Federal Prison Residential Drug Treatment
Reduces Substance Use and Arrests After Release

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Prison Drug Program Outcomes

Abstract

Objective: The effectiveness of Federal prison-based residential drug and alcohol treatment programs was evaluated using event history procedures which addressed the problem of selection bias and included a wide range of control variables. Methods: The sample comprised 760 treatment subjects and 809 comparison subjects. Treatment subjects were from 20 different prisons of medium, low and minimum security levels. Comparison subjects were drawn from over 30 prisons. Results: The results indicated that individuals who entered and completed in-prison residential treatment were less likely to experience the critical post-release outcomes of new arrests and substance use during the first 6 months following release. Conclusions: Without controlling for selection bias, the effects of treatment would most likely have been attenuated. The results have greater generalizability than other studies of prison-based treatment. This study occurred within a multi-site context of 20 programs serving both male and female inmates, and operating within different security levels and different geographic regions.

Prison Drug Program Outcomes

Introduction

In recent years, the number of persons incarcerated in the United States has increased dramatically. Between 1985 and 1996 the incarcerated population increased by an average of 7.8 percent per year (1). Additionally, 62.2 percent of state prison inmates and 42.1 percent of Federal inmates report being regular drug users (2). The combination of a rapidly increasing prison population and the high percentage of prisoners with a history of drug use have led policymakers and correctional practitioners to call for increased funding of prison and community-based drug treatment programs. As a result, prison-based treatment programs for substance abusing offenders expanded greatly in the 1990's.

Evaluation of the effectiveness of treatment programs among criminal justice populations has largely focused on non-incarcerated populations and has examined the effectiveness of drug treatment as an alternative to incarceration or prosecution, or as a condition of probation or parole (3-7). Even though in-prison residential programs have been designed and implemented throughout the United States, evaluations of these programs are relatively few. Outcome evaluations have been limited to programs in six state prison systems. Those evaluations typically reported that treatment lowered recidivism (as measured by arrests, reconvictions and return to prison), decreased post-release drug use, and curtailed self-reported illegal activities (8-19). There are, however, significant methodological weaknesses in the majority of these studies, the most common and most important being the lack of attention to the problem of selection bias.

Under ideal circumstances, causal inferences can be imputed from research designs which use random assignment of subjects to different intervention protocols. There are three related problems in implementing experimental designs in applied social settings. First, there are obvious administrative and ethical reasons that make it difficult to implement random assignment studies in applied settings. Second, even when random assignment is possible, a problem arises in that subjects inevitably select themselves, or are selected by others, *out of* treatment (and control) groups by dropping out of treatment or refusing follow-up interviews. This is often called the problem of noncompliance (20). Third, when random assignment is not possible, the problem of noncompliance is compounded by the inevitability of subjects selecting themselves *into* treatment.

In our study, there were four prominent selection processes that filtered respondents into and out of the study: self-selection, administrative — or clinical — selection, treatment selection, and post-treatment selection.

The first process, self-selection, was created by internal motivational states and/or external incentives that disposed some people to volunteer for treatment. The second process, administrative selection, reflected the clinical judgment exercised by treatment providers and other administrators who determined whether someone was chosen for a program. The third process, treatment selection, weeded out clients who could not or refused to meet the program demands after entering treatment. Although clients weeded themselves out by dropping out of treatment, treatment providers in different programs exercised clinical judgment in expelling participants from treatment. The last selection pressure occurred when clients were lost to follow-up. The end result of such selection influences was that the treatment and control groups differed for unintended and uncontrolled reasons, and these differences potentially introduced bias into the outcomes of the study.

Collectively, this set of selection processes is known as selection bias. When selection bias has changed the composition of the control group, the treatment group, or both groups, observed differences between the control group and the treatment group may be due in part, or entirely, to differences in the composition of the respective groups. In short, the effect of treatment is confounded with the effect of selection processes, and there is no simple means of unraveling the two (21, 22). One of the major challenges inherent in evaluation research is to find design, measurement, and analysis methods to minimize the effects of selection bias. Such methods must be able to differentiate the effects attributable to the selection processes from those attributable to treatment.

Many researchers assume that selection bias usually results in a treatment group composed of subjects who are more motivated to change and who have an inherently lower risk of post-intervention failure. However, it is also possible that selection processes operate in an opposite manner. For example, there might be an incentive structure that encourages higher risk subjects, rather than lower risk subjects, to enter treatment. Another possibility is that treatment selection

is tightly controlled by providers who reserve treatment beds for the most difficult cases. Given the pervasive and difficult problems of selection bias, most evaluations have not employed methods that allow the evaluators to move beyond speculation about whether selection bias exists, and if it does, about the direction of the selection bias. We attempt to rectify these shortcomings in our study.

We address selection bias using the Heckman approach (23, 24). This approach allowed us to identify whether selection bias occurred and the nature of the selection bias. We also conducted analyses using two other analytic strategies. Although we do not report those results in this paper, they, along with additional details about the research design, are available from the authors (25). These analyses provided a form of 'sensitivity' analysis to test the robustness of our findings.

In addition to addressing selection bias issues, this study also improved upon the external validity of prior research. Most previous studies of in-prison intensive substance abuse treatment programs evaluated a single implementation of a treatment program. Results of such studies were dependent upon the unique characteristics of that one site, whether of the staff, the inmate drug treatment volunteers, or the extraordinary attention and support provided to such a treatment program. The present study included male and female inmates from treatment programs in 20 federal prisons of minimum, low and medium security levels. Thus, our results are more generalizable.

The purpose of this article is to describe the Bureau of Prisons (BOP's) residential substance abuse programs and the positive impact of these programs on substance use and arrests during the first 6 months after release. We provide an overview of other substance abuse treatment services received by both treatment and comparison subjects. We also provide an overview of the research design, including a description of the analytic procedure which identified whether selection bias was present and the nature of the selection bias.

BOP Residential Substance Abuse Treatment Programs

Following a conference in September 1988 organized by Michael Quinlan, then Director of the BOP, program staff met to plan the development of residential substance abuse programs.

Initially, the number of programs established was small. By 1991, eight programs were operational. This was followed by a rapid expansion of programs wherein another 22 programs were approved for activation by September 1992. The growth of programs subsequently slowed down and, as of June 1998 there were 42 residential programs with a total bed capacity of 6,000.

The federal prison system offered two types of residential programs. The first type, the moderate intensity programs, which included the majority of programs in this evaluation effort, offered 500 hours of treatment over a nine-month period and had a staff-to-inmate ratio of 1:24. The second type, the high intensity programs, was offered at three institutions. These programs provided 1,000 hours of treatment over a twelve-month period and had a staff-to-inmate ratio of 1:12. Participation in both types of programs was voluntary. Incentives for participation varied over time and across programs. These ranged from small items such as caps and pens to increased halfway-house time, to, with the passage of the Violent Crime Control and Law Enforcement Act of 1994, up to a one-year reduction in sentence length for successful program completion among non-violent offenders.

All 20 residential substance abuse treatment programs (RDAP's) were unit-based; that is, all program participants lived together — separate from the general population — for the purpose of building a treatment community. Each unit had a capacity of approximately 100 inmates. Ordinarily, treatment was conducted on the unit for a half-day in two consecutive 2-hour sessions. The other half day, inmates participated in typical institution activities (*e.g.*, work and school), during which time, as well as during meals, treatment participants interacted with general population inmates.

All residential substance abuse treatment programs were based on a cognitive-behavioral model which attempted to identify, confront, and alter the attitudes, values, and thinking patterns that led to criminal behaviors and drug or alcohol use. Most program content was standardized through dissemination of curriculum manuals to be used in every treatment program.

This substance abuse treatment effort also consisted of two "transitional" components. One component was required only of those RDAP graduates who were not immediately released from prison after graduation. These program graduates were required to receive no less than one

hour of individual or group counseling per month for the first 12 months following treatment completion or until transfer to a halfway house or release, whichever came first. This treatment was referred to as "institution transitional services".

The second transitional component was available only to those individuals — approximately two-thirds of our subjects — who received a halfway house placement before their release from BOP custody. Individuals who received a halfway house placement spent up to the last 6 months of their sentence in a halfway house with some individuals spending some of the time under home confinement. All RDAP program graduates were required to participate in transitional services when and if they were transferred from the institution to a halfway house. This phase of treatment was referred to as "transitional services" and consisted, on average, of two hours per week of group, individual, or family counseling. In order to increase continuity of service, the BOP and the Administrative Office of the U.S. Courts signed a Memorandum of Understanding (MOU). The agreement specified that for individuals with halfway house placements, the BOP would use the same contractors, in other words "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 in a halfway house to post-release supervision by a probation officer.

Substance Abuse Treatment Services Stages

Although the focus of our evaluation was in-prison residential substance abuse treatment, we note that any inmate — including comparison subjects who did not participate in RDAP — may have received other substance abuse treatment services within the BOP as well as after release from prison, that is, during their post-release supervision period. During their incarceration, inmates could take advantage of outpatient services and self-help groups. During the post-release supervision period, whether they were treatment or comparison subjects, inmates had access to a variety of treatment services, including residential services.

Stage 1: Prison

In addition to residential treatment (described in previous section), outpatient substance abuse treatment services and self-help groups such as Alcoholics Anonymous, Narcotics Anonymous and Cocaine Anonymous, were available to all inmates. The primary purpose of the outpatient substance abuse treatment services was to provide services to those not eligible for the residential treatment programs as well as to individuals who did not volunteer for the residential programs. However, both RDAP treatment subjects as well as comparison subjects received such treatment within prison.

Stage 2: Halfway House

Transitional services, although required of all RDAP graduates, were also made available to individuals who entered but did not complete RDAP as well as to individuals who never entered RDAP. Although some transitional services participants who had not been RDAP graduates received these services voluntarily, most did not. Participation was a result either of BOP staff recommending treatment as a condition of halfway house placement or of two new program initiatives implemented in 1994. Both initiatives required halfway house participants to be screened for drug and alcohol treatment needs and then referred, if appropriate, to transitional services.

Stage 3: Post-Release Supervison

U.S. Probation offers a wide-range of treatment services for its offender population. The majority of treatment services consisted of individual and group outpatient counseling. Some individuals received inpatient, residential, or detoxification services. In general, offenders did not participate in these treatment services voluntarily. Treatment was usually imposed as a condition of supervision by a judge or the probation officer. While not all releasees were required to participate in post-release treatment, the majority of them were required to participate in urinalysis testing. Requiring post-release treatment was not contingent upon participation in the BOP's in-prison residential treatment program. Inmates leaving BOP custody for supervision by a U.S. probation officer may have remained in substance abuse treatment with the same treatment provider the BOP used during the halfway house transition program.

The complexity of the program structure and treatment options available both to treatment and comparison subjects allowed many opportunities for bias to be introduced into the treatment participation process. Before describing the analyses we used to control for selection bias, we provide a description of the research design.

Methods

Sample Selection

We identified residential treatment subjects from four female and 16 male RDAP's in institutions ranging from minimum to low and medium security. Three of the programs consisted of 1,000 hour programs and the remaining 17 consisted of 500 hour programs.

To explicitly test for selection bias, our study design required selecting two types of comparison subject groups in addition to the treated group. The first comparison group consisted of individuals with a substance use history who were housed at a residential substance abuse treatment institution (i.e., had treatment available) but did not volunteer for treatment. The second group consisted of individuals with a substance use history who came from institutions without a residential treatment program and who thus did not have the opportunity to volunteer for residential treatment.

Our sample consisted of 1,288 men and 281 women for a total sample size of 1,569. Our analyses were limited to those subjects released to supervision by a U.S. probation officer because outcome data on substance use were available only for those subjects. Thus, subjects not released to supervision and subjects with a detainer were not included in our analyses. The subjects were released between July 1992 and December 1995 with two-thirds being released between January and December 1995.

Of the 3,112 subjects initially identified in prison, 1,033 were excluded from the analyses due to a lack of background interview information deemed essential for our analyses. There were 683 subjects for whom no background data were available due to administrative reasons, 340 subjects who refused to participate and 57 subjects with lost or unusable data. The administrative reason for not collecting background data was due to insufficient staff for the frequent travel to the more than 30 institutions required to interview inmates before they were transferred to

another institution or released from prison. Logistic regression analyses demonstrated that no individual characteristics were predictive of having missed data collection or refusing research.

Table 1 shows the distribution of the various types of research subjects by gender. Of the 1,288 male subjects included in this report, 610 (47 percent) entered residential treatment. The remaining 678 (53 percent) were comparison subjects who never entered residential treatment. Of these, 382 were comparison subjects who had in-prison residential treatment available but did not volunteer, and 296 were comparison subjects who did not have treatment available. Of the 610 who entered treatment, 75 percent completed the treatment program, 5 percent voluntarily dropped out of the program, 7 percent were removed for disciplinary reasons, and 13 percent did not complete the program. This "incomplete" category, in general, comprised inmates who were unable to complete treatment due to transfers to another institution or release from BOP custody.

Of the 281 female subjects, 150 (53 percent) entered residential treatment. The remaining 131 (47 percent) were comparison subjects who never entered residential treatment. Of these, 98 were comparison subjects who had in-prison residential treatment available but did not volunteer, and 33 were comparison subjects who did not have such treatment available. Of the 150 who entered treatment, 59 percent completed the treatment program, 8 percent voluntarily dropped out of the program, 10 percent were removed for disciplinary reasons, and 23 percent did not complete the program before release from BOP custody or transfer to another prison.

Data Collection Procedures

Inmate background information was obtained from automated data files as well as from interviews and surveys. Interviews were generally administered to treatment subjects within the first 6 weeks after admission to the treatment program and to comparison subjects approximately three to nine months before release from prison. Survey data were collected from treatment subjects, on average, within the first two weeks of treatment and for comparison subjects at the same time the interviews were administered. Halfway house treatment information was obtained from automated data files. We obtained 6-month post-release information for subjects released to supervision through telephone interviews with U.S. probation officers.

<u>Predictor measures</u>

We selected a set of background and other predictor factors because past research has found them to be important predictors of recidivism, they are relevant to substance-using populations, they have been found to predict treatment outcomes, or because we considered them to be important control variables omitted in previous studies.

The background characteristics obtained from official records included gender, race, ethnicity, prior commitments (yes/no), history and recency of violence (less than or greater than 5 years before incarceration or no history), amount of time served (months), and age upon release from incarceration. Self-report information obtained through interviews included criminal justice status at time of incarceration (supervised yes/no), employment status during the month before incarceration – employed full- or part-time, not in work force, unemployed, unemployed but looking for work – , history of illegal activity for at least once year as a primary source of support, number of years of education, and frequency of previous substance use for eight drug categories – ranging from no use (1) to daily use (5). This information also included predicted values for DSM-III-R diagnoses for drug dependence and alcohol dependence (26).

In-prison treatment information, in addition to residential treatment, included both outpatient treatment and self-help group participation. Involvement in transitional services during halfway house placement was recorded for both treatment and comparison subjects. Individuals receiving post-release treatment (i.e., while under supervision by a U.S. probation officer) were categorized according to whether the treatment provided was contractual or non-contractual in nature. Post-release services were primarily outpatient in nature although some individuals received inpatient services. Post-release services also included information on involvement in self-help groups such as Alcoholics Anonymous and Narcotics Anonymous.

Because outcomes may be affected by how closely an individual is monitored after release from custody, we included several measures representing differing types and levels of supervision: halfway house placement before release, average number of urinalysis tests per month during supervised release, and placement (yes/no) in a halfway house by a probation officer during supervised release.

The post-release variables included in the analyses were: living with a spouse, percent of post-release period employed full-time (32 or more hours per week), number of violations of conditions of supervision for substance use, and number of violations of conditions of supervision for other reasons.

Outcome measures

Our measure of arrest consisted of the first occurrence of an arrest for a new offense during the first 6 months after release from custody. Our outcome measure of substance use referred to the first occurrence of evidence of substance use, either of an illicit drug or alcohol. Evidence of drug use was defined as: a positive urinalysis (u/a) test, refusal to submit to a urinalysis test, admission of drug use to the probation officer, or a positive breathalyser test. We limited the measure to the first incident of substance use because, in some probation districts, an individual is revoked after the first positive urinalysis, whereas, in others, the individual is provided with additional treatment. In addition, for obvious reasons, we limited this outcome measure to individuals who received urinalysis testing. There were 142 individuals excluded from the analyses of drug use: 134 individuals had no urinalysis testing and eight individuals had missing data. These individuals were, however, included in the analyses of arrest for a new offense.

Analyses

We conducted three types of analyses to increase confidence in our findings. We limit the presentation of findings to the results obtained using the Heckman approach (23, 24). We had the most confidence in the findings obtained from this approach because it directly addressed selection bias issues. This procedure represents one of the two alternative procedures we used to ensure that effects of treatment were not confounded with the effects of the selection pressures discussed in the introduction. The Heckman approach directly controlled for selection bias, identified whether selection bias occurred, and provided information about the nature of the selection bias. The Heckman approach explicitly modeled the effects of selection bias and tested for the impact of treatment under conditions of selection bias. In modeling the selection process, we used the non-treated comparison subjects, both those who did not have treatment available

(non-RDAP comparisons) and those who had treatment available but did not volunteer (RDAP comparisons) as "controls." By explicitly modeling the selection process, we statistically capitalized on all comparison subjects to increase the power of the treatment versus non-treatment contrast. This information on selection into treatment was then incorporated into a model which simultaneously measured treatment effects in the presence of selection bias.

We analyze the data using event history techniques where we model the length of time of non-occurrence of an event – arrest or drug use – during the risk period, the time after release from custody during which a subject is being supervised. Thus, the results reported refer to the effects of variables on the survival time, that is, time until first arrest or time until first evidence of substance use. Thus, significant positive coefficients imply that a variable increased the survival time of an offender, while significant negative coefficients imply survival time was decreased. The selection-bias adjustment was made to the survival function associated with the time until an offender was arrested and the time until the offender used drugs or alcohol. By modeling selection bias explicitly, we were able to test whether and to what extent selection bias increased or decreased survival time. If it increased survival time, this was evidence that lower-risk inmates were selected into treatment. If it decreased survival time, this suggested that higher-risk inmates were brought into treatment. If the selection-bias parameter was not significant, then we could conclude there was no selection bias.

Because there were a large number of background and control variables, we adopted a hierarchical procedure for selecting blocks of variables to include in the model for each outcome measure. This procedure tested the difference between a "base" model of core background characteristics and four additional blocks of variables. These additional blocks of variables comprised motivation for change measures, treatment indicators (other than in-prison residential treatment), level of post-release supervision indicators and post-release behaviors. A block of variables was retained in the final model only if the combined set of variables from a particular block was significantly different from the "base" model. Thus, the variables included as predictor variables differed between the two outcome measures.

We used effect vector coding for the background predictor variables that were nominal

variables. Effects coding provides a contrast between a particular group and *all* the groups in the sample (e.g., all subjects on average). This differs from dummy coding where the contrast is between a particular group and the group which serves as referent category. Thus, for example, our coefficient for women represents the likelihood of arrest or drug use as compared with the likelihood for men and women combined. In addition to the background variables, the models we present include variables representing the time period over which an inmate was released. This release cohort variable was intended to proxy any effects on outcomes that may be attributable to changes in the release environment over time. For example, economic conditions can vary over time and can have an effect on employment and recidivism.

Results

In reporting the results, we first focus on treatment effects. We report treatment coefficients significant at the .05 level using a one-tailed test since we hypothesize the direction of the relationship. Significance is tested with the standard t-statistic calculated by dividing the coefficient by the standard error. We hypothesize that individuals who received and completed in-prison drug and alcohol treatment had more favorable outcomes than those who did not receive drug and alcohol treatment. For all other coefficients, we used a two-tailed test. Our two outcome results tables—Tables 2 and 3—contain only those variables which were statistically significant. For constructs coded with the effects vector variables, all of the corresponding variables are presented if any individual component is significant.

Arrest for a New Offense

Approximately 12.5 percent of the subjects had at least one arrest for a new offense during the 6-month post-release period. The model of arrest for a new offense is presented in Table 2. Based on a graphical inspection of the data not reported here, we concluded that the time until an arrest followed a lognormal distribution wherein the probability of arrests increases with time until it reaches a maximum and then decreases. Thus, we chose the lognormal mathematical form for our survival model. The results show a positive effect of treatment on arrests. Since the treatment variable is the conditional probability of completing treatment, the results in Table 1 show that the effect of treatment was statistically significant and positive. Inmates who began and

completed treatment had longer survival times until arrest than untreated comparison subjects or subjects who began but did not complete treatment.

The variable BIASCOEF in Table 2 represents selection bias. It controls for whether subjects who volunteered for treatment were different from those who did not, all other factors included in the model being held constant. The coefficient for BIASCOEF was significant and negative, indicating that inmates who were more likely of being arrested for a new offense selected themselves into the treatment groups. Rather than "creaming" the best candidates for post-release success whether treatment was provided or not, the selection processes worked so that inmates who were less likely to succeed upon release from prison were provided with drug and alcohol treatment.

Because subjects who selected treatment were more likely to be rearrested upon release, we can conclude that the selection process operating in this study would have muted the observed effects of treatment if we had not used appropriate methods to deal with it. By measuring and controlling for this bias, we were able to detect a rather strong treatment effect upon post-release arrests.

We estimated the size of the treatment effect by computing the change in the probability of being arrested within the first 6 months depending on whether a subject was treated or not. We computed the probabilities of re-arrest for treated and untreated subjects with other variables in the model set to their respective means. Untreated subjects had a probability of .167 of being arrested in the first 6 months, while treated subjects had a probability of .031 of being arrested in the first 6 months. Thus, treated inmates were 73 percent less likely to be re-arrested than untreated inmates.

Two background variables were related to post-release arrests: age at release and prior commitments. Older individuals had a lower probability of arrest, whereas those with a prior commitment had a higher probability of arrest. Conversely, individuals with no prior commitments had a lower probability of arrest. We found several post-release factors to be related to arrest. Higher rates of urinalysis testing during supervision, a greater number of hours worked after release, and living with a spouse were all associated with a lower likelihood of arrest. Individuals

who lived alone after release had higher arrest rates. We note that all the background factors we found predictive of post-release arrests were significant in bivariate models of recidivism. It is noteworthy that these factors remained significant when we controlled for treatment effects and the other background factors.

First Detected Substance Use

Twenty-nine percent of the subjects had evidence of substance use during the 6-month post-release period. Unlike the lognormal mathematical form used for the model of arrests, the drug use data suggested an exponential form. This mathematical form indicates a risk for drug use which is constant over time. The results for post-release substance use are presented in Table 3. Once again, the results in Table 3 show a positive effect of treatment on post-release outcome. Looking at the substance use results, we found that, as was the case with the model for arrests, the coefficient which represents the conditional probability of entering and completing treatment, was significant and positive. This indicates that individuals who entered and completed treatment had a longer time until first substance use than those who did not. The variable BIASCOEF, which represents the level of selection bias, was negative. Although BIASCOEF was not statistically significant, our general experience with selection bias models has been that the power for tests of selection bias is very low. To err on the side of caution, we put considerable weight on the possibility of Type 2 error. In other words, we deemed it to be prudent to proceed as though selection bias existed.

Further reason to reject the null hypothesis of no selection bias was provided in the implied correlation. The implied correlation of -.26 for substance use was close to the value calculated for re-arrest (-.33). The correlation of the error term is calculated with the formula: correlation = 1-2/(1+exp(COVARIAN)). Thus, for both arrests and substance use, the size of the correlation indicated that the highest-risk subjects were selected into treatment. This means that individuals who completed treatment were likely to have shorter survival times, independent of treatment, 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 most likely have been attenuated.

Similar to our analysis of arrest within the first 6 months, we computed the estimated size of the treatment effect. When we measured treatment as the conditional probability of completing treatment, untreated subjects had a probability of .367 of using drugs or alcohol in the first 6 months, while treated subjects had a probability of .205 of using drugs or alcohol in the first 6 months. Thus, those who completed drug treatment were 44 percent less likely than those who had not received treatment or did not complete treatment to use drugs within the first 6 months after release.

As was the case with the results for arrests, a history of prior commitments was related to post-release substance use. Individuals with prior commitments had a higher probability of substance use. Unlike our model of arrests we found gender, race and ethnicity to be related to substance use. Women, white offenders, and non-Hispanic offenders had a lower likelihood of substance use whereas men and Hispanics had a higher likelihood of substance use. We note, however, that the coefficient for gender approached significance (p<.10) in the model of arrests.

We also found that individuals with higher levels of past marijuana use, but not other drugs, were more likely to return to post-release substance use. Furthermore, individuals without a history of violence and individuals who served longer sentences were less likely to return to post-release substance use. In addition to in-prison residential treatment, several other treatment variables were significant: in-prison outpatient treatment and post-release treatment services. Having received in-prison outpatient treatment and *not* having received post-release treatment services decreased the likelihood of substance use, whereas having received no in-prison outpatient services and having received both contract and non-contract services after release both increased the likelihood of substance use. Anecdotal information from probation officers indicated that some individuals receiving both contract and non-contract services received the non-contract services only after having had a positive urinalysis. This means that receipt of non-contract services may have represented an effect rather than a cause of detected substance use. Failing the BOP's halfway house placement increased the probability of substance use whereas successful completion of such a placement or not having had such a placement decreased the probability of substance use. Two post-release behaviors — non-drug or alcohol related supervision violations

and living alone — were related to an increased probability of substance use. Similar to our arrest results, the post-release behaviors of employment and living with a spouse were related to a lower probability of substance use. However, unlike our results for arrest, we found a higher number of non-drug related supervision violations was associated with an increased probability of drug use. As with our model of arrests, all the factors found to be significant in our multivariate model of post-release drug use were also significant in bivariate event history analyses.

Discussion

Selection bias presents a serious problem for conducting evaluation research, one that is often not recognized or adequately addressed. To increase our confidence in the BOP's drug treatment evaluation results, we attempted to address the selection bias problem by modeling the outcomes in three different ways. While all three approaches indicated similar results, we presented the results for our most efficient approach: it not only controlled for selection bias but provided information on the nature of the selection bias. We found evidence suggesting that in-prison residential drug and alcohol treatment does seem to "work." We also found evidence of selection bias: individuals who were more likely to be arrested or use drugs were selected into the treatment groups. Thus, without controlling for selection bias our positive findings would have been attenuated.

Individuals who entered and completed in-prison residential drug and alcohol treatment were less likely to experience the critical post-release outcomes of new arrests and substance use in the first 6 months following release. The positive results we found for both outcome measures occurred within a multi-site context of 20 programs serving both male and female inmates and operating within different security levels and different geographic regions. Thus, our results have greater generalizability than a study with treatment subjects from one or two treatment programs.

While our findings suggest the utility of replicating the BOP's cognitive-behavioral programs in other prison settings, we point out that we cannot readily identify which component(s) — criminal lifestyle, relapse prevention, cognitive skills, etc. — were most crucial. On the other hand, we must also consider that *all* components may be crucial and additive in their effects, and that the elimination of any one component may decrease the effectiveness of the

16

programs.

The control variables found to be significant point to background factors which reduce or increase failure when controlling for treatment exposure. All of the control variables which were significant in our multivariate analyses were also significant on a bivariate basis. Although the focus of our analyses was upon detecting treatment effects, the background factors which remain significant when controlling for treatment effects and other background characteristics point to other important predictors of post-release outcomes. Such factors suggest the need to understand how treatment effects might be dampened or facilitated and the need, therefore, for treatment providers to pay greater attention to individuals of specific backgrounds. For example, individuals with prior commitments were more likely to have post-release arrests and substance use. This finding, which is consistent with the literature on recidivism, suggests that treatment programs might want to pay greater attention to or develop specialized treatment protocols for these individuals who are inherently at a higher risk of failure. Results also indicated that being employed full-time a greater percentage of the post-release time period was related to a reduced likelihood of both arrests and substance use. This finding suggests that treatment effectiveness might be enhanced by including training which enables individuals to acquire or increase employment skills. In addition, supervising probation officers may want to emphasize skills which enable individuals not only to find employment but maintain employment.

During the first 6 months after release the percentage of individuals who had evidence of substance use was greater than that for arrest (29% versus 12.5% percent, respectively). Our models of arrests showed that substance use increased the likelihood of an arrest. In addition, the mathematical function which best represented the survival distribution differed between substance use and arrest. The exponential function for drug use presumes that the survival rate was constant over time whereas the lognormal function for arrests presumes that arrests increased to a maximum value and then decreased. Other analyses not reported here indicate that drug use was highest within the first two months and then began to decrease somewhat whereas the likelihood of an arrest increased during each of the 6 months. Consideration of this difference in survival curves suggests that treatment providers focus on targeting substance use behaviors *immediately*

after release. Placing a priority on targeting substance use in the early weeks of release may further help reduce future criminal behavior. This will require ensuring that the referral and admission to treatment occurs immediately after release. In addition, the first priority of probation officers and treatment providers may need to be upon identifying how to avoid the high-risk situations for drug use and on finding alternative coping mechanisms to resist the temptation to use drugs during the first two months after release.

While our results show promising outcomes for in-prison treatment, we must continue to follow the research subjects over a longer period of time to determine whether the initial positive findings are maintained. Due to sample size limitations, our preliminary analyses did not allow for separate analyses by gender. Future research is necessary to determine whether and how the programs differentially affect men and women. In addition, future research which examines the interaction effects between variables such as treatment and level of drug addiction is necessary to help treatment providers identify the types of individuals who may require increased treatment intensity or additional services.   Identifying the extent to which differences in program quality — e.g., program maturity, staff qualifications — may affect post-release outcomes is yet another goal of future research.

Proximal outcomes, those outcomes which represent the intervening mechanism through which the treatment program affects the outcomes of interest such as arrests and drug use, were not included in the reported analyses. Examining proximal outcomes facilitates causal inferences about program effects because it tests the mechanisms that are associated with program outcomes (27). This approach is referred to as theory-driven evaluation (28). Our data collection incorporated several pre- and post-treatment measures of proximal outcomes which are indicative of the problem areas addressed through relapse prevention theory. These measures included The Ways of Coping scale (29) and the Drug-Taking Confidence Questionnaire (30). Our future examination of proximal outcomes will serve two purposes. First of all, it will allow us to assess the extent to which the population served had the purported deficiencies and the extent to which these deficiencies were remedied. This will help us understand how the treatment programs work. Secondly, it may allow us to strengthen the causal link between treatment and the distal outcomes

if a strong association is found between treatment and proximal outcomes as well as between proximal outcomes and distal outcomes (31-34).

Identification of what type of program is most cost-effective requires future research to address questions related to treatment intensity. As noted in reviews of drug treatment evaluation studies, length of treatment has generally been shown to be associated with positive outcomes (31, 32). Yet, most programs offered are of a single duration and intensity, and studies comparing different planned treatment intensity are limited. There is little extant research which examines the effects of differential treatment intensity. Nemes, Wish & Messina (35) conducted a study of cocaine abusers comparing individuals randomly assigned to two treatment programs which were of similar duration, 12 months, but which differed in intensity. They found that type of treatment program — that is, treatment intensity — was not as important as treatment completion for post-release drug use and criminal activity. The only effect of treatment intensity was found for post-release employment. The data available for the analyses we reported did not have sufficient power to detect differences between the high intensity and moderate intensity programs. Such analyses will be conducted in the future. Although such analyses will not definitively answer the question about the most cost-effective treatment intensity, if we were to find that the moderate intensity programs were as successful as the high intensity programs, we would know that the more costly high intensity program is not necessary.

Before policymakers continue to implement costly in-prison residential drug treatment programs, further research is needed to determine whether providing intensive treatment is more cost-effective when provided during a halfway house placement or while under supervision. Inciardi, Martin, Butzin, Hooper, & Harrison (16) found that individuals who participated in a therapeutic community work-release program (e.g., during a halfway house placement) did better than individuals who participated in an in-prison therapeutic community without aftercare during their halfway house placement. In addition, those who participated in both in-prison and post-release therapeutic communities were more successful than those who participated only after release. Nonetheless, the difference between these latter two groups was not as pronounced as the difference between those who received treatment while incarcerated and those who received

treatment while in a halfway house. These results indicate that research which assesses whether the more intensive phase of treatment is better provided in a prison environment or a halfway house environment would provide information useful to policymakers. Furthermore, future research should also consider addressing the following question: is in-prison treatment best conceived of as preparing individuals for intensive aftercare treatment, and, if yes, what level of treatment is necessary to prepare individuals for aftercare?

Table 1

Type of Subject by Gender

	Male		Female	
	Number	Percent	Number	Percent
Type of Subject				
Treatment	610	47%	150	53%
Comparison	678	53%	131	47%
Treatment available	382	30%	98	35%
Treatment not available	296	23%	33	12%
Total	1,288	100%	281	100%
Treatment				
Program graduate	458	75%	89	59%
Drop-out	30	5%	12	8%
Disciplinary discharge	43	7%	15	10%
Other reason incomplete	79	13%	34	23%
Total	610	100%	150	100%

Table 2

Arrests for Supervised Subjects

Variable	b	se(b)
Background Variables		
INTERCEPT	5.4181***	0.6098
COMPLETED TREATMENT	1.2406***	0.3377
PRIOR COMMITMENTS	-0.4426***	0.0988
NO PRIOR COMMITMENTS	0.4426***	0.0988
AGE AT RELEASE	0.0485***	0.0107
Supervision Variables		
FREQUENCY OF URINE TESTING/AVG. PER MONTH	0.0911**	0.0399
Post-Release Variables		
LIVED WITH SPOUSE	0.5493***	0.1626
LIVED WITH COMMON-LAW SPOUSE	-0.1436	0.1488
LIVED WITHOUT SPOUSE	-0.4057***	0.1126
% TIME WORKED FULL-TIME (32+ HRS./WEEK)	0.0044***	0.0018
Selection Bias Variables		
BIASCOEF	-0.7738***	0.2555
SIGMA (STANDARD ERROR)	1.6808***	0.1046

* p < .10   ** p < .05   ***p < .01

Table 3

First Evidence of Substance Use, Supervised Subjects with Urine Testing

Variable	b	se(b)
Background Variables		
INTERCEPT	6.0021***	0.5711
COMPLETED TREATMENT	0.6829**	0.3157
BLACK	-0.1604	0.1485
OTHER RACE	-0.3313	0.2500
WHITE	0.4917***	0.1439
HISPANIC	-0.2704**	0.1190
NON-HISPANIC	0.2704**	0.1190
FEMALE	0.3390***	0.0989
MALE	-0.3390***	0.0989
PRIOR COMMITMENTS	-0.1956***	0.0794
NO PRIOR COMMITMENTS		0.1956***
	0.0794	
RECENT VIOLENT HISTORY (< 5 YEARS AGO)	-0.2066*	0.1139
PAST VIOLENT HISTORY (5 + YEARS AGO)	-0.0236	0.1003
NO VIOLENT HISTORY	0.2302***	0.0910
AMOUNT OF TIME SERVED (MONTHS)	0.0682**	0.0343
HIGHEST FREQUENCY OF MARIJUANA USE	-0.0769**	0.0380
Treatment Variables		
RECEIVED IN-PRISON OUTPATIENT TREATMENT	0.2905**	0.1420
NO IN-PRISON OUTPATIENT TREATMENT	-0.2905**	0.1420
POST-RELEASE CONTRACT SERVICES ONLY	0.3103	0.1772
POST-RELEASE NON-CONTRACT SERVICES ONLY	0.2322	0.2859
BOTH CONTRACT AND NON-CONTRACT SERVICES	-0.8881***	0.2507

Table 3

First Evidence of Substance Use, Supervised Subjects with Urine Testing

Variable	b	se(b)
NO-POST RELEASE TREATMENT	1.4578***	0.2022
POST-RELEASE TREATMENT INFORMATION MISSING	-1.1132	0.5817
Supervision Variables		
NO HALFWAY HOUSE PLACEMENT	0.2735***	0.1138
FAILED HALFWAY HOUSE PLACEMENT -	0.5684***	0.1095
COMPLETED HALFWAY HOUSE PLACEMENT	0.2949***	0.0912
Post-Release Variables		
LIVED WITH SPOUSE	0.2684**	0.1182
LIVED WITH COMMON-LAW SPOUSE	0.0064	0.1233
LIVED WITHOUT SPOUSE	-0.2748***	0.0901
# OF SUPERVISION VIOLATIONS (NON-DRUG)	-0.2421***	0.0400
% TIME WORKED FULL-TIME (32+ HRS./WEEK)	0.0065***	0.0015
Selection Bias Variables		
BIASCOEF	-0.5354	0.4060
SIGMA (STANDARD ERROR)	0.8999***	0.1323

* p < .10   ** p < .05   *** p< .01

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