PRISON-BASED CHEMICAL DEPENDENCY TREATMENT IN MINNESOTA:
AN OUTCOME EVALUATION

MINNESOTA DEPARTMENT OF CORRECTIONS

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March 2010

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INTRODUCTION

The impact of substance use on the criminal justice system is substantial. Research has long shown, for example, that alcohol and/or illicit drugs figure prominently in criminal offending. In Marvin Wolfgang's landmark study on homicide in Philadelphia during the 1950s, he reported that alcohol was consumed by either the victim or the offender in approximately two-thirds of the cases (Wolfgang, 1958). In a survey of nearly 7,000 jail inmates, Karberg and James (2005) found that 33 percent reported being under the influence of alcohol at the time of the offense. And, in a recent study of 224 Minnesota sex offenders who recidivated with a sex crime, either the victim or the offender had used alcohol and/or drugs at the time of the offense in at least 31 percent of the assaults (Duwe, Donnay, and Tewksbury, 2008; Minnesota Department of Corrections, 2007c).

Among state and federal prisoners incarcerated in 2004, Mumola and Karberg (2006) reported that 32 percent committed their offenses under the influence of drugs, and 56 percent had used drugs in the month preceding the offense. The highest percentages of drug use were found for drug offenders, followed closely by those incarcerated for property offenses. For example, 44 percent of drug offenders and 39 percent of property offenders indicated using drugs at the time of the offense. Moreover, the rate of drug use in the month prior to the offense was 72 percent for drug offenders and 64 percent for property offenders.

The use and abuse of substances is linked not only to involvement in criminal activity, but also to the growth of the prison population, particularly over the last few decades. Due in part to increased penalties resulting from the "war on drugs," the federal and state prison population has more than doubled in size over the last 20 years (Beck and Gilliard, 1995; Sabol, Couture, and Harrison, 2007). Drug offenses, moreover, accounted for 53 percent of all federal prisoners in 2006 and 20 percent of state inmates in 2005 (Harrison and Beck, 2006; Sabol, Couture, and Harrison, 2007). Within Minnesota, the percentage of drug offenders in the total inmate population grew from 4 percent in 1989 to 20 percent in 2008 (Minnesota Department of Corrections, 2007b; Minnesota Department of Corrections, 2008). The percentage of drug offenders, however, represents only a fraction of those who are in need of chemical dependency (CD) treatment. Indeed, approximately 85 percent of the offenders entering Minnesota state prisons during 2006 were determined to be chemically abusive or dependent (Minnesota Department of Corrections, 2007a).

Given the relatively high rate of substance abuse and dependency among incarcerated offenders, efforts to reduce their risk of reoffense often include the provision of prison-based CD treatment. Previous evaluations of prison-based CD treatment have concentrated mainly on programs based on the therapeutic community (TC) model. Originating in England during the late 1940s, the TC model regards CD as a symptom of an individual's problems rather than the problem itself (Patenaude and Laufersweiller-Dwyer, 2002). Viewing substance abuse as a disorder that affects the whole person, the TC model attempts to promote comprehensive pro-social changes by encouraging participants to contribute to their own therapy, as well as that of others, through activities such as therapy, work, education classes, and recreation (Klebe and O'Keefe, 2004). Individual and group counseling, encounter groups, peer pressure, role models, and a system of incentives and sanctions often comprise the core of treatment interventions within a TC program (Welsh, 2002). Moreover, to foster a greater sense of community, participants within a prison setting are housed separately from the rest of the prison population.

Previous studies have evaluated prison-based TC programs for federal prisoners (Pelissier et al., 2001) as well as for state prisoners in California (Prendergast, Hall, Wexler, Melnick, and Cao, 2004; Wexler, Melnick, Lowe, and Peters, 1999), Delaware (Inciardi, Martin, Butzin, Hooper, and Harrison, 1997; Inciardi, Martin, and Butzin, 2004), New York (Wexler, Falkin, and Lipton, 1990), Oregon (Field, 1985), Pennsylvania (Welsh, 2007) and Texas (Knight, Simpson, Chatham, and Camacho, 1997; Knight, Simpson, and Hiller, 1999). In general, the findings from these studies suggest that prison-based treatment can be effective in reducing recidivism and relapse. Indeed, in the most recent meta-analysis of the incarceration-based drug treatment literature, Mitchell, Wilson, and MacKenzie (2007) found that treatment significantly decreased subsequent criminal offending and drug use in their review of 66 evaluations. The average treatment effect sizes for recidivism and drug use were odds ratios of 1.37 and 1.28, respectively (Mitchell et al., 2007).

The most promising outcome results have been found for offenders who complete prison-based TC programs, especially those who participate in post-release aftercare (Inciardi, Martin, and Butzin, 2004; Mitchell et al., 2007; Pearson and Lipton, 1999). In addition, Wexler, Falkin, and Lipton (1990) reported that treatment effectiveness is related to the length of time an individual remains in treatment, but only up to a point. As time in the TC program increased, so, too, did the time until rearrest. Time to rearrest was shorter, however, for offenders who had been in the TC program longer than 12 months.

Despite the positive findings from prior outcome evaluations, most of these studies have been limited in one or more ways. Welsh (2002) notes, for example, that previous evaluations have had small sample sizes, faulty research designs, and devoted too little attention to interactions between inmate characteristics, treatment processes, and treatment outcomes. Moreover, Pelissier and colleagues (2001) identified selection bias as the most significant shortcoming of prior studies on prison-based CD treatment. In evaluations of treatment effectiveness, selection bias refers to differences—both observable and unobservable—between the treated and untreated groups that make it difficult to determine whether the observed effects are due to the treatment itself or to the different group compositions. Therefore, although previous evaluations have found that recidivism rates are generally lower for offenders who participate in treatment, this difference may not necessarily be due to the treatment itself but, rather, to other differences between treated and untreated offenders.

In their evaluation of the Federal Bureau of Prison's Drug Abuse Treatment Program, Pelissier and colleagues (2001) used two methods—the instrumental variable approach and the Heckman selection bias model—to control for selection bias.[1] After doing so, Pelissier et al. (2001) still found that, within three years of release, 31 percent of treated male offenders had been rearrested in comparison to 38 percent of the untreated male offenders, which amounted to a recidivism reduction of 19 percent. Although treated female offenders were not significantly less likely to recidivate than untreated female offenders, they were 18 percent less likely to use drugs in the 36 months following release from prison. Treated male offenders, meanwhile, were 15 percent less likely to have post-release drug use than untreated male offenders.

[1] The instrumental variable approach involves locating a variable that is related to selection into treatment but is unrelated to the outcome variable. The variance from the instrumental variable is then used to estimate the impact of treatment on the outcome measure. The Heckman method, on the other hand, requires that the selection pressures be jointly modeled into the sample and post-release outcome (Pelissier et al., 2001).

PRESENT STUDY

Using a retrospective quasi-experimental design, this study evaluates the effectiveness of CD treatment provided within the Minnesota Department of Corrections (DOC) by comparing recidivism outcomes between treated and untreated offenders released from prison in 2005. As discussed later in more detail, propensity score matching (PSM) was used to individually match the untreated offenders with those who received CD treatment. Similar to the instrumental variable and Heckman approaches used by Pelissier and colleagues (2001), PSM is a method designed to control for selection bias. More specifically, PSM minimizes the threat of selection bias by creating a comparison group whose probability of entering treatment was similar to that of the treatment group. Although PSM has been used in at least one recent study on community-based CD treatment (Krebs, Strom, Koetse, and Lattimore, 2008), this study is one of the first to use it in a prison-based treatment evaluation.

In addition to PSM, this study attempts to further control for rival causal factors by analyzing the data with Cox regression, which is widely regarded as the most appropriate multivariate statistical technique for recidivism analyses. Moreover, by comparing 926 treated offenders with a matched group of 926 untreated offenders, the sample size used for this study (N = 1,852) is one of the larger prison-based CD treatment studies to date. Finally, to achieve a more complete understanding of the effects of prison-based treatment, multiple treatment and recidivism measures were used.

Despite these strengths, there are several limitations worth noting. First, in measuring the effectiveness of CD treatment, the two most common outcome measures are substance abstention and criminal recidivism. Although abstention is an important and arguably more sensitive measure of CD treatment effectiveness, data on post-release substance use were not available for this study. Therefore, in focusing exclusively on recidivism, this study may not fully capture whether CD programming is effective. Second, in providing a continuum of care from the institution to the community, aftercare programming is often considered a critical component to effective CD treatment. Data on post-release aftercare programming, however, were not available on the offenders examined here. As a result, the differences observed between the treatment and comparison groups (or lack thereof) may be attributable, in part, to differences in the extent to which offenders participated in aftercare programming while in the community.

These limitations notwithstanding, this study attempts to address several questions central to the substance abuse treatment literature. First, does treatment reduce offender recidivism? Second, what effect does treatment outcome (i.e., drop out or complete) have on reoffending? Finally, what impact does program duration have on recidivism?

In the following section, this study describes the provision of CD treatment within the DOC. After discussing the data and methods used in this study, the results from the statistical analyses are presented. This study concludes by discussing the implications of the findings for the prison-based treatment literature.

CD TREATMENT IN THE DOC

Shortly after their admission to prison in Minnesota, offenders undergo a brief (20-40 minutes) CD assessment conducted by a licensed assessor. Of the newly-admitted offenders who receive a CD assessment, approximately 85 percent are directed to enter CD treatment because they are determined to be chemically abusive or dependent. In making CD diagnoses, which are based on both self-report and collateral information, CD assessors utilize DSM-

IV criteria for substance abuse. Among the criteria for abuse are problems at work or school, not taking care of personal responsibilities, financial problems, engaging in dangerous behavior while intoxicated, legal problems, problems at home or in relationships, and continued use despite experiencing problems. The criteria for dependence, meanwhile, include increased tolerance; withdrawal symptoms; greater use than intended over a relatively long period of time, inability to cut down or quit; a lot of time spent acquiring, using, or recovering from use; missing important family, work, or social activities; and knowledge that continued use would exacerbate a serious medical or psychological condition. Although the vast majority of newly-admitted offenders are considered to be CD abusive or dependent, not all treatment-directed offenders have the opportunity to participate in prison-based treatment since the number of treatment-directed offenders (nearly 3,000 annually) exceeds the number of treatment beds available (about 1,800 annually).

The DOC currently uses information relating to offender needs and recidivism risk in prioritizing inmates for treatment. This information, however, was not routinely considered from 2002-2005, the period of time covered in this study. Rather, among offenders directed to treatment, prioritization decisions were based primarily on the amount of time remaining to serve. Offenders with shorter lengths of time until their release from prison were often selected over those with more time to serve.

During the 2002-2005 period, the DOC provided CD programming to both male and female offenders in six of the ten state facilities that house adult inmates. Although there are variations among the different programs provided at each facility, all of the CD treatment offered by the DOC is modeled on TC concepts. Housed separately from the rest of the prison population, offenders admitted to treatment were involved in 15-25 hours of programming per week. The CD programs, which maintained a staff-to-inmate ratio of 1:15, emphasized each offender's personal responsibility for identifying and acknowledging criminal and addictive thinking and behavior. Moreover, the CD programming generally included educational material that addressed the signs and symptoms of CD, the effects of drug use on the body, the effects of chemical use on family and relationships, and the dangers of drug abuse. In addition to completing an autobiography that focused on prior chemical use, program participants completed work relating to relapse prevention.

The DOC offered short-term (90 days), medium-term (180 days), and long-term (365 days) CD programming during the 2002-2005 period. The short-term programs, which were primarily psycho-educational with minimal individual counseling, emphasized the relationship between substance abuse issues and criminal behavior. Participants in these programs were expected to increase their level of active participation as they progressed through the program. The medium- and long-term programs, on the other hand, included education, individual counseling, and group counseling components. Therefore, aside from program duration, the main distinction between the short-term programs and the medium- and long-term programs was that the former contained little emphasis on individual or group counseling, primarily due to the relatively short period of time over which to deliver the programming.

In 2006, the DOC refocused its CD programs to long-term treatment of at least six months or more. The decision to discontinue the short-term programming was due, in part, to evidence which seemed to suggest that short-term programs are not as effective as ones that are longer in duration (Minnesota Office of the Legislative Auditor, 2006). More specifically, in its report on substance abuse treatment across the state, the Minnesota Office of the

Legislative Auditor found that recidivism rates for short-term program participants were higher than those for offenders who participated in medium- and long-term programs. However, the simple bivariate analyses performed by the Minnesota Office of the Legislative Auditor did not control for factors known to affect recidivism (e.g., criminal history, age at release, institutional disciplinary history, type of offense, etc.). Therefore, rather than demonstrating that short-term treatment is less effective, the higher recidivism rates for short-term participants may simply reflect that they had, in comparison to the medium- and long-term participants, a greater risk of reoffense prior to entering treatment.

DATA AND METHODOLOGY

This study uses a retrospective quasi-experimental design to determine whether CD programming has an impact on recidivism. More specifically, the effectiveness of CD treatment was evaluated by comparing recidivism outcomes between treated offenders and a matched comparison group of untreated offenders who were released from prison in 2005. To ensure that offenders in the comparison group were similar to those in the treatment group, the population for this study consisted only of inmates who received a positive CD assessment (i.e., they were determined to be chemically abusive or dependent) and were directed to enter CD treatment prior to their release from prison. In addition, because valid and reliable CD treatment data were not available prior to 2002, the population from which the treatment and comparison groups were drawn includes only offenders who were admitted to prison after December 31, 2001. As a result, this study does not include offenders with longer sentences who were directed to CD treatment.[2] Still, the study captured the vast majority of offenders released in 2005 who were directed to CD treatment given that only 8 percent of the releasees from 2005 were admitted to prison prior to 2002.

Overall, there were 3,499 offenders directed to CD treatment who were admitted to prison after 2001 and released during 2005. Of these 3,499 offenders, there were 1,164 who participated in CD treatment while in prison. Of the remaining 2,335 offenders, there were 35 who refused to enter CD treatment. Because the 35 treatment refusers did not participate in treatment, these offenders were removed from the study so as not to bias the results from the statistical analyses. Before doing so, however, an attempt was made to remove an additional source of bias by using PSM to identify a comparison group of offenders from the pool of untreated offenders (N = 2,300) who were not offered treatment, often due to a lack of available treatment beds. The procedures used to address potential bias resulting from treatment refusers are discussed later in this section.

Dependent Variable

Recidivism, the dependent variable in this study, was defined as a 1) rearrest, 2) felony reconviction or 3) reincarceration for a new sentence. Recidivism data were collected on offenders through December 31, 2008. Considering that offenders from both the treatment and comparison groups were released during 2005, the follow-up time for the offenders examined in this study ranged from 36-48 months. Data on arrests and convictions were obtained electronically from the Minnesota Bureau of Criminal Apprehension. Reincarceration data were derived from the Correctional Operations Management System (COMS)

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[2] In Minnesota, the sentences for offenders committed to the commissioner of corrections consist of two parts: a minimum prison term equal to two-thirds of the total executed sentence, and a supervised release term equal to the remaining one-third.

database maintained by the DOC. The main limitation with using these data is that they measure only arrests, convictions or incarcerations that took place in Minnesota. As a result, the findings presented later likely underestimate the true recidivism rates for the offenders examined here.

To accurately measure the total amount of time offenders were actually at risk to reoffend (i.e., "street time"), it was necessary to account for supervised release revocations in the recidivism analyses by deducting the amount of time they spent in prison from the time of release to the end of the observation period or to the first recidivism event, whichever came first. Failure to deduct time spent in prison as a supervised release violator would artificially increase the length of the at-risk periods for these offenders. Therefore, the time that an offender spent in prison as a supervised release violator was subtracted from his/her at-risk period, but only if it preceded a rearrest, a reconviction, a reincarceration for a new offense, or if the offender did not recidivate (i.e., no rearrest, reconviction, or reincarceration for a new offense) prior to January 1, 2009.

Treatment Variables

Given that the central purpose of this study is to determine whether CD programming has an impact on recidivism, CD treatment is the principal variable of interest. In an effort to achieve a more complete understanding of its potential impact on recidivism, six different treatment measures were used in this study.

The first CD treatment variable compares offenders who entered CD treatment with a comparison group of similar offenders who did not. As such, CD treatment was measured as "1" for offenders who participated in treatment between the time of admission (after 2001) and release (2005) from prison. Offenders who did not participate in CD treatment (the comparison group) were given a value of "0."

Two measures were used to assess the impact of treatment outcome on reoffending. The variable, treatment completer, compares offenders who completed treatment or successfully participated until release (1) with untreated offenders (0). The treatment dropout variable, on the other hand, compares offenders who quit or were terminated from treatment (1) with untreated offenders (0).

Three measures were created to assess the effects of program duration. As noted above, during the 2002-2005 period, the DOC had short-, medium-, and long-term CD treatment programs. The variable, short-term program compares short-term participants (1) with untreated offenders (0). The medium-term program variable contrasts medium-term participants (1) with untreated offenders (0), whereas the long-term program variable is dichotomized as long-term participants (1) or as untreated offenders (0).

Independent Variables

The independent, or control, variables included in the statistical models were those that were not only available in the COMS database but also might theoretically have an impact on whether an offender recidivates. These variables cover the salient factors that are either known or hypothesized to have an impact on recidivism. The following lists these variables and describes how they were created:

Offender Sex: dichotomized as male (1) or female (0).

Offender Race: dichotomized as minority (1) or white (0).

Age at Release: the age of the offender in years at the time of release based on the date of birth and release date.

Prior Felony Conviction: the number of prior felony convictions, excluding the conviction(s) that resulted in the offender's incarceration.

Metro Area: a rough proxy of urban and rural Minnesota, this variable measures an offender's county of commitment, dichotomizing it into either metro area (1) or Greater Minnesota (0). The seven counties in the Minneapolis/St. Paul metropolitan area include Anoka, Carver, Dakota, Hennepin, Ramsey, Scott, and Washington. The remaining 80 counties were coded as non-metro area or Greater Minnesota counties.

Offense Type: five dichotomous dummy variables were created to quantify offense type; i.e., the governing offense at the time of release.[3] The five variables were person offense (1 = person offense, 0 = non-person offense); property offense (1 = property offense, 0 = non-property offense); drug offense (1 = drug offense, 0 = non-drug offense); felony driving while intoxicated (DWI) offense (1 = DWI offense, 0 = non-DWI offense); and other offense (1 = other offense, 0 = non-other offense). The other offense variable serves as the reference in the statistical analyses.

Length of Stay (LOS): the number of months between prison admission and release dates.

Institutional Discipline: the number of discipline convictions received during the term of imprisonment prior to release.

Dependency Assessment: dichotomized as either (1) chemically dependent or (0) chemically abusive for offenders who received positive CD assessments at intake.

Length of Post-Release Supervision: the number of months between an offender's first release date and the end of post-release supervision; i.e., the sentence expiration or conditional release date, the greater of the two.

Type of Post-Release Supervision: four dichotomous dummy variables were initially created to measure the level of post-release supervision to which offenders were released. The four variables were intensive supervised release (ISR) (1 = ISR, 0 = non-ISR); supervised release (SR) (1 = SR, 0 = non-SR); work release (1 = work release, 0 = non-work release); and discharge (1 = discharge or no supervision, 0 = released to supervision). Discharge is the variable that serves as the reference in the statistical analyses.

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[3] The "governing offense" is the crime carrying the sentence on which an offender's scheduled release date is based. Although offenders may be imprisoned for multiple offenses, each with its own sentence, the governing offense is generally the most serious crime for which an offender is incarcerated.

Supervised Release Revocations (SRRs): the number of times during an offender's sentence that s/he returned to prison as a supervised release violator.

PROPENSITY SCORE MATCHING (PSM)

PSM is a method that estimates the conditional probability of selection to a particular treatment or group given a vector of observed covariates (Rosenbaum & Rubin, 1984). The predicted probability of selection, or propensity score, is typically generated by estimating a logistic regression model in which selection (0 = no selection; 1 = selection) is the dependent variable while the predictor variables consist of those that theoretically have an impact on the selection process. Once estimated, the propensity scores are then used to match individuals who entered treatment with those who did not. Thus, one of the main advantages with using PSM is that it can simultaneously "balance" multiple covariates on the basis of a single composite score. Although there are a number of different matching methods available, this study used a "greedy" matching procedure that utilized a without replacement method in which treated offenders were matched to untreated offenders who had the closest propensity score (i.e., "nearest neighbor") within a caliper (i.e., range of propensity scores) of 0.10.[4]

In matching untreated offenders with treated offenders on the conditional probability of entering treatment, PSM reduces selection bias by creating a counterfactual estimate of what would have happened to the treated offenders had they not participated in treatment. PSM has several limitations, however, that are worth noting. First, in order to produce unbiased treatment effect estimates, the selection model must contain all of the variables related to the selection process and the outcome variable, and these variables must be measured without error (Berk, 2003). Consequently, because propensity scores are based on observed

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[4] The greedy procedure is a matching algorithm that generates fixed matches. In contrast, optimal matching algorithms produce matches after reconsidering all previously made matches.

covariates, PSM is not robust against “hidden bias” from unmeasured variables that are associated with both the assignment to treatment and the outcome variable. Second, there must be substantial overlap among propensity scores between the two groups in order for PSM to be effective (Shadish, Cook & Campbell, 2002); otherwise, the matching process will yield incomplete or inexact matches. Finally, as Rubin (1997) points out, PSM tends to work best with large samples.

Although somewhat limited by the data available, an attempt was made to address potential concerns over unobserved bias by including as many theoretically-relevant covariates (17) as possible in the propensity score models. More important, however, Rosenbaum bounds sensitivity analyses were conducted to evaluate the extent to which the treatment effects obtained are robust to the possibility of hidden bias. In addition, this study later demonstrates that there was substantial overlap in propensity scores between the treated and untreated offenders. Further, the sample size limitation was addressed by assembling a relatively large number of cases (N = 3,394) on which to conduct the propensity score analyses.

Matching Treatment Refusers and Non-Refusers

In an effort to minimize the bias resulting from treatment refusers, an attempt was made to identify a comparison group of untreated offenders who were not offered treatment in order to remove these offenders from the comparison group pool. Propensity scores were computed for the 35 treatment refusers and the 2,300 untreated offenders by estimating a logistic regression model in which the dependent variable was refusal of treatment (i.e., the 35 treatment refusers were assigned a value of “1”, while the 2,300 untreated offenders in the comparison group pool received a value of "0"). The predictors were the 17 control variables described earlier. After obtaining propensity scores on the 2,335 offenders, a greedy matching procedure was used to match 35 untreated offenders not offered treatment with the 35 treatment refusers.

Of the 1,199 offenders who received a treatment offer, there were 35 who refused, resulting in a refusal rate of three percent.[5] If a similar refusal rate is assumed among the 2,300 offenders not offered treatment, then approximately 70 of the untreated offenders would have refused a treatment offer. As a result, it was necessary to remove an additional 35 untreated offenders who were not offered treatment. Accordingly, after removing the 35 untreated offenders who were matched to the treatment refusers, a second logistic regression model was estimated to generate propensity scores on the 35 offenders who refused treatment and the remaining 2,265 who did not receive a treatment offer. A greedy matching procedure was then used, once again, to match 35 untreated offenders without a treatment offer with the 35 treatment refusers. Along with the 35 treatment refusers, the 70 matched offenders not offered treatment were removed from the remaining analyses. In doing so, the number of untreated offenders in the comparison group pool was reduced by 105 from 2,335 to 2,230.

Matching Treated and Untreated Offenders

Similar to the approach described above with treatment refusers, propensity scores were calculated for the 1,164 treated offenders and the 2,230 untreated offenders by estimating a logistic regression model in which the dependent variable was participation in prison-based treatment (i.e., the 1,164 group offenders were assigned a value of "1", while the 2,230

[5] The 1,199 offenders include the 1,164 who participated in treatment and the 35 who refused to enter treatment.

Table 1. Logistic Regression Model for Assignment to Treatment

Predictors	*Coefficient*	*Standard Error*
Male	-0.315*	0.134
Minority	-0.288**	0.085
Age at Release (years)	-0.002	0.005
Metro	0.003	0.084
Prior Felonies	-0.023	0.013
Offense Type		
Person Offenders	-0.027	0.138
Property Offenders	0.027	0.139
Drug Offenders	-0.008	0.136
DWI Offenders	2.051**	0.338
Assessed as Dependent	0.535**	0.081
Institutional Discipline	-0.046**	0.012
Length of Stay (months)	0.056**	0.004
Length of Supervision (months)	-0.013**	0.003
Supervision Type		
ISR	1.542**	0.253
Supervised Release	2.143**	0.236
Work Release	1.814**	0.260
SR Revocations	0.056	0.062
Constant	-2.795	0.330
N	3,394	
Log-likelihood	3805.104	
Nagelkerke R^2	0.210	

** $p < .01$
* $p < .05$

offenders in the comparison group pool received a value of "0"). The predictors were the 17 control variables used in the statistical analyses (see Table 1). As shown in Figure 1, there was substantial overlap in propensity scores between the treated and untreated offenders, even though the difference in mean propensity score was statistically significant at the .01 level (see Table 2).

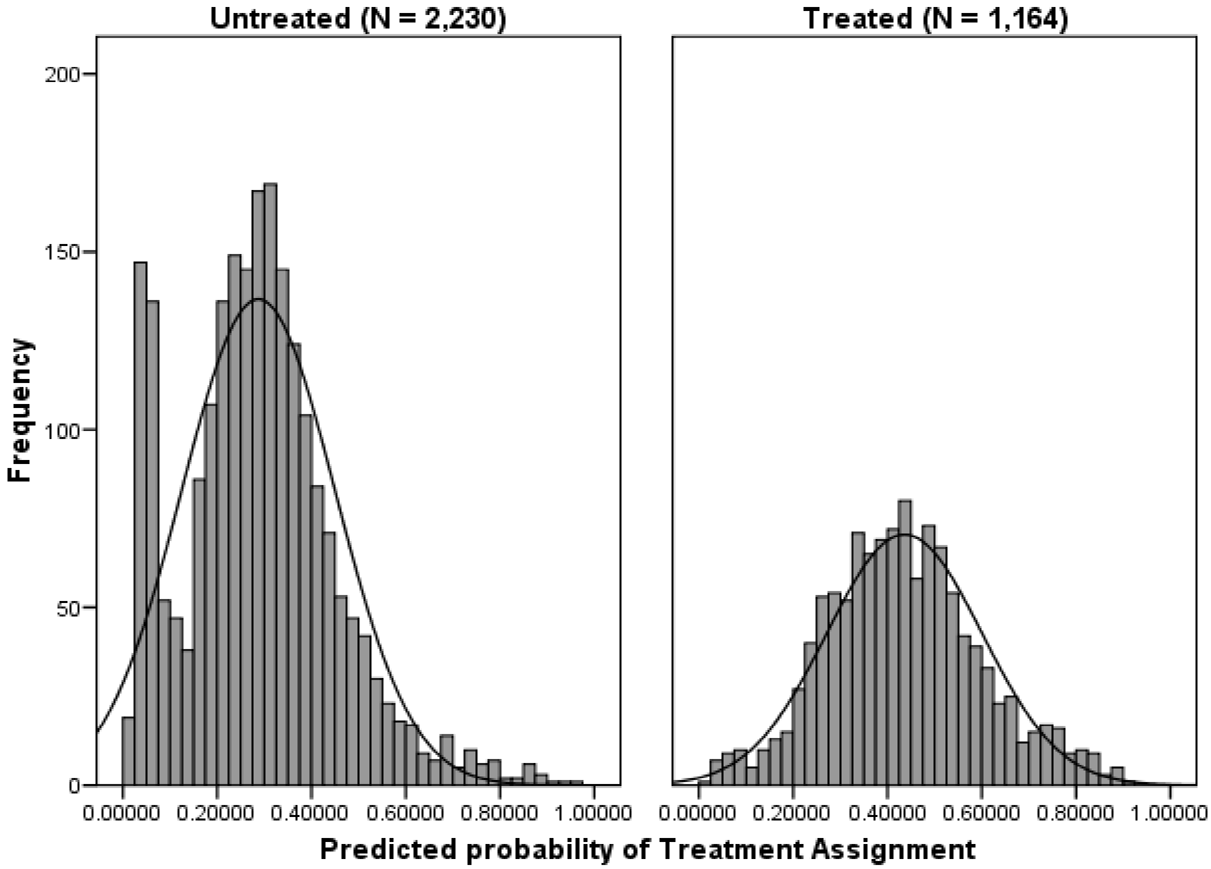

Figure 1. Distribution of Propensity Scores by Treatment Assignment

After obtaining propensity scores for the 3,394 offenders, a greedy matching procedure was used to match the untreated offenders with the treated offenders. Because the matching process is often a trade-off between the size of the bias reduction and the proportion of cases that can be matched (DiPrete & Gangl, 2004), matches were not obtained for all of the treated offenders. However, in using a relatively narrow caliper of 0.10, matches were found for 926 treatment participants, which accounts for 80 percent of the total number of treated offenders (N = 1,164).

Table 2. Propensity Score Matching and Covariate Balance for Treatment

Variable	*Sample*	*Treated Mean*	*Untreated Mean*	*Bias (%)*	*Bias Reduction*	*t test p Value*
Propensity Score	Total	0.44	0.29	74.28		0.00
	Matched	0.40	0.40	3.17	-95.74%	0.40
Male	Total	89.60%	90.72%	3.02		0.30
	Matched	89.85%	88.55%	3.44	13.69%	0.37
Minority	Total	40.81%	50.36%	15.77		0.00
	Matched	43.52%	44.92%	2.31	-85.36%	0.54
Age at Release (Years)	Total	33.55	32.97	5.12		0.08
	Matched	33.44	33.26	1.61	-68.51%	0.67
Metro	Total	49.74%	52.87%	5.11		0.08
	Matched	51.30%	51.51%	0.35	-93.10%	0.93
Prior Felony	Total	2.45	2.51	1.62		0.58
	Matched	2.55	2.55	0.16	-90.42%	0.97
Person Offenders	Total	27.41%	34.84%	13.30		0.00
	Matched	28.62%	28.94%	0.58	-95.61%	0.88
Property Offenders	Total	24.66%	24.84%	0.35		0.91
	Matched	24.62%	25.38%	1.43	304.00%	0.71
Drug Offenders	Total	30.41%	27.85%	4.59		0.12
	Matched	30.24%	32.07%	3.24	-29.31%	0.39
DWI Offenders	Total	5.24%	0.81%	19.13		0.00
	Matched	4.21%	1.51%	12.34	-35.48%	0.00
Other Offenders	Total	12.29%	11.66%	1.58		0.59
	Matched	12.31%	12.10%	0.54	-65.91%	0.89
Assessed as Dependent	Total	63.66%	51.66%	20.10		0.00
	Matched	58.75%	61.66%	4.85	-75.85%	0.20
Institutional Discipline	Total	2.36	2.86	9.61		0.00
	Matched	2.50	2.66	3.19	-66.84%	0.40
Length of Stay (months)	Total	17.46	11.55	47.86		0.00
	Matched	16.29	16.19	0.74	-98.46%	0.86
Length of Supervision (months)	Total	18.95	17.60	4.14		0.25
	Matched	18.60	17.06	6.56	58.72%	0.47
Intensive Supervised Release	Total	18.30%	25.38%	14.33		0.08
	Matched	21.38%	20.41%	1.95	-86.42%	0.61
Supervised Release	Total	64.95%	46.86%	30.47		0.00
	Matched	62.10%	63.17%	1.82	-94.03%	0.63
Work Release	Total	14.86%	12.51%	5.52		0.06
	Matched	14.15%	13.82%	0.76	-86.21%	0.84
Discharge	Total	1.89%	15.25%	46.23		0.00
	Matched	2.38%	2.59%	1.14	-97.53%	0.77
Supervised Release Revocations	Total	0.42	0.39	3.75		0.01
	Matched	0.48	0.48	0.12	-96.73%	0.98

Total Treated N = 1,164 Matched Treated N = 926
Total Untreated N = 2,230 Matched Untreated N = 926

Table 2 presents the covariate and propensity score means for both groups prior to matching ("total") and after matching ("matched"). In addition to tests of statistical significance (“t test p value”), Table 2 provides a measure (“Bias”) developed by Rosenbaum and Rubin (1985) that quantifies the amount of bias between the treatment and control

$$\text{Bias} = \frac{100(\overline{X}_t - \overline{X}_c)}{\sqrt{\frac{(S_t^2 + S_c^2)}{2}}}$$

samples (i.e., standardized mean difference between samples), where $\overline{X}_t$ and S_t^2 represent the sample mean and variance for the treated offenders and $\overline{X}_c$ and S_c^2 represent the sample mean and variance for the untreated offenders. If the value of this statistic exceeds 20, the covariate is considered to be unbalanced (Rosenbaum & Rubin, 1985). As shown in Table 2, the matching procedure reduced the bias in propensity scores between treated and untreated offenders by 96 percent. Whereas the p value was 0.00 in the unmatched sample, it was 0.40 in the matched sample. In the unmatched sample, there were three covariates that were significantly imbalanced (i.e., the bias values exceeded 20). But in the matched sample, covariate balance was achieved insofar as there were no covariates with bias values greater than 20. The average reduction in bias for the 17 covariates was 46 percent.

Matching for Treatment Outcome and Program Duration

As noted above, this study also examines the effects of treatment outcome and program duration on recidivism. Because untreated and treated offenders were matched individually, it is possible to estimate the effects of treatment outcome by separately comparing completers and dropouts with their untreated counterparts in the comparison group. Likewise, the effects of program duration can be analyzed by separately comparing short-, medium-, and long-term program participants with their matched pairs of untreated offenders. Yet, using the matched pairs produced by the propensity score model for treatment participation could yield biased estimates of the effects for treatment outcome and program duration, considering that the initial match between treated and untreated offenders was based on a different measure of treatment (participation).[6]

To address this issue, separate propensity score models were estimated for each of the five additional measures of treatment: 1) treatment completers, 2) treatment dropouts, 3) short-term participants, 4) medium-term participants, and 5) long-term participants. Specifically, five logistic regression models were estimated in which the 17 aforementioned predictors were regressed against dependent variables that contrasted the untreated offenders (N = 2,230) with the treatment completers (N = 843), treatment dropouts (N = 321), short-term participants (N = 671), medium-term participants (N = 393), and long-term participants (N = 100). After obtaining propensity scores from the five logistic regression models, untreated offenders were then matched—using a caliper of 0.10—with treated offenders for each of the five treatment measures. The matching process yielded match rates of 84 percent (708 of 843) for treatment completers, 96 percent (306 of 321) for treatment dropouts, 90 percent (606 of 671) for short-term participants, 90 percent (352 of 393) for medium-term participants, and 98 percent (98 of 100) for long-term participants. Comparisons between the matched pairs for the five treatment measures revealed that all propensity score and covariate means had bias values less than 20.

[6] It is worth noting that results from Cox regression models analyzing treatment outcome and program duration based on matches from the treatment participation propensity score model were similar to those reported in this study. That is, completing treatment significantly reduced recidivism, whereas dropping out of treatment had no effect. Similarly, for program duration, short-term programs significantly decreased recidivism, while long-term programs did not have a statistically significant impact. Medium-term programs significantly reduced rearrest and reconviction but did not have a statistically significant effect on reincarceration.

ANALYSIS

In analyzing recidivism, survival analysis models are preferable in that they utilize time-dependent data, which are important in determining not only whether offenders recidivate but also when they recidivate. As a result, this study uses a Cox regression model, which uses both "time" and "status" variables in estimating the impact of the independent variables on recidivism. For the analyses presented here, the "time" variable measures the amount of time from the date of release until the date of first rearrest, reconviction, reincarceration, or December 31, 2008, for those who did not recidivate. The "status" variable, meanwhile, measures whether an offender reoffended (rearrest, reconviction, or reincarceration for a new crime) during the period in which s/he was at risk to recidivate. In the analyses presented below, Cox regression models were estimated for each of the three recidivism measures for all six treatment variables (participation, completer, dropout, short-term, medium-term, and long-term).

RESULTS

Compared to the untreated offenders, those who received treatment had lower rates of reoffending for all three recidivism measures. As shown in Table 3, which breaks out recidivism rates by treatment participation, outcome, and program type, offenders who completed treatment or successfully participated until their release had lower reoffense rates than treatment dropouts for all three recidivism measures. In addition, offenders who participated in medium-term programs had the lowest recidivism rates, followed by those who entered long-term programs.

Table 3. Recidivism Rates by Treatment Participation, Outcome, and Program Length

	Rearrest	*Reconviction*	*Reincarceration*	*N*
Untreated Offenders	63.5	39.5	29.6	926
Treated Offenders	59.8	33.7	23.8	926
Treatment Outcome				
Treatment Completers	57.1	29.8	20.6	650
Treatment Dropouts	66.3	42.8	31.2	276
Length of Program				
Short-Term Treatment	67.1	36.8	25.6	562
Medium-Term Treatment	46.7	27.5	20.3	291
Long-Term Treatment	56.2	34.2	23.3	73

These findings suggest that: 1) prison-based treatment may have an impact on recidivism, 2) completing treatment may significantly lower the risk of recidivism, and 3) medium- and long-term programs may be more effective at reducing recidivism than short-term programs. It is possible, however, that the observed recidivism differences between treated and untreated offenders, treatment completers and dropouts, and short-term and other treatment participants are due to other factors such as time at risk, prior criminal history, discipline history, or post-release supervision. To statistically control for the impact of these other factors on reoffending, Cox regression models were estimated for each of the three recidivism variables across all six treatment measures (participation, completers, dropouts, short-term, medium-term, and long-term).

THE IMPACT OF CD TREATMENT ON RECIDIVISM

Treatment Participation

The results in Table 4 indicate that, controlling for the effects of the other independent variables in the statistical model, participation in a prison-based CD treatment program significantly reduced the hazard ratio for all three recidivism measures (rearrest, reconviction, and reincarceration for a new offense). Put another way, treated offenders recidivated less often and more slowly than untreated offenders; as a result, those who participated in treatment survived longer in the community without committing a new offense. In particular, CD treatment decreased the hazard by 17 percent for rearrest, 21 percent for reconviction, and 25 percent for reincarceration for a new crime.

Table 4. Cox Regression Models for Treatment Participation

Variables	*Rearrest*		*Reconviction*		***Reincarceration***	
	Hazard Ratio	SE	Hazard Ratio	SE	Hazard Ratio	SE
CD Treatment	0.828**	0.060	0.792**	0.077	0.746**	0.091
Male	1.448**	0.104	1.665**	0.148	1.964**	0.185
Minority	1.276**	0.064	1.273**	0.083	1.350**	0.098
Age at Release (years)	0.981**	0.004	0.981**	0.005	0.982**	0.006
Metro	1.118	0.064	1.378**	0.084	1.321**	0.100
Prior Felonies	1.083**	0.008	1.088**	0.009	1.100**	0.009
Offense Type						
Person Offenders	0.896	0.103	1.034	0.131	0.984	0.153
Property Offenders	1.058	0.099	1.121	0.125	1.107	0.144
Drug Offenders	0.930	0.102	0.804	0.134	0.783	0.159
DWI Offenders	2.400**	0.265	2.436**	0.346	4.003**	0.412
Assessed as Dependent	1.034	0.062	1.064	0.081	1.006	0.095
Institutional Discipline	1.038**	0.008	1.024*	0.010	1.035**	0.011
Length of Stay (months)	0.983**	0.003	0.988**	0.004	0.992	0.005
Length of Supervision (months)	0.979**	0.003	0.982**	0.004	0.975**	0.006
Supervision Type						
Intensive Supervised Release	0.697	0.192	0.586*	0.229	0.530*	0.264
Supervised Release	0.860	0.170	0.734	0.199	0.718	0.226
Work Release	0.741	0.195	0.571*	0.238	0.518*	0.280
Supervised Release Revocations	0.919	0.049	1.193**	0.056	1.152*	0.065
N	1,852		1,852		1,852	

** $p < .01$
* $p < .05$

The results also showed that the hazard ratio was significantly greater for males (all three measures), minorities (all three measures), younger offenders (all three measures), offenders with a metro-area county of commitment (reconviction and reincarceration), offenders with prior felony convictions (all three measures), DWI offenders (all three measures), offenders with institutional discipline convictions (all three measures), offenders with

Table 5. Cox Regression Models for Treatment Outcome

	Treatment Completer						*Treatment Dropout*					
Variables	Rearrest		Reconviction		Reincarceration		Rearrest		Reconviction		Reincarceration	
	Hazard Ratio	SE	Hazard Ratio	SE	Hazard Ratio	SE	Hazard Ratio	SE	Hazard Ratio	SE	Hazard Ratio	SE
Treatment Outcome												
Complete	0.783**	0.069	0.800*	0.093	0.730**	0.113						
Drop out							1.022	0.100	1.067	0.130	0.882	0.148
Male	1.344*	0.116	1.349	0.162	1.699*	0.212	1.220	0.185	1.360	0.253	1.810	0.306
Minority	1.427**	0.075	1.398**	0.101	1.557**	0.122	1.117	0.110	1.135	0.143	1.365	0.163
Age at Release (years)	0.982**	0.004	0.984**	0.006	0.990	0.007	0.976**	0.006	0.972**	0.008	0.962**	0.010
Metro	1.069	0.075	1.311**	0.100	1.325*	0.122	1.115	0.106	1.347*	0.140	1.063	0.157
Prior Felonies	1.069**	0.010	1.081**	0.010	1.091**	0.011	1.077**	0.015	1.090**	0.018	1.113**	0.020
Offense Type												
Person Offenders	0.857	0.126	0.944	0.163	0.861	0.196	0.847	0.178	1.093	0.231	1.034	0.270
Property Offenders	1.082	0.119	1.098	0.153	1.193	0.179	0.987	0.175	1.076	0.230	1.118	0.266
Drug Offenders	0.842	0.121	0.665*	0.162	0.633*	0.198	0.971	0.199	0.967	0.270	0.888	0.315
DWI Offenders	1.684	0.324	1.600	0.460	1.785	0.606	3.554**	0.430	3.519*	0.557	6.487**	0.681
Assessed as Dependent	0.957	0.072	1.006	0.098	1.026	0.118	1.079	0.106	1.270	0.140	1.207	0.157
Institutional Discipline	1.036*	0.015	1.028	0.019	1.039	0.023	1.017*	0.008	1.021*	0.010	1.026*	0.011
Length of Stay (months)	0.980**	0.004	0.987*	0.006	0.989	0.007	0.980**	0.005	0.981**	0.007	0.987	0.008
Length of Supervision (months)	0.982**	0.003	0.983*	0.005	0.976**	0.007	0.980*	0.006	0.982*	0.008	0.976*	0.011
Supervision Type												
Intensive Supervised Release	1.292	0.347	1.023	0.454	1.053	0.509	1.200	0.281	0.703	0.339	0.439*	0.397
Supervised Release	1.652	0.324	1.513	0.420	1.386	0.464	1.209	0.254	0.929	0.305	0.724	0.354
Work Release	1.372	0.338	1.203	0.441	0.965	0.497	0.437	0.579	0.466	0.669	0.497	0.697
Supervised Release Revocations	0.930	0.060	1.218**	0.070	1.274**	0.081	0.891	0.081	1.288**	0.092	1.268*	0.104
N	1,416		1,416		1,416		612		612		612	

** $p < .01$
* $p < .05$

supervised release revocations (reconviction and reincarceration), and offenders with shorter lengths of stay in prison (rearrest and reconviction) and time under post-release supervision (all three measures). The risk (hazard) was significantly less, however, for offenders released to intensive supervised release (reconviction and reincarceration) and work release (reconviction and reincarceration).

The results for the control variables were, for the most part, similar across all six measures of treatment (participation, completer, dropout, short-term, medium-term, and long-term). As such, the ensuing discussion of the results presented in Tables 5-8 will focus strictly on the effects found for the other five treatment measures.

Treatment Outcome

As shown in Table 5, which analyzes the impact of treatment outcome on reoffending, dropping out of treatment—either quitting or being terminated—did not have a statistically significant effect on any of the three recidivism measures. Completing treatment, however, had a significant impact on all three types of recidivism, reducing the hazard by 22 percent for rearrest, 20 percent for reconviction, and 27 percent for reincarceration.

Program Duration

As shown earlier in Table 3, offenders who entered medium-term programs had the lowest recidivism rates, whereas short-term participants had the highest rates. The results presented in Tables 6-8, however, show that both the short- and medium-term programs had statistically significant effects on all three recidivism measures. In contrast, long-term programs did not have a statistically significant impact on any type of recidivism. The hazard ratio for short-term participants was, relative to their untreated counterparts, 18 percent lower for rearrest, 18 percent lower for reconviction, and 24 percent lower for reincarceration. In addition, compared to their untreated matched pairs, the hazard ratio for medium-term participants was 32 percent lower for rearrest, 28 percent lower for reconviction, and 30 percent lower for reincarceration.

Table 6. Cox Regression Models for Program Duration: First Rearrest

Variables	*Short-Term*		*Medium-Term*		***Long-Term***	
	Hazard Ratio	SE	Hazard Ratio	SE	Hazard Ratio	SE
Program Duration						
Short-Term Treatment	0.821**	0.070				
Medium-Term Treatment			0.683**	0.107		
Long-Term Treatment					1.052	0.227
Male	1.396**	0.128	2.531*	0.425	1.669	0.294
Minority	1.281**	0.077	1.355*	0.113	1.617*	0.227
Age at Release (years)	0.976**	0.004	0.986*	0.007	0.961**	0.013
Metro	1.245**	0.076	1.080	0.113	1.015	0.221
Prior Felonies	1.075**	0.010	1.087**	0.018	1.148**	0.033
Offense Type						
Person Offenders	0.909	0.127	0.885	0.165	1.193	0.358
Property Offenders	1.024	0.117	1.264	0.194	1.629	0.356
Drug Offenders	0.881	0.125	0.933	0.165	1.191	0.358
DWI Offenders	1.708	0.385	2.489**	0.332	2.079	0.563
Assessed as Dependent	0.954	0.072	1.023	0.112	0.891	0.237
Institutional Discipline	1.019	0.010	1.033*	0.013	1.021	0.025
Length of Stay (months)	0.982**	0.004	0.989*	0.005	0.973**	0.011
Length of Supervision (months)	0.989**	0.004	0.979**	0.004	0.989	0.008
Supervision Type						
Intensive Supervised Release	1.257	0.244	0.477*	0.330	0.969	0.818
Supervised Release	1.423	0.211	0.492*	0.317	1.533	0.775
Work Release	1.164	0.247	0.463*	0.336	0.780	0.896
Supervised Release Revocations	0.922	0.062	0.976	0.080	0.684*	0.171
N	1,212		704		196	

** $p < .01$
* $p < .05$

Given that medium-term participants had the lowest recidivism rates, it is perhaps not that surprising to find that medium-term programming had a statistically significant effect on all three recidivism measures. Interestingly, however, the results suggest that short-term programming was more effective than long-term programming even though the latter had lower recidivism rates. Although short-term participants had the highest rates of reoffense,

Table 7. Cox Regression Models for Program Duration: First Reconviction

Variables	*Short-Term*		*Medium-Term*		***Long-Term***	
	Hazard Ratio	SE	Hazard Ratio	SE	Hazard Ratio	SE
Program Duration						
Short-Term Treatment	0.820*	0.093				
Medium-Term Treatment			0.725*	0.143		
Long-Term Treatment					0.994	0.302
Male	1.492*	0.184	1.614	0.604	1.205	0.382
Minority	1.238*	0.100	1.406*	0.153	1.262	0.286
Age at Release (years)	0.980**	0.006	0.982	0.010	0.967	0.018
Metro	1.453**	0.100	1.191	0.155	1.006	0.283
Prior Felonies	1.078**	0.011	1.144**	0.022	1.226**	0.045
Offense Type						
Person Offenders	0.949	0.166	0.921	0.209	2.335	0.528
Property Offenders	1.056	0.151	0.755	0.257	1.550	0.520
Drug Offenders	0.790	0.167	0.659	0.219	2.155	0.539
DWI Offenders	1.896	0.503	2.555*	0.434	5.648*	0.819
Assessed as Dependent	1.021	0.096	0.898	0.153	1.132	0.326
Institutional Discipline	1.010	0.014	1.043**	0.015	0.993	0.033
Length of Stay (months)	0.989*	0.006	0.987*	0.007	0.992	0.013
Length of Supervision (months)	0.988*	0.006	0.982**	0.006	0.980	0.011
Supervision Type						
Intensive Supervised Release	0.787	0.312	0.651	0.418	0.849	0.857
Supervised Release	1.118	0.262	0.763	0.394	0.865	0.815
Work Release	0.810	0.317	0.678	0.427	0.159	1.311
Supervised Release Revocations	1.311**	0.072	1.209*	0.087	0.933	0.201
N	1,212		704		196	

** $p < .01$
* $p < .05$

they also had more prior felony convictions, shorter lengths of stay in prison, shorter post-release supervision periods, and were less likely to be released to supervision—all factors that significantly increased the risk of recidivism. Yet, after controlling for the effects of these and other factors such as time at risk, it was participation in the short-term programs—as opposed to the long-term programs—that had a statistically significant effect on all three recidivism measures.

Table 8. Cox Regression Models for Program Duration: First Reincarceration

Variables	*Short-Term*		*Medium-Term*		***Long-Term***	
	Hazard Ratio	SE	Hazard Ratio	SE	Hazard Ratio	SE
Program Duration						
Short-Term Treatment	0.760*	0.111				
Medium-Term Treatment			0.705*	0.173		
Long-Term Treatment					0.841	0.373
Male	2.093**	0.254	3.033	1.024	1.656	0.475
Minority	1.330*	0.120	1.484*	0.185	1.174	0.340
Age at Release (years)	0.978**	0.007	0.981	0.012	0.977	0.021
Metro	1.481**	0.120	1.065	0.188	1.030	0.333
Prior Felonies	1.092**	0.011	1.187**	0.025	1.203**	0.051
Offense Type						
Person Offenders	0.981	0.197	0.974	0.253	2.329	0.658
Property Offenders	1.218	0.175	0.719	0.303	1.586	0.655
Drug Offenders	0.786	0.203	0.710	0.266	2.235	0.669
DWI Offenders	3.881*	0.601	3.610*	0.514	15.800*	1.224
Assessed as Dependent	0.980	0.114	0.893	0.186	0.866	0.380
Institutional Discipline	1.007	0.016	1.055**	0.016	1.009	0.036
Length of Stay (months)	0.999	0.007	0.987	0.008	0.991	0.016
Length of Supervision (months)	0.980*	0.008	0.980**	0.008	0.957*	0.020
Supervision Type						
Intensive Supervised Release	0.596	0.346	0.508	0.466	1.136	0.933
Supervised Release	0.808	0.278	0.683	0.430	0.770	0.891
Work Release	0.579	0.360	0.478	0.485	0.284	1.381
Supervised Release Revocations	1.299**	0.080	1.222*	0.100	0.785	0.250
N	1,212		704		196	

** $p < .01$
* $p < .05$

SENSITIVITY ANALYSES

Rosenbaum Bounds

Although the results suggest that prison-based CD treatment reduces recidivism, PSM controlled only for bias among the observed covariates. As a result, the possibility exists that unobserved selection bias may account for the significant treatment effects. Hidden bias can occur when two offenders with the same observed covariates have different chances of receiving treatment due to an unobserved covariate. If this unobserved covariate is related to the outcome (recidivism) affected by treatment, then the failure to account for this hidden bias can alter conclusions drawn about the effects of treatment.

The sensitivity of the results to hidden bias was tested by using a method developed by Rosenbaum (2002) that calculates a bound on how large an effect an unobserved covariate would need to have on the treatment selection process in order to reverse inferences drawn about the effects of treatment. The Rosenbaum bounds sensitivity analysis produces a test statistic, gamma, that measures the threshold at which an unobserved covariate would cause the estimated treatment effect to no longer be statistically significant (i.e., $p > .05$). More specifically, the closer the gamma value is to 1, the stronger the possibility that the effect can be explained away by an unobserved covariate. Therefore, an estimated treatment effect with a gamma value of 1.5, for example, would be more sensitive to hidden bias than an effect with a gamma value of 2.0.

It is important to emphasize, however, that the Rosenbaum bounds method is limited in two important ways. First, the sensitivity analysis does not indicate whether unobserved bias exists. Rather, it simply identifies how large the hidden bias would need to be to nullify the estimated treatment effect. Second, as DiPrete and Gangl (2004) point out, the Rosenbaum bounds method is a "worst-case" scenario to the extent that it assumes the hypothetical unobserved covariate is an almost perfect predictor of the outcome variable (recidivism).

The results from the sensitivity analyses reveal that the estimated treatment effects are not particularly robust to hidden bias. With a gamma value of 1.05, the rearrest findings are the most sensitive to the possibility of hidden bias, followed by reconviction (gamma = 1.08) and reincarceration (gamma = 1.10). These results suggest that if an unobserved covariate that almost perfectly predicted rearrest differed between matched pairs of treated and untreated offenders by a factor of 1.05 or more, it would be sufficient to undermine the conclusions regarding the treatment effect. To put this statistic in perspective, institutional discipline would be a hidden bias equivalent in that, as shown earlier in Table 1, it had a comparable impact on the treatment selection process (b = -0.046). Therefore, if an unobserved covariate existed that perfectly predicted rearrest and had an impact on the treatment selection process similar to institutional discipline, it would be sufficient to invalidate the treatment effect for rearrest. Still, it is worth reiterating, however, that the Rosenbaum bounds method is a "worst-case" scenario. Although existing research has identified a number of factors that are significantly associated with recidivism, none have yet to be shown to be a nearly perfect predictor of reoffending, which is what the Rosenbaum bounds approach assumes.

CONCLUSION

This study is limited by the absence of data on post-treatment substance use and participation in post-release aftercare programming. Despite these limitations, however, the results are consistent with previous findings showing that prison-based CD treatment significantly reduces offender recidivism. Still, the size of the treatment effect was relatively modest. For example, entering treatment lowered the hazard ratio by 17-25 percent across all three types of recidivism. These results translate into odds ratios of 1.17 for rearrest, 1.28 for reconviction, and 1.35 for reincarceration (Lösel & Schmucker, 2005), which can, in turn, be converted into Cohen's d values of 0.09 for rearrest, 0.14 for reconviction, and 0.17 for reincarceration (Sánchez-Meca, Marín-Martínez, & Chacón-Moscoso, 2003). In their meta-analysis of incarceration-based drug treatment studies, Mitchell et al, (2007) reported a treatment effect odds ratio of 1.37, which was based primarily on rearrest as a measure of recidivism. The rearrest odds ratio (1.17) for the treatment effect observed in this evaluation is therefore quite a bit lower than what Mitchell et al. (2007) found among drug treatment studies in general. Moreover, the Cohen's d values for all three recidivism measures were under 0.20, which is indicative of a small effect size (Cohen, 1988).

The findings also indicated that dropping out of treatment did not have a significant effect on recidivism, while completing treatment lowered the risk of reoffending from 20-27 percent. Consistent with previous research (Wexler, Falkin, and Lipton, 1990), the results suggest that more treatment is not always better. That is, increased treatment time appeared to lower the risk of recidivism, but only up to a point. Although short-term (90 days) and medium-term (180 days) programs had a statistically significant impact on all three recidivism measures, no statistically significant effects were found for long-term (365 days) programming.

The results regarding program duration have implications not only for the DOC, but also for the prison treatment literature in general. Recall that the DOC discontinued its short-term programming in 2006, a decision that was based, in part, on evidence which seemed to suggest that better recidivism outcomes were associated with longer program durations. This evidence, however, consisted primarily of simple recidivism comparisons similar to those presented in Table 3. Yet, as this study has shown, controlling for rival causal factors is critical in determining whether a program (or type of program) has an impact on the outcome measure.

This study suggests that short-term programs can be an effective form of treatment, which is an important consideration given that the DOC has had, over the last several years, a growing influx of offenders admitted to prison as either probation or supervised release violators (Minnesota Department of Corrections, 2007b). Because these offenders tend to have relatively short lengths of stay in prison (an average of eight months), developing (or reinstituting) a treatment program for these offenders, even if it is short in duration, may yield a benefit in terms of reduced recidivism.

The growing number of probation and supervised release violators admitted to prison is not unique to Minnesota, however. Probation and parole violators have figured prominently in the dramatic growth in the state and federal prison systems and are projected to have a sizeable impact on future prison populations (JFA Associates, 2007). Therefore, implementing short-term treatment programs for offenders with shorter lengths of stay (e.g., probation and parole violators) may produce a modest recidivism reduction and, in so doing, help limit the growth of prison populations.

Although this study suggests that prison-based CD treatment and, more narrowly, short-term programs can be effective, more evaluations of prison-based programs are needed. Due to the many variations among state and federal correctional populations, it is unlikely that a single study—regardless of how rigorous the design—can conclusively determine whether prison-based treatment works. Rather, by quantitatively reviewing evaluations from multiple jurisdictions, meta-analyses could help better identify what works best for whom under which circumstances. In order to do so, however, the meta-analyses need to be based on an accumulation of rigorous evaluations that effectively control for threats to validity, not least selection bias.

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