIA_FRQ	2.3736	2.1726	0.2313	2.1311
POT_FRQ	2.7747	1.4710	2.9582**	1.4253
BARB_FRQ	4.5700**	2.0172	3.6906*	2.1072
STIM_FRQ	4.0786**	1.6667	2.8735*	1.6205
DEPLOGTA	-2.1973***	0.8324	-1.3691	0.8802
DEPLOGTD	-6.6055***	1.5892	-5.7293***	1.5056

Table 62 — Continued

Variable	Base Model b	Base Model se(b)	Final Model b	Final Model se(b)
Other Background Factors				
EPSTDGTX			-7.2670**	3.1555
EPSTETOH			-3.2428	7.7473
EDIAGNO			-0.2233	4.6179
EDIAGDEP			-0.8293	6.2872
EDIAGASP			-14.3311**	6.1159
EDIAGBTH			-3.2677	7.3209
EPSTMHTX			-6.6580**	3.0297
Treatment Variables				
ENRGENY			2.1505	5.6322
ENRSUPY			-4.4092	7.9864
ETSYES			6.0215**	3.0279
ECTRONLY			-9.2331	9.6198
EOTHONLY			2.5367	17.5175
EBOTH			-13.4968	15.9204
ETXMISS			19.8380	28.5482
EAAYES			18.2420	12.6264
EAAMISS			-23.9299	23.8895
Post-Release Variables				
ESPOUSE			-7.6487	5.7708
ECOM_LAW			-3.0093	6.0982
SUPVVIOL			-3.1191	3.3753
DRUGVIOL			-6.1659**	1.9143
ECOHO1_6			-0.6759	7.9887
ECOHO7			-16.0996	14.9781
ECOHO8			10.9226	9.2785
ECOHO9			10.6195	8.3168
ECOHO10			- 7.1430	8.6438
ECOHO11			2.1817	8.8995
ECOHO12			4.4315	6.9258
ECOHO13			-3.3400	6.9230
R-square	0.4454		0.6078	
Adjusted R-square	0.3791		0.4988	

* p < .10
** p < .05
*** p < .01

Table 63
Employment, Male Subjects, Complete
Heckman Model

Variable	Final Model b	se(b)
Base Model Variables		
CONSTANT	-32.1110	33.0520
COMPLETE	17.2258	20.7844
EBLACK	-10.0642	8.2328
ERACEOTH	-0.7299	13.6061
EHISP	-7.3069	6.1439
EPRIORCM	-4.4277	4.0312
ERECVIOL	-7.8160	6.2269
EPASTVIO	4.7354	5.3460
TIMESRVD	4.2533***	1.6597
ECJSPVNM	5.9392	18.5640
ECJSPVNY	-1.7189	10.0180
AGERLSE	-1.2592***	0.4273
ESUPILL	-12.8703***	3.5538
EWORKJOB	33.2225***	8.0514
ELEGITUN	-67.5976***	14.6025
EUNEMP	8.6471	11.0839
EJOB_UNK	14.6921	24.1302
GRADEA	7.0702***	1.6470
COC_FRQ	-0.2528	1.8236
CRK_FRQ	-1.6577	1.9754
HAL_FRQ	-2.8671	2.7733
HER_FRQ	-2.3122	2.0165
OPIA_FRQ	-1.4065	3.0048
POT_FRQ	-0.9506	2.0245
BARB_FRQ	-1.9828	2.7868
STIM_FRQ	0.4170	2.1667
DEPLOGTA	-0.2646	0.9916
DEPLOGTD	5.0719***	1.6508
Motivation for Change Variables		
ECLUST1	-3.2774	7.0053
ECLUST2	3.2775	7.2702
ECLUST3	2.7983	7.3101
ECLUST4	-1.9732	10.0991
ECLUST5	4.8922	9.6808
ECLUST6	1.5480	12.0130
Treatment Variables		
ENRGENY	-8.2659	6.6498
ENRSUPY	12.7788	10.5683

Table 63 — Continued

Variable	Final Model b	se(b)
ETSYES	8.5452**	4.3213
ECTRONLY	36.2152***	12.5768
EOTHONLY	28.2776	18.4097
EBOTH	7.6860	18.6364
ETXMISS	-108.8187***	43.7235
EAAYES	15.4717*	11.5112
EAAMISS	-5.6524	21.1216
Supervision Variables		
ECCCNO	-22.9065***	6.0379
ECCCFAIL	-13.6094*	6.2678
UARATE	0.6791	1.8480
HHSE_STR	39.2389*	21.6417
Post-Release Variables		
ESPOUSE	10.4433*	5.8034
ECOM_LAW	-4.1095	6.1736
SUPVVIOL	-13.0330***	2.9608
DRUGVIOL	-3.6384**	1.7792
ECOHO1_6	-31.4436***	11.0559
ECOHO7	10.9644	16.2027
ECOHO8	-4.7308	13.0873
ECOHO9	-7.2790	12.2725
ECOHO10	-5.6929	10.4097
ECOHO11	2.0520	9.1687
ECOHO12	9.8891	7.5761
ECOHO13	12.8270*	7.7957
COVARIAN	-0.1731	0.2322
SIGMA	96.3157***	4.0892

* $p < .10$
** $p < .05$
*** $p < .01$

Table 64
Employment, Female Subjects, Complete
Heckman Model

Variable	Final Model b	se(b)
Base Model Variables		
CONSTANT	-247.2121**	136.5464
COMPLETE	-25.4524	39.4746
EBLACK	2.2213	16.5481
ERACEOTH	-22.7347	29.3515
EHISP	-41.9586***	14.0349
EPRIORCM	6.5783	6.4969
ERECVIOL	-29.1517*	16.8577
EPASTVIO	27.5197	17.8484
TIMESRVD	-2.7949	4.7383
ECJSPVNY	-5.5018	20.6673
ECJSPVNM	15.8639	37.3754
AGERLSE	-0.6517	0.9272
ESUPILL	3.5440	6.2005
EWORKJOB	46.4459***	15.0039
ELEGITUN	33.7117	22.3265
EUNEMP	-14.6981	20.1353
EJOB_UNK	-113.9005***	46.5668
GRADEA	3.2357	3.1933
COC_FRQ	4.7075	3.0739
CRK_FRQ	3.3980	2.9440
HAL_FRQ	11.7363**	5.4208
HER_FRQ	2.5719	3.7321
OPIA_FRQ	1.5998	4.4917
POT_FRQ	6.2249**	3.0237
BARB_FRQ	5.9649	4.3535
STIM_FRQ	4.3169	3.2972
DEPLOGTA	-0.5535	1.7808
DEPLOGTD	-11.6221***	3.0589
Other Background Factors		
EPSTDGTX	-19.9376***	6.9881
EPSTETOH	-14.6600	14.8143
EDIAGNO	7.5648	9.8947
EDIAGDEP	-6.2374	12.6390
EDIAGASP	-20.5639	14.2575
EDIAGBTH	10.7854	15.9964
EPSTMHTX	-21.8755***	6.6828
Treatment Variables		
ENRGENY	10.5016	12.8803
ENRSUPY	-41.1975**	17.9066
ETSYES	23.5507***	7.9883
ECTRONLY	6.9478	20.9722
EOTHONLY	-20.2095	31.0567
EBOTH	-65.6195	44.6805
ETXMISS	51.4462	64.7839
EAAYES	129.7196	126.6303
EAAMISS	-216.0057	252.5413
Post-Release Variables		
ESPOUSE	-24.3226*	13.1360
ECOM_LAW	4.4116	12.8448
SUPVVIOL	-1.7698	6.5649
ECOHO1_6	-42.0943*	18.5261

Table 64 — Continued

Variable	Final Model b	se(b)
ECOHO7	-13.8106	40.0700
ECOHO8	16.0893	21.5139
ECOHO9	41.2103**	20.4657
ECCCNO	-5.8564	12.4212
ECOHO10	-34.4378**	20.0645
ECOHO11	19.0277	17.4111
ECOHO12	5.0852	15.2581
ECOHO13	12.0990	14.4356
COVARIAN	-0.0127	0.5828
SIGMA	72.0446***	5.7980

* $p < .10$
** $p < .05$
*** $p < .01$

Table 65
Arrests, Male and Female Subjects, Supervised and Unsupervised Subjects
Treatment Subjects Only

Variable	Sites Only b	Sites Only se(b)	Sites Only OR	Final Model b	Final Model se(b)	Final Model OR
INTERCPT	-3.7881***	0.4897	.	-2.6817*	1.4784	.
COMPLETE	-0.4423**	0.1727	0.643	-0.2799	0.2224	0.756
DISCIP	0.2810	0.2514	1.324	0.0207	0.2778	1.021
WITHDRAW	0.3040	0.2968	1.355	0.2885	0.3323	1.334
Institution Indicators						
ELEXSITM	-0.6851	0.4232	0.504	-0.7925	0.7767	0.453
EBUTSITM	-0.2426	0.3431	0.785	-0.4267	0.5725	0.653
EFAISITM	0.3371	0.3397	1.401	0.0296	0.3824	1.030
ELOFSITM	-0.1472	0.7058	0.863	0.2392	0.9370	1.270
EMCKSITM	0.5373	0.5754	1.711	0.6146	0.7299	1.849
EMNASITM	0.4834	0.3814	1.622	0.1112	0.4689	1.118
EOXFSITM	0.4879	0.3293	1.629	-0.2089	0.4691	0.811
EPHXSITM	0.3505	0.4938	1.420	0.3088	0.5535	1.362
ERCHSITM	-0.0178	0.4647	0.982	-0.1593	0.5630	0.853
ESEASITM	-0.3702	0.5885	0.691	-0.0316	0.6891	0.969
ESHESITM	0.3125	0.3919	1.367	-0.0099	0.4822	0.990
ETALSITM	0.0708	0.3446	1.073	-0.4333	0.6046	0.648
ETRMSITM	0.1709	0.5775	1.186	0.9381	0.8191	2.555
EYANSITM	-0.3609	0.3871	0.697	0.0681	0.5431	1.071
EMRGSITM	-0.9267*	0.5174	0.399	-0.2475	0.7867	0.781
COHTIME	0.0091	0.0129	1.009	0.0269	0.0323	1.027
TIMETORL	-0.0360**	0.0165	0.965	-0.0173	0.0359	0.983
Base Model Variables						
EBLACK				-0.0805	0.2332	0.923
ERACEOTH				0.1058	0.3443	1.112
EHISP				0.0777	0.2379	1.081
EPRIORCM				0.3510**	0.1596	1.420
ERECVIOL				0.3634*	0.1924	1.438
EPASTVIO				-0.0407	0.1868	0.960
TIMESRVD				0.0988*	0.0596	1.104
ECJSPVNY				-0.2744	0.2691	0.760
ECJSPVNM				0.4202	0.4721	1.522
AGERLSE				-0.0606***	0.0180	0.941
ESUPILL				-0.0768	0.1289	0.926
EWORKJOB				0.0357	0.3099	1.036
ELEGITUN				0.2187	0.5643	1.244
EUNEMP				0.2437	0.4209	1.276
EJOB_UNK				-1.0386	0.9131	0.354
GRADEA				0.0112	0.0537	1.011
COC_FRQ				0.0369	0.0659	1.038
CRK_FRQ				-0.1486**	0.0727	0.862
HAL_FRQ				-0.0014	0.0921	0.999
HER_FRQ				0.1084	0.0678	1.114
OPIA_FRQ				-0.0817	0.1035	0.922
POT_FRQ				-0.0417	0.0701	0.959
BARB_FRQ				0.0870	0.0976	1.091
STIM_FRQ				-0.0952	0.0766	0.909
DEPLOGTA				-0.0351	0.0349	0.966
DEPLOGTD				0.0309	0.0560	1.031

Table 65 — Continued

Variable	Sites Only b	Sites Only se(b)	Sites Only OR	Final Model b	Final Model se(b)	Final Model OR
Supervision Variables						
ECCCNO				-0.1687	0.1822	0.845
ECCCFAIL				0.4493**	0.1926	1.567
SUP_REL				-0.0108	0.3480	0.989
Post-Release Variables						
ECOHO1_6				0.3816	0.5568	1.465
ECOHO7				-0.0306	0.5019	0.970
ECOHO8				0.5803	0.3707	1.787
ECOHO9				-0.5831	0.4524	0.558
ECOHO10				0.7502***	0.2731	2.117
ECOHO11				-0.8075	0.4929	0.446
ECOHO12				0.0401	0.3809	1.041
ECOHO13				0.3438	0.4303	1.410
Time Variables						
D_T2	0.4500	0.3774	1.568	0.4720	0.3829	1.603
D_T3	0.3015	0.3920	1.352	0.3494	0.3977	1.418
D_T4	0.2073	0.4050	1.230	0.3089	0.4108	1.362
D_T5	0.8413**	0.3616	2.319	0.9977***	0.3688	2.712
D_T6	0.3854	0.3985	1.470	0.5700	0.4053	1.768
-2 Log Likelihood						
Intercept Only		913.772			913.772	
Intercept and Covariates		879.189			803.653	
DF		24			61	
Concordant Pairs		65.0%			78.1%	
Somer's D		0.342			0.584	
Hosmer-Lemeshow Goodness of Fit	15.218 with 8 DF (p=0.0550)			4.8296 with 8 DF (p=0.7756)		

* $p < .10$
** $p < .05$
*** $p < .01$

NOTE: Cases from Danbury, Dublin, LaTuna, and Three Rivers were deleted due to no failures, and cases from Alderson deleted due to quasi-complete separation.

Table 66
Arrests, Male and Female Subjects, Supervised Subjects Only
Treatment Subjects Only

Variable	Sites Only b	Sites Only se(b)	Sites Only OR	Final Model b	Final Model se(b)	Final Model OR
INTERCPT	-3.6895***	0.5370	.	-2.1701	1.6661	.
COMPLETE	-0.4389**	0.1903	0.645	-0.0694	0.2713	0.933
DISCIP	0.3557	0.2839	1.427	-0.1829	0.3401	0.833
WITHDRAW	0.3368	0.3199	1.400	0.3133	0.4058	1.368
Institution Indicators						
ELEXSITM	-0.2787	0.4326	0.757	-0.6892	0.8726	0.502
EBUTSITM	-0.2248	0.3748	0.799	-0.8250	0.6591	0.438
EFAISITM	0.0480	0.4124	1.049	-0.5022	0.4848	0.605
ELOFSITM	-0.0449	0.7147	0.956	0.0387	1.0276	1.039
EMCKSITM	0.5300	0.5862	1.699	0.7149	0.8125	2.044
EMNASITM	0.5305	0.3940	1.700	0.2956	0.5320	1.344
EOXFSITM	0.6815**	0.3387	1.977	-0.1814	0.5045	0.834
EPHXSITM	0.4443	0.5043	1.559	0.8566	0.5956	2.355
ERCHSITM	0.2144	0.7060	1.239	0.3642	0.8628	1.439
ESEASITM	-0.9344	0.9712	0.393	-0.2416	1.0603	0.785
ESHESITM	0.3916	0.4071	1.479	-0.0228	0.5679	0.977
ETALSITM	0.2337	0.3746	1.263	-0.3054	0.6448	0.737
ETRMSITM	-0.0319	0.6945	0.969	1.3015	0.9730	3.675
EYANSITM	-0.2352	0.4194	0.790	0.1192	0.6454	1.127
EMRGSITM	-1.3239	0.7052	0.266	-0.9232	0.9619	0.397
COHTIME	0.0038	0.0138	1.004	0.0323	0.0355	1.033
TIMETORL	-0.0374**	0.0174	0.963	-0.0038	0.0391	0.996
Base Model Variables						
EBLACK				0.0107	0.2745	1.011
ERACEOTH				-0.1936	0.4114	0.824
EHISP				0.1077	0.2786	1.114
EPRIORCM				0.3576*	0.1945	1.430
ERECVIOL				0.0297	0.2363	1.030
EPASTVIO				0.3554	0.2206	1.427
TIMESRVD				0.1707***	0.0637	1.186
ECJSPVNY				-0.6832**	0.3149	0.505
ECJSPVNM				0.9882*	0.5217	2.686
AGERLSE				-0.0873***	0.0219	0.916
ESUPILL				-0.0050	0.1571	0.995
EWORKJOB				0.2660	0.3506	1.305
ELEGITUN				-0.0271	0.6039	0.973
EUNEMP				0.1205	0.5062	1.128
EJOB_UNK				-1.0000	0.9732	0.368
GRADEA				0.0514	0.0690	1.053
COC_FRQ				0.0312	0.0818	1.032
CRK_FRQ				-0.2479***	0.0946	0.780
HAL_FRQ				-0.0194	0.1100	0.981
HER_FRQ				0.1129	0.0805	1.119
OPIA_FRQ				-0.2269*	0.1343	0.797
POT_FRQ				-0.0724	0.0852	0.930
BARB_FRQ				0.2308**	0.1164	1.260
STIM_FRQ				-0.0677	0.0942	0.935
DEPLOGTA				0.0075	0.0400	1.008
DEPLOGTD				0.0053	0.0654	1.005

Table 66 — Continued

Variable	Sites Only b	Sites Only se(b)	Sites Only OR	Final Model b	Final Model se(b)	Final Model OR
Supervision Variables						
ECCCNO				-0.3837*	0.2280	0.681
ECCCFAIL				0.2013	0.2245	1.223
UARATE				-0.0954	0.0795	0.909
Post-Release Variables						
ESPOUSE				-0.5624*	0.3067	0.570
ECOM_LAW				0.1971	0.2978	1.218
SUPVVIOL				0.0849	0.1107	1.089
DIRTY				0.3433	0.3126	1.410
EMP_HRS				-0.0518***	0.0084	0.950
ECOHO1_6				0.4346	0.6407	1.544
ECOHO7				-0.4526	0.6882	0.636
ECOHO8				0.8136*	0.4603	2.256
ECOHO9				-0.8550	0.6158	0.425
ECOHO10				1.1181***	0.3393	3.059
ECOHO11				-1.0942*	0.5829	0.335
ECOHO12				0.2009	0.4805	1.222
ECOHO13				0.4901	0.4837	1.633
Time Variables						
D_T2	0.3833	0.4193	1.467	0.4222	0.4368	1.525
D_T3	0.3415	0.4259	1.407	0.4149	0.4459	1.514
D_T4	0.1314	0.4524	1.140	0.3563	0.4776	1.428
D_T5	0.8431**	0.3969	2.324	1.2044***	0.4279	3.335
D_T6	0.5051	0.4265	1.657	0.9099**	0.4593	2.484
-2 Log Likelihood						
Intercept Only		762.944			762.944	
Intercept and Covariates		730.505			584.937	
DF		24			66	
Concordant Pairs		66.6%			87.3%	
Somer's D		0.372			0.760	
Hosmer-Lemeshow Goodness of Fit (p=0.1619)	4.3931 with 8 DF (p=0.8200)			11.767 with 8 DF		

* p < .10
** p < .05
*** p < .01

NOTE: Cases from Danbury, Dublin, LaTuna, and Three Rivers deleted due to no failures and cases from Alderson deleted due to quasi-complete separation. CCC placement during supervision (HHSE_STR) variable deleted due to small sample size.

Table 67
Arrests or Revocations, Male and Female Subjects, Treatment Subjects Only

Variable	Sites Only b	Sites Only se(b)	Sites Only OR	Final Model b	Final Model se(b)	Final Model OR
INTERCPT	-3.5532***	0.4380	.	-2.2143	1.4347	.
COMPLETE	-0.3167**	0.1528	0.729	0.0039	0.2368	1.004
DISCIP	0.3276	0.2334	1.388	-0.3928	0.3030	0.675
WITHDRAW	0.1800	0.2668	1.197	0.4671	0.3538	1.595
Institution Indicators						
EALDSITM	-0.6540	0.4397	0.520	-0.5836	0.7422	0.558
EDANSITM	-0.7320	0.9885	0.481	-1.0540	1.1137	0.349
EDUBSITM	-0.5136	0.9760	0.598	0.2137	1.1297	1.238
ELEXSITM	-0.0069	0.3531	0.993	0.0034	0.7793	1.003
EBUTSITM	-0.2477	0.3346	0.781	-0.7538	0.6699	0.471
EFAISITM	-0.1318	0.3885	0.877	-1.6557	0.4719	0.519
ELATSITM	-0.0758	0.9808	0.927	-0.3335	1.1177	0.716
ELOFSITM	-0.0305	0.5944	0.970	0.7376	0.8509	2.091
EMCKSITM	0.7359	0.4715	2.087	-0.1984	0.7127	0.820
EMNASITM	0.7073**	0.3219	2.029	0.3056	0.4458	1.357
EOXFSITM	0.6152	0.3079	1.850	0.3186	0.5113	1.375
EPHXSITM	0.7448	0.4022	2.106	1.3566**	0.5351	3.883
ERCHSITM	0.2874	0.5973	1.333	1.5374**	0.7450	4.652
ESEASITM	-0.5860	0.7162	0.557	-0.0309	0.9076	0.970
ESHESITM	0.8224***	0.3073	2.276	0.6897	0.4620	1.993
ETALSITM	0.1792	0.3345	1.196	0.1442	0.6495	1.155
ETRMSITM	0.3744	0.5132	1.454	0.2269	0.7911	1.255
ETRVSITM	-0.2931	0.9753	0.746	-1.2439	1.1185	0.288
EYANSITM	-0.1947	0.3533	0.823	0.5979	0.5151	1.818
EMRGSITM	-1.0004*	0.5166	0.368	-1.2278	0.8312	0.279
TIMETORL	-0.0395***	0.0141	0.961	-0.0139	0.0327	0.986
COHTIME	0.0024	0.0112	1.002	0.0061	0.0305	1.006
Base Model Variables						
EBLACK				0.2048	0.2462	1.227
ERACEOTH				-0.4369	0.3743	0.646
EHISP				0.1500	0.2331	1.162
EFEM						
EPRIORCM				0.1446	0.1601	1.156
ERECVIOL				0.1683	0.1982	1.183
EPASTVIO				0.3025	0.1876	1.353
TIMESRVD				0.1627***	0.0549	1.177
ECJSPVNY				-0.3901	0.2470	0.677
ECJSPVNM				1.1693***	0.4126	3.220
AGERLSE				-0.0383**	0.0165	0.962
ESUPILL				-0.0563	0.1306	0.945
EWORKJOB				0.3438	0.2617	1.410
ELEGITUN				-0.4237	0.4722	0.655
EUNEMP				-0.1207	0.4283	0.886
EJOB_UNK				-0.1300	0.6441	0.878
GRADEA				-0.0242	0.0534	0.976
COC_FRQ				0.0382	0.0664	1.039
CRK_FRQ				-0.1077	0.0643	0.898
HAL_FRQ				0.0267	0.0943	1.027
HER_FRQ				0.0124	0.0677	1.013
OPIA_FRQ				-0.1012	0.1033	0.904
POT_FRQ				-0.0918	0.0691	0.912
BARB_FRQ				0.1437	0.0925	1.155

Table 67 — Continued

Variable	Sites Only b	Sites Only se(b)	Sites Only OR	Final Model b	Final Model se(b)	Final Model OR
STIM_FRQ				-0.0958	0.0759	0.909
DEPLOGTA				-0.0349	0.0309	0.966
DEPLOGTD				0.0806	0.0567	1.084
Treatment Variables						
ENRGENY				-0.1267	0.2577	0.881
ENRSUPY				0.5322	0.3812	1.703
ETSYES				-0.2560	0.1692	0.774
ECTRONLY				-0.1171	0.2090	0.890
EOTHONLY				0.8942	0.3739	2.445
EBOTH				-0.9315**	0.3780	0.394
EAAYES				0.7140**	0.3103	2.042
EAAMISS				-1.0270	0.5179	0.358
Supervision Variables						
ECCCNO				-0.6034**	0.2465	0.547
ECCCFAIL				0.3574	0.2098	1.430
UARATE				-0.1605**	0.0726	0.852
HHSE_STR				-1.6848	0.9720	0.185
Post-Release Variables						
ESPOUSE				-0.4634	0.2765	0.629
ECOM_LAW				-0.0896	0.2888	0.914
SUPVVIOL				0.2642***	0.0754	1.302
DIRTY				1.1931***	0.2538	3.297
EMP_HRS				-0.0631***	0.0073	0.939
ECOHO1_6				-0.0908	0.5432	0.913
ECOHO7				-0.2940	0.5151	0.745
ECOHO8				0.2199	0.4121	1.246
ECOHO9				-0.6566	0.4265	0.519
ECOHO10				0.7141**	0.2975	2.042
ECOHO11				-0.6531	0.3732	0.520
ECOHO12				0.6516	0.3827	1.919
ECOHO13				0.4079	0.4160	1.504
Time Variables						
D_T2	0.1724	0.3712	1.188	0.4196	0.4233	1.521
D_T3	0.7880**	0.3334	2.199	1.1563***	0.3923	3.178
D_T4	0.5967	0.3478	1.816	1.0397**	0.4104	2.828
D_T5	0.9349***	0.3319	2.547	1.4639***	0.3977	4.323
D_T6	0.6983**	0.3482	2.010	1.2127***	0.4139	3.362
-2 Log Likelihood						
Intercept Only		1168.728			1139.942	
Intercept and Covariates		1111.686			804.256	
DF		29			80	
Concordant Pairs		67.4%			89.3%	
Somer's D		0.376			0.796	
Hosmer-Lemeshow Goodness of Fit	6.0254 with 8 DF (p=0.6444)			9.1881 with 8 DF (p=0.3267)		

* p < .10
** p < .05
*** p < .01

Table 68
Drug Use, Male and Female Subjects
Treatment Subjects Only

Variable	Sites Only b	Sites Only se(b)	Sites Only OR	Final Model b	Final Model se(b)	Final Model OR
INTERCPT	-2.1676***	0.3347	.	-3.1879***	1.2173	.
COMPLETE	-0.1126	0.1469	0.893	-0.2581	0.1964	0.773
DISCIP	0.4284*	0.2263	1.535	0.3106	0.2689	1.364
WITHDRAW	-0.4167	0.3049	0.659	0.0724	0.3354	1.075
Institution Indicators						
EALDSITM	-0.0704	0.2901	0.932	-0.1939	0.5574	0.824
ELEXSITM	-0.6854*	0.3574	0.504	-0.5203	0.6937	0.594
EBUTSITM	-0.2465	0.2769	0.782	0.1128	0.5293	1.119
EFAISITM	0.3953	0.2532	1.485	0.5640	0.3480	1.758
ELATSITM	1.0413**	0.5313	2.833	1.2254*	0.6948	3.406
ELOFSITM	0.3077	0.4091	1.360	0.3923	0.6377	1.480
EMCKSITM	-1.7336*	0.9608	0.177	-2.9868***	1.0354	0.050
EMNASITM	0.5428**	0.2718	1.721	0.6189*	0.3467	1.857
EOXFSITM	0.6565***	0.2499	1.928	0.2656	0.4212	1.304
EPHXSITM	0.1472	0.4570	1.159	0.2760	0.5198	1.318
ERCHSITM	0.3795	0.4762	1.462	0.6945	0.5794	2.003
ESEASITM	0.0178	0.4814	1.018	0.8866	0.6066	2.427
ESHESITM	0.1116	0.3100	1.118	-0.1174	0.4014	0.889
ETALSITM	-0.1226	0.2921	0.885	0.6181	0.5764	1.855
ETRMSITM	0.3157	0.4233	1.371	-0.2933	0.5842	0.746
ETRVSITM	-0.8557	0.9674	0.425	-1.4215	1.0782	0.241
EYANSITM	0.0678	0.2661	1.070	0.0691	0.3934	1.072
EMRGSITM	-0.2689	0.3042	0.764	-0.1902	0.5604	0.827
TIMETORL	-0.0288**	0.0119	0.972	-0.0179	0.0284	0.982
COHTIME	-0.0002	0.0094	1.000	-0.0142	0.0266	0.986
Base Model Variables						
EBLACK				0.3913**	0.2026	1.479
ERACEOTH				-0.1190	0.3285	0.888
EHISP				-0.0331	0.1967	0.967
EPRIORCM				0.2158*	0.1139	1.241
ERECVIOL				0.2950*	0.1683	1.343
EPASTVIO				-0.0588	0.1518	0.943
TIMESRVD				-0.0525	0.0626	0.949
ECJSPVNY				0.1812	0.2196	1.199
ECJSPVNM				0.0447	0.3878	1.046
AGERLSE				-0.0068	0.0128	0.993
ESUPILL				0.0040	0.0975	1.004
EWORKJOB				-0.1963	0.2033	0.822
ELEGITUN				-0.1216	0.3711	0.885
EUNEMP				-0.1413	0.2877	0.868
EJOB_UNK				0.3812	0.5156	1.464
GRADEA				0.0727*	0.0429	1.075
COC_FRQ				-0.0312	0.0504	0.969
CRK_FRQ				0.0904**	0.0452	1.095
HAL_FRQ				0.0157	0.0699	1.016
HER_FRQ				0.0092	0.0511	1.009
OPIA_FRQ				0.1481**	0.0720	1.160
POT_FRQ				0.0430	0.0546	1.044
BARB_FRQ				-0.1065	0.0712	0.899

Table 68 — Continued

Variable	Sites Only b	Sites Only se(b)	Sites Only OR	Final Model b	Final Model se(b)	Final Model OR
STIM_FRQ				0.0385	0.0544	1.039
DEPLOGTA				-0.0408*	0.0247	0.960
DEPLOGTD				0.0261	0.0443	1.026
Treatment Variables						
ENRGENY				-0.3075*	0.1893	0.735
ENRSUPY				-0.0204	0.2650	0.980
ETSYES				0.1240	0.1498	1.132
EOTHONLY				-0.1935	0.3840	0.824
ECTRONLY				-0.4595*	0.2494	0.632
EBOTH				0.8917***	0.3380	2.439
ETXMISS				1.3357	0.8251	3.803
EAAYES				0.3737	0.2506	1.453
EAAMISS				-0.9230**	0.4479	0.397
Supervision Variables						
ECCCNO				0.0475	0.2227	1.049
ECCCFAIL				0.4522***	0.1658	1.572
UARATE				0.0912*	0.0497	1.095
HHSE_STR				0.1366	0.5946	1.146
Post-Release Variables						
ESPOUSE				-0.2595	0.1849	0.771
ECOM_LAW				0.0077	0.1945	1.008
SUPVVIOL				0.3485***	0.0641	1.417
EMP_HRS				-0.0104**	0.0049	0.990
ECOHO1_6				-0.2241	0.4658	0.799
ECOHO7				-0.4429	0.4276	0.642
ECOHO8				-0.1896	0.3449	0.827
ECOHO9				-0.4811	0.3039	0.618
ECOHO10				0.1221	0.2736	1.130
ECOHO11				0.5673**	0.2582	1.763
ECOHO12				0.5646**	0.2785	1.759
ECOHO13				-0.0283	0.3421	0.972
Time Variables						
D_T2	-0.1808	0.2143	0.835	-0.0318	0.2345	0.969
D_T3	-0.2046	0.2205	0.815	0.1469	0.2412	1.158
D_T4	-0.4491*	0.2411	0.638	0.0067	0.2613	1.007
D_T5	-0.5054**	0.2501	0.603	0.0778	0.2701	1.081
D_T6	-1.5019***	0.3657	0.223	-0.8177**	0.3797	0.441
-2 Log Likelihood						
Intercept Only		1519.130		1519.130		
Intercept and Covariates		1450.624		1206.570		
DF						
Concordant Pairs		65.7%		83.2%		
Somer's D		0.331		0.671		
Hosmer-Lemeshow Goodness of Fit	4.0023 with 8 DF (p=0.8569)			7.0056 with 8 DF (p=0.5360)		

* p < .10
** p < .05
*** p < .01

NOTE: Six cases from Dublin were omitted from the models due to zero cells (*i.e.*, no failures). In addition, eight cases from Danbury were deleted due to quasi-complete separation.

Table 69
CCC Failure, Male and Female Subjects, Treatment Subjects Only

Variable	Sites Only b	Sites Only se(b)	Sites Only OR	Final Model b	Final Model se(b)	Final Model OR
INTERCPT	-1.4875***	0.4931	.	-0.2713	1.1315	.
COMPLETE	-0.5148**	0.2069	0.598	-0.5885**	0.2346	0.555
DISCIP	0.7843**	0.3440	2.191	0.8309**	0.3887	2.295
WITHDRAW	-0.3716	0.4126	0.690	-0.0901	0.4560	0.914
Institution Indicators						
EALDSITM	0.2373	0.3831	1.268	0.6862	0.4728	1.986
EBUTSITM	-0.7560*	0.4301	0.470	-0.8019*	0.4731	0.448
EDANSITM	-0.2715	1.1474	0.762	-0.6516	1.3059	0.521
EDUBSITM	1.3000	0.9780	3.669	1.0322	1.0922	2.807
EFAISITM	-0.5884	0.4911	0.555	-0.9504*	0.5684	0.387
ELATSITM	2.0990***	0.6585	8.158	2.1748***	0.7872	8.801
ELEXSITM	-2.4045***	0.7553	0.090	-3.5497***	0.9278	0.029
ELOFSITM	-1.1358	1.0135	0.321	-1.1711	1.0615	0.310
EMCKSITM	0.9725	0.7365	2.645	0.7900	0.8034	2.203
EMNASITM	0.4069	0.3980	1.502	0.3167	0.4675	1.373
EMRGSITM	-0.2805	0.4215	0.755	0.2242	0.4867	1.251
EOXFSITM	0.1086	0.3834	1.115	0.0009	0.4568	1.001
EPHXSITM	0.0470	0.5752	1.048	0.2782	0.6414	1.321
ERCHSITM	-0.2872	0.6564	0.750	-0.3836	0.7342	0.681
ESEASITM	-0.4159	0.6640	0.660	0.0506	0.7215	1.052
ESHESITM	0.4053	0.4166	1.500	0.2279	0.5264	1.256
ETALSITM	0.1141	0.3853	1.121	0.5120	0.4477	1.669
ETRMSITM	0.1275	0.6804	1.136	0.1298	0.7272	1.139
ETRVSITM	0.0329	1.0897	1.033	0.4245	1.2375	1.529
EYANSITM	0.2889	0.3388	1.335	0.6600	0.4267	1.935
COHTIME	0.0129	0.0147	1.013	0.0174	0.0169	1.018
TIMETORL	0.0156	0.0167	1.016	0.0312	0.0199	1.032
Base Model Variables						
EBLACK				0.2615	0.2878	1.299
ERACEOTH				-0.2645	0.4579	0.768
EHISP				0.0362	0.2468	1.037
EPRIORCM				0.4986***	0.1602	1.646
ERECVIOL				-0.3615	0.2743	0.697
EPASTVIO				0.2175	0.2159	1.243
TIMESRVD				0.0195	0.0796	1.020
ECJSPVNY				-0.2012	0.2754	0.818

Table 69 — Continued

Variable	Sites Only b	Sites Only se(b)	Sites Only OR	Final Model b	Final Model se(b)	Final Model OR
ECJSPVNM				0.4638	0.4809	1.590
AGERLSE				-0.0444***	0.0178	0.957
COC_FRQ				-0.0045	0.0661	0.995
CRK_FRQ				-0.0063	0.0671	0.994
HAL_FRQ				-0.0896	0.0989	0.914
HER_FRQ				0.1160	0.0725	1.123
OPIA_FRQ				-0.0004	0.0923	1.000
POT_FRQ				-0.0465	0.0734	0.955
BARB_FRQ				0.0353	0.0940	1.036
STIM_FRQ				-0.0936	0.0761	0.911
DEPLOGTA				0.0286	0.0381	1.029
DEPLOGTD				0.1017*	0.0604	1.107
ESUPILL				-0.1303	0.1317	0.878
EWORKJOB				-0.0911	0.2797	0.913
ELEGITUN				-0.8875	0.5657	0.412
EUNEMP				-0.1412	0.4332	0.868
EJOB_UNK				0.3611	0.6665	1.435
GRADEA				-0.0291	0.0602	0.971
Change Assessment Variables						
ECLUST1				0.4184*	0.2426	1.519
ECLUST2				-0.4340	0.2808	0.648
ECLUST3				-0.0834	0.3491	0.920
ECLUST4				0.4168	0.3020	1.517
ECLUST5				0.2385	0.4115	1.269
ECLUST6				0.0017	0.6295	1.002
Additional Background Variables						
EPSTDGTX				-0.1355	0.1400	0.873
EPSTETOH				-0.0290	0.2711	0.971
EDIAGNO				0.0545	0.2188	1.056
EDIAGDEP				-0.0816	0.3492	0.922
EDIAGASP				-0.2131	0.2521	0.808
EDIAGBTH				0.5325	0.4150	1.703
EPSTMHTX				0.0949	0.1623	1.100
-2 Log Likelihood						
Intercept Only	633.409			633..409		
Intercept and Covariates	585.988			511.883		
Concordant Pairs			68.6%	79.8		
Somer's D	0.378			0.599		
Hosmer-Lemshow Goodness of Fit	4.85 with 8 DF (p=0.7735)			9.6741 with 8 DF (p=0.2886)		

* p < .10
** p < .05
*** p < .01

Table 70
Employment, Male Subjects, Treatment Subjects Only

Variable	Sites Only b	Sites Only se(b)	Final Model b	Final Model se(b)
INTERCEP	38.7495***	7.2791	30.6567	22.6200
COMPLETE	11.3253***	3.2174	3.3670	3.7303
DISCIP	-10.3042*	5.5566	-0.5381	5.5207
WITHDRAW	-9.9472*	6.0105	-8.7872	6.0340
Institution Indicators				
EBUTSITM	5.5338	5.2200	8.1419	9.2079
EFAISITM	-11.4393**	5.5163	-5.0006	6.2276
ELOFSITM	11.9363	9.4912	1.7157	11.7128
EMCKSITM	-18.2962	10.0178	-8.4229	10.8313
EMNASITM	12.0731	6.3280	13.2705**	6.4449
EMRGSITM	13.5867**	6.3120	3.5959	9.8491
EOXFSITM	-16.6510***	5.8436	-7.7553	7.6801
ERCHSITM	1.6550	10.3131	-1.2167	10.7080
ESEASITM	4.6932	9.1964	-1.7873	10.5701
ESHESITM	6.5996	6.5081	4.7255	7.2023
ETALSITM	-0.7683	5.9878	4.4062	9.4668
ETRMSITM	-9.3956	9.6671	-7.9623	11.7833
ETRVSITM	-13.5148	13.3971	-14.4127	14.1510
EYANSITM	13.9876***	5.2701	10.7020	6.5497
COHTIME	0.3379	0.2216	0.1797	0.4598
TIMETORL	0.9370***	0.2498	0.6076	0.4954
Base Model Variables				
EBLACK			-1.3437	3.9582
ERACEOTH			-4.9109	6.1037
EHISP			1.1529	3.5635
EPRIORCM			-1.8591	2.0270
ERECVIOL			-7.4356**	3.2583
EPASTVIO			5.5369**	2.8103
TIMESRVD			0.4225	0.9482
ECJSPVNY			-2.8101	4.7201
ECJSPVNM			5.4680	8.3580
AGERLSE			-0.3028	0.2319
ESUPILL			-3.6451	1.9427
EWORKJOB			7.2200	3.9002
ELEGITUN			-12.1341	8.3415
EUNEMP			3.4986	5.9480
EJOB_UNK			-6.2709	10.3687
GRADEA			1.6459**	0.7792
COC_FRQ			-0.7529	0.9695
CRK_FRQ			1.0265	0.9917
HAL_FRQ			-0.7901	1.3497
HER_FRQ			0.2400	1.0812
OPIA_FRQ			-1.6642	1.5077
POT_FRQ			-0.4556	1.0100
BARB_FRQ			-0.6734	1.3922
STIM_FRQ			1.4907	1.0988
DEPLOGTA			0.8004	0.5103
DEPLOGTD			0.2564	0.8155

Table 70 — Continued

Variable	Base Model b	Base Model se(b)	Final Model b	Final Model se(b)
Treatment Variables				
ENRGENY			-3.8281	3.1819
ENRSUPY			7.0113	4.4415
ETSYES			2.6255	2.8161
ECTRONLY			5.2732	6.4047
EOTHONLY			2.9580	9.1898
EBOTH			-9.9466	8.8634
ETXMISS			-4.9454	22.4388
EAAYES			1.7481	7.0945
EAAMISS			-0.3514	13.3841
Supervision Variables				
ECCCNO			-3.3175	4.0947
ECCCFAIL			-10.3152***	3.5034
UARATE			0.2792	1.1016
HHSE_STR			2.5239	9.7071
Post-Release Variables				
ESPOUSE			4.8795	2.8943
ECOM_LAW			-0.2148	3.1480
SUPVVIOL			-6.3430***	1.6151
DRUGVIOL			-0.3313	1.0681
ECOHO1_6			-6.1697	8.2326
ECOHO7			3.7840	8.1534
ECOHO8			-0.9205	6.6688
ECOHO9			2.6910	5.5651
ECOHO10			2.8452	4.7694
ECOHO11			-1.8155	5.1634
ECOHO12			4.9572	5.4542
ECOHO13			-2.7609	6.2530
R-square	0.1017		0.3121	
Adjusted R-square	0.0722		0.2068	

* p < .10
** p < .05
*** p < .01

Table 71
Employment, Female Subjects
Treatment Subjects Only

Variable	Sites Only b	Sites Only se(b)	Final Model b	Final Model se(b)
INTERCEP	61.0837***	18.8709	-42.8151	57.7088
COMPLETE	3.3093	6.4204	-15.6595*	8.4658
WITHDRAW	-12.6133	10.9331	5.5565	11.1068
DISCIP	2.0879	10.5692	15.8256	11.5945
Institution Indicators				
EALDSITF	-5.3871	12.0890	-7.5404	24.8843
EDUBSITF	1.2791	13.2501	8.3793	17.0461
EDANSITF	0.2654	14.4700	9.9221	16.0893
ELEXSITF	3.8370	9.8438	-10.7610	25.4852
COHTIME	-0.5407	0.5499	-0.6510	1.2074
TIMETORL	0.3497	0.6114	-0.0450	1.1648
Base Model Variables				
EBLACK			-13.9794	12.3872
ERACEOTH			7.5346	23.2314
EHISP			-14.0500	9.4380
EPRIORCM			2.7749	4.5347
ERECVIOL			-16.4935	13.2149
EPASTVIO			14.6624	12.1796
TIMESRVD			-2.6116	4.4719
ECJSPVNY			-21.8618	13.3840
ECJSPVNM			46.8719	24.2315
AGERLSE			0.7641	0.6304
ESUPILL			6.3584	4.2381
EWORKJOB			15.4427	9.3212
ELEGITUN			17.5113	15.6152
EUNEMP			-10.4508	13.4391
EJOB_UNK			-25.3884	25.8912
GRADEA			3.6736	2.4873
COC_FRQ			0.1853	2.1312
CRK_FRQ			5.9633***	1.9387
HAL_FRQ			4.3003	3.6152
HER_FRQ			2.4945	2.3689
OPIA_FRQ			-0.5656	3.0347
POT_FRQ			4.8097**	2.2622
BARB_FRQ			3.0478	3.0274
STIM_FRQ			-0.4217	2.2608
DEPLOGTA			0.0825	1.1768
DEPLOGTD			-5.0124**	1.9809

Table 71 — Continued

Variable	Base Model b	Base Model se(b)	Final Model b	Final Model se(b)
Other Background Factors				
EPSTDGTX			-11.5537**	4.9696
EPSTETOH			4.2091	11.7114
EDIAGNO			0.2065	6.4146
EDIAGDEP			4.6496	10.5420
EDIAGASP			-0.0049	9.5346
EDIAGBTH			13.1529	10.8568
EPSTMHTX			-9.8249**	4.7917
Treatment Variables				
ENRGENY			-18.6746	14.2786
ETSYES			16.5595***	5.0622
ECTRONLY			-8.2499	10.2734
EOTHONLY			13.4910	19.7962
EBOTH			-21.2734	25.9017
EAAYES			36.2222**	17.1660
EAAMISS			-47.1299	33.2446
Post-Release Variables				
ESPOUSE			-9.5264	9.7654
ECOM_LAW			-4.2805	10.6947
SUPVVIOL			2.2984	5.1067
DRUGVIOL			-3.6110	3.1805
ECOHO1_6			-11.9649	22.1765
ECOHO7			1.8024	29.5588
ECOHO8			16.2751	15.3845
ECOHO9			12.9008	12.5676
ECOHO10			-6.6371	15.4663
ECOHO11			5.2367	10.1319
ECOHO12			-9.3290	13.6844
ECOHO13			1.4909	15.1372
R-square	0.0288		0.5846	
Adjusted R-square	-0.0315		0.2609	

* $p < .10$
** $p < .05$
*** $p < .01$

NOTE: ENRSUPY and ETXMISS are not included in this model because no women DAP subjects had a value of one for these variables.

APPENDIX A: SUPPLEMENTARY TABLES

Table A1
HDU Refusals — Male Subjects

Variable	b	se(b)	OR
INTERCPT	-1.8906	0.2049	.
DRACEB	-0.2613***	0.0877	0.770
DRACEO	-0.5633	0.3400	0.569
DSECLEVM	0.2031	0.1324	1.225
DSECLEVL	-0.2288**	0.1160	0.796
DETHNO	-0.2333**	0.1184	0.792
DOFFSEVM	0.2871***	0.1019	1.333
DOFFSEVH	0.1502	0.1388	1.162
DOFFSEVG	0.3300**	0.1309	1.391
DPRIORM	-0.1875	0.1336	0.829
DPRIORS	0.0105	0.1064	1.011
DVIOLM	0.2146	0.1180	1.239
DVIOLS	0.2986**	0.1179	1.348
AGE	0.0162***	0.0040	1.016
-2 Log Likelihood			
Intercept Only	4178.965		
Intercept and Covariates	4070.609		
Concordant Pairs	60.5%		
Somer's D	0.217		
Hosmer-Lemeshow Goodness of Fit	7.6473	with 8 DF (p=0.4687)	

** $p < .05$
*** $p < .01$

Table A2
HDU Refusals — Female Subjects

Variable	b	se(b)	OR
INTERCPT	-3.6313	0.4570	.
DRACEB	0.4923**	0.1918	1.636
DRACEO	-0.3275	0.6397	0.721
DSECLEVL	0.3694	0.1946	1.447
DOFFSEVM	0.5301**	0.2298	1.699
DOFFSEVH	0.6937**	0.2898	2.001
DOFFSEVG	0.7013	0.3938	2.016
DVIOLM	0.6696**	0.3094	1.953
DVIOLS	-0.5518	0.5475	0.576
AGE	0.0224**	0.0096	1.023
-2 Log Likelihood			
Intercept Only	830.383		
Intercept and Covariates	801.602		
Concordant Pairs	63.1%		
Somer's D	0.273		
Hosmer-Lemeshow Goodness of Fit	4.9634	with 8 DF (p=0.7615)	

** $p < .05$
*** $p < .01$

Table A3
Missed Research — Male Subjects

Variable	b	se(b)	OR
INTERCPT	-1.8360	0.2694	.
DETHN	0.2197	0.1730	1.246
DRACEB	-0.1214	0.1329	0.886
DRACEO	-0.0114	0.3691	0.989
DCPTR	1.4919***	0.1283	4.446
DNONDAP	-2.2187***	0.2168	0.109
AGE	-0.0117	0.0069	0.988
-2 Log Likelihood			
Intercept Only	2110.361		
Intercept and Covariates	1881.339		
Concordant Pairs	72.4%		
Somer's D	0.459		
Hosmer-Lemeshow			
Goodness of Fit	4.9446	with 8 DF (p=0.7635)	

** $p < .05$
*** $p < .01$

Table A4
Missed Research — Female Subjects

Variable	b	se(b)	OR
INTERCPT	-2.9544	0.5488	.
DOFFSEVM	-0.4313	0.2366	0.650
DOFFSEVH	-0.0633	0.3375	0.939
DOFFSEVG	-0.1742	0.4852	0.840
DCPTR	1.1466***	0.2277	3.148
DPRIORA	0.2778	0.2754	1.320
DPRIORB	0.2241	0.2640	1.251
DSECLEVL	0.5111**	0.2367	1.667
AGE	0.0225	0.0137	1.023
-2 Log Likelihood			
Intercept Only	605.203		
Intercept and Covariates	563.189		
Concordant Pairs	68.3%		
Somer's D	0.370		
Hosmer-Lemeshow Goodness of Fit	5.7372	with 8 DF (p=0.6766)	

** p < .05
*** p < .01

Table A5
Research Refusals — Male Subjects

Variable	b	se(b)	OR
INTERCPT	-2.7777	0.3421	.
DETHN	0.1700	0.2160	1.185
DRACEB	-0.2808	0.1660	0.755
DRACEO	-0.6562	0.6100	0.519
DCPTR	0.9539***	0.1723	2.596
DPRIORA	-0.3949	0.2410	0.674
DPRIORB	-0.1261	0.1933	0.882
DVIOLL	-0.2215	0.2450	0.801
DVIOLH	0.3117	0.1951	1.366
DNONDAP	-0.2446	0.1797	0.783
AGE	0.0087	0.0083	1.009
-2 Log Likelihood			
Intercept Only	1417.312		
Intercept and Covariates	1363.944		
Concordant Pairs	64.3%		
Somer's D	0.298		
Hosmer-Lemeshow Goodness of Fit	5.2222	with 8 DF (p=0.7336)	

** $p < .05$
*** $p < .01$

Table A6
Missed Intake 1 — Male Subjects

Variable	b	se(b)	OR	
INTERCPT	-1.7818	0.3693	.	
DCPTR	-0.6954***	0.2196	0.499	
DNONDAP	-1.8146***	0.4908	0.163	
AGE	-0.0059	0.0102	0.994	
-2 Log Likelihood				
Intercept Only		1037.665		
Intercept and Covariates		971.820		
Concordant Pairs		63.9%		
Somer's D		0.334		
Hosmer-Lemeshow				
Goodness of Fit		4.5583	with 8 DF	(p=0.8036)

** $p < .05$
*** $p < .01$

APPENDIX B: CODEBOOK OF VARIABLES USED IN TRIAD ANALYSES

Variables Used in Subject Attrition Analyses

Age at Time of Interview
AGE: A continuous variable.

Comparison vs. Treatment Subject
DCOMPTRMT: Dummy variable coded as 1 if comparison subject.
Excluded group is treatment subject.

DAP vs. non-DAP site (site where subject identified)
DNONDAP: Dummy variable coded as 1 if non-DAP site.
Excluded group is DAP site.

Ethnicity
DETHNO: Dummy variable coded as 1 if non-Hispanic.
Excluded group is Hispanic.

History of Violence (Dummy variables)
DVIOLM: Coded as 1 if minor history of violence.
DVIOLS: Coded as 1 if serious history of violence.
Excluded group has no history of violence.

Institution
This was coded as an effects vector that included all institutions where a research subject was identified.

Offense Severity (Dummy variables)
DOFFSEVM: Coded as 1 if moderate offense severity.
DOFFSEVH: Coded as 1 if high offense severity.
DOFFSEVG: Coded as 1 if great offense severity.
Excluded group is low moderate offense severity.

Prior Commitments (Dummy variables)
DPRIORM: Coded as 1 if minor prior commitments.
DPRIORS: Coded as 1 if serious prior commitments.
Excluded group has no prior commitments.

Race (Dummy variables)
DRACEB: Coded as 1 if black.
DRACEO: Coded as 1 if other race.
Excluded group is white.

Recency of Violence (Dummy variables)
DVIOLR: Coded as 1 if violence within past 5 years.
DVIOLP: Coded as 1 if violence more than 5 years ago.
Excluded group has no history of violence. (Note: Recency of violence was not included in the HDU analyses.)

Security Level of Institution (Dummy variables)
DSECLEVL: Coded as 1 if low security level..
DSECLEVM: Coded as 1 if medium security level.
Excluded group is minimum security level. (Note: female subjects were housed only at minimum and low security facilities.)

Variables Used in Outcome Analyses

Unless otherwise indicated, all of the nominal variables are coded as effects vectors.

Age at Time of Release from Current Incarceration
AGERLSE: A continuous variable.

Alcohol Dependency
DEPLOGTA: log odds of alcohol dependence; a continuous variable.

Alcohol Treatment History
EPSTETOH: Coded 1 if there was no previous inpatient or outpatient alcohol treatment.
Excluded group had previous history of alcohol treatment.

Amount of Time Served for Current Offense
TIMESRVD: Number of years served; a continuous variable.

Criminal Justice Status at Most Recent Incarceration
ECJSPVNY: Coded 1 if under supervision at time of incarceration.
ECJSPVNM: Coded 1 if supervision at time of incarceration unknown.
Excluded group not supervised at time of incarceration.

DAP Maturity
COHTIME: number of months between implementation of program and subject's admission to program; a continuous variable.

Diagnoses of Depression and Antisocial Personality
EDIAGNO: Coded 1 if neither diagnosis.
EDIAGDEP: Coded 1 if diagnosis of depression only.
EDIAGASP: Coded 1 if diagnosis of antisocial personality only.
EDIAGBTH: Coded 1 if both diagnoses.
Excluded group has missing diagnosis information.

Drug Dependency
DEPLOGTD: log odds of drug dependence; a continuous variable.

Drug Treatment History
EPSTDGTX: Coded 1 if no previous inpatient or outpatient drug treatment.
Excluded group had previous history of drug treatment.

Drug Use
The following variables for different drugs represent the frequency of use during the period of heaviest use, with the following values: 0 = never used or used fewer than 5 times, 1 = less than once per month, 2 = 1 to 3 days per week, 3 =1 to 2 days per week, 4 =3 to 4 days per week, and 5 = daily.
BARB_FRQ: barbiturates.
COC_FRQ: cocaine.
CRK_FRQ: crack.
HAL_FRQ: hallucinogens.
HER_FRQ: heroin.
OPIA_FRQ: opiates.
POT_FRQ: marijuana.
STIM_FRQ: stimulants.

Education
GRADEA: Number of years of education: A continuous variable.

Employment (Employment status in month before incarceration)
EWORKJOB: Coded 1 if working full- or part-time.
ELEGITUN: Coded 1 if unemployed because in school, a homemaker, retired, or disabled.
EUNEMP: Coded 1 if unemployed but looking for work.
EJOB_UNK : Coded 1 if employment status unknown.
Excluded group is composed of those unemployed because of involvement in illegal drug use or illegal activities, because the individual has never been employed, or due to other reasons.

Ethnicity
EHISP: Coded 1 for Hispanics, with non-Hispanics serving as excluded group.

History of Violence
ERECVIOL: Coded 1 if violence occurred less than 5 years ago.
EPASTVIO: Coded 1 if violence occurred 5 or more years ago.
Excluded group had no history of violence.

Illegal Source of Income
ESUPILL: Coded 1 if ever supported self mainly through illegal activity for at least one year.
Excluded group did not ever support self mainly through illegal activity for at least one year.

In-Prison Outpatient Treatment
ENRGENY: Coded 1 if received outpatient treatment in prison.
Excluded group did not receive outpatient treatment.

In-Prison Self-Help Group
ENRSUPY: Coded 1 if involved in self-help group (*e.g.*, NA, CA, or AA) in prison.
Excluded group not involved in self-help group.

Involved in Post-Release Self-Help Group
EAAYES: Coded 1 if involved in self-help group.
EAAMISS: Coded 1 if self-help group participation unknown/missing.
Excluded group did not participate in self-help after release from prison.

Level of selection bias
COVARIAN—Ordinal variable.

Living with Spouse
ESPOUSE: Coded 1 if living with spouse.
ECOM_LAW: Coded 1 if living with common-law partner.
Excluded group not living with spouse or common-law partner.

Mental Health Treatment History
EPSTMHTX: Coded 1 if subject received no previous inpatient or outpatient mental health treatment.
Excluded group had previous history of mental health treatment

Monthly Rate of Urine Testing
UARATE: Average number of urinalysis tests per month.

Month Since Release from BOP Custody
D_T2, D_T3, D_T4, D_T5, D_T6: Represents successive months of release, starting with second month of release. Excluded group is first month of release.

Motivation for Change
ECLUST1: Coded 1 for contemplation cluster.
ECLUST2: Coded 1 for preparation cluster.
ECLUST3: Coded 1 for reluctant cluster.
ECLUST4: Coded 1 for action cluster.
ECLUST5: Coded 1 for precontemplation cluster.
ECLUST6: Coded 1 for uninvolved cluster.
Excluded group has motivation for change information missing.

Placement in Halfway House After Release
HHSE_STR: Time-dependent covariate for event history analyses. In the models of employment, this is a dummy variable representing whether a halfway house placement occurred during supervision.

Post-Release Drug Use
A) DIRTY: Time dependent covariate: dummy variable representing whether drug or alcohol use occurred during the month in question. Used in event history analyses for the traditional and Bloom models.

B) DRUGVIOL: Ordinal variable: number of drug-related supervision violations throughout the supervision time period or until arrest or until end of supervision, whichever occurred first. Used in Heckman model.

Post-Release Employment
A) PRCTFULL: Percent of post-release period employed full-time: Used in Heckman model.

B) EMP_HRS: Number of hours worked during each post-release month. Time dependent covariate used for event history analyses in traditional and Bloom models.

Post-Release Supervision Violations
This consists of any violation of a condition of supervision except one related to drug use.

SUPVVIOL: Ordinal variable — number of violations of conditions of supervision throughout the supervision time period or until arrest or until end of supervision, whichever occurred first.

Prior Commitments
EPRIORCM: Coded 1 if had a major or minor prior commitment.
Excluded group had no prior commitment.

Race
EBLACK: Coded as 1 if black
ERACEOTH: Coded as 1 if of other race. Whites serve as excluded group.

Received CCC Placement/CCC Outcome
ECCCNO: Coded 1 if did not receive CCC placement.
ECCCFAIL: Coded 1 if failed CCC placement.
Excluded group completed CCC placement.

Received Transitional Services During CCC Placement
ETSYES: Coded 1 if received transitional services.
Excluded group did not receive transitional services.

Release Cohort of Subject
ECOHO1_6 : Coded 1 if released between July 1992 and December 1993.
ECOHO7: Coded 1 if released between January and March 1994.
ECOHO8: Coded 1 if released between April and June 1994.
ECOHO9: Coded 1 if released between July and September 1994.
ECOHO10: Coded 1 if released between October and December 1994.
ECOHO11: Coded 1 if released between January and March 1995.
ECOHO12: Coded 1 if released between April and June 1995.
ECOHO13: Coded 1 if released between July and September 1995.
Excluded group is cohort 14—released between October and December 1995.

Sex
EFEM: Coded 1 if female, with males serving as the excluded group.

Supervised After Release
SUP_REL: Time dependent covariate for event history analyses.

Time Served After Release from DAP
TIMETORL: Number of months between DAP discharge and release from custody.

Type of Post-Release Treatment
ECTRONLY: Coded 1 if received contract services only.
EOTHONLY: Coded 1 if received non-contract services.
EBOTH: Coded 1 if received both contract and non-contract services.
ETXMISS: Coded 1 if service received is unknown or missing.
Excluded group had no post-release treatment.

Type of Subject (These are dummy-coded variables with non-DAP controls serving as excluded group)
COMPDAP: Coded 1 if DAP comparison.
INCOMPTX: Coded 1 if DAP incomplete.
WITHDRTX: Coded 1 if DAP dropout.
DISCIPTX: Coded 1 if DAP disciplinary discharge.
GRAD9MO: Coded 1 if completed 9-month DAP.
GRAD12MO: Coded 1 if completed 12-month DAP.
Excluded group is non-DAP control.

Type of Subject (used for Bloom model)
SUBJECT: Coded 1 if DAP subject — comparison or treatment.
Excluded group is non-DAP control.

Type of Subject (used for Heckman model)
COMPLETE: Conditional probability of completing treatment if entered treatment.

Type of Treatment Subject (used for treatment subjects-only models)
WITHDRAW: Coded 1 if DAP dropout.
DISCIP: Coded 1 if DAP disciplinary discharge.
COMPLETE: Coded 1 if completed 9-month or 12-month DAP.
Excluded group is DAP incomplete.

APPENDIX C: MODELS FOR DEALING WITH SELECTION BIAS IN AN EVALUATION OF A PRISON-BASED DRUG TREATMENT PROGRAM

Introduction

The Federal Bureau of Prisons (BOP) wanted to assess whether in-prison modified therapeutic community treatment programs improve the post-release behavior of drug-involved offenders following release from the Bureau's custody. The Bureau sought to learn whether or not treatment:

- reduced halfway house placement failure.
- increased the percentage of time that offenders were employed full-time during the 6 months following release from BOP custody.
- decreased the rate of relapse to drug use following release from prison and halfway house confinement.
- decreased the rate of criminal recidivism, defined alternately as:
  (1) being arrested during the 6 months following release from BOP custody, and
  (2) being arrested or otherwise being revoked from supervised release during the 6 months following release from BOP custody.

The Bureau was unable to assign subjects randomly to treatment and to no treatment conditions, so it devised a quasi-experimental design to test for treatment effectiveness. Some Federal prisons had therapeutic community treatment programs (hereafter referred to as DAP facilities) and others did not (hereafter non-DAP facilities). Prisoners in DAP facilities did not differ materially from prisoners in non-DAP facilities, so the two populations were comparable for evaluation purposes. Within the DAP facilities, some offenders were offered and accepted treatment (hereafter the DAP treatment group) while others either were not offered treatment or declined treatment that was offered (hereafter the DAP comparison group). Of course, those offenders who were housed in non-DAP facilities did not receive treatment (hereafter the non-DAP control group).

The Bureau wanted to learn whether treatment improved the post-release performance for those who received treatment. However, the Bureau was concerned that a simple comparison of the outcomes for offenders who were treated (the DAP treatment group) with the outcomes for offenders who were not treated (the non-DAP control group and the DAP comparison group) could be misleading because of selection bias. In this case, the concern was that some unmeasured factors (such as motivation to change) that affect the decision to enter treatment might also affect post-release performance, so the relationship between treatment and post-release performance could be partly or wholly spurious. The Bureau consulted with Abt Associates about procedures for dealing with this problem. Abt Associates suggested three instrumental variable models for dealing with this form of selection bias: the *Bloom approach*, a *standard instrumental variables approach*, and a *Heckman selection bias approach.*

The *Bloom approach* (1984) is the most straightforward of the three approaches. Because a prisoner's assignment to a specific prison had nothing to do with whether he or she needed substance abuse treatment, selection bias does not affect a comparison between the outcomes for the non-DAP control group and the *combined* outcomes for the DAP treatment and comparison groups. To illustrate this approach, suppose that every prison comprises two populations of offenders: those who would enter treatment if it were offered to them and those who would not

enter treatment if it were offered. When treatment is offered, these populations can be identified, and when treatment is not offered, they cannot be identified. Let:

P_{accept}	The percentage of a prison population that would accept treatment if given the opportunity. Call this group A.
$1-P_{accept}$	The percentage of a prison population that would decline treatment if given the opportunity. Call this group B.
F_{accept}	The fraction of group A that would recidivate if treatment were not provided.
$F_{decline}$	The fraction of group B that would recidivate.

Then if treatment were provided to no one:

$$F_{untreated\ population} = P_{accept}\ F_{accept} +(1-P_{accept})F_{decline}.$$

This is the expected value of the observed proportion of failures in the non-DAP control group.

Suppose that, on average, treatment reduced the proportion of inmates who recidivate by an amount D. If treatment were provided to everyone who would accept it:

$$F_{treated\ population} = P_{accept}((F_{accept} -D) +(1-P_{accept})F_{decline}$$

This is the expected value of the observed proportion of failures in the combined DAP groups. A test of treatment effectiveness can be based on the differences between $F_{treated\ population}$ and $F_{untreated\ population}$. Some algebra shows that the expected value of the effect from treatment is:

$$D = (F_{untreated\ population}-F_{treated\ population})/P_{accept}.$$

The Bloom approach affords an estimate of the treatment effect D and a measure of its statistical significance despite the fact that the treated and untreated groups may have failure rates that differ from each other for reasons that have nothing to do with the receipt of treatment.

The Bloom approach to evaluating treatment effectiveness is not much complicated by introducing control variables and using regression models. The introduction of control variables has three benefits. First, by reducing unexplained variance, the regression can reduce the standard error of estimate for the treatment effect. Second, the control variables can help adjust for any population difference between DAP and non-DAP facilities. And, third, the parameters associated with control variables have policy relevance for the Bureau. Thus, the BOP selected the Bloom approach for the analysis reported in the main study. It will not be discussed further in this appendix, except to compare it with two other approaches.

The second approach is to use *standard instrumental variables* (Davidson and MacKinnon, 1993, for example). Suppose an analyst were to combine data from all three sources (non-DAP controls, DAP comparisons, and DAP treatment subjects), assign a dummy variable coded one to those who received treatment and coded zero for those who did not, and then regress the outcome variable on this dummy variable and any control variables that seem appropriate. The problem

with this approach is well known. The estimated regression parameter associated with the dummy variable will be biased and inconsistent if the dummy variable and the error term are not independent. Independence seems unlikely if any unmeasured factor (such as motivation) affects both the receipt of treatment and the error term.

A solution is to identify an instrumental variable that is highly correlated with the dummy variable but that is distributed as independent of the error term. One suitable instrument is the estimated probability of entering treatment, where this instrument might be estimated from a probit model. The dependent variable in the probit model is a dummy variable indicating whether the offender entered treatment. This probit model is estimated using just those data from the DAP subjects, since the non-DAP subjects have a zero probability by definition, so the instrument is set to zero for them. By substituting the instrument for the dummy variable and estimating the regression, the parameter estimate associated with the instrument is a consistent estimate of the treatment effect.

The standard instrumental variable approach is more difficult than is the Bloom approach, and we might ask: Why bother? This is a difficult question to answer in the abstract. Because the standard instrumental variable approach uses more information than does the Bloom approach, we might expect that parameter estimates based on the standard instrumental variable approach would have the potential to be more efficient than would be parameter estimates based on the Bloom approach. Monte Carlo testing shows that this is not necessarily true, however.

How these two approaches compare depends partly on the size of the non-DAP sample relative to the rest of the sample. The Bloom approach can yield relatively poor estimates when the proportion of non-DAP cases is small, but the estimates improve as the proportion approaches 50 percent of the study sample. Also, the instrumental variable estimates are poor when (1) the instrument is not highly correlated with the dummy variable, or (2) the instrument is highly collinear with other variables in the regression. Experience from limited Monte Carlo testing is not much to go on, but we note two facts. The first is that the non-DAP sample is only about 20 percent of the available data. The second is that the instrument is fairly strong, partly because the non-DAP subjects ensure a zero value for the instrumental variable, and partly because the probability of entering treatment varies systematically across the 20 DAP facilities and increases over time.[64] We return to these issues later.

The *Heckman selection bias* approach (Heckman, 1979; Maddala, 1983) is somewhat more difficult to apply than is the instrumental variable approach. It requires the analyst to jointly model the selection into the sample and into the post-release outcome. We describe this approach at length below. For now, note that the selection bias approach has much in common with the standard instrumental variable approach, and if the analyst is willing to limit his or her analysis to

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[64]These are important identifying conditions. For identification, we want the instrument to be strongly correlated with the receipt of treatment. The facts that treatment is unavailable to inmates from non-DAP facilities, that treatment availability varies widely across the DAP facilities, and that treatment availability increases over time means that the receipt of treatment can be predicted with fairly good accuracy by knowing the facilities where the inmate was housed and when he was housed there. The prediction should be strongly correlated with the receipt of treatment. However, there is little or no reason to believe that the place where the inmates was housed and when he was housed in that facility should have a strong effect on his subsequent misconduct following release. Consequently, the instrument should be distributed as independent of unmeasured variables that also affect recidivism. In technical terms, the model is identified.

a linear-additive regression model, there is little to recommend the selection bias approach over the instrumental variable approach. However, as explained by Maddala (1983, p.261), the Heckman selection bias model can be used to study more complicated models where treatment interacts with other variables. For that reason, we developed the selection bias model in this appendix.

In addition to selection bias, another problem complicates this comparison between those who were treated and those who were not treated. The Bureau sampled DAP comparison subjects, but it did not know the probability that an inmate from a DAP facility who declined treatment would be included in the sample. This is a serious problem because all three of the methods discussed above require that this sampling probability be known.[65]

In summary, the Bureau asked Abt Associates to help it develop statistical models for analyzing treatment effectiveness in terms of the outcomes identified earlier. It asked Abt Associates to help it deal with the problem of not knowing the sampling probability for DAP comparison subjects, to develop model-based adjustments for selection bias that are suitable for the Bureau's evaluation, and to demonstrate the use of these models using the Bureau's data. The Bureau provided those data, specified the variables that had to be included in the modeling, and specified that the model had to be linear-additive in those variables and their parameters. The reason for these restrictions was to ensure that inferences based on the Abt Associates' models could be compared readily with inferences based on the models estimated by the Bureau. Two of the Bureau's interests were to estimate models that ignore the selection bias problem and to compare results with findings from models that attempt to adjust for selection bias. Results from this comparison are presented in the main report.

The analysis reported here is preliminary. It is based on 6 months of follow up data, almost certainly too short of a period to judge the effectiveness of treatment. But the Bureau continues to collect data, increasing both the size of its sample and the length of the follow-up period. Given that future data will provide a much better basis for judging treatment effectiveness, and given

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[65]The Bureau of Prisons operates a number of facilities across the nation. Some are maximum security, others are medium security, and still others are minimum security. An inmate's original placement depends on his or her security requirements, prison space availability, and geography (so that offenders will be as near their homes as is practicable). Offenders often begin their terms in high or medium security facilities and progress through less secure facilities over time. They also transfer from facility to facility for a variety of other reasons, including the availability of specialized health care and other programs. However, with a few exceptions, inmates did not transfer from one facility to another to receive substance abuse treatment.

The Bureau developed a procedure to estimate the probability of receiving treatment based on an inmate's progression through the prison system. They assigned this probability—PROB in the text—to every member of the study sample. Our problem was to use PROB to estimate the probability of selection into the study sample for offenders at the time they were released from prison. This would be a relatively simple matter if the Bureau could divide the number of DAP comparison subjects who were selected for the study sample by the total number of DAP comparison subjects who were released, but at the time of release, the Bureau could no longer identify the population of DAP comparison subjects. This required us to infer the sampling probability for a releasee cohort.

that future analysis of those data will afford the opportunity for a more rigorous analysis, this appendix focuses on methodology rather than on substantive findings. The prospect of future analysis is the reason why we have developed the selection bias model when, given the constraints of a linear additive model imposed by the Bureau on this analysis, the instrumental variable model would seem to do quite well.

This appendix provides a technical exegesis of the statistical models developed by Abt Associates. There are four models:

- a lognormal survival model (Section 2.0).
- an exponential survival model (also Section 2.0).
- a probit model (Section 3.0).
- a two-limit tobit model (Section 4.0).

Each of these four models has an adjustment for selection bias. The appendix also explains how Abt Associates dealt with the problem of estimating sampling probabilities for DAP comparison cases. Finally, it provides computer output of the results from using the Bureau's data to estimate the four models, and a copy of the computing algorithm, as attachments.

Recidivism: Survival Models That Are Based on the Lognormal and Exponential Distributions

This section of the appendix develops two mathematical models of recidivism. One is based on a lognormal survival model and the other is based on an exponential survival model. We discuss both models (in section 2.1) because they raise similar analytic problems, which have similar solutions. In section 2.2, we introduce a form of selection bias into both models and develop an estimation procedure (maximum likelihood) that yields consistent parameter estimates of the treatment effect, provided the model is true. Deriving those estimates requires a model of the process by which subjects get into treatment, which we develop in section 2.3, and a procedure for deriving sampling weights, which we develop in section 2.4.

The Basic Recidivism Model

Upon release from prison (including release from a half-way house), every offender has a *propensity* to recidivate. Recidivism means either that the offender was rearrested or that illegal drug use was detected. These two events are analyzed separately. The propensity to recidivate can be expressed as a non-negative, increasing function of an underlying latent propensity score, Z. This score is in turn assumed to be a linear function of a dummy variable (coded 1 when the offender was treated and coded zero otherwise) and a vector of control variables. Thus, the propensity score is written:

$$Z_i = \alpha_0 + \alpha_1 TR_i + \alpha_2 X_i + \sigma\epsilon_{1i} \qquad \textbf{(8)}$$

where:

Z_i a latent variable, measured on a continuous scale, so that within a specified time the probability of recidivism for the i^{th} individual decreases as Z_i increases.

TR_i a dummy variable coded 1 when the i^{th} offender was treated and coded 0 otherwise.

X_i a column vector of control variables such as age, gender, and race.

α_0 a scalar parameter — the constant term.

α_1 a scalar parameter — the treatment effect.

α_2 a row vector of parameters associated with the control variables.

ϵ_{1i} a random error term, identically and independently distributed as standard normal across the sample of offenders. We use ϵ as an error term in other equations, so the superscript "1" is introduced to distinguish error terms across equations.

σ A scalar parameter. Alternatively, we might drop σ from (8) and assume that ϵ is distributed as normal with a mean of zero and variance of σ^2, but the derivations are simplified by using this first specification.

We eventually adopt two different assumption about how the latent variable Z affects the distribution of time until recidivism, but it is useful first to define the density and distribution functions for time until recidivism generically, and then substitute parametric distribution functions to get the lognormal and exponential models. Let:

t_i represent time until recidivism.

$\phi(t_i)$ represent the density function for time until recidivism.

$\Phi(t_i)$ represent the cumulative distribution function for time until recidivism.

The follow up period lasts 6 months. If recidivism occurs within 6 months, then we observe the time at which it occurred. Otherwise we observe that recidivism did not occur within those 6 months. The generic likelihood function for recidivism during the first 6 months is written:

$$L_1 = \prod_i \phi(T_i)^{R_i}(1-\Phi(6))^{1-R_i} \quad \textbf{(9)}$$

where:

L_1 is the generic likelihood function for a survival model with censoring at 6 months.

T_i is the time (in months) until recidivism for the i^{th} subject when recidivism is observed.

R_i is coded 1 when recidivism happens within the 6-month follow up period and is coded 0 otherwise.

This generic likelihood function is standard for survival models (Kalbfleisch and Prentice, 1980; Lancaster, 1990). It is readily changed into the likelihood for the lognormal survival model by substituting the lognormal density and distribution functions into the generic form, and, likewise, it is transformed into a variation of the exponential survival model by substituting density and distribution functions based on a modification of the exponential distribution. We take those steps below.

Based on inspection of the data, it appears reasonable to assume that the time until an arrest follows a lognormal distribution.[66] In this case, $\ln(t_i) = Z_i$, and the density function for time until an arrest is written:

$$\phi_A(t_{A_i}) = \frac{e^{-0.5\frac{(\ln(t_{A_i})-\alpha_0-\alpha_1 TR_i-\alpha_2 X_i)^2}{\sigma^2}}}{t_{A_i}\sqrt{2\pi\sigma^2}} \qquad \textbf{(10)}$$

where:

$\phi_A(t_{Ai})$ represents the lognormal density function for the distribution of time until arrest.

t_{Ai} time of arrest.

Substituting the lognormal density (10) and its distribution function into the generic likelihood function (9) yields the likelihood function for the lognormal survival model.

Based on preliminary analysis, time until a positive urine screen seems to follow an exponential distribution. The propensity to recidivate (8) is now written in the form:

$$\lambda_i = e^{Z_i} \qquad \textbf{(11)}$$

Unlike the usual exponential model, this specification has an error term ϵ_1 that must be taken into account in the analysis (see Trussel and Richards, 1985). This introduction of an error term is a convenient and realistic way to introduce selection bias into the model, although it does complicate the mathematics behind the development of the survival model. Thus, the density function for the time until recidivism is now written as the integral of a mixture distribution:

$$\phi_U(t_{U_i}) = \int_{\epsilon=-\infty}^{\epsilon=+\infty} \lambda_i e^{-\lambda_i t_{U_i}} \eta(\epsilon_1) d\epsilon_1 \qquad \textbf{(12)}$$

[66]The inspection is based on the Allison model, discussed in the main text, which treats each month's events as independently distributed but censored by occurrence of the first event. The Bureau estimated this model.

where:

$\phi_U(t_{Ui})$ represents the density function for the distribution of time until drug use was detected.

t_{Ui} time until detection of drug use.

$\eta(\epsilon_1)$ the standard normal density function.

The integration removes the unobserved ϵ_1 from the distribution.[67] However, the presence of ϵ_1 will not be innocuous in discussions to follow. Equation (12) has no closed-form equivalent expression and requires numerical integration. Of course, this is also true of its cumulative distribution function, which requires a second integration over t_{Ui} from 0 to T_i.

Introducing Selection Bias

A problem occurs when subjects who receive treatment are selected on a non-random basis. This may happen because subjects self-select for treatment or because treatment personnel are selective, or both. To build selection bias into the lognormal and exponential models, we introduce a second latent variable, the propensity to enter treatment:

$$Y_i = \beta_0 + \beta_1 X_i + \epsilon_{2i} \qquad \textbf{(13)}$$

Here:

Y_i a latent variable. The higher the value of Y, the more likely a person will enter treatment.

X_i a column vector of control variables, the same as defined earlier.

β_0 a scalar parameter.

β_1 a row vector of parameters conformable with X.

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[67]The models developed here are sometimes called mixture models (Lancaster, 1990), and the $\eta(\epsilon)$ is sometimes called the mixture distribution. Estimates of the parameters in the distribution of greatest interest to us (e.g., the exponential) are sensitive to the assumptions made about the mixture distribution (Yamaguchi, 1986). A literature on criminal careers (Spelman, 1994) reports that offense rates have a skewed distribution across offenders, and this finding might be extended to assume that time until recidivism will be similarly skewed, so that the error distributions chosen for this analysis have some justification. Others (Schmidt and Witte, 1988; Rhodes, 1989) have found the lognormal to be a useful distribution for explaining recidivism. Nevertheless, future analyses will test the sensitivity of results to alternative assumptions made about the mixture distribution. For example, by using a power transformations (such as the Box-Cox power transformation), the distribution $\eta(\epsilon)$ can be extremely flexible.

ϵ_{2i} a random error term that is distributed as standard normal.

and:

when $Y \geq 0$, then treatment occurs (TR=1), and

when $Y < 0$, then treatment does not occur (TR=0).

Unless ϵ_1 and ϵ_2 are statistically independent, the variable representing treatment (TR) will not be independent of ϵ_1. It seems unlikely that the two will be independent, because they both are affected by excluded variables, such as motivation to change behavior. This correlation will cause the parameter estimate of the treatment effect (α_1) to be biased and inconsistent unless it is taken into account in the analysis.

One approach to overcoming this problem is to assume a parametric form for the joint distribution between ϵ_1 and ϵ_2, and to take that joint distribution into account in the likelihood functions (equation 9). Assuming that the two are distributed as bivariate normal, two cases are pertinent: the first for time until an arrest and the second for time until detection of drug use. Considering the first case (the lognormal distribution), the density function expressed previously as equation (10) is correct only for those cases that come from non-DAP facilities. For people who receive treatment, we use the conditional density function as represented by equation (7) in place of (10).

$$\phi_A(t_{A_i}|TR_i=1) = \frac{e^{-0.5\frac{(\ln(t_{A_i})-\alpha_0-\alpha_1 TR_i-\alpha_2 X_i)^2}{\sigma_i^2}}}{t_{A_i}\sqrt{2\pi\sigma_i^2}} \frac{H\left(\frac{\beta_0+\beta_1 X_i+\rho\frac{\ln(t_{A_i})-\alpha_0-\alpha_1 TR_i-\alpha_2 X_i}{\sigma_i}}{\sqrt{1-\rho^2}}\right)}{H(\beta_0+\beta_1 X_i)} \qquad \textbf{(14)}$$

and for people who do not enter treatment and were members of the DAP comparison group, we use the conditional density function represented by (8) in place of (10).

$$\phi_A(t_{A_i}|TR_i=0) = \frac{e^{-0.5\frac{(\ln(t_{A_i})-\alpha_0-\alpha_2 X_i)^2}{\sigma_i^2}}}{t_{A_i}\sqrt{2\pi\sigma_i^2}} \frac{H_c\left(\frac{\beta_0+\beta_1 X_i+\rho\frac{\ln(t_{A_i})-\alpha_0-\alpha_2 X_i}{\sigma_i}}{\sqrt{1-\rho^2}}\right)}{H_c(\beta_0+\beta_1 X_i)} \qquad \textbf{(15)}$$

where:

H the standard normal cumulative distribution function.

H_c the complement of the standard normal cumulative distribution function.

ρ the correlation between ϵ_1 and ϵ_2.

The conditional density functions (7) and (8) have cumulative distribution counterparts, which must also be substituted into (9). We do not show those distribution functions because they are just the appropriate specification of the bivariate normal cdf divided by the unconditional probability that the subject was treated (7) or was not treated (8).

The general approach to deriving this likelihood is explained in Maddala (1983, p. 266). Briefly, we start with the bivariate normal density involving ϵ_1 and ϵ_2. This can be written as $\eta(\epsilon_1)\eta(\epsilon_2|\epsilon_1$). We integrate this over the appropriate range for ϵ_2 to get the joint probability of t_A and entering treatment (equation (**7**)) or not entering treatment (equation **(15)**). We divide the results by the unconditional probability of entering treatment (equation (**7**)) or not entering treatment (equation **(15)**).

In essence, then, the likelihood functions differ depending on whether the subject came from a non-DAP facility, from a DAP facility but did not enter treatment, or from a DAP facility and entered treatment. Nevertheless, the generic likelihood (9) holds; we just substitute the correct density and distribution function depending on whether the subject is a member of the non-DAP control group, the DAP comparison group, or the DAP treatment group.

The generic likelihood function also has to be modified when the exponential model is used. When a subject comes from a non-DAP facility, equation (12) represents the density function. When the subject comes from a DAP facility and receives treatment, we use (16) in place of (12)

$$\phi_U(t_{U_i}|TR_i=1) = \int_{\epsilon=-\infty}^{\epsilon=+\infty} \lambda_i e^{-\lambda_i t_{U_i}} \eta(\epsilon_1|TR_i=1)d\epsilon_1 \qquad \textbf{(16)}$$

and when the subject comes from a DAP facility but does not receive treatment then we use (17) in place of (12)

$$\phi_U(t_{U_i}|TR_i=0) = \int_{\epsilon=-\infty}^{\epsilon=+\infty} \lambda_i e^{-\lambda_i t_{U_i}} \eta(\epsilon_1|TR_i=0)d\epsilon_1 \qquad \textbf{(17)}$$

where:

$\eta(\epsilon_1|TR_i=1)$ is the normal density function conditional on $TR_i = 1$.

$\eta(\epsilon_1|TR_i=0)$ is the normal density function conditional on $TR_i = 0$.

and numerical integration is used to get these conditional distributions, because there is no closed-form equivalent expression. The density function for the error terms in (9) and (10) conditional on TR can be written:

$$\eta(\epsilon_1|TR_i=1) = \frac{\int_{\epsilon_2=-\beta_0-\beta_1 X_1}^{\infty} \eta_b(\epsilon_1,\epsilon_2,\rho)d\epsilon_2}{\int_{\epsilon_2=-\beta_0-\beta_1 X_i}^{\infty} \eta(\epsilon_2)d\epsilon_2} \quad \textbf{(18)}$$

$$= \frac{H\left(\frac{\beta_0+\beta_1 X_1+\rho\epsilon_1}{\sqrt{(1-\rho^2)}}\right)}{H(\beta_0+\beta_1 X_i)}\eta(\epsilon_1) \quad \textbf{(19)}$$

where:

η_b represents the density function for the bivariate normal (standard normal in this case), and

ρ represents the correlation between ϵ_1 and ϵ_2.

and a similar expression exists for $\eta(\epsilon_1|TR_i=0)$. As before, the density functions have cumulative distribution (over t_U) function counterparts. These must be numerically computed with a double integral and substituted, as appropriate, into (9).

The likelihood function is different depending on whether the subject came from the non-DAP control group, the DAP comparison group, or the DAP treatment group. The generic likelihood (9) holds; we substitute the correct density and distribution function depending on whether the subject is a member of the non-DAP control group, the DAP comparison group, or the DAP treatment group.

Estimating the Probability of Selection Into Treatment

Applying the adjustment described above for selection bias requires an estimate of β. Although the α and β parameters could be estimated jointly, it is easier (although less efficient) to estimate the β parameters from the probit model (equation (13)) and then maximize the likelihood expression (equation 9, after the appropriate substitutions) conditional on those estimates of β. Estimation of the probit model was not straightforward. Because the Bureau sampled the DAP comparison cases, we had to take that sampling into account by including the probability of being sampled as part of the likelihood function for the probit model. Thus, the probit model needs to be based on the joint probability of two events: entering treatment or not entering treatment, and being selected into the study sample. DAP treatment cases were selected with certainty, so they have a conditional selection probability equal to one, and non-DAP cases do not enter into this estimation, because those cases have a zero probability of entering treatment. The likelihood for this model is written:

$$L_2 = \prod_i \frac{H(\beta_0 + \beta_1 X_i)^{TR_i}\left(PS_i\left(1 - H(\beta_0 + \beta_1 X_i)\right)\right)^{1-TR_i}}{H(\beta_0 + \beta_1 X_i) + (PS_i(1 - H(\beta_0 + \beta_1 X_i))} \quad \textbf{(20)}$$

where:

PS_i is the probability of selection into the study sample for the i^{th} case. When the subject received treatment, the probability is 1, because all treated subjects were included in the sample.

The logic of this approach is that the probit model represents the probability of occurrence of two events. In the first event, a subject either is selected for treatment or he is not selected for treatment. The second event — being included in the sample — is then conditional on the outcome of the first event. If the subject entered treatment, then he was included in the sample, but if he did not enter treatment, he was included in the sample with a probability of PS_i. The likelihood function reflect the joint probability of those two events.

Determining PS, the Probability of Selection Into the Study Sample[68]

Estimation of the likelihood function (20) requires knowing PS_i, the probability of being selected into the study sample given that an offender was from a DAP facility and either entered treatment or did not enter treatment. As we commented in the introduction to this appendix, the Bureau did not record this probability. In fact, if would be difficult to do so, because offenders move from prison to prison, so the DAP comparison group comprises inmates who spent at least some time in a DAP facility, but did not necessarily spend their entire incarceration periods in a DAP facility. We sought to identify a justifiable way of determining the probability of estimating PS_i.

The key to this estimation was to use an estimate of the proportion of inmates who entered treatment given (1) the DAP facilities where an inmate was incarcerated during his imprisonment, (2) how long he spent in each of those facilities, and (3) and the proportion of persons who spent time in those facilities who entered treatment. The Bureau estimated that probability $PROB_i$ using procedures described in the main report. Given $PROB_i$, we can estimate the implied value of PS_i that makes the proportion of DAP comparison subjects and DAP treatment subject observed in the data consistent with PROB.

Define:

$PROB_i$ The probability that the i^{th} subject enters treatment, as estimated by BOP. The probability of being selected into the study sample is 1 conditional on entering treatment. Thus, the joint probability of entering treatment and being selected into the study sample is $PROB_i$.

[68] The solution presented in this section is provisional. We continue to work with the Bureau to develop better ways to estimate the implied sampling weights.

$PS_i(1\text{-}PROB_i)$ The probability that the i^{th} subject at a DAP facility does not enter treatment ($1\text{-}PROB_i$) times the conditional probability that a subject who does not enter treatment is selected for the study sample — PS_i. PS_i needs to be estimated.

We assume that PS_i is a function of the subject's release cohort (that is, that PS_i varies over time), the prison from which the subject was recruited (that is, that PS_i varies across facilities), and some other variables. We write the relationship between PS_i and the independent variables as:

$$PS_i = \frac{1}{1+e^{\sum_{j=1}^{J}\xi_{1j}ECOHO_{ij}+\sum_{k=1}^{K}\xi_{2k}INST_{ik}+\xi_3 X_i}} \quad \textbf{(21)}$$

where:

$ECOHO_{ij}$ is a dummy variable representing the i^{th} subject's release in cohort (j=1...J). Names of variables, as they appear in this appendix, were chosen by BOP and correspond to usage in the main report.

$INST_{ik}$ is a dummy variable representing the k^{th} institution.

ξ parameters to be estimated.

The likelihood function for estimating the ξ is:

$$L_3 = \prod_i \frac{\left(PS_i\ (1-PROB_i)\right)^{1-S_i} PROB_i^{\ S}}{\left(PS_i\ (1-PROB_i)\right)+PROB_i} \quad \textbf{(22)}$$

where:

L_3 The likelihood.

S_i is a dummy variable coded 1 when the i^{th} subject enters treatment and coded zero when he does not enter treatment.

The reader might note that, for purposes of the analysis discussed in this appendix, the estimation of PS is unnecessary. We could have estimated a linear probability model using PROB as the dependent variable and X as the independent variables and then developed the likelihood function (13) based on this linear probability model instead of the probit model. The reason for taking the extra step of estimating PS is that the Bureau needed the sampling probability, PS, to apply the Bloom solution discussed earlier. Although we will not discuss findings in detail, we did in fact estimate a model using this simpler approach, and found that it provided similar results to those based on the more complicated approach.

To summarize, the estimation requires three steps. In the first step, we estimate the sampling weights for the DAP comparison group members. Using those sampling weights, in the second step we estimate the probability that an offender housed in a DAP facility will be treated. Conditional on that probability, in the third step we estimate the treatment effect.

A Probit Model of Halfway House Failures

The Bureau chose to analyze failures in halfway house assignments as a dichotomous dependent variable — failure (coded 1) and success (coded 0). This decision suggested that a probit model would be an appropriate way to analyze outcomes. As before, we assume that every individual who is placed in a halfway house has a propensity to fail, expressed as a latent variable:

$$Z_i = \alpha_0 + \alpha_1 TR_i + \alpha_2 X_i + \epsilon_{3i} \quad \textbf{(23)}$$

We have reused notation from above because there seems to be little risk of confusion. The Z again represents the latent variable, but now it applies to the propensity to fail in a halfway house confinement.

An inmate fails when:

$$Z_i \geq 0$$

and he or she succeeds when:

$$Z_i < 0$$

Assuming that ϵ_3 and ϵ_2 are distributed as bivariate normal, the likelihood function for estimating the α can be written as:

$$L_{41} = \prod_{i \in nonDAP\ CONTROL} \mathrm{H}(\alpha_0 + \alpha_2 X_i)^{h_i} \left[1 - \mathrm{H}(\alpha_0 + \alpha_2 X_i)\right]^{1-h_i} \quad \textbf{(24)}$$

for the non-DAP control group, as:

$$L_{42} = \prod_{i \in DAP\ COMPARISON} \mathrm{N}(\alpha_0 + \alpha_2 X_i | TR_i = 0)^{h_i} \left[1 - \mathrm{N}(\alpha_0 + \alpha_2 X_i | TR_i = 0)\right]^{1-h_i} \quad \textbf{(25)}$$

for the DAP comparison group, and as:

$$L_{43} = \prod_{i \in DAP\ TREATED} \mathrm{N}(\alpha_0 + \alpha_1 TR_i + \alpha_2 X_i | TR_i = 1)^{h_i} \left[1 - \mathrm{N}(\alpha_0 + \alpha_1 TR_i + \alpha_2 X_i | TR_i = 1)\right]^{1-h_i} \quad \textbf{(26)}$$

for the DAP treatment group, where:

h_i equals 1 when the subject failed and equals 0 otherwise.

$N(\alpha_0+\alpha_2 X_i|TR_i=0)$ represents the probability of failing in a halfway house conditional on the i^{th} subject's not being treated and $N(\alpha_0+\alpha_1 TR_i+\alpha_2 X_i|TR_i=1)$ represents the probability of failure conditional on the i^{th} subject's being treated:

$$N(\alpha_0+\alpha_1 TR_i+\alpha_2 X_i|TR_i=1) = \frac{H_b(\alpha_0+\alpha_1 TR_i+\alpha_2 X_i, \beta_0+\beta_1 X_i, \rho)}{H(\beta_0+\beta_1 X_i)} \quad \textbf{(27)}$$

where:

H_b is the bivariate normal distribution function (standard normal in this case).
and a similar expression exists for $N(\alpha_0+\alpha_2 X_i|TR_i=0)$.

The likelihood function is then written:

$$L_4 = L_{41}L_{42}L_{43}$$

A Two-Limit Tobit Model of Employment

The Bureau chose to measure post-release employment as the percentage of time employed during the 6-month follow up period. This could range from 0 percent for those who were never employed to 100 percent for those who were always employed. Both extremes were observed in the data.

Although an ordinary least squares regression might be used to analyze this outcome, OLS regression suffers from three problems when applied in this context. The first problem is that parameter estimates will be biased and inconsistent because the outcomes have upper and lower limits, which are not taken into account by the estimation procedure. The second problem is that the standard errors will be inconsistent, because the error terms will necessarily be heteroscedastic. The third problem is that selection bias still needs to be taken into account. Although feasible generalized least squares can be used to deal with all these problems, an alternative approach is to use a two-limit tobit model (Maddala, p. 160).

As used here, this model assumes that the every offender has a propensity to be employed. Reusing the earlier notation, we write this propensity as:

$$Z_i = \alpha_0+\alpha_1 TR_i+\alpha_2 X_i+\epsilon_{4i} \quad \textbf{(29)}$$

The subject is unemployed at all times when

$Z_i < 0$,

and he is employed full time when

$Z_i > 100$,

and, otherwise, time employed (TE_i) equals the latent variable, so:

$TE_i = Z_i$ when $Z_i \geq 0$ and $Z_i \leq 100$

The unknown parameters can be estimated by maximum likelihood. As before, we have to account for three conditions. When the study subject comes from the non-DAP control group, the likelihood is:

$$L_{51} = \prod_{i \in nonDAP\ CONTROL} \mathrm{H}\left(\frac{-\alpha_0 - \alpha_2 X_i}{\sigma} \right)^{E_{1i}} \left[\frac{\eta\left(\frac{TE_i - \alpha_2 X_i}{\sigma} \right)}{\sigma} \right]^{E_{2i}}$$

$$\left[1 - \mathrm{H}\left(\frac{100 - \alpha_0 - \alpha_2 X_i}{\sigma} \right) \right]^{1 - E_{1i} - E_{2i}} \qquad \textbf{(30)}$$

When the subject come from the DAP treatment group, the likelihood is:

$$L_{52} = \prod_{i \in DAP\ TREATED} \mathrm{N}\left(\frac{-\alpha_0 - \alpha_1 TR_i - \alpha_2 X_i}{\sigma} | TR_i = 1 \right)^{E_{1i}} \left[\frac{\nu\left(\frac{TE_i - \alpha_0 - \alpha_1 TR_i - \alpha_2 X_i}{\sigma} | TR_i = 1 \right)}{\sigma} \right]^{E_{2i}}$$

$$\left[1 - \mathrm{N}\left(\frac{100 - \alpha_0 - \alpha_1 TR_i - \alpha_2 X_i}{\sigma} | TR_i = 1 \right) \right]^{1 - E_{1i} - E_{2i}} \qquad \textbf{(31)}$$

and when the subject comes from the DAP comparison group, the likelihood is:

$$L_{53} = \prod_{i \in DAP\ COMPARISON} \mathrm{N}\left(\frac{-\alpha_0 - \alpha_2 X_i}{\sigma} \middle| TR_i = 0 \right)^{E_{1i}} \left[\frac{\nu\left(\frac{TE_i - \alpha_0 - \alpha_2 X_i}{\sigma} | TR_i = 0 \right)}{\sigma} \right]^{E_{2i}}$$

$$\left[1 - \mathrm{N}\left(\frac{100 - \alpha_0 - \alpha_2 X_i}{\sigma} | TR_i = 0 \right) \right]^{1 - E_{1i} - E_{2i}} \quad \textbf{(32)}$$

where:

E_{1i} equals 1 when the subject was unemployed for the entire follow up period, and otherwise equals zero.

E_{2i} equals 1 when the subject was employed during part (but not all) of the follow up period, and otherwise equals zero.

As before, N and ν represent the conditional distribution and density function, respectively. The conditional distribution function has already been presented as part of the probit model, and the density is similar to that for the uncensored part of the lognormal model, except that the dependent variable is in natural rather than logarithmic units. Thus, we do not show them explicitly.

The likelihood function for the two-limit tobit model is written:

$$L_5 = L_{51} L_{52} L_{53}$$

Summary

Parameter estimates are not provided in this appendix; they appear in the main report. Researchers at the Bureau compared the parameter estimates from these selection bias models with estimates derived from other models that do not take selection bias into account. Findings are discussed in the main report.

We consider these estimates to be preliminary. The Bureau continues to collect data for more offenders and for longer follow-up periods. When additional data become available, we will estimate the models reported in this appendix using those data. For this round of estimation, we have used models that have a structural form specified by the Bureau. We expect to experiment with improvements to that structural form in future analyses. Additionally, the models developed in this paper make some strong assumptions, especially about mixture distributions. Some of these

assumptions can be relaxed,[69] and we will seek to do this in future analyses. Finally, we continue to search for better ways to weight the data.

[69]The derivations in this appendix rely heavily on assumptions about the joint distributions between two error terms. The bivariate normal has played a large role in those assumptions. Although it is difficult to substitute other distributional assumptions without that substitution leading to major changes to these models, it is practical to introduce power transformation (such as the Box-Cox power transformation) to provide more flexibility to the model. Given the possible sensitivity of such models to distributional assumptions (Yamaguchi, 1986), this seems like a prudent step to plan for future analysis.

GLOSSARY OF TERMS

BOP
Federal Bureau of Prisons

CCC
Community Corrections Center. These facilities also are referred to as halfway houses. Inmates can be placed in a CCC either prior to the end of their sentences while under BOP custody or while under supervision by a Probation officer. Although not applicable to the subjects in this study, an individual may be placed in a CCC for his or her entire sentence.

DAP
Drug Abuse Program — the BOP Residential Drug Abuse Treatment Programs serving individuals with histories of drug or alcohol abuse.

DAP comparison
Subject group consisting of individuals who were housed at one or more DAP sites at times in their incarcerations when they could have volunteered for and been accepted into a DAP.

Non-DAP control
Subject group consisting of individuals who were never housed at a DAP facility or were housed at such a facility only in the last few months of their incarcerations when it would have been too late to volunteer for DAP.

DAP complete
Subject group consisting of individuals who completed the in-prison residential DAP.

DAP incomplete
Subject group consisting of individuals who did not complete the in-prison residential DAP for the following reasons: (1) they were released from BOP custody or released to a CCC before completion, (2) they transferred to another institution before completion, or (3) they went out on writ before completion.

DAP dropout
Subject group consisting of individuals who did not complete the in-prison residential DAP because they voluntarily dropped out.

DAP disciplinary discharge
Subject group consisting of individuals who did not complete the in-prison residential DAP because they were discharged for disciplinary reasons *(i.e.,* did not adhere to program rules or were found guilty of committing a disciplinary infraction).

DSM-III-R
Diagnostic and Statistical Manual for Mental Disorders (American Psychiatric Association, 1987).

FCI
Federal Correctional Institution. A BOP facility housing sentenced inmates.

FMC
Federal Medical Center. A BOP facility serving inmates with medical problems.

INS
Immigration and Naturalization Service.

NCIC
National Crime Information Center. This is an automated database maintained by the Federal Bureau of Investigation with information on Federal and State arrests.

SENTRY
Automated BOP database with comprehensive information on currently and formerly incarcerated inmates. This database holds large amounts of information, including background characteristics, sentence length and conditions, program involvement, disciplinary infractions, institutional transfers, and Community Corrections Center placements.

TRIAD
Acronym for this drug treatment evaluation project. TRIAD stands for Treating Inmates' Addiction to Drugs.

UA
Urinalysis. These tests occur in the halfway house and while under supervision. During a halfway house placement, urinalysis testing may be completed by halfway house staff, as well as by treatment providers for cases in which the individual is enrolled in transitional services.

The standard drugs tested for when an individual is being supervised by a U.S. Probation officer include cocaine metabolites, opiate metabolites, phencyclidine, amphetamines, barbiturates, benzodiazepine, and methadone. As of February 1996, marijuana was added to the regular drug screen test. Other drugs are tested upon special request. The initial screening levels used by Probation officers were those approved by the U.S. Department of Health and Human Services.

VCCLEA
1994 Violent Crime Control and Law Enforcement Act. This law contains provisions for eligible inmates who complete all phases of the BOP's DAP to earn as much as a one-year reduction in their statutory release dates.

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