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

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[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 —

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[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.