THE IMPACT OF PRISON-BASED TREATMENT ON SEX OFFENDER RECIDIVISM: EVIDENCE FROM MINNESOTA

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INTRODUCTION

Over the last 50 years, dozens of studies from a number of countries have examined whether sex offender treatment reduces recidivism. Reviews of the earliest studies drew pessimistic conclusions about the effectiveness of treatment. For example, in their 1989 review of the treatment literature, Furby, Weinrott, and Blackshaw argued that, due to methodological shortcomings, there was insufficient evidence to support the notion that treatment decreases sex offender recidivism. Several years later, Quinsey, Harris, Rice, and Lalumiere (1993) reached a similar conclusion in their review of existing treatment studies.

Since the mid-1990s, however, meta-analyses of the treatment literature have, with a few notable exceptions (Kenworthy et al., 2004; Rice & Harris, 2003), found lower sexual recidivism rates for treated sex offenders in comparison to untreated offenders (Alexander, 1999; Gallagher, Wilson, Hirschfield, Coggeshall & MacKenzie, 1999; Hall, 1995; Hanson et al., 2002; Lösel & Schmucker, 2005). Among the meta-analyses that have found a treatment effect, the rate of sexual reoffense has been between 5-10 percentage points less for those who participated in treatment, resulting in a mean effect size (Cohen's d) ranging from 0.12 to 0.47. The evidence from these studies further indicates that cognitive-behavioral techniques with relapse prevention components have, by and large, been found to be the most effective in reducing recidivism.

Despite the generally positive findings from the meta-analytical reviews, it is nevertheless true that most of the existing treatment studies have lacked methodological rigor. In the Lösel and Schmucker (2005) study, which is the most comprehensive meta-analysis to date, they examined 80 comparisons (69 studies) between treated and untreated sex offenders. Of these comparisons, only six (seven percent) used a randomized experimental de-

sign—most notably, the research by Marques and colleagues (Marques, Day, Nelson & West, 1994; Marques, 1999; Marques, Wiederanders, Day, Nelson & von Ommeren, 2005)—while seven (nine percent) used individual matching or statistical control in an effort to achieve equivalence between the treatment and comparison groups. Instead, most treatment studies have used either nonequivalent comparison groups (60 percent) or research designs in which equivalence was assumed between the treated and untreated groups (24 percent).

Given the relatively large percentage (84 percent) of studies that have not used random assignment or matching techniques, selection bias is, as some have pointed out (Harkins & Beech, 2006; Jones, Pelissier & Klein-Saffran, 2006; Rice & Harris, 2003), a problem that has plagued the sex offender treatment literature. 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 reduced for sex offenders who participate in treatment, this effect may not necessarily be due to the treatment itself, but rather to other differences between treated and untreated offenders.

In addition to selection bias, the vast majority of existing studies share a number of limitations. For example, of the studies reviewed by Lösel and Schmucker (2005), only 11 (13%) had a sample size in excess of 500. Moreover, the follow-up periods for many studies have been relatively short, as only one-fourth of those examined by Lösel and Schmucker tracked offenders for more than seven years.

PRESENT STUDY

In evaluating the effectiveness of sex offender treatment in Minnesota prisons, this study does not use a randomized experimental design. Furthermore, due to a lack of available data, it does not control for the possible impact that post-release participation in community-based treatment may have on reoffending. Despite these limitations, however, the present study contains a number of strengths that have been lacking from most prior treatment studies. First, as discussed later in more detail, a propensity score matching (PSM) technique was used to individually match treated and untreated sex offenders. In doing so, this study minimizes the threat of selection bias by creating a comparison group whose probability of entering treatment was similar to that of the treatment group. Second, in addition to being one of the first studies in the sex offender treatment literature to use PSM (Caldwell, Skeem, Salekin & Van Rybroek, 2006; Skeem, Monahan & Mulvey, 2002), this study further controls 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. Third, by comparing 1,020 treated sex offenders with a matched group of 1,020 untreated sex offenders, the sample size used for this study (N = 2,040) is one of the larger sex offender treatment studies to date. Fourth, to gain a more precise assessment of the effectiveness of treatment, multiple measures of treatment participation and criminal recidivism were used. Finally, because recidivism data were collected on the 2,040 sex offenders through the end of 2006, the average follow-up period for these offenders was 9.3 years. This study thus provides a robust assessment of treatment effectiveness by tracking offenders over a relatively lengthy period of time.

In examining prison-based treatment in Minnesota, there was an attempt to address several questions central to the sex offender treatment literature. First, does treatment participation reduce offender recidivism? Second, what effect does treatment outcome (i.e., drop out, complete, successfully participate until release, etc.) have on reoffending? Finally, are there certain types of sex offenders for whom treatment is more effective?

In the following section, the provision of sex offender treatment within the Minnesota Department of Corrections (DOC) is described. The data and methods used in this study are then discussed, followed by a presentation of the results. The report concludes by discussing the implications of the findings for the sex offender treatment literature.

PROGRAM DESCRIPTION: SEX OFFENDER TREATMENT IN THE DOC

In 1978, the DOC began providing sex offender treatment to incarcerated adult men when it opened the Transitional Sex Offender Treatment Program (TSOP)—a 30-bed program for offenders preparing to return to the community—at the Minnesota Correctional Facility (MCF)-Lino Lakes. Consistent with the name of the program, offenders were treated in the last year of their incarceration with the emphasis on preparation for release. As a component of the program design, services were continued for men post-release in a halfway house setting in conjunction with the staff of the halfway house.

In 1983, a second prison-based sex offender treatment program was established at the MCF-Oak Park Heights, Minnesota's lone maximum-custody facility. In addition to providing sex offender treatment, this 52-bed program treated chemically dependent inmates as well as those with dual diagnoses (i.e., needing treatment for both chemical dependency [CD] and sexual offending). In 1994, this program relocated to the MCF-Lino Lakes, a medium-

custody facility, and eventually integrated with the original TSOP, which had grown and evolved since 1978.

In 1991, the Sexual Education and Evaluation Center (SEEC), a small (36-bed) sex offender treatment program opened at the MCF-Stillwater. Psycho-education and therapy groups were the primary services offered by this program, which was designed to provide a short-term but intensive treatment experience. Three years later, a 50-bed sex offender treatment program opened at the MCF-Moose Lake. Designed to provide long-term, intensive sex offender treatment, this program also offered some specialized groups for offenders with intellectual or cognitive deficits.

Due to prison population growth and increased sentence lengths for sex offenders, the size of the program at the MCF-Lino Lakes increased to 110 beds by 1997 and then further expanded to 150 when the SEEC program was transferred to Lino Lakes and integrated with the program at that site. In 2000, it expanded once again with the transfer and integration of the MCF-Moose Lake program to Lino Lakes.

Over the last three decades, sex offender treatment programming at Lino Lakes evolved to keep pace with changing practices in the field, while attempting to maintain the most unique and seemingly valuable components of each of the programs that were melded into the program that exists today. For example, the CD treatment component was maintained. In doing so, the program now known as the Sex Offender Treatment Program (SOTP) addressed chemical abuse issues and their relationship to sexual offending in a more integrated manner than would likely occur in a stand-alone CD treatment program. In addition, psycho-education classes and therapy designed to accommodate the needs of the offender with intellectual or cognitive functioning limitations were maintained. Moreover, the

number and variety of psycho-education classes designed to impart information and enhance skill building were expanded at the SOTP. Finally, an emphasis on preparation for release was also maintained.

Using a cognitive-behavioral framework, the SOTP attempted to provide long-term intensive sex offender and CD treatment consistent with a risk-needs-responsivity model. To be eligible for treatment, offenders had to have at least nine months to serve in prison. Moreover, offenders who minimized their offenses (as described in official documents) were eligible to enter treatment in the DOC, whereas those who completely denied committing a sexual offense were not eligible. Given the fact that treatment capacity did not keep pace with the overall growth in inmate population, the SOTP attempted to target moderate- to high-risk sex offenders for treatment. Offenders considered to be lower risk were less likely to be admitted to sex offender treatment programming while incarcerated. However, offenders required to enter treatment but were unable to do so while incarcerated were recommended to participate in community-based treatment at the time of release.

Under the current process, offenders are prioritized for treatment primarily on the basis of their scores from the following actuarial instruments: Static-99, Rapid Risk Assessment for Sex Offense Recidivism (RRASOR), and Minnesota Sex Offender Screening Tool-Revised (MnSOST-R). Earlier attempts, however, to identify and prioritize cases for treatment were based on more primitive tools such as the Public Risk Monitoring (PRM) criteria, which were developed by DOC staff. Offenders who met the PRM criteria, which were never formally validated on the sex offender population, were directed to participate in treatment programming. As shown later, the PRM criteria did not appear to be very effective in distinguishing offenders on the basis of recidivism risk. Indeed, the recidivism risk (as

reflected by the risk score measure developed for this study) was not significantly different between offenders who were offered treatment and those who were not.

After receiving a treatment directive, offenders had the right to refuse treatment. There were consequences, however, for those who exercised this right. In particular, offenders who failed to comply with a treatment directive had their wages frozen and were subject to extended incarceration disciplinary time that lengthened their stay in prison. In addition, treatment participation and outcome are items on the MnSOST-R, which has been used to guide decisions regarding community notification levels and civil commitment consideration referral. Therefore, even though offenders can refuse the directive to enter treatment, the "carrot and stick" approach used by the DOC likely motivated many offenders to enter treatment programming who might have otherwise opted not to do so if the choice were entirely voluntary.

SOTP participants were housed in two adjacent wings of a larger living unit. This arrangement allowed for some movement across the wings in the living unit, but no movement either to or from the other wings where general population inmates were housed. The living units operated within a modified therapeutic milieu with clear living unit/program structure and rules, and there was an expectation that inmates would support and hold one another accountable throughout their day. The SOTP was not an entirely closed living unit as there was some interaction with the general inmate population during movement and activities such as dining, religious services, educational programming, etc. A 30-day assessment and orientation phase, individual and group therapy, and psycho-educational programming were held next to the living unit in two buildings where the offices of clinical staff were also located. Although individualized treatment plans varied widely among treatment participants,

the average dosage consisted of approximately 10-15 hours of direct, staff-facilitated services per week for a duration that often ranged from one to three years.

In 1999, the DOC implemented legislatively-mandated rules for residential sex offender treatment that require programs to meet specified staff training and ratios; group and class size limitations; and have a written, theory-based treatment protocol and demonstrate that it is applied to the assessment, treatment, and therapeutic milieu. The program was audited on a biennial basis. Given the state of research and practice in the field of sex offender treatment, the SOTP continues to be a work in progress. The following section describes the main components of the program.

SOTP COMPONENTS

Assessment: This 30-day phase of the program includes psychological testing; completion of assignments to facilitate the assessment of treatment needs; a review of offending history and offense dynamics; and use of lecture, discussion, and videotapes to provide information on treatment participation and expectations, defenses and denial, sexual assault dynamics, victim impact, chemical dependency, etc. A clinical interview, collateral information, client observations, and test results were used to develop a written psychosexual assessment report and an individualized treatment plan.

Therapy: Following the assessment phase of the program, inmates participated in an average of six hours per week of staff-facilitated group therapy sessions. Therapy groups specific to the needs of the inmate with cognitive/intellectual limitations were provided. Additional individual therapy was offered based on the needs of the inmate and the availability of staff. Therapy was provided in progressive phases and in-

cluded transitional programming and aftercare. Ongoing therapy and post-release programming in the community were provided under contract with or through grants from the DOC to private agencies.

CD Treatment: Offenders entering the DOC were formally screened, assessed, and diagnosed for chemical abuse or dependency. Treatment directives were provided contingent on the outcome of those assessments. For those sex offenders needing treatment for alcohol or drug dependency, CD treatment was typically provided following completion of the assessment phase in the SOTP.

Family/Support Person Education: To prepare offenders for their return to the community while also helping them reach specific treatment goals, education sessions were facilitated between program participants and members of their family and/or support system. These sessions were used to provide clarification about the nature and impact of their offending, to inform support persons about the risk for reoffense, and to identify response strategies for the offender and the support person.

Psycho-educational Programming: Program members participated in psycho-educational programming, which varied according to the offenders' individualized treatment plans. Psycho-educational classes were typically provided for 1.5 hour sessions 3 to 4 times per week in 12-week (quarter) sessions. Each quarter, program participants were each enrolled in one or two classes, which included Emotions Management, Alcohol and Drug Education, Cognitive Restructuring and Criminal Thinking, Sexuality Education, Sexual Assault Dynamics, Reoffense Prevention, Victim Empathy, Personal Victimization, Grief and Loss, Morals and Values, Sexual Behaviors, and Transitional Curriculum. Classes specific to the needs of the inmate with cognitive

limitations included modifications of some of the classes above. In addition to these psycho-educational classes, offenders were assigned per their individual treatment plan to participate in a parenting class provided in the institution under a contract with a non-profit agency. Offenders in the program also participated in additional educational, pre-release, and transitional planning (housing, employment, transportation, etc.), which drew heavily on resources from the community.

Support Groups: On a weekly basis, offenders attended additional support group meetings held in the institution such as Alcoholics Anonymous, Narcotics Anonymous, and Sex Abusers Anonymous. Each of these groups met for 1.5 hours per week and were monitored but not facilitated by program staff.

Community Meetings: Inmates met weekly in a large group with other members of their living unit to address general housekeeping issues, community milieu, and provide support to one another as a community.

DATA AND METHODOLOGY

To determine whether treatment provided within the DOC has had an impact on sex offender recidivism, a retrospective quasi-experimental design was used. That is, the effectiveness of sex offender treatment was evaluated by comparing recidivism outcomes between treated offenders and a matched comparison group of untreated offenders who were released between 1990 and 2003. During this 14-year period, there were 3,440 sex offenders released from Minnesota prisons.

Of these offenders, 1,493 (43%) participated in prison-based treatment prior to their release. Of the remaining 1,947 offenders, 105 refused to enter treatment while the other 1,842 offenders were not given the opportunity to participate. The 105 treatment refusers

were removed from the study so as not to bias the results from the statistical analyses. Before doing so, however, an additional source of bias was removed by using PSM to identify a comparison group of 105 offenders from the pool of untreated offenders (N = 1,842) who were not offered treatment.

PROPENSITY SCORE MATCHING

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 assignment (0 = no assignment; 1 = assignment) is the dependent variable while the predictor variables consist of those that theoretically have an impact on the selection process. Shadish, Cook, and Campbell (2002) note that unless the predictors are unrelated to the outcome variable or are not proper covariates, they should be included in the propensity score model even if they are only weakly associated with the outcome (recidivism for this study). Once estimated, the propensity scores are then used to match individuals who entered treatment (or refused to enter 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, a "greedy" matching procedure was used 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 (defined in terms of probabilities).

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 is not without its limitations, however. First, and foremost, because propensity scores are based on observed 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, it was later demonstrated 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 = 3,440) on which to conduct the propensity score analyses.

Table 1. Propensity Score Matching and Covariate Balance for Refusers

Variable	*Sample*	*Refusers Mean*	*Non-Refusers Mean*	*Bias (%)*	*Bias Reduction*	*t test p Value*
Propensity Score	Total	0.11	0.05	53.25		0.00
	Matched	0.11	0.11	0.76	-98.58%	0.95
Minority	Total	38.10%	37.79%	0.52		0.95
	Matched	38.10%	39.05%	1.59	205.38%	0.89
Age at Release (years)	Total	32.89	33.04	1.08		0.89
	Matched	32.89	32.84	0.37	-65.72%	0.98
Metro	Total	49.52%	45.44%	6.66		0.41
	Matched	49.52%	59.05%	15.58	134.05%	0.17
Prior Sex Crimes	Total	41.90%	20.20%	37.98		0.00
	Matched	41.90%	44.76%	4.70	-87.64%	0.68
Prior Felony	Total	68.57%	63.79%	8.28		0.32
	Matched	68.57%	66.67%	3.31	-60.04%	0.77
Stranger	Total	10.48%	9.50%	2.64		0.74
	Matched	10.48%	12.38%	4.91	85.96%	0.67
Acquaintance	Total	66.67%	60.42%	10.66		0.20
	Matched	66.67%	64.76%	3.28	-69.24%	0.77
Adult Female	Total	16.19%	18.78%	5.61		0.51
	Matched	16.19%	14.29%	4.26	-24.02%	0.70
Male Child	Total	3.81%	5.54%	6.89		0.45
	Matched	3.81%	1.90%	8.86	28.51%	0.41
Length of Stay (months)	Total	29.13	23.65	15.43		0.01
	Matched	29.13	29.24	0.28	-98.18%	0.98
Discipline	Total	1.38	1.45	2.30		0.82
	Matched	1.38	1.13	10.90	374.76%	0.32
Supervision (months)	Total	50.58	34.17	42.00		0.00
	Matched	50.58	43.33	18.00	-57.13%	0.11
Intensive Supervised Release	Total	32.38%	18.19%	26.10		0.00
	Matched	32.38%	24.76%	13.58	-47.99%	0.22
Supervised Release	Total	61.90%	79.59%	31.30		0.00
	Matched	61.90%	70.48%	14.65	-53.19%	0.19
Supervised Release Revocations	Total	1.34	0.76	34.69		0.00
	Matched	1.34	1.36	0.89	-97.42%	0.93
Community Notification	Total	14.30%	2.40%	32.33		0.00
	Matched	14.30%	14.30%	0.00	-100.00%	1.00
Release Year	Total	1998.61	1996.93	32.13		0.00
	Matched	1998.61	1997.76	16.02	-50.14%	0.16
Risk Score	Total	4.39	3.95	19.80		0.02
	Matched	4.39	4.32	3.16	-84.05%	0.78
	Total					
Total Recidivism	Matched					
Sex Rearrest	Total	41.0%	16.1%			0.00
	Matched	41.0%	36.2%			0.48
Violent Rearrest	Total	55.2%	33.6%			0.00
	Matched	55.2%	47.6%			0.27
Any Rearrest	Total	66.7%	59.8%			0.16
	Matched	66.7%	69.5%			0.66

Total Refusers N = 105
Total Non-Refusers N = 1,842
Matched Refusers N = 105
Matched Non-Refusers N = 105

Matching Treatment Refusers and Non-Refusers

In an effort to minimize the bias resulting from treatment refusers, there was an attempt 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 for the 105 treatment refusers and the 1,842 untreated offenders were computed by estimating a logistic regression model in which the dependent variable was refusal of treatment (i.e., the 105 treatment refusers were assigned a value of "1" while the 1,842 untreated offenders in the comparison group pool received a value of "0"). The predictors were the 17 control variables, which are described later, that were used in the statistical analyses. After obtaining propensity scores on the 1,947 offenders, a greedy matching procedure was used to match 105 untreated offenders not offered treatment with the 105 treatment refusers.

In Table 1, the covariate, propensity score, and recidivism outcome means are presented for both groups prior to matching ("total") and after matching ("matched"). In addition to tests of statistical significance ("t test p value"), a measure ("bias") developed by Rosenbaum and Rubin (1985) is provided that quantifies the amount of bias between the

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

treatment and control 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 1, the matching procedure reduced the bias in propensity scores between treatment refusers and those not offered treatment by 99 percent. Whereas the p value was 0.00 in the unmatched sample, it was 0.95 in the matched sample. Although risk score was not used as a predictor in the logistic regression analysis, the means for this variable are also presented to illustrate the differences between the two groups before and after matching. In the unmatched sample, there were nine covariates that were significantly imbalanced (i.e., the difference between the treatment refusers and those not offered treatment was significant at the .05 level and 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 or with significant differences between the treatment refusers and those not given a treatment opportunity. Just as important, when examining the outcome data for these two groups of offenders within the unmatched sample, it is apparent that the treatment refusers had significantly higher rates of sexual and violent recidivism. In the matched sample, however, recidivism outcomes were not significantly different between the two groups. Along with the 105 treatment refusers, the 105 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 210 from 1,947 to 1,737.

Matching Treated and Untreated Sex Offenders

Similar to the approach described above with treatment refusers, propensity scores for the 1,493 treated offenders and the 1,737 untreated offenders were calculated by estimating a logistic regression model in which the dependent variable was participation in prison-based treatment (i.e., the 1,493 treatment group offenders were assigned a value of “1”, while the

1,737 offenders in the comparison group pool received a value of "0"). The predictors were the 17 control variables (excluding risk score) used in the statistical analyses (see Table 2). 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 3).

After obtaining propensity scores for the 3,230 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), especially in situations like

Table 2. Logistic Regression Model for Assignment to Treatment

Predictors	*Coefficient*	*Standard Error*
Minority	-0.292**	0.088
Age at Release (years)	9.0 E-4*	3.9 E-4
Metro	0.290**	0.081
Prior Sex Crime Convictions	0.789**	0.095
Prior Felony Convictions	-0.348**	0.082
Stranger Victims	-0.048	0.153
Acquaintance Victims	-0.039	0.089
Adult Female Victims	-0.093	0.106
Male Child Victims	0.145	0.160
Length of Stay (months)	0.021**	2.0 E-4
Discipline	-0.076**	0.019
Supervision (months)	6.5 E-4**	1.6 E-4
Intensive Supervised Release	0.980**	0.368
Supervised Release	0.902*	0.358
Supervised Release Revocations	0.160**	0.033
Community Notification	0.703**	0.238
Release Year	-0.017	0.014
Constant	31.997	27.806
N	3,230	
Log-likelihood	3929.875	
Nagelkerke R^2	0.202	

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

this one where the treated offenders (N = 1,493) accounted for nearly half (46 percent) of the offenders (N = 3,230), it was not possible to obtain matches for all of the treated offenders. However, using a relatively narrow caliper of 0.10, it was possible to achieve 1,020 matches, which amounts to 68 percent of the total number of treated offenders (N = 1,493).

As shown in Table 3, the matching procedure reduced the bias in the propensity score (i.e., probability of entering treatment) by 98 percent. Again, the means for risk score are presented even though it was not used as a predictor in the logistic regression model. In the unmatched sample, more than half of the covariates (9) had bias values greater than 20 and all but three were significantly different at the .05 level. In the matched sample, however, the covariates are balanced to the extent that all bias values are less than 20 and there are no statistically significant differences in covariates between the treated and untreated offenders. The average reduction in bias for the 18 covariates (including risk score) was 81 percent.

Table 3. Propensity Score Matching and Covariate Balance for Treatment

Variable	*Sample*	*Treated Mean*	*SD*	*Untreated Mean*	*SD*	*Bias (%)*	*Bias Reduction*	*t test p Value*
Propensity Score	Total	0.55	0.19	0.39	0.17	70.31		0.00
	Matched	0.47	0.17	0.47	0.17	1.19	-98.30%	0.74
Minority	Total	33.09%	0.47	37.71%	0.48	7.94		0.01
	Matched	35.20%	0.48	35.10%	0.48	0.15	-98.06%	0.96
Age at Release (years)	Total	36.10	10.48	33.05	11.06	23.33		0.00
	Matched	34.88	10.23	34.94	11.80	0.52	-97.79%	0.90
Metro	Total	51.44%	0.50	44.62%	0.50	11.16		0.00
	Matched	47.94%	0.50	48.53%	0.50	0.84	-92.48%	0.79
Prior Sex Crimes	Total	39.38%	0.49	18.71%	0.39	36.84		0.00
	Matched	27.55%	0.45	28.14%	0.45	1.01	-97.27%	0.77
Prior Felony	Total	54.39%	0.50	63.62%	0.48	15.29		0.00
	Matched	58.33%	0.49	56.96%	0.50	1.88	-87.72%	0.53
Stranger	Total	11.72%	0.32	9.33%	0.29	6.27		0.03
	Matched	10.69%	0.31	9.90%	0.30	2.09	-66.67%	0.56
Acquaintance	Total	53.65%	0.50	60.16%	0.49	10.72		0.00
	Matched	57.25%	0.49	56.67%	0.50	0.81	-92.47%	0.79
Adult Female	Total	20.29%	0.40	19.06%	0.39	2.53		0.38
	Matched	21.47%	0.41	20.20%	0.40	2.47	-2.38%	0.48
Male Child	Total	7.37%	0.26	5.76%	0.23	5.21		0.06
	Matched	6.67%	0.25	7.16%	0.26	1.54	-70.43%	0.66
Length of Stay (mos.)	Total	36.63	24.50	23.31	21.25	46.33		0.00
	Matched	29.90	20.68	29.42	24.44	1.92	-95.86%	0.64
Discipline	Total	0.99	1.94	1.46	2.92	16.61		0.00
	Matched	1.10	2.02	1.09	1.89	0.34	-97.96%	0.94
Supervision (months)	Total	44.60	31.19	33.61	30.07	29.11		0.00
	Matched	39.14	28.84	37.88	31.86	3.94	-86.45%	0.35
Intensive Supervised Release	Total	33.29%	0.47	17.79%	0.38	28.52		0.00
	Matched	24.71%	0.43	23.33%	0.42	2.51	-91.20%	0.47
Supervised Release	Total	65.91%	0.47	80.14%	0.40	25.79		0.00
	Matched	74.31%	0.44	75.59%	0.43	1.70	-93.39%	0.51
Supervised Release Revocations	Total	0.93	0.27	0.73	0.13	12.63		0.00
	Matched	0.85	0.21	0.83	0.17	1.60	-87.37%	0.69
Comm. Notification	Total	7.64%	3.10	1.67%	4.14	21.26		0.00
	Matched	4.41%	3.20	2.84%	4.12	7.33	-65.51%	0.58
Release Year	Total	1997.86	1.44	1996.88	1.41	23.09		0.00
	Matched	1997.34	1.44	1997.07	1.46	0.86	-96.27%	0.09
Risk Score	Total	3.77	1.88	3.93	1.83	7.17		0.01
	Matched	3.80	1.89	3.84	1.86	1.83	-74.47%	0.61

Notes: SD = Standard Deviation
Total Treated N = 1,493
Total Untreated N = 1,737
Matched Treated N = 1,020
Matched Untreated N = 1,020

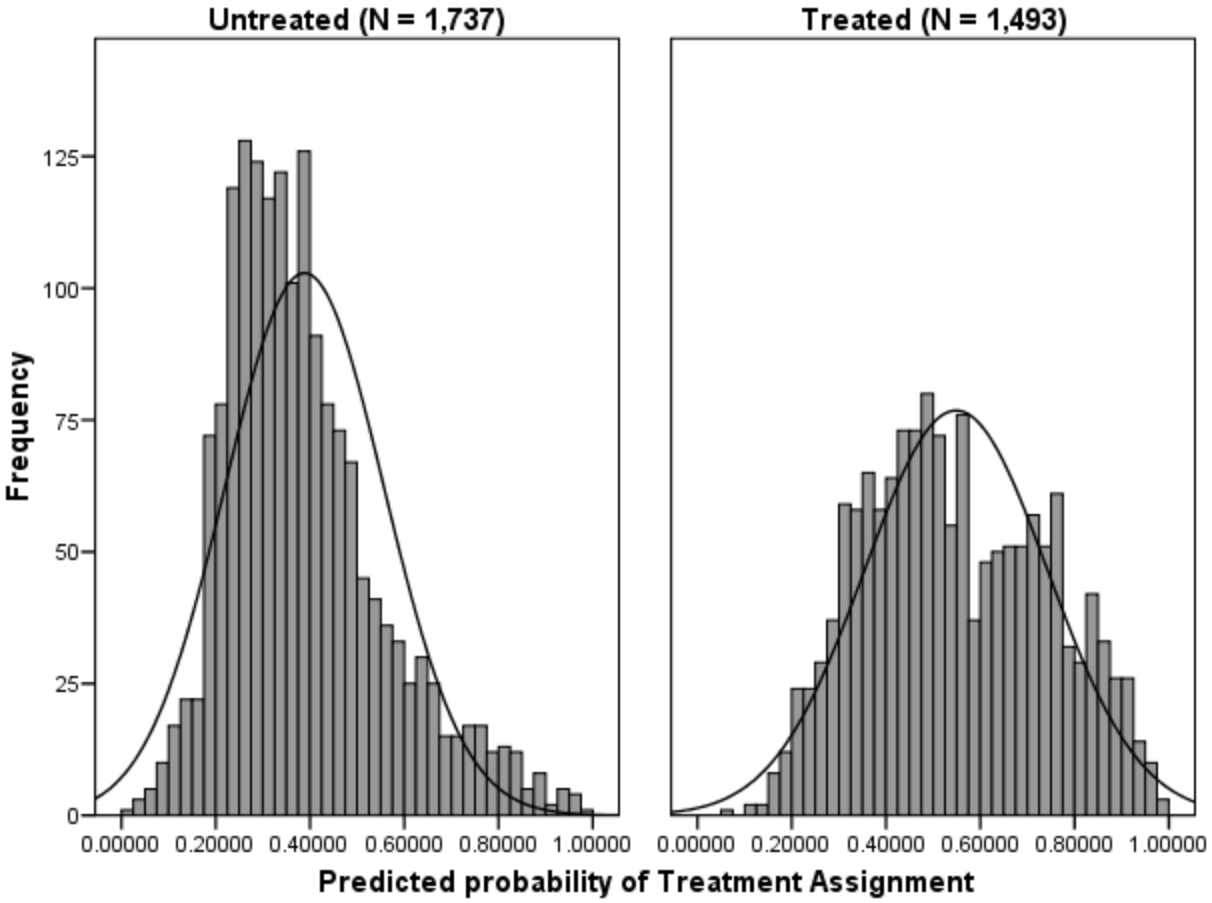

Figure 1. Distribution of Propensity Scores by Treatment Assignment

MEASURES

Dependent Variable

Recidivism, the outcome variable, was measured nine different ways in this study. It was first operationalized as 1) rearrest, 2) reconviction, or 3) reincarceration in a Minnesota correctional facility (MCF) for a new offense following an offender's first release from prison. Because it is important to know whether offenders recidivate with a sex offense, recidivism was further distinguished by the type of reoffense: 1) sex offense, 2) violent offense (including sex offenses), and 3) any offense. Sex offense was defined here as a 1st-5th

degree Criminal Sexual Conduct (CSC) offense. According to Minnesota statutes, CSC 1st-4th degree are felony-level offenses, and CSC 5th degree is a gross misdemeanor offense. In addition to sex crimes, violent offenses included homicide, assault, robbery, and kidnapping.

Arrest, conviction, and incarceration data were collected on offenders through December 31, 2006. The minimum follow-up period, then, was three years, while the maximum was 17 years. Data on arrests (misdemeanor, gross misdemeanor, and felony) and convictions (misdemeanor, gross misdemeanor, and felony) were obtained electronically from the Minnesota Bureau of Criminal Apprehension (BCA), whereas incarceration data were derived from the DOC's Correctional Operations Management System (COMS) database. Consequently, a limitation with these data is that they measure only arrests, convictions, or incarcerations that took place in Minnesota. Moreover, as with any recidivism study, official criminal history data will likely underestimate the actual extent to which the sex offenders examined here recidivated.

An arrest, conviction, and/or incarceration was considered a recidivism event only if it pertained to an offense that had taken place following release. There were a handful of offenders who returned to prison for a "new" sex offense that had been committed prior to the beginning of their previous prison term; e.g., an offender who was incarcerated from 1997 to 2000 (the beginning of the at-risk period) returns to prison in 2002 for an offense committed in 1995. In these instances, the offenses were not considered recidivism events, but the time that offenders served in prison was deducted from their at-risk period.

Treatment Variables

In the statistical analyses presented later, recidivism is the dependent variable. Given that the central purpose of this study is to determine whether sex offender treatment has an impact on recidivism, treatment is the principal variable of interest. In an effort to acquire a more refined understanding of its potential effect on recidivism, two separate treatment measures were used.

The first treatment variable compared offenders who entered sex offender treatment with a comparison group of similar offenders who did not. As such, treatment was measured as "1" for treatment participants and "0" for non-participants. The second treatment variable measured the impact of treatment outcome on reoffending. To this end, three dichotomous dummy variables were created: completion/successfully participated until the time of release (1 = completion/successful participation, 0 = treatment dropout or non-participants); terminated from treatment or voluntarily quit (1 = treatment termination/quit, 0 = other); and non-participants (1 = comparison group, 0 = treatment participants).

Control Variables

The control, or independent, 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. Prior research indicates that sex offender recidivism is predicted by factors such as prior sexual criminal history, victim characteristics, the intensity and length of post-release supervision, and broad community notification (Duwe & Donnay, 2008; Hanson & Morton-Bourgon, 2004; Minnesota Department of Corrections, 2007, 2008). To control for potential rival causal factors, it was necessary to include va-

riables such as these in the statistical analyses. The following lists these variables, which include pre-treatment and post-treatment measures, and describes how they were created. The univariate relationships between these variables and the three types of recidivism are presented in the Appendix.

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

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: offenders who had at least one prior felony conviction (excluding the instant offense) were given a value of 1, whereas those without a prior felony conviction were assigned a value of 0.

Prior Sex Crime Conviction: offenders who had at least one prior sex crime conviction (excluding the instant offense) were given a value of 1, whereas those without a prior sex crime conviction were assigned a value of 0.

Victim-Offender Relationship: three dichotomous dummy variables were created to measure the offender's relationship to the victim for the instant sex offense; i.e., the crime for which the offender was incarcerated. The three variables were stranger victims (1 = stranger victim, 0 = known or non-stranger victim); acquaintance victims (1 = acquaintance victim, 0 = non-acquaintance victim); and family member victims (1 = family member victim, 0 = non-family member victim). The family member victim variable, which is a proxy for incest offenders, serves as the reference in the statistical analyses.

Male Child Victims: dichotomized as either male child victims (1) or non-male child victims (0), this variable measures whether offenders victimized a male under the age of 13 in their instant offense.

Adult Female Victims: dichotomized as either adult female victims (1) or non-adult female victims (0), this variable quantifies whether offenders victimized women over the age of 17 in their instant offense. Accordingly, this variable is a proxy for adult rapists.

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-county metro area (i.e., Minneapolis, St. Paul, and surrounding suburbs) counties are Anoka, Carver, Dakota, Hennepin, Ramsey, Scott, and Washington. The remaining 80 counties were coded as non-metro area or Greater Minnesota counties.

Recent Disciplinary History: this variable measures the number of formal disciplinary convictions that an offender received in the final 12 months prior to his initial release from prison. Because sex offenders often serve relatively long sentences, disciplinary convictions at the end of the term of imprisonment may be a more valid predictor of post-release behavior than the total number of convictions throughout the full prison term.

Risk Score: because formal risk assessment data were not available for the full 14-year period over which offenders were released, the approach developed by Hanson, Broom, and Stephenson (2004) was followed through the creation of a recidivism risk score using the data available from the 10 preceding control variables. Offenders received a value of "1" if they were under the age of 30 at the time of release, had at least one institutional discipline conviction in the 12 months prior to release, or had a value of "1" for the remaining eight controls (minority race, prior felony conviction, prior sex crime conviction, stranger victims, acquaintance victims, male child victims, adult female victims, and metro area). Thus, the maximum total score was 10, while the minimum score was 0.

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

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: three dichotomous dummy variables were created to measure the level of post-release supervision to which offenders were released. The three variables were intensive supervised release (ISR) (1 = ISR, 0 = non-ISR); supervised release (SR) (1 = SR, 0 = non-SR); and discharge (1 = discharge or no su-

pervision, 0 = released to supervision). Discharge is the variable that serves as the reference in the statistical analyses.

Supervised Release Revocations (SRRs): the number of times during an offender's sex crime sentence when he returned to prison as a supervised release violator for a technical violation.

Broad Community Notification: dichotomized as either (1) broad community notification or (0) no broad community notification, this variable measures whether offenders were given a Level III risk level assignment prior to their release from prison and, thus, were subjected to broad community notification.

Release Year: measuring the year in which offenders were first released from prison for the instant sex offense, this variable is included to control for any unobserved differences between the 14 different release year cohorts from 1990-2003.

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, the statistical technique used was a Cox regression model, which utilizes both "status" and "time" variables in estimating the impact of the independent variables on recidivism. For the analyses presented here, the "status" variable was one of the recidivism variables mentioned above; e.g., sex crime rearrest, violent crime rearrest. The "time" variable, on the other hand, measured the amount of time (in days) from the date of release until the date of first rearrest, reconviction, reincarceration, or December 31, 2006, for those who did not recidivate.

To accurately measure the total amount of time an offender was actually at risk to reoffend (i.e., "street time"), it was necessary to account for instances in which an offender was not at risk to recidivate following release from prison. Failure to do so would bias the

findings by artificially increasing the lengths of offenders' at-risk periods. Accordingly, the time offenders spent in prison as supervised release violators was subtracted from their total at-risk period as long as it 1) preceded a reincarceration for a new offense, or 2) occurred prior to January 1, 2007 (the end of the follow-up period) for those who did not recidivate. In addition, when recidivism was defined as a sex reoffense, time spent in prison was deducted for offenders reincarcerated for either a violent or a non-sex reoffense.

Because civilly-committed offenders are incapacitated in a mental health institution, it was necessary to account for those who were civilly committed between September 1991—when Minnesota courts began more aggressively applying the civil commitment statute to released sex offenders—and December 31, 2006. Of the 3,533 sex offenders released from Minnesota prisons between 1990 and 2003, 93 were excluded because they were later civilly committed without ever spending any time in the community. Of these, 70 entered prison-based treatment, with 32 dropping out and the remaining 38 completing or participating until release. As expected, these offenders had a higher average risk score (4.82) than the other 3,440 offenders (3.89), which suggests that they had a greater recidivism risk than the sex offender population in general.

Included in the study were 54 offenders who had spent time in the community but had later been civilly committed following a return to prison for either a supervised release violation or a new crime. Offenders who returned to prison for a supervised release revocation were "right censored" at the time of their civil commitment; that is, their at-risk period ended when they were civilly committed. For offenders who were civilly committed following a reincarceration for a new offense, they were right censored at the time of their commitment if the offense type was different from the type of recidivism being measured (e.g., sexual or

violent). For example, when recidivism was measured as a violent reoffense, offenders were right censored at the time of their civil commitment following a return to prison for either a new sex or a non-sex crime.

Cox regression models were estimated for each of the nine recidivism measures for both treatment variables (participation and outcome). However, given that the reconviction and reincarceration results were substantively similar to those for rearrest for all three reoffense types, only the findings for rearrest are presented because it is the most sensitive recidivism measure. In addition, to determine whether there are certain types of offenders for whom treatment may be more effective, interaction models were estimated for each measure of recidivism. Similar to stepwise regression, all first-order interactions with treatment were examined and non-significant terms removed until only the significant interactions (at the .05 level) remained in the model.

RESULTS

As shown in Table 4, which breaks out recidivism rates by treatment participation and outcome, treated offenders had lower reoffense rates than untreated offenders for each of the three types of recidivism—sexual, violent, and general. Not surprisingly, the best recidivism outcomes were found for offenders who completed treatment or successfully participated until their release. These results suggest that the risk of recidivism may be significantly lowered by participating in prison-based treatment, especially for those who complete treatment or successfully participate until release. It is possible, however, that the observed recidivism differences between treated and untreated offenders as well as between treatment

Table 4. Three-Year and Total Recidivism Rates by Treatment Participation and Outcome

Recidivism	*Treatment Completers*	*Treatment Dropouts*	*Treatment Participants*	*Treatment Non-participant*
Sexual Rearrest				
Three Years	7.1% (51)	10.6% (32)	8.1% (83)	11.6% (118)
Total	13.4% (96)	16.2% (49)	14.2% (145)	19.5% (199)
Violent Rearrest				
Three Years	13.4% (96)	16.9% (51)	14.4% (147)	19.3% (197)
Total	29.0% (208)	35.1% (106)	30.8% (314)	34.1% (348)
General Rearrest				
Three Years	29.1% (209)	33.1% (100)	30.3% (309)	38.5% (393)
Total	55.4% (398)	59.3% (179)	56.6% (577)	58.1% (593)
N	718	302	1,020	1,020

completers and dropouts are due to other factors such as 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 measure of recidivism across both treatment variables (participation and outcome).

THE IMPACT OF TREATMENT ON SEX OFFENDER RECIDIVISM

For each measure of recidivism, two separate Cox regression models were initially run to estimate the effects of prison-based treatment. The first model, risk score, contained the ten-factor risk score measure along with the institutional and post-release controls. The individual predictor model, on the other hand, was similar to the risk score model except that it showed the unique effects of the ten predictors used to calculate the risk score. Because the results from the individual predictor models were similar to those from the risk score models for all three types of recidivism, only the findings from the risk score models are presented here.

Sexual Recidivism

The results shown in Table 5 indicate that, controlling for other factors, prison-based treatment significantly reduced the hazard ratio for a new sex offense rearrest, decreasing it by 27 percent. That is, sex offenders who participated in treatment recidivated less often and more slowly than untreated offenders; as a result, treated sex offenders survived longer in the community without committing a new sex offense (see Figure 2). In the individual predictor model, the hazard ratio was 28 percent lower for treatment participants.

Table 5. Cox Regression Model: Time to First Sex Offense Rearrest

Variables	*Coefficient*	*SE*	*Hazard Ratio*
Prison-Based Treatment	-0.317	0.110	0.729**
Risk Score	0.135	0.030	1.145**
Length of Stay (Months)	-0.010	3.0 E-3	0.990**
Supervision Length (Months)	-3.1 E-4	2.3 E-3	1.000
Intensive Supervised Release	-1.041	0.358	0.353**
Supervised Release	-1.484	0.327	0.227**
Supervised Release Revocations	-0.066	0.049	0.936
Community Notification	-1.242	0.589	0.289*
Release Year	-0.093	0.021	0.911**
N	2,040		

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

Although not shown in Table 5, Cox regression models were also estimated that analyzed the impact of treatment outcome on sexual recidivism. Compared to the untreated offenders, the effect of dropping out of treatment—either quitting or being terminated—was in the negative direction and did not have a significant effect on sexual recidivism. Completing treatment, however, did significantly decrease the risk (hazard) relative to not receiving treatment, reducing it by 33 percent in the risk score model and 34 percent in the individual predictor model.

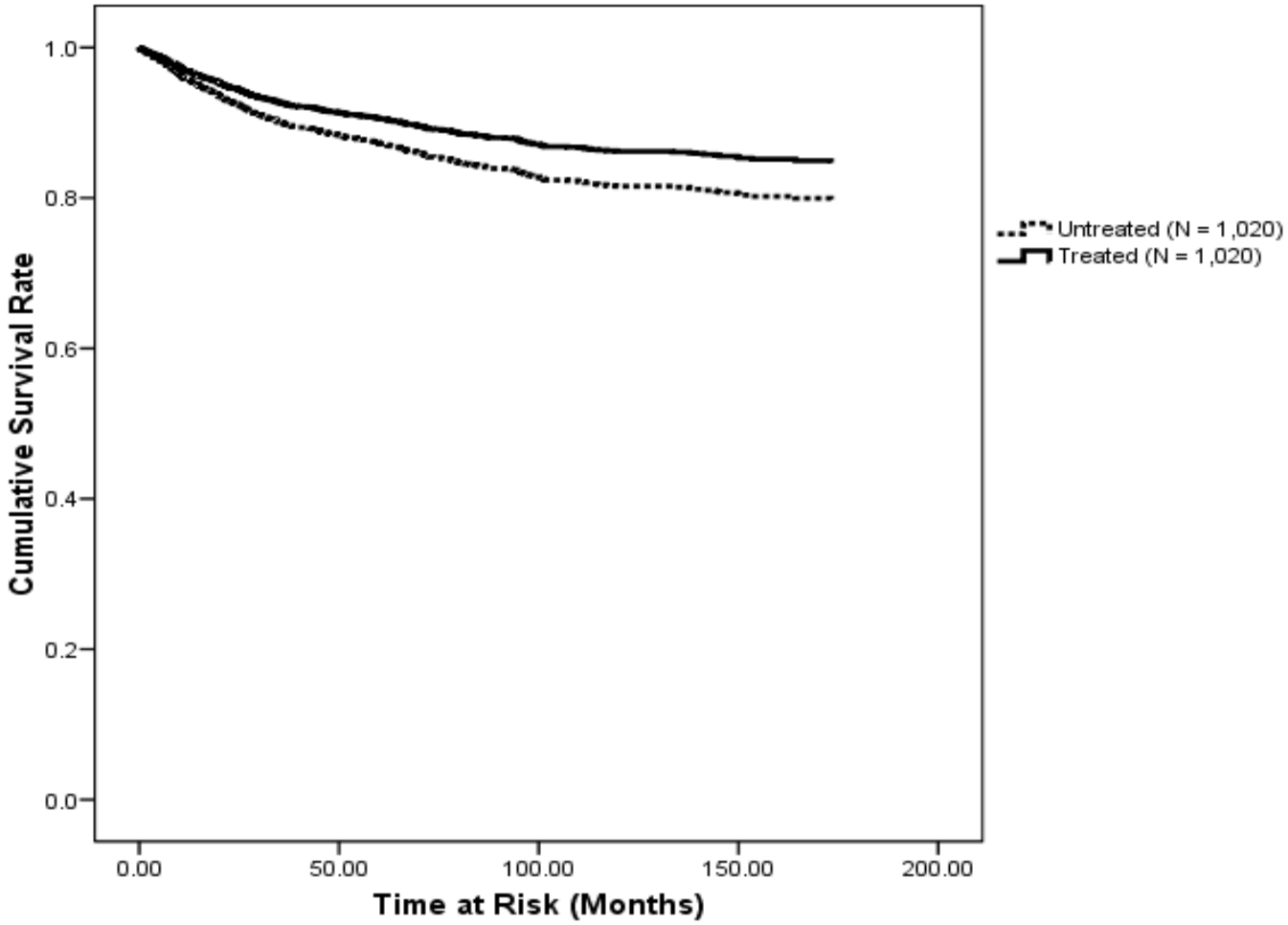

Figure 2. Survival Curves for Sexual Rearrest

Interactions between the controls and the two treatment variables (participation and outcome) were tested in both the risk score and individual predictor models, but none were statistically significant. The results from all four models, however, showed that longer lengths of stay in prison, post-release supervision (ISR or supervised release), broad community notification, and release year were associated with a reduced risk of rearrest. The findings from both risk score models (treatment participation and outcome) revealed that risk score was a significant predictor of sexual recidivism; in the treatment participation model, a one-unit increase in risk score increased the hazard ratio for sexual recidivism by 15 percent.

In the two individual predictor models, minority offenders, younger offenders, and prior sex crime convictions were significantly associated with an increased risk of sexual recidivism.

Violent Recidivism

The results in Table 6 show that treatment had a statistically significant impact on violent offense recidivism. The hazard ratio for a violent rearrest was 18 percent lower for treated sex offenders in the risk score model and 19 percent lower in the individual predictor model (also see Figure 3). Compared to the untreated offenders, completing treatment reduced the risk (hazard) by 23 percent in the risk score model and by 24 percent in individual predictor model, whereas the effect of dropping out of treatment was in the

Table 6. Cox Regression Model: Time to First Violent Offense Rearrest

Variables	*Coefficient*	*SE*	*Hazard Ratio*
Prison-Based Treatment	-0.194	0.079	0.824*
Risk Score	0.157	0.022	1.170**
Length of Stay (Months)	-3.5 E-3	2.0 E-3	0.996
Supervision Length (Months)	-4.1 E-3	1.7 E-3	0.996*
Intensive Supervised Release	-1.379	0.301	0.252**
Supervised Release	-1.426	0.284	0.240**
Supervised Release Revocations	0.169	0.029	1.184**
Community Notification	-0.531	0.282	0.588
Release Year	-0.039	0.015	0.962**
N	2,040		

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

negative direction and was not significant in either model. Similar to the sexual recidivism results, statistically significant interactions were not found between any of the controls and either treatment variable.

The findings from all four models (treatment participation/risk score, treatment participation/individual predictor, treatment outcome/risk score, and treatment out-

come/individual predictor) suggested that longer post-release supervision periods, post-release supervision (ISR and supervised release), and release year were significantly

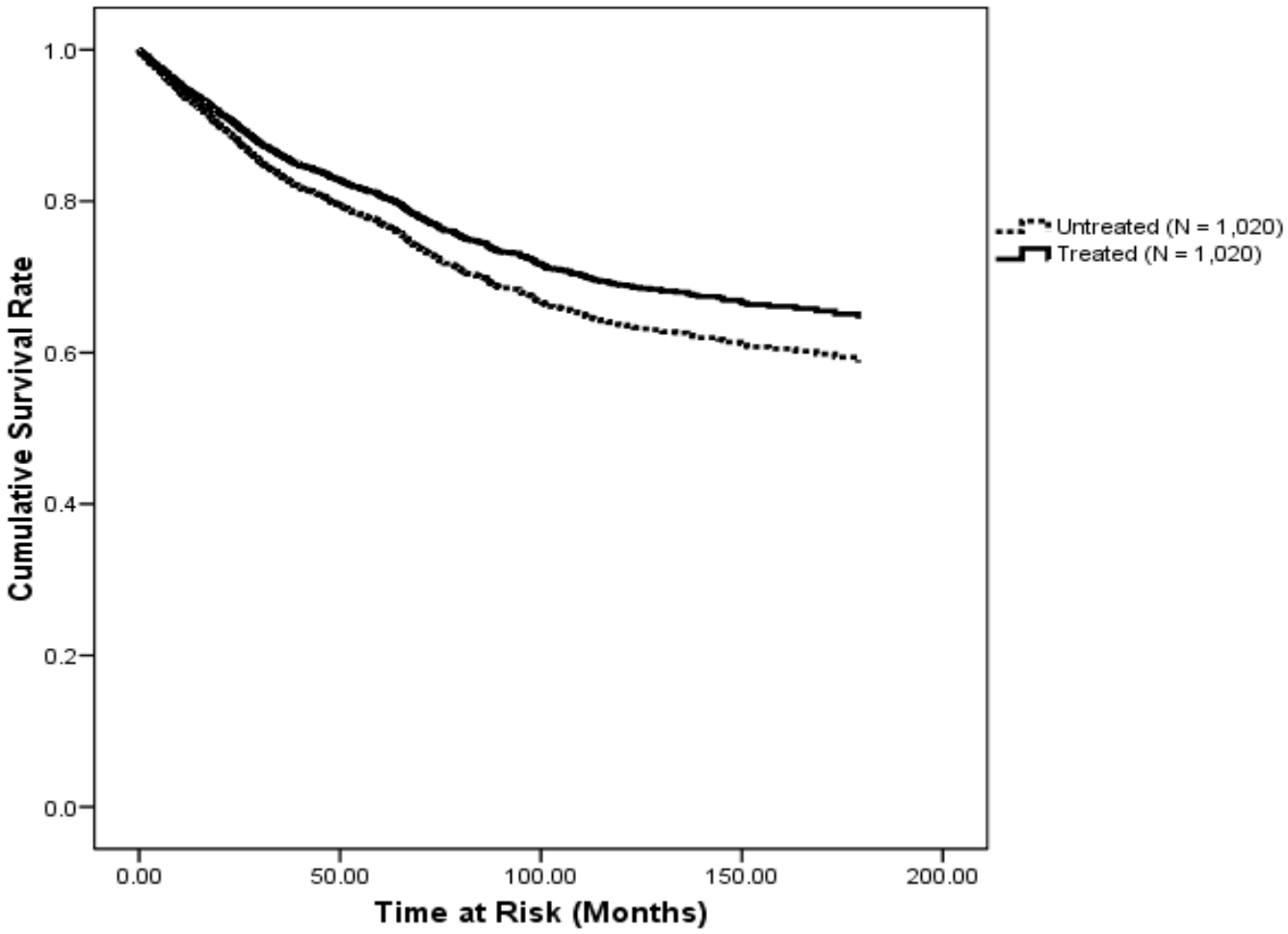

Figure 3. Survival Curves for Violent Rearrest

associated with a reduced risk of rearrest for a violent offense. Supervised release revocations, on the other hand, significantly increased the risk in all four models. Risk score was a significant predictor of violent recidivism in both treatment variable models, whereas minority offenders, younger offenders, and prior felony convictions significantly increased the hazard ratio for rearrest in the individual predictor models.

General Recidivism

As shown in Table 7, participating in treatment had a statistically significant effect on general recidivism, reducing the hazard ratio for rearrest for any offense by 12 percent (also see Figure 4). In the individual predictor model, the hazard ratio was 14 percent lower for treated offenders. The treatment outcome results suggest that, compared to untreated offenders, completing treatment significantly decreased the risk (hazard) for any offense by 15 percent in the risk score model and by 17 percent in the individual predictor model. The effect of dropping out of treatment, relative to not participating in treatment, was in the negative direction and was not statistically significant in both models. Once again, statistically significant interactions were not found between the controls and either treatment variable.

Table 7. Cox Regression Model: Time to First Rearrest for Any Offense

Variables	*Coefficient*	*SE*	*Hazard Ratio*
Prison-Based Treatment	-0.123	0.059	0.884*
Risk Score	0.198	0.016	1.219**
Length of Stay (Months)	-5.2 E-3	1.5 E-3	0.995**
Supervision Length (Months)	-3.9 E-3	1.3 E-3	0.996**
Intensive Supervised Release	-0.675	0.291	0.501*
Supervised Release	-0.536	0.281	0.585
Supervised Release Revocations	0.083	0.026	1.089*
Community Notification	-0.870	0.213	0.419**
Release Year	0.026	0.011	1.027*
N	2,040		

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

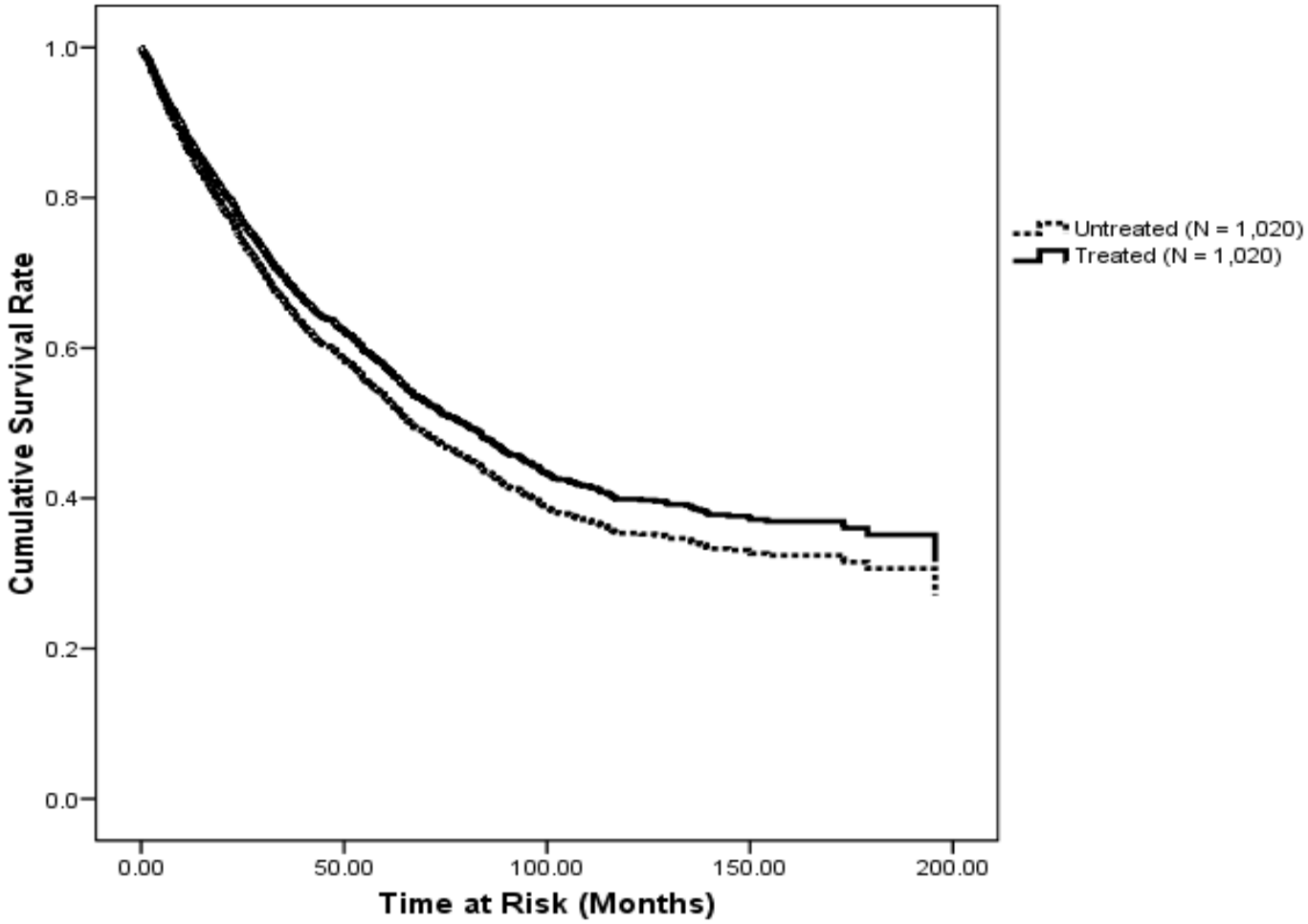

Figure 4. Survival Curves for General Rearrest

Longer periods of post-release supervision, longer lengths of stay in prison, ISR, and broad community notification significantly decreased the hazard ratio for rearrest in all four models, whereas release year and supervised release revocations were significantly and positively associated with recidivism risk. Risk score significantly increased the risk of rearrest in both treatment models. In the individual predictor models, minority offenders, younger offenders, prior felony convictions, a history of victimizing acquaintances, and recent institutional disciplinary convictions significantly increased the risk of general recidiv-

ism. In contrast, prior sex crime convictions and a history of victimizing male children significantly decreased the risk.

SENSITIVITY ANALYSES

Intent to Treat

The results presented above suggest that prison-based treatment in Minnesota significantly reduces sexual, violent, and general recidivism. But in using PSM to identify the untreated offenders most likely to refuse treatment, it is possible that this study underestimates the number of offenders who would have refused to enter treatment had it been offered to them. For example, treatment refusers (105) accounted for 6.6 percent of the offenders (1,598) who were offered treatment. Yet the 105 offenders matched to the treatment refusers made up 5.7 percent of those not offered treatment (1,842). If the rate of refusal was the same among the 1,842 not offered treatment, 121 offenders (6.6% of 1,842) would have refused a treatment offer. The results from the preceding analyses could be biased, then, to the extent that 16 additional offenders needed to be removed from the comparison group pool but were not.

To address potential treatment refuser bias, intent-to-treat (ITT) analyses were conducted based on whether offenders were offered treatment. ITT analysis does not measure the effectiveness of treatment administered insofar as treatment refusers are considered to be "treated" offenders. It can be used, however, to test whether the results are robust to possible treatment refuser bias.

The ITT analyses began by using PSM to individually match offenders not offered treatment with those who received a treatment offer. A logistic regression model was esti-

mated in which the dependent variable was a treatment offer (i.e., the 1,598 offenders offered treatment were assigned a value of "1" while the 1,842 untreated offenders not offered treatment received a value of "0"). The predictors were the 17 control variables (excluding risk score) described earlier. After obtaining propensity scores on the 3,440 offenders, a greedy matching procedure was used to individually match offenders from both groups. Using a caliper of 0.10, a sample of 2,224 was obtained in which all of the covariates were balanced. The number of matches (1,112) accounted for 70 percent of the total number of offenders offered treatment (N = 1,598). Moreover, of the 1,112 matched pairs, 85 were treatment refusers (81 percent of the 105 refusers).

Using Cox regression, risk score models were estimated for sexual, violent, and general recidivism. The results from these models showed that the hazard ratios for offenders offered treatment were significantly lower for all three types of recidivism. In particular, the hazard ratios were 19 percent lower for sexual recidivism (B = -0.209; SE = 0.101), 14 percent lower for violent recidivism (B = -0.156; SE = 0.073), and 16 percent lower for general recidivism (B = -0.175; SE = 0.056). Compared to the recidivism analyses reported in the previous section, the hazard ratios were smaller for sexual and violent recidivism but were slightly larger for general recidivism.

Rosenbaum Bounds

As indicated by the results from the ITT analyses, the treatment effects were robust against possible treatment refuser bias. Yet, given that PSM controls only for bias among the observed covariates, the possibility exists that unobserved selection bias may account for the significant treatment effects. Hidden bias can occur when two offenders with the same ob-

served 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 were 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, say, 1.5 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 especially robust to hidden bias. With a gamma value of 1.02, the general recidivism findings are the most sensitive to the possibility of hidden bias, followed by violent recidiv-

ism (gamma = 1.09) and sexual recidivism (gamma = 1.15). These results suggest that if an unobserved covariate that almost perfectly predicted general recidivism differed between matched pairs of treated and untreated offenders by a factor of 1.02 or more, it would be sufficient to undermine the conclusions regarding the treatment effect. To put this statistic in perspective, Length of Stay (LOS) would be a hidden bias equivalent in that, as shown earlier in Table 2, it had a comparable impact on the treatment selection process (b = 0.02). Therefore, if an unobserved covariate existed that perfectly predicted general recidivism and had an impact on the treatment selection process similar to LOS, it would be sufficient to invalidate the treatment effect for general recidivism. Furthermore, most of the significant predictors of treatment selection shown earlier in Table 2 had effect sizes (b > 0.15) that exceeded the gamma value for sexual recidivism (1.15), which was the least sensitive to possible hidden bias. 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 sex offender recidivism, none have yet to be shown to be a nearly perfect predictor of reoffending, which is what the Rosenbaum bounds approach assumes.

CONCLUSION

The results from this study suggest that prison-based treatment in Minnesota produces a significant, albeit relatively modest, reduction in sex offender recidivism. Indeed, entering treatment lowered the risk of rearrest for a new offense by 12 percent for general recidivism, 18 percent for violent recidivism, and 27 percent for sexual recidivism. The average sexual recidivism rate was 27 percent lower for treated offenders (14.2 percent) than for untreated

offenders (19.5 percent), which is similar to the reduction reported by Hanson et al. (2002) but lower than that (37 percent) reported by Lösel and Schmucker (2005) in their meta-analyses of the treatment evaluation literature. Moreover, the effect size for sexual recidivism (d = 0.21), which translates to an odds ratio of 1.46 (Lösel & Schmucker, 2005; Sánchez-Meca, Marín-Martínez, & Chacón-Moscoso, 2003), falls within the lower end of the range (d = 0.12-0.47) observed in previous meta-analytic reviews.

Dropping out of treatment did not significantly increase the risk of recidivism, although completing treatment lowered it for sexual rearrest by 33 percent, violent rearrest by 23 percent, and any arrest by 15 percent. In addition, treatment was not found to be significantly more or less effective for certain types of sex offenders. The absence of any significant interactions is important in its own right, however, for it suggests that treatment is similarly effective not only for adult rapists and child molesters, but also for incest offenders and those who victimize acquaintances or strangers.

Although the findings reported here support the notion that prison-based sex offender treatment is moderately effective in Minnesota, there are several limitations worth reiterating. First, due to a lack of data, this study did not account for the potential impact that community-based treatment may have had on recidivism. Prior research has shown that community-based treatment significantly lowers the extent to which sex offenders recidivate, particularly with regard to sex offenses (Aytes, Olsen, Zakrajsek, Murray & Ireson, 2001; Marshall & Barbaree, 1988; Marshall, Eccles & Barbaree, 1991; McGrath, Hoke & Vojtisek, 1998). It is possible, then, that the sexual recidivism differences between treated and untreated offenders may reflect variations in the extent to which each group participated in community-based

treatment; i.e., more prison-treated offenders may have participated in community-based treatment than untreated offenders.

Recall, however, that sex offenders who do not receive treatment while incarcerated are directed, as part of their post-release supervision conditions, to enter treatment while in the community. As a result, it is unlikely that prison-treated offenders were significantly more likely to participate in community-based treatment than offenders who were not treated in prison. If anything, sex offenders who were untreated in prison likely had a higher rate of participation in community-based treatment than prison-treated offenders. Accordingly, if community-based treatment is as effective in Minnesota as suggested by prior research, it may have moderated the observed effect for prison-based treatment. Therefore, the actual effect of prison-based treatment may be stronger than what was reported in this study.

Second, because this study did not use a randomized experimental design, some may argue that it does not provide an adequate assessment of the effectiveness of prison-based treatment—in Minnesota or in general. However, random assignment does not guarantee equivalence between treated and untreated offenders (Marques, Wiederanders, Day, Nelson & von Ommeren, 2005). Moreover, as Hanson, Broom, and Stephenson (2004) point out, no single study—regardless of how rigorous the design—is sufficient to determine whether treatment works. Instead, Hanson and colleagues argue that advances in the understanding of sex offender treatment will be made when individual studies improve, and the cumulative results from these studies are meaningfully integrated through meta-analyses. This study thus contributes to the advancement of the sex offender treatment literature by not only examining a relatively large number of sex offenders, but also by using multiple treatment and outcome measures, a lengthy follow-up period, a matching technique that controlled for

observable selection bias, and sensitivity analyses that addressed treatment refuser and hidden selection bias.

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APPENDIX

Univariate Relationships Between Control Variables and Recidivism

Control Variables	*Sexual Rearrest*	*Violent Rearrest*	*Any Rearrest*
Dichotomous Variables	Odds Ratios	Odds Ratios	Odds Ratios
Minority	1.002	1.658**	1.804**
Metro	1.314**	1.359**	1.346**
Prior Sex Crimes	1.363**	0.903	0.746**
Prior Felony	1.161	1.451**	1.997**
Stranger	1.369*	1.498**	1.246
Acquaintance	1.152	1.311**	1.676**
Adult Female	1.092	1.387**	1.427**
Male Child	1.272	0.762	0.608**
Intensive Supervised Release	0.788*	0.660**	0.640**
Supervised Release	1.082	1.331**	1.508**
Community Notification	0.608*	0.632*	0.476**
Ordinal/Interval Variables	AUC	AUC	AUC
Age at Release (Years)	0.360**	0.404**	0.455**
Length of Stay (Months)	0.467*	0.461**	0.425**
Discipline	0.543**	0.583**	0.595**
Supervision (Months)	0.443**	0.415**	0.438**
Supervised Release Revocations	0.522	0.579**	0.562**
Release Year	0.377**	0.371**	0.400**
Risk Score	0.564**	0.622**	0.649**
N	3,440	3,440	3,440

** $p < .01$

* $p < .05$

Notes: AUC = Area Under the Curve