

**AN OUTCOME EVALUATION OF MINNCOR'S EMPLOY
PROGRAM**



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RESEARCH SUMMARY

This study evaluated the effectiveness of EMPLOY, a prisoner reentry employment program, by examining recidivism and post-release employment outcomes among 464 offenders released from Minnesota prisons between 2006 and 2008. Because outcome data were collected on the 464 offenders through the end of June 2010, the average follow-up period was 28 months. Observable selection bias was minimized by using propensity score matching to create a comparison group of 232 non-participants who were not significantly different from the 232 EMPLOY offenders. Results from the Cox regression analyses revealed that participating in EMPLOY reduced the hazard ratio for recidivism by 32-63 percent. The findings further showed that EMPLOY increased the odds of gaining post-release employment by 72 percent. Although EMPLOY did not have a significant impact on hourly wage, the overall post-release wages for program participants were significantly higher because they worked a greater number of hours. The study concludes by discussing the implications of these findings.

INTRODUCTION

Offenders generally face a number of barriers in gaining employment after they leave prison. Often undereducated, inmates frequently lack vocational skills and a legitimate work history when they enter prison (Petersilia, 2003). Although the majority of state and federal prisons offer educational, employment, and vocational programming opportunities, research suggests that most prisoners do not participate in programming while incarcerated (Lynch and Sabol, 2001). Therefore, when offenders get released from prison, they tend to leave with the same educational and vocational deficits with which they arrived (Solomon, Dedel Johnson, Travis, and McBride, 2004). Yet, when prisoners attempt to find work after their release to the community, they often encounter the stigmatizing effects of a criminal record. Indeed, employers are generally reluctant to hire released prisoners, especially those who are minorities (Pager, 2003).

The potential importance of work as a buffer against crime and, more narrowly, recidivism has been recognized by the major theories within criminology. Social control theory, for example, posits that employment decreases the likelihood of crime by providing individuals with a stake in conformity and involvement in conventional activities (Hirschi, 1969). According to strain theory, work can reduce economic need and, therefore, strain by providing a legitimate means to achieve material success (Merton, 1938). Social learning and differential association theories also point out, however, that relationships with co-workers may inhibit criminal activity by fostering pro-social values, attitudes, and behaviors (Akers, 1998; Sutherland, 1947). Whereas rational choice theory suggests that work curbs crime by increasing the perceived benefits of conventional behavior (Becker, 1968), labeling theory argues that the stigma of criminality can increase the likelihood of future criminal offending

by limiting access to employment (Needels, 1996). Lastly, life course theory proposes that employment can provide a critical turning point in helping individuals desist from crime, particularly for older adults (Uggen, 2000).

Despite the theoretical salience of work as a protective factor against crime, relatively few evaluations have examined the effects of employment programming, either in prison or the community, on offender recidivism. In their meta-analysis of corrections-based educational, vocational, and work programs, Wilson, Gallagher, and MacKenzie (2000) were able to identify only four comparisons between offenders who participated in a correctional work/industry program and offenders who did not participate in this type of programming. Although the odds ratio for these four contrasts was 1.48, which amounts to a recidivism reduction of 20 percent, the effect was not statistically significant.

Among the correctional work/industry program evaluations analyzed by Wilson et al. (2000) were studies of New York's Prison Industry Research Project (PIRP) (Maguire, Flanagan, and Thornberry, 1988) and the Federal Bureau of Prison's Post Release Employment Project (PREP) (Saylor and Gaes, 1997). In their evaluation of PIRP, Maguire and colleagues did not find a statistically significant difference in recidivism between offenders who worked in prison industries and those who did not. Although the PIRP evaluation statistically controlled for a variety of inmate characteristics, it did not control for selection bias. In contrast, Saylor and Gaes (1997) used propensity score matching and a Cox proportional hazards model to control for rival causal factors, including selection bias and time at risk. Using a more sophisticated and rigorous design than the one employed by Maguire et al. (1988), Saylor and Gaes (1997) found that prison employment significantly lowered recidivism, increased employment, and reduced prison misconduct.

Compared to research on correctional work/industry programs, more evaluations have been published on community-based employment programming for offenders. The number of completed evaluations is still relatively small, however, as Visher, Winterfield, and Coggeshall (2005) analyzed only eight studies in their meta-analysis of this literature. On the basis of their findings, Visher and colleagues concluded that community employment programs do not have a significant effect on recidivism. Yet, given that only one of the studies (Rossman, Sridharan, Gouvis, Buck, and Morley, 1999) examined a contemporary sample of offenders, Visher et al. observed that these evaluations were mostly out-of-date. Furthermore, because there were wide differences among the offenders who participated in these programs and the type of programming delivered, Visher and colleagues cautioned against generalizing these findings to all employment programs for former prisoners.

Of the studies analyzed by Visher and colleagues, the one conducted by Uggen (2000) is notable for a few reasons. First, Uggen reanalyzed evaluation data from a program, the National Supported Work Demonstration (NSWD), that was considered to be ineffective by the initial evaluation (Piliavin and Gartner, 1981). Second, Uggen's reanalysis suggested that the effectiveness of the employment program depended on the age of participants. That is, for offenders over the age of 26, NSWD significantly reduced recidivism. The program had no effect, however, for those 26 and younger. Whereas work hastened the desistance from crime for older offenders, Uggen (2000) reasoned that it did not have the same effect for younger offenders because they are generally less attached to the labor market.

The overall evidence from the program evaluations is not overwhelmingly positive. Nevertheless, there are several considerations that work against drawing the conclusion that employment programming cannot lower offender recidivism. First, research suggests that

individuals are less likely to commit crime when they work more often (Uggen, 1999) and have employment that is stable (Crutchfield and Pitchford, 1997), considered satisfying (Uggen, 1999), and perceived as having career potential (Huiras, Uggen, and McMorris, 2000). Second, as noted above, evaluations of contemporary employment programs, regardless of whether they are based in prison or the community, are virtually non-existent. Finally, existing research has examined offender employment programs that have delivered services primarily either in prison or the community. To date, no evaluations have examined a program that focuses on providing a continuum of employment programming by delivering services in both the institution and the community. The findings from prisoner reentry research suggest that providing a continuity of programming from prison to the community can improve recidivism outcomes by fostering a more seamless reentry to society (Pullman, Kerbs, Koroloff, Veach-White, Gaylor, and Sieler, 2006; Taxman, 1998). Similarly, providing employment programming to offenders could yield more favorable outcomes when it is delivered not only in the institution but also in the community after they leave prison.

PRESENT STUDY

This study uses a retrospective quasi-experimental design to evaluate EMPLOY¹, a prisoner reentry employment program operated by the Minnesota Department of Corrections (MNDOC). The main goal of EMPLOY is to help program participants find and retain employment after they are released from prison. To this end, EMPLOY provides participants with employment assistance from the last several months of their confinement period through the first year following their release from prison. As such, EMPLOY is different not only from institutional-based programs that provide inmates with work opportunities or vocational

¹ EMPLOY is not an acronym but is the actual name of the program.

training while they are incarcerated, but also from community-based programs that focus on delivering services to offenders when they are out of prison.

This evaluation assesses the effectiveness of EMPLOY by comparing recidivism and post-release employment outcomes among 232 offenders who participated in the program and 232 offenders who were eligible but did not participate. The 464 offenders were released from Minnesota prisons between 2006 and 2008 and outcome data were collected through June 2010, resulting in an average follow-up period of 28 months. Like the Saylor and Gaes (1997) study, propensity score matching (PSM) was used to individually match the non-participants with those who entered EMPLOY.

In the following section, the EMPLOY program is described in greater detail. 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 criminological theory and correctional practice.

MINNESOTA'S EMPLOY PROGRAM: A DESCRIPTION

Minnesota prisons have provided inmates with employment opportunities since the late nineteenth century. In 1994, however, the state's prison industry program, MINNCOR, was formed to integrate and centralize administration and sales functions of the MNDOC's various industry operations. To help inmates capitalize on the work experiences and skills acquired as MINNCOR employees, the EMPLOY program was implemented in 2006. Designed to help offenders locate, gain, and retain employment at a livable wage, EMPLOY provides incarcerated participants with assistance to enhance their readiness for post-release employment and offers them community support for one full year following release from

prison. The program is entirely voluntary and offenders must complete and submit an application for consideration.

To be eligible for EMPLOY, inmates must be within the last five years of their sentence. In addition, offenders must have at least six months of current or prior MINNCOR work experience. If, however, an offender has been terminated on negative terms from a MINNCOR position, then s/he is ineligible for the program even if s/he has the requisite six months of experience. Offenders with at least six months of experience are still eligible, however, if they were terminated due to a layoff or transfer to another facility.

Because participants are held accountable for their actions, they are expected to have a clean discipline record. If participants are placed in segregation for disciplinary reasons within the last year of their confinement period or have any discipline convictions with the final six months, they are placed on a hold status and are required to submit a one-page letter explaining what they will do differently to ensure their future success. The letter must be received within one month of receiving their discipline letter in order for the participant to remain eligible for EMPLOY. Participants are given only one opportunity to explain any disciplinary action. If they receive any more disciplinary convictions prior to their release from prison, they are automatically dropped from the program.

After eligible offenders have been accepted into EMPLOY, they meet with a job training specialist approximately 60-90 days prior to their release date for two sessions, which each last eight hours. Under normal circumstances, these sessions are group meetings that involve 4-6 participants. During these meetings, the job training specialist covers material relating to skills assessments, resumes, job searching techniques, and interviewing

skills. To remain in good standing with EMPLOY, participants must attend the job training sessions and complete a resume prior to their release.

The week before a participant gets released from prison, a job development specialist will begin searching for job leads based on the participant's vocational skills and the geographic area where s/he will be released. The job development specialist will locate current open positions by searching the internet and making phone calls to employers. Potential employers are informed about the participant's vocational skills, work history, and criminal background. The job development specialist will also make further inquiries to determine if an employer has a blanket policy against hiring persons with felony backgrounds or if they exclude those with certain offenses (e.g., crimes committed against a vulnerable adult). In addition, employers are notified that they are eligible for the Work Opportunity Tax Credit if they decide to hire an EMPLOY participant. Further, employers are given information regarding the Minnesota Federal Bonding Service, which protects employers against employee theft of money or property.

As soon as participants get released from prison, a retention specialist schedules an appointment that takes place in the community. At this meeting, the retention specialist provides participants with a portfolio that contains copies of their resume, any certification submitted to EMPLOY, job leads, and any additional resources or tools (e.g., bus fare, interview clothing, supplies, etc.) to assist them with their job search. After the initial meeting, the retention specialist maintains contact with each participant and provides support and/or referral, as needed. The retention specialist conducts follow-up meetings with participants one month after their release and then again at 3, 6, and 12 months.

If participants do not keep in contact with the retention specialist, they are dropped from the program. Participants who maintain contact with the retention specialist for one full year after release are considered program completers regardless of whether they obtain employment. Some participants, however, may have less than 12 months remaining on their sentence at the time of their release from prison. Participants with less than a year of community supervision who maintain contact with the retention specialist until the end of their sentence are not considered program completers. Instead, they are regarded as offenders who successfully participated until the expiration of their sentence, irrespective of whether they were able to secure employment while participating in the program. Although the retention specialist does not meet with successful participants (different from the completers) for retention visits after their sentences have expired, EMPLOY staff provides ongoing support within the first year to those who request assistance after the expiration of their sentence.

DATA AND METHODOLOGY

This study uses a retrospective quasi-experimental design to determine whether EMPLOY has had an impact on recidivism and post-release employment.² The effectiveness of EMPLOY was evaluated by comparing recidivism and employment outcomes between EMPLOY participants and a matched comparison group of non-participants who were released from prison between July 2006 and December 2008. This 30-month period was selected because the initial EMPLOY participants began to be released from prison in July

² Although the data collected for this study did not require offender consent, a proposal for the evaluation was reviewed by the MNDOC's Research and Evaluation Advisory Committee and was ultimately approved by the commissioner of corrections.

2006. In addition, to allow a sufficient follow-up period for the recidivism and employment analyses, this study includes offenders released through 2008.

Between July 2006 and December 2008, there were 249 offenders who participated in EMPLOY. During this same 30-month period, there were 13,242 individual offenders released from Minnesota prisons who did not participate in EMPLOY. Of these offenders, 4,928 had been MINNCOR employees while incarcerated. Offenders who had fewer than six months of MINNCOR experience, had been in segregation during the final 12 months of their stay in prison, or had received discipline in the last six months of their prison term were removed from the population. Because pre-incarceration employment data were not available, the education/employment domain score on the Level of Service Inventory-Revised (LSI-R) was used as a proxy to control for pre-incarceration employment history. However, not all offenders, including some EMPLOY participants, had been administered an LSI-R. After excluding the offenders with missing LSI-R data as well as the non-participants who would have been ineligible due to discipline or lack of MINNCOR experience, there were 4,191 offenders in the sample. Of these offenders, 232 were EMPLOY participants while the remaining 3,959 offenders were eligible for EMPLOY but did not participate.

DEPENDENT VARIABLES

As discussed above, two main outcome measures—recidivism and post-release employment—were used to assess the effectiveness of EMPLOY. The following discusses how each outcome measure was operationalized.

Recidivism

In this study, recidivism was defined as a 1) rearrest, 2) reconviction, 3) reincarceration for a new sentence, or 4) revocation for a technical violation. It is important

to emphasize that the first three recidivism variables strictly measure new criminal offenses. In contrast, technical violation revocations (the fourth measure) represent a broader measure of rule-breaking behavior. Offenders can have their supervision revoked for violating the conditions of their supervised release. Because these violations can include activity that may not be criminal in nature (e.g., use of alcohol, failing a community-based treatment program, failure to maintain agent contact, failure to follow curfew, etc.), technical violation revocations do not necessarily measure reoffending.

Recidivism data were collected on offenders through June 30, 2010. Considering that offenders from both the EMPLOY and comparison groups were released between 2006 and 2008, the follow-up time for the offenders examined in this study ranged from 16-45 months. Data on arrests and convictions were obtained electronically from the Minnesota Bureau of Criminal Apprehension. Reincarceration and revocation data were derived from the Correctional Operations Management System (COMS) database maintained by the MNDOC. 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. More specifically, for the three recidivism variables that strictly measure new criminal offenses (rearrest, reconviction, and new offense reincarceration), it was necessary to deduct the amount of time they spent in prison for technical violation revocations from their total at-risk period. 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, to achieve a more accurate measure of “street time,” 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 prior to July 1, 2010.

Post-Release Employment

Data on post-release employment were obtained from the Minnesota Department of Employee and Economic Development (DEED). The main caveat with using these data is that it does not capture any labor (or compensation for that labor) not reported to DEED, which can occur in situations where employees are paid “under the table” for their labor. Still, the DEED data provide important information not only on whether offenders obtained employment, but also on how much they worked and the extent to which they were compensated. Because the employment data are compiled on a quarterly basis, information was not available on the specific date(s) when offenders entered and/or exited a job. As a result, the post-release employment measures included: 1) any employment (dichotomized as “1” for employment and “0” for no employment), 2) total number of hours worked, 3) hours worked per quarter, 4) total wages earned, and 5) hourly wage.

EMPLOY VARIABLE

The main objective of this evaluation is to determine whether EMPLOY has had an impact on recidivism and post-release employment. For this variable, EMPLOY participants were assigned a value of “1”, whereas those in the comparison group received a value of “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 recidivism and post-release employment. The following lists these variables and describes how they were created:

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

Offender Race: dichotomized as non-white (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.

LSI-R Score: the LSI-R is a risk assessment tool designed to predict an offender's risk of recidivism. In general, the higher an offender's LSI-R score, the greater the risk of recidivism. The total score, which ranges from a low of 0 to a high of 54, was used from the most recent LSI-R administered in prison prior to release.

LSI-R Education/Employment: because pre-incarceration employment history data were unavailable, this domain score derived from the LSI-R was used as a proxy to assess education and employment needs.

MINNCOR Time: the total number of months an offender had spent as a MINNCOR employee, including both current and prior commitments to prison.

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

Prior Supervision Failures: the number of prior revocations while under correctional supervision (probation or supervised release).

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.

Admission Type: three dummy variables were created to measure prison admission type. The three variables were new commitment (1 = new commitment, 0 = probation or release violator), probation violator (1 = probation violator, 0 = new commitment or release violator), and release violator (1 = release violator, 0 = new commitment or probation violator). Release violator serves as the reference in the statistical analyses.

Offense Type: five dummy variables were created to quantify offense type, which was the governing offense at the time of release. As the crime carrying the sentence on which the release date is based, the governing offense is generally the most serious offense for which an offender is incarcerated. 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 person offense variable serves as the reference in the statistical analyses.

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

General Equivalency Degree (GED) or High School Diploma (HSD): data were collected on whether offenders had earned a GED or HSD by the time they were released from prison.

This variable was dichotomized as GED/HSD (1) and no GED/HSD (0).

Chemical Dependency (CD) Treatment: this variable measures whether offenders had, by the time they were released from prison, entered CD treatment (1) or were untreated (0) during their current prison sentence.

Sex Offender Treatment: this variable measures whether offenders had, by the time they were released from prison, entered sex offender treatment (1) or were untreated (0) during their current prison sentence.

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

Type of Post-Release Supervision: five dummy variables were created to measure the level of post-release supervision to which offenders were released. The five 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); Challenge Incarceration Program (CIP) (1 = CIP, 0 = discharge), and discharge (1 = discharge or no supervision, 0 = released to supervision). Work release and CIP are early release programs operated by the MNDoc. Offenders placed on work release are subject to regular supervised release, whereas offenders who complete the institutional phase of CIP are placed on intensive supervised release. Supervised release serves as the reference in the statistical analyses.

Release Year: measuring the year in which offenders were first released from prison for the instant offense, this variable is included to control for any unobserved differences between the different release year cohorts from 2006-2008.

Supervised Release Revocations (SRRs): to control for the potential effects of technical violation revocations on reoffending, this measure was included in the models that specifically examined new criminal offenses (rearrest, reconviction, and reincarceration for a new offense). This variable measured the number of times an offender returned to prison as a

supervised release violator between the date of his/her release from prison and the date of his/her first reoffense (for those who reoffended), or June 30, 2010, (the end of the follow-up period) for those who did not reoffend.

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, 1985). 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, an advantage with using PSM is that it can simultaneously “balance” multiple covariates on the basis of a single composite score.

In matching EMPLOY participants with non-participants on the conditional probability of entering EMPLOY, PSM reduces selection bias by creating a counterfactual estimate of what would have happened to the EMPLOY offenders had they not participated in the program. PSM has several limitations, however, that are worth noting. 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 and 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 (26) as possible in the propensity score model. In addition, this study later demonstrates there was substantial overlap in propensity scores between the treated and untreated offenders. Further, the sample size limitation was addressed by assembling a large number of cases (N = 4,191) on which to conduct the propensity score analyses.

MATCHING EMPLOY PARTICIPANTS AND NON-PARTICIPANTS

Propensity scores were calculated for the 232 EMPLOY participants and the 3,959 non-participants in the comparison group pool by estimating a logistic regression model in which the dependent variable was participation in EMPLOY (see Table 1). The predictors were the 26 control variables used in the statistical analyses. Even though the difference in mean propensity score between EMPLOY participants and non-participants was statistically significant at the .01 level (see Table 2), there was substantial overlap in propensity scores. Indeed, the vast majority of offenders in both groups (91 percent for EMPLOY and 99 percent for non-EMPLOY) had propensity scores less than 0.20.

After obtaining propensity scores for the 4,191 offenders, a greedy matching procedure was used to match the EMPLOY offenders with the non-participants. Using a relatively narrow caliper of 0.10, matches were found for all 232 EMPLOY participants. 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

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

(1985) that quantifies the amount of bias between the treatment and comparison samples (i.e., standardized mean difference between samples), where \bar{X}_t and S_t^2 represent the sample mean and variance for the treated offenders and \bar{X}_c and S_c^2 represent the sample mean and

Table 1. Logistic Regression Model for EMPLOY Selection

<i>Predictors</i>	<i>Coefficient</i>	<i>Standard Error</i>
Male	-1.154**	0.196
Non-White	-0.012	0.155
Age at Release (years)	0.008	0.008
LSI-R Score	-0.036*	0.014
Education/Employment	-0.017	0.040
MINNCOR Time	-0.006*	0.002
Prior Felony Convictions	0.018	0.023
Prior Supervision Failures	0.081	0.056
Metro Commit	0.229	0.154
Admission Type		
New Commitment	0.437	0.277
Probation Violator	0.341	0.299
Offense Type		
Criminal Sexual Conduct	0.098	0.290
Property	-0.208	0.238
Drugs	0.04	0.220
Felony DWI	-0.194	0.376
Other	-0.517	0.295
Institutional Discipline	-0.046*	0.023
GED or HSD at Release	0.707*	0.355
Entered Drug Treatment	-0.254	0.194
Entered Sex Offender Treatment	-0.183	0.454
Length of Stay (months)	0.006*	0.003
Supervision Type		
ISR	0.493*	0.205
Work Release	1.051*	0.211
CIP	-0.497	0.414
Discharge	0.545	0.375
Release Year	0.453**	0.105
Constant	-912.172	209.837
N	4,191	
Log-likelihood	1646.793	
Nagelkerke R ²	0.099	

** $p < .01$

* $p < .05$

Table 2. Propensity Score Matching and Covariate Balance for EMPLOY Participation

<i>Variable</i>	<i>Sample</i>	<i>EMPLOY Mean</i>	<i>Non-EMPLOY Mean</i>	<i>Bias (%)</i>	<i>Bias Reduction</i>	<i>t test p Value</i>
Propensity Score	Total	9.71%	5.29%	49.39		0.00
	Matched	9.71%	9.75%	0.38	-99.22%	0.96
Male	Total	78.45%	89.90%	24.67		0.00
	Matched	78.45%	81.03%	5.19	-78.95%	0.49
Non-White	Total	46.12%	46.63%	0.83		0.88
	Matched	46.12%	42.67%	5.65	577.83%	0.46
Age at Release (years)	Total	37.03	35.29	15.37		0.01
	Matched	37.03	37.52	4.14	-73.06%	0.59
LSI-R Score	Total	24.76	27.01	24.06		0.00
	Matched	24.76	24.78	0.14	-99.43%	0.99
Employment/Education Domain	Total	4.47	5.21	23.11		0.00
	Matched	4.47	4.63	4.84	-79.04%	0.52
MINNCOR Time	Total	36.78	40.23	7.26		0.22
	Matched	36.78	40.76	8.36	15.26%	0.28
Prior Felony Convictions	Total	3.24	3.44	3.74		0.47
	Matched	3.24	3.03	3.77	0.77%	0.62
Prior Supervision Failures	Total	0.93	0.97	2.29		0.69
	Matched	0.93	0.96	1.53	-33.35%	0.84
Metro	Total	60.78%	53.90%	11.41		0.04
	Matched	60.78%	65.52%	7.98	-30.04%	0.29
New Commit	Total	64.66%	59.33%	9.00		0.11
	Matched	64.66%	64.22%	0.75	-91.69%	0.92
Probation Violator	Total	23.28%	23.04%	0.46		0.93
	Matched	23.28%	26.29%	5.72	1133.59%	0.45
Sex Offenders	Total	10.34%	8.21%	5.90		0.25
	Matched	10.34%	10.78%	1.17	-80.16%	0.88
Property Offenders	Total	21.98%	23.21%	2.41		0.67
	Matched	21.98%	16.81%	10.51	336.51%	0.16
Drug Offenders	Total	29.74%	27.18%	4.61		0.40
	Matched	29.74%	29.31%	0.77	-83.34%	0.92
DWI Offenders	Total	5.17%	6.72%	5.46		0.36
	Matched	5.17%	6.47%	4.61	-15.60%	0.55
Other Offenders	Total	7.33%	12.50%	14.77		0.02
	Matched	7.33%	5.60%	5.63	-61.90%	0.45
Institutional Discipline	Total	2.60	2.84	5.35		0.34
	Matched	2.60	2.34	6.06	13.21%	0.42
GED/HSD	Total	96.12%	91.92%	15.44		0.02
	Matched	96.12%	96.98%	3.79	-75.46%	0.61
Entered Drug Treatment	Total	21.98%	25.23%	6.30		0.27
	Matched	21.98%	25.86%	7.49	18.94%	0.33
Entered Sex Offender Treatment	Total	3.02%	2.88%	0.68		0.90
	Matched	3.02%	3.02%	0.00	-100.00%	1.00
Length of Stay (months)	Total	31.26	25.25	17.69		0.00
	Matched	31.26	36.63	12.33	-30.30%	0.14
Intensive Supervised Release	Total	24.57%	22.81%	3.36		0.54
	Matched	24.57%	25.86%	2.43	-27.82%	0.75
Work Release	Total	16.81%	5.46%	27.86		0.00
	Matched	16.81%	14.66%	4.77	-82.87%	0.53
CIP	Total	3.45%	4.72%	5.39		0.37
	Matched	3.45%	4.74%	5.48	1.57%	0.48
Discharge	Total	5.60%	7.20%	5.44		0.36
	Matched	5.60%	3.45%	8.16	49.91%	0.27
Release Year	Total	2007.42	2007.15	32.62		0.00
	Matched	2007.42	2007.44	2.70	-91.74%	0.73

Total EMPLOY N = 232

Total Non-EMPLOY N = 3,959

Matched EMPLOY N = 232

Matched Comparison N = 232

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 the EMPLOY and non-EMPLOY offenders by 99 percent. Whereas the p value was 0.00 in the unmatched sample, it was 0.96 in the matched sample. In the unmatched sample, there were five covariates that were significantly imbalanced (i.e., the bias values exceeded 20). But in the matched sample, covariate balance was achieved given that no covariates had bias values greater than 20.

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, technical violation revocation, or June 30, 2010, for those who did not recidivate. The “status” variable, meanwhile, measures whether an offender recidivated (rearrest, reconviction, reincarceration for a new crime, and technical violation revocation) 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 four recidivism measures. In addition, to determine whether the effectiveness of EMPLOY was dependent on offender age, interaction models were estimated for each measure of recidivism.

As noted above, the DEED data are compiled on a quarterly basis and, thus, do not provide specific information on the specific date(s) when offenders entered and/or exited

employment. Because employment start date information would be needed to use Cox regression, multivariate logistic regression was used to assess the impact of EMPLOY on finding any employment. Considering that logistic regression assumes the lengths of follow-up periods do not vary among offenders, the follow-up period was capped at 12 months, or four quarters, for all offenders (i.e., for the most recently released offenders, four was the maximum number of quarters for which DEED data were available). Because the four remaining employment variables (total numbers of hours worked, hours worked per quarter, total wages earned, and hourly wage) were ratio-level measures, ordinary least squares (OLS) regression was used to estimate the impact of EMPLOY on these four outcomes.

RESULTS

Of the 232 EMPLOY participants, 65 (28 percent) completed the program, 43 (19 percent) successfully participated until their sentence expired, and the other 124 (53 percent) dropped out. Of the dropouts, 49 were dropped prior to their release from prison and the

Table 3. Recidivism and Employment by EMPLOY Participation and Outcome

<i>Outcomes</i>	<i>Comparison</i>	<i>EMPLOY</i>	<i>Completed</i>	<i>Successfully Participated</i>	<i>Dropped Out</i>
<u>Recidivism</u>					
Rearrest	50%	42%	28%	37%	51%
Reconviction	31%	25%	14%	16%	34%
Reincarceration	14%	9%	3%	5%	14%
Revocation	38%	21%	8%	5%	33%
<u>Employment</u>					
Employment	67%	76%	91%	81%	67%
Avg. Total Hours	904	1,288	1,787	1,412	984
Avg. Hrs./Quarter	137	185	255	171	153
Avg. Total Wages	\$11,478	\$16,523	\$23,033	\$19,655	\$12,025
Avg. Hourly Wage	\$8.46	\$8.94	\$11.80	\$9.49	\$7.25
N	232	232	65	43	124

remaining 75 were dropped post-release in the community. The main reasons why the 49 dropped out prior to release include a refusal of services provided, failure to complete a resume, getting fired from a MINNCOR job, and institutional discipline. Among the 75 post-release dropouts, failure to maintain contact was the main reason for dropping out.

Compared to the non-participants, offenders who entered EMPLOY had lower rates of recidivism for all four measures, had a higher rate of post-release employment, earned more total wages, and worked a greater number of hours. As shown in Table 3, which breaks out the recidivism and employment data by EMPLOY outcome (completed, successfully participated until expiration, or dropped out), offenders who completed EMPLOY had the lowest recidivism rates followed closely by those who successfully participated until the expiration of their sentence. In contrast, we see that recidivism rates were higher, but relatively similar, for the offenders who dropped out and those in the comparison group.

The post-release employment data yield similar outcomes. At 91 percent, completers had the highest employment rate, followed closely by those who successfully participated until discharge (81 percent).³ In addition, we see that, on average, completers worked the greatest number of hours, earned more total wages, and had the highest hourly wage. The offenders who successfully participated until discharge had the next highest averages for

³ It is worth noting that because the DEED data include only those instances in which employers reported offenders' employment to the state, the employment rates presented in Table 3 likely underestimate the true employment rates. For example, data collected by MINNCOR staff on program completers and successful participants revealed that some offenders who were, according to the DEED data, unemployed during the first year had actually obtained employment, which MINNCOR staff verified with these offenders' employers. Because similar data were unavailable for EMPLOY dropouts and offenders in the comparison group, it is unknown whether there was much underreporting for these offenders and, if so, to what extent.

these three measures. The post-release employment data were similar, however, for the drop outs and the offenders in the comparison group.

These findings suggest that participation in EMPLOY, particularly those that complete or successfully participate, may have an impact on both recidivism and post-release employment. It is possible, however, that the observed recidivism and employment differences between the EMPLOY and comparison group offenders 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 four recidivism measures. In addition, logistic and OLS regression models were estimated to assess the impact on post-release employment.

THE IMPACT OF EMPLOY ON RECIDIVISM

The results in Table 4 indicate that, controlling for the effects of the other independent variables in the statistical model, participation in EMPLOY significantly reduced the hazard ratio for all four recidivism measures (rearrest, reconviction, reincarceration for a new offense, and technical violation revocation). Because EMPLOY offenders recidivated less often and more slowly than those in the comparison group, they survived longer in the community without committing a new offense or getting revoked for a technical violation. In particular, participation in the EMPLOY decreased the hazard by 35 percent for rearrest, 32 percent for reconvictions, 55 percent for reincarcerations for a new crime, and 63 percent for technical violation revocations.

The results also showed that the hazard ratio was significantly greater for males (all three reoffending measures), younger offenders (all four measures), offenders with higher LSI-R scores (reconviction and reincarceration), offenders with a metro-area county of commitment (rearrest), offenders with prior felony convictions (all three reoffense measures),

Table 4. Cox Regression Models: Impact of EMPLOY on Time to First Recidivism Event

	<i>Rearrest</i>		<i>Reconviction</i>		<i>Reincarceration</i>		<i>Revocation</i>	
	<u>Hazard Ratio</u>	<u>SE</u>	<u>Hazard Ratio</u>	<u>SE</u>	<u>Hazard Ratio</u>	<u>SE</u>	<u>Hazard Ratio</u>	<u>SE</u>
EMPLOY	0.652**	0.145	0.678*	0.190	0.451**	0.307	0.368**	0.186
Male	1.763*	0.221	2.759**	0.317	3.17*	0.572	1.315	0.275
Non-White	1.052	0.155	0.923	0.204	1.021	0.329	1.228	0.187
Age at Release (years)	0.953**	0.010	0.972*	0.012	0.938**	0.022	0.968**	0.012
LSI-R Score	1.032*	0.013	1.018	0.017	1.06*	0.027	1.021	0.017
Education/Employment	0.963	0.038	0.996	0.048	0.883	0.076	0.984	0.049
MINNCOR Time	1.002	0.002	0.999	0.003	0.998	0.004	1.000	0.003
Prior Felony Convictions	1.066**	0.017	1.061**	0.021	1.147**	0.029	1.038	0.023
Prior Supervision Failures	1.237**	0.048	1.177*	0.063	1.268*	0.095	1.178**	0.058
Metro Commit	1.399*	0.168	1.170	0.216	1.311	0.373	1.154	0.207
Admission Type								
New Commitment	0.894	0.293	0.874	0.360	1.031	0.623	1.040	0.359
Probation Violator	1.067	0.307	0.907	0.383	0.827	0.698	1.083	0.380
Offense Type								
Criminal Sexual Conduct	0.745	0.359	0.932	0.434	1.566	0.698	1.801	0.367
Property	1.450	0.246	1.233	0.307	0.995	0.504	1.804	0.314
Drugs	1.563	0.231	0.955	0.293	1.142	0.493	1.414	0.311
Felony DWI	0.940	0.396	0.790	0.499	1.153	0.813	1.478	0.510
Other	0.623	0.374	0.670	0.463	0.367	1.069	1.128	0.438
Institutional Discipline	1.044	0.024	1.036	0.037	0.987	0.063	1.034	0.028
GED or HSD at Release	0.995	0.417	1.121	0.544	0.956	0.697	1.229	0.523
Entered Drug Treatment	1.504	0.213	1.737	0.292	2.408	0.497	1.192	0.277
Entered Sex Offender Treatment	0.350	0.633	0.213	1.063	0.000	425.104	0.549	0.669
Length of Stay (months)	0.991*	0.004	0.985*	0.007	0.994	0.010	0.997	0.004
Supervision Type								
ISR	1.053	0.227	0.904	0.287	1.057	0.506	1.556	0.281
Work Release	1.146	0.208	1.123	0.272	2.146	0.401	3.286**	0.229
CIP	0.267*	0.557	0.308	0.781	0.000	383.164	0.192	1.049
Discharge	0.653	0.409	1.110	0.460	1.051	0.781	0.000	165.866
Release Year	0.880	0.120	0.937	0.155	0.863	0.270	0.656**	0.140
Supervised Release Revocations	0.734	0.165	1.146	0.163	0.866	0.266		
N	464		464		464		464	

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

offenders with prior supervision failures (all four measures), offenders with shorter lengths of stay in prison (rearrest and reconviction), and those who were placed on work release (technical violation revocation). The risk (hazard) was significantly less, however, for offenders who participated in CIP (rearrest) and those released more recently (release year) from prison (technical violation revocation).⁴

⁴ It is unclear why release year was a significant predictor for technical violation revocations and, as shown later, for several of the post-prison employment measures. It is possible that

Although models for each measure of recidivism were estimated that tested for an interaction between EMPLOY and age at release, none of the interaction terms were statistically significant. To further test for whether the effectiveness of EMPLOY depended on offender age, the age at release variable was modified so that offenders 26 and older were given a value of “1” and those under 26 were assigned a value of “0”. Even with this modification, however, none of the interaction terms reached statistical significance.

THE IMPACT OF EMPLOY ON POST-RELEASE EMPLOYMENT

The results from the multivariate logistic regression model, which are shown in Table 5, reveal that participation in EMPLOY significantly increased the chances of securing employment within the first 12 months after release from prison by 72 percent. The findings also show that offenders with a GED or high school degree at the time of release were four times more likely to gain employment, whereas those offenders placed on work release were 2.5 times more likely. The odds of finding a job were significantly greater for younger offenders and those with an earlier release year.

The effects of EMPLOY on total wages, hourly wage, total hours worked, and hours worked per quarter are presented in Table 6. Controlling for the effects of the other predictors, EMPLOY participants earned more than \$5,400 than offenders in the comparison group following their release from prison. Offenders who participated in CIP and those who

the better outcomes for the earliest participants are due to an early program effect in which there was greater energy, enthusiasm, and dedication among staff and participants alike during the initial start-up phase. It is also possible, however, that the better outcomes, particularly for employment, reflect changes in the economy. In particular, more recent releases in both groups (EMPLOY and comparison) entered the community and were trying to find employment under worse economic conditions than those offenders who were released in 2006 or 2007.

were released from prison earlier during the release period earned more total wages.

Conversely, total wages were significantly less for offenders with higher LSI-R scores.

Table 5. Logistic Regression Model for Post-Release Employment

<i>Predictors</i>	<i>Odds Ratio</i>	<i>Standard Error</i>
EMPLOY	1.715*	0.228
Male	0.928	0.337
Non-White	0.950	0.249
Age at Release (years)	0.964**	0.013
LSI-R Score	0.982	0.022
Education/Employment	0.965	0.062
MINNCOR Time	1.000	0.004
Prior Felony Convictions	0.954	0.033
Prior Supervision Failures	0.969	0.087
Metro Commit	0.949	0.260
Admission Type		
New Commitment	0.736	0.507
Probation Violator	0.669	0.530
Offense Type		
Criminal Sexual Conduct	0.620	0.457
Property	0.949	0.396
Drugs	0.975	0.353
Felony DWI	1.303	0.654
Other	1.150	0.515
Institutional Discipline	0.988	0.039
GED or HSD at Release	4.052*	0.614
Entered Drug Treatment	1.598	0.351
Entered Sex Offender Treatment	2.610	0.881
Length of Stay (months)	1.002	0.004
Supervision Type		
ISR	1.138	0.333
Work Release	2.582*	0.386
CIP	0.515	0.661
Discharge	0.307	0.671
Release Year	0.530**	0.186
Constant		373.795
N	464	
Log-likelihood	488.180	
Nagelkerke R ²	0.190	

** $p < .01$

* $p < .05$

Table 6. OLS Regression Models: Impact of EMPLOY on Post-Release Employment

Predictors	Total Wages (N = 464)		Hourly Wage (N = 464)		Total Hours (N = 464)		Hours Per Quarter (N = 464)	
	B	SE	B	SE	B	SE	B	SE
EMPLOY	5432.407**	1917.318	0.586	1.397	408.322**	123.752	52.730**	13.784
Male	1251.426	2773.278	2.342	2.021	-95.471	178.999	13.987	19.937
Non-White	-3860.061	2122.192	-1.467	1.546	-213.295	136.975	-32.761*	15.257
Age at Release (years)	-51.341	113.187	0.066	0.082	-3.103	7.306	-0.919	0.814
LSI-R Score	-545.979**	188.064	-0.209	0.137	-35.452**	12.138	-3.963**	1.352
Education/Employment	-396.251	528.363	0.456	0.385	-16.190	34.103	-1.641	3.798
MINNCOR Time	-34.861	31.657	-0.020	0.023	-2.021	2.043	0.008	0.228
Prior Felony Convictions	17.599	295.059	-0.222	0.215	-3.648	19.044	-1.455	2.121
Prior Supervision Failures	-858.463	780.362	-0.386	0.569	-42.969	50.368	-6.523	5.610
Metro Commit	-928.935	2189.729	-0.859	1.595	-98.786	141.334	0.582	15.742
Admission Type								
New Commitment	-1876.972	4189.416	0.039	3.052	-261.237	270.402	-40.106	30.118
Probation Violator	4372.583	4401.694	-0.884	3.207	102.262	284.103	-21.161	31.644
Offense Type								
Criminal Sexual Conduct	-700.252	4048.56	-1.196	2.950	-138.668	261.311	-25.636	29.105
Property	-2305.992	3387.912	0.728	2.468	-238.593	218.670	-15.171	24.356
Drugs	-2052.802	2984.429	1.874	2.174	-155.015	192.627	-4.769	21.455
Felony DWI	-3122.264	5073.876	0.497	3.697	-34.667	327.489	20.831	36.476
Other	1938.078	4304.847	1.945	3.137	140.735	277.852	26.164	30.948
Institutional Discipline	-188.734	323.738	-0.122	0.236	-9.382	20.895	-0.463	2.327
GED or HSD at Release	8596.877	5460.905	5.125	3.979	674.546	352.469	76.479	39.259
Entered Drug Treatment	-3541.865	2744.798	-0.607	2.000	-280.635	177.161	15.112	19.733
Entered Sex Offender Treatment	7908.441	6479.072	3.007	4.721	915.246*	418.186	83.161	46.579
Length of Stay (months)	40.540	37.197	0.001	0.027	2.983	2.401	0.221	0.267
Supervision Type								
ISR	1345.558	2844.898	0.193	2.073	53.577	183.621	32.150	20.452
Work Release	5598.356	2862.037	0.493	2.085	503.525**	184.728	56.882**	20.575
CIP	15115.302**	5600.072	-2.002	4.080	1133.167**	361.452	60.979	40.259
Discharge	-8343.535	5931.419	-3.250	4.322	-667.917	382.838	-76.647	42.641
Release Year	-6495.439**	1483.757	-0.232	1.081	-445.258	95.768	-21.507*	10.667
Constant	1.31E+07**	2977877.88	469.968	2169.708	895593.08*	192204.459	43394.914*	21408.178
Adjusted R ²	0.150		0.042		0.172		0.195	

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

Participation in EMPLOY did not have a significant effect on hourly wage. The results from this model show that none of the covariates had a statistically significant effect on hourly wage. Compared to the non-participants, EMPLOY offenders worked 408 more hours in the follow-up period, net of the effects of the control variables in the model. Offenders who participated in prison-based sex offender treatment, work release, and CIP worked significantly more hours, whereas LSI-R scores were negatively associated with total hours worked. To help control for varying lengths of follow-up periods for offenders, a model was estimated for hours worked per quarter. The results show that, controlling for the other predictors, EMPLOY participants worked nearly 53 more hours per quarter than offenders in the comparison group. Minorities, offenders with higher LSI-R scores, and more recent releases from prison (release year) worked significantly fewer hours per quarter, whereas work release significantly increased the number of hours employed per quarter.

CONCLUSION

In evaluating the effectiveness of EMPLOY, this study was limited by the absence of data on pre-incarceration employment history. Because offenders who participated in EMPLOY may have had more extensive pre-prison legitimate work histories than those in the comparison group, the findings obtained for post-release employment may be due to this difference rather than to the effects of the program itself. Despite this limitation, however, the findings suggest that, on the whole, EMPLOY is an effective employment program for released prisoners. Offenders who participated in EMPLOY earned more total wages than those in the comparison group because they were not only more likely to find employment, but they were also more likely to work more hours. Participation in EMPLOY did not have a significant effect on hourly wage, however, which is not surprising given that the main

objective of the program is to provide offenders with assistance in finding and retaining a job, not necessarily a higher paying job. Yet, the lack of a significant effect for hourly wage also undermines the argument that EMPLOY had an impact on post-release employment simply because participants were more skilled or had more impressive prior work histories than offenders in the comparison group. If true, then it would be reasonable to expect that this pre-incarceration difference, if it exists, would result in a significantly higher hourly wage for the EMPLOY group, which was not the case.

Although offenders from the EMPLOY and comparison groups were not matched on prior work history, they were matched on prior criminal history in addition to a host of other covariates. The results showed that EMPLOY lowered the hazard ratio by 32-55 percent across the three types of recidivism that strictly measured new criminal offending. The largest effect, however, was for technical violation revocations, as participation in EMPLOY significantly reduced the risk of revocation by 63 percent. These results translate into odds ratios of 1.41 for rearrest, 1.38 for reconviction, 1.65 for new offense reincarceration, and 2.43 for revocation, which can, in turn, be converted into Cohen's *d* values of 0.19 for rearrest, 0.18 for reconviction, 0.28 for new offense reincarceration, and 0.49 for revocation.

Why did EMPLOY increase the likelihood of employment and lower the risk of recidivism? For various reasons, there is seldom a strong connection between programming delivered in the institution and that provided in the community after an offender gets released from prison. As offenders transition from prison to the community, the fragmented service delivery system makes it difficult to produce a seamless provision of programming. Findings from the prisoner reentry literature suggest, however, that providing a continuum of care from prison to the community is critical in helping offenders successfully reenter society

(Pullman et al., 2006; Ventura Miller and Miller, in press). Operating on the continuum of care principle, EMPLOY not only provides post-release employment assistance to offenders while they are in the institution, but also during the first year after they are released from prison. The results from this evaluation suggest that providing programming in both the institution and the community over a relatively lengthy period of time (approximately 15 months) helped offenders find and retain employment.

The findings further suggest that by increasing the extent to which offenders found and maintained employment, EMPLOY reduced recidivism. That employment lowered the risk of recidivism is consistent with the growing body of literature that has demonstrated the protective effects of employment against crime. While employment reduces economic need, it also expands informal social control by giving individuals a greater stake in conformity and involvement in conventional activities, which inhibit opportunities for criminal behavior. Further, associating with others who are employed increases the likelihood that offenders will develop or maintain pro-social values, beliefs, and attitudes.

But given the absence of significant interaction effects for offender age, the findings did not provide strong support for the notion derived from age-graded life-course theory that EMPLOY would be effective only for older offenders. Instead, the findings suggest that EMPLOY significantly reduced recidivism regardless of offender age. It is worth pointing out, however, that offenders under the age of 26 who participated in EMPLOY had higher rearrest (52 percent) and reconviction (30 percent) rates than those from the comparison group (45 percent rearrested and 24 percent reconvicted) in the same age cohort. Despite the lack of statistical significance for the EMPLOY-age interaction effects, particularly for

rearrest and reconviction, this is an issue that warrants further scrutiny in future evaluations of prisoner reentry employment programs.

Although the above findings suggest that employment programming delivered in both the institution and the community can lower recidivism and increase the extent to which offenders work following their release from prison, no single study is sufficient to determine whether employment programming works. It remains to be seen, for example, whether these findings can be generalized to other correctional populations or whether they are unique to Minnesota. Yet, the promising results from this evaluation combined with the scarcity of contemporary evaluations underscore the need for future research to evaluate the effectiveness of employment programming delivered in the institution, community, or both. When meaningfully integrated through meta-analyses, the results from future evaluations could not only provide a more definitive answer as to whether employment programming works, but also identify what works best for whom under which circumstances.

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