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An Experimental Evaluation of a Comprehensive Employment-Oriented Prisoner Re-entry Program

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Abstract

Objectives While the economic model of crime suggests that improving post-prison labor market prospects should reduce recidivism, evaluations of previous employment-oriented re-entry programs have mixed results, possibly due to the multi-faceted challenges facing prisoners at the time of their release. We present an evaluation of an experiment that combines enhanced employment opportunities with wrap around services before and after release.

Methods This paper presents what we believe is the first randomized controlled trial (RCT) of a re-entry program that combines post-release subsidized work with “reach-in”

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social services provided prior to release. The sample was 236 high-risk offenders in Milwaukee with a history of violence or gang involvement.

Results We observe increased employment rates and earnings during the period when ex-offenders are eligible for subsidized jobs, and these gains persist throughout the year. The intervention has significant effects ($p < 0.01$) in reducing the likelihood of rearrest. The likelihood that the treatment group is re-imprisoned during the first year after release is lower than for controls (22 vs. 26 %) but the difference is not statistically significantly different from zero.

Conclusions The results of our RCT suggest that “reach-in” services to help improve human capital of inmates prior to release, together with wrap around services following release, boosts employment and earnings, although whether there is sufficient impact on recidivism for the intervention to pass a benefit-cost test is more uncertain. Average earnings for both treatment and control groups were very low; legal work simply does not seem that important in the economic lives of released prisoners.

Keywords Recidivism · Experiment · Employment · Prisoners · Gang

Introduction

About 700,000 prisoners are released annually from state and federal prisons. As suggested by the high recidivism rates—about two-thirds are re-arrested and over half re-incarcerated within 3 years (Durose et al. 2014)—the correctional system would appear to do little to “correct” or prepare inmates for life after release. Developing cost-effective rehabilitation programs is hence a high priority. In recent years there has been a substantial investment in this domain by the US Department of Justice. But just “what works” in reducing recidivism rates among released prisoners remains unclear.

The standard economic theory of crime (Becker 1968; Cook 1980) assumes that potential criminals choose whether to engage in illegal activities based on the rewards of such activity compared with the potential costs. The perceived costs associated with arrest and punishment will generally be greater for someone who has relatively good licit options. Given this analysis, the high recidivism rate of released prisoners is unsurprising, since their licit options tend to be meager at best. In particular, as a group they have poor employment prospects due to lack of education or work experience and a serious criminal record, quite possibly compounded by drug abuse and other disabilities (Travis et al. 2014). Given this analysis, it is plausible to suppose that interventions that are effective in improving employment opportunities will reduce the allure of crime for some released prisoners and thus reduce the recidivism rate (Cook 1975). But to date the evidence on this matter is mixed at best. Indeed, based on their review of the literature, including two important experiments with transitional jobs, Shawn Bushway and Robert Apel (2012) concluded that “work doesn’t work”.

One potential limitation of previous efforts to improve the employment outcomes for re-entering prisoners is that they only start providing services after exit from prison. It may be that post-release programs start too late to help ex-offenders deal effectively with the multiple challenges associated with employment, family relations, substance abuse, and other aspects of re-entry. Early failures in these domains may lead to backsliding and resumption of a pre-imprisonment life style (Visher and Travis 2011). Indeed, we know that the risks are greatest immediately following release. For example, among Wisconsin prisoners who wind up re-offending and re-entering prison within 3 years of their release,

fully one-quarter do so within the first 5 months following release and one-half re-offend within the first year (WIDOC 2012). Leading scholars have identified reach-in services as one key aspect of a successful re-entry program (Petersilia 2003; Travis 2005).

The present paper presents the results of a randomized controlled trial (RCT) of a re-entry program for violent criminals implemented by the Wisconsin Department of Corrections (WIDOC). We believe this is the first RCT of a re-entry program that includes some attempt to “reach in” to provide fairly intensive programming to inmates while they are still in prison—which in this case began about 6 months prior to release and included services intended to help them stay off drugs and away from gangs, as well as prepare them for legitimate employment. The sample included 236 high-risk offenders with a history of violence or gang involvement who were scheduled to be released in Milwaukee. Following release they were eligible for subsidized employment for 6 months and assistance in making the transition to unsubsidized employment.

Our evaluation of program impacts over 1 year following prison release utilizes intention to treat analysis (ITT) that preserves the strength of the randomized experimental design. We find that the treatment group did work more than the control group, with an increase in median earnings of \$2,200, which provides some support for the idea that even the highest-risk ex-offenders are willing to work if jobs are available. Interestingly, and contrary to other recent employment-oriented re-entry experiments, the employment effect is sustained after the first 6 months, suggesting some success in transitioning to unsubsidized employment. There is also evidence that recidivism is reduced during the first year, although the strength of that evidence differs depending on definition.

Stepping back from our experimental design, a striking result in our data is how little these released offenders earn. Half of the controls earned <\$500 during the first year. The treatment group did better, but not much, and none of them earned enough to support a family above the poverty line. Employment, at least of the sort that parolees are willing to report to their parole officers, simply is not that important in the economic lives of released prisoners.

We begin with a discussion of employment-oriented programs for released prisoners, including a review of experimental evaluations of several such programs. Subsequent sections describe the Milwaukee Safe Street Prisoner Release Initiative (PRI) evaluation, explain our data sources, and assess the extent to which randomization produced similar groups of offenders. We then present our empirical findings, first for employment and earnings, and then for recidivism. The final section summarizes and concludes, with a discussion of why better work opportunities apparently did not do more to encourage desistance from crime.

Background and Literature Review

In recent years, there has been an increase of interest and considerable policy action involving prisoner reentry issues.¹ For example in 2003, the U.S. Departments of Justice,

¹ Early work by the U.S. Department of Justice-sponsored Reentry Partnership Initiative informed a significant push at the national level to identify effective strategies for reentry (Taxman et al. 2001, 2002; Travis 2005). For example, the Offender Reentry and Community Safety Act of 2001, the Second Chance Act of 2004, and the Second Chance Act of 2007 were influenced by this early work (www.lac.org). The Reentry Partnership Initiative started under the direction of then Attorney General Janet Reno in 1999 and was supported by President Bush in his second State of the Union Address (Travis 2005). The initiative encouraged state and local policymakers to develop multi-pronged strategies for successful reentry and continued community safety (Taxman et al. 2002; Travis 2005).

Labor, Housing and Urban Development, and Health and Human Services established the Serious and Violent Offender Reentry Initiative (SVORI), a program providing over \$100 million to 69 grantees to develop programming, training, and state-of-the-art reentry strategies at the community level (www.svori.org). The SVORI programs were intended to reduce recidivism, as well as to improve employment, housing, and health outcomes of participating released prisoners. The 69 grantees operated 89 distinct programs that served generally small numbers of released prisoners (Lattimore et al. 2004). The services provided by these programs have focused on enhancing employment, community integration, family unification, substance abuse treatment, and useful skills building; some included reach-in services prior to release (Winterfield and Lindquist 2005). RTI International and the Urban Institute conducted a quasi-experimental evaluation of SVORI in 16 selected sites (12 adult, 4 juvenile). Only two sites utilized random assignment, and the evaluation utilized quasi-experimental methods (www.svori-evaluation.org).

The main findings were quite discouraging. SVORI provided modest enhancements in services to offenders before and after release, and appears to have had some effect on intermediate outcomes like self-reported employment, drug use, housing, and criminal involvement. However, there was no reduction in recidivism as measured by administrative data on arrest and conviction (Lattimore et al. 2010).

Randomized trials of employment-oriented re-entry programs are rare. Table 1 summarizes what is in our view the most important of these experiments, which date back to the 1970s.² The final column reports the treatment effects on key outcomes at 12 months, with an indication of whether those estimated effects were significantly different from zero. Two early experiments—LIFE (Mallar and Thornton 1978) and TARP (Rossi et al. 1980)—found that providing financial assistance and job search assistance had no detectable effects on recidivism. Another carried out in Florida found that work release during the last few months of imprisonment also had no detectable effects on a measure of post-release arrests (Waldo and Chiricos 1977).

The most prominent experiment of the decade of the 1970s was the National Supported Work Demonstration program, which provided recently released prisoners and other high-risk groups with employment opportunities on an experimental basis. While there were no statistically significant effects on earnings or recidivism rates for ex-offenders, there was heavy attrition from the program among participants that undercut the strength of the experiment (Manpower Demonstration Research Corporation 1980). A re-analysis by Christopher Uggen (2000) which combined the ex-offenders with illicit-drug abusers and youthful dropouts found some reduction in arrests for older participants (over age 26), but not for the younger group. He has speculated that older offenders are more amenable to employment-oriented interventions (Uggen and Staff 2001), perhaps because they are more motivated.

After a long hiatus, two important employment-oriented field experiments for released prisoners have been mounted in the last decade. First was an experimental evaluation of transitional jobs provided by the Center for Employment Opportunities (CEO) in New York City (Redcross et al. 2012). The control group received job-placement assistance, while the treatment group was offered a 4-day pre-employment life-skills class, followed

² We do not include the evaluation of the Minnesota Comprehensive Offender Reentry Plan Pilot by Grant Duwe (2013). The intervention did not have a strong focus on employment. Further, while prisoners were initially assigned at random to the experimental or control group, over half of each group (63 % of treatment group, 52 % of control group) was disqualified following random assignment and not included in the outcomes evaluation. The author does not report an intention-to-treat set of results that would preserve the experimental design.

Table 1 Experimental evaluations of employment-oriented programs for released offenders

Name and source	Intervention	Study population	Sample size	Outcomes	Effect-size estimate, year 1 (T-C)
LIFE (Mallar and Thornton 1978)	Transitional aid T1: \$60/week and job placement T2: \$60/week T3: job placement C: data collection	Baltimore High risk males <45	T1: 108 T2: 108 T3: 108 C: 108	Arrested?	+1.9 % (T3-C)
TARP (Rossi et al. 1980)	Transitional aid T1: UI, 26 weeks T2: UI, 13 weeks T3: UI, 13 weeks var.) T4: Job placement C1: interviewed C2: no interview	Georgia Most released prisoners	T1: 176 T2: 199 T3: 199 T4: 201 T5: 201 T6: 1,031	Ever employed? Earnings Arrested? Average number of arrests	(T4-C2) +1.3 % -\$443 +0.4 % +0.05
TARP (Rossi et al. 1980)	Transitional aid T1: UI, 26 weeks T2: UI, 13 weeks T3: UI, 13 weeks var.) T4: Job placement C1: interviewed C2: no interview	Texas Most released prisoners	T1: 175 T2: 200 T3: 200 T4: 200 C1: 200 C2: 1,000	Ever employed? Earnings Arrested? Average number of arrests	(T4-C2) +6.9 % +\$26 -1.5 % +0.11
Work release experiment (Waldo and Chiricos 1977)	Work release from prison T: Work release C: No	Florida Inmates who meet criteria for release	T: 188 C: 93	Arrested?	+3.7 % (thru year 2)
National supported work (Uggen 2000)	Transitional jobs T: 12–18 months subsidized work and increasing stress	Arrest record & member of target group	T + C ≤ 26 years: 2,125 T + C > 26 years: 980	Arrested? Arrested?	0.0% -8.0 %

Table 1 continued

Name and source	Intervention	Study population	Sample size	Outcomes	Effect-size estimate, year 1 (T-C)
CEO (Redcross et al. 2012)	Transitional jobs T: Class, then day labor with city/state work crews	New York Referred by parole officer	T: 568 C: 409	Ever employed? Arrested? Any arrest, conviction, or return to prison	+24.5 ^{***} -1.7 -6.7 % ^{**}
TJRD (Redcross et al. 2010)	Transitional jobs T: subsidized work plus services	Four cities in midwest Released from state prison within 90 days	T: 912 C: 901	Ever employed? Earnings Arrested?	+33.2 % ^{***} +\$1,419 ^{***} +3.5 %
Southern California (Farrabee et al. 2014)	Job placement T: employment readiness course plus job placement assistance	Southern California Released from jail/prison within 180 days	T: 115 C: 102	Ever employed (full-time)? Arrested?	+2.7 % -4.2 %

*** $p < 0.01$, ** $p < 0.05$

by temporary, minimum-wage jobs with crews that worked under contract to city and state agencies. When treatment group members were deemed ready they were offered help in finding permanent jobs. By one measure the treatment effects on employment were strong during the first year, increasing the likelihood of some employment from 55.5 to 80 %. But the total earnings from those jobs averaged just \$535. What is more, the increase in employment was limited to the transitional jobs. Employment rates during the second and third years (when transitional jobs were not offered) were unaffected.

We would expect, then, that the CEO treatment effect on recidivism would be strongest for the first year of the program when there was at least some effect on employment. But during the first year there was no discernible difference in the prevalence of arrest or conviction or total days incarcerated in prison or jail. A significant difference does emerge when several outcomes are combined: the percent who were “ever arrested, convicted, or incarcerated” was 45 % for the control group, but 39 % for the treatment group. Furthermore, using a 3-year follow-up, “ever convicted” also emerges as significant. The subset of subjects who came to this program within 3 months of release appeared to benefit more from the guaranteed job than other ex-inmates (Bloom et al. 2007). And there is some evidence that those at high risk of recidivating benefited more than others, although the impact on the high risk group does not emerge until the second year following release (Zweig et al. 2011).

The second recent experiment was the Transitional Jobs Reentry Demonstration (TJRD), conducted in four Midwestern cities, including Milwaukee (Redcross et al. 2010). Recruitment for this large experiment occurred in 2007/8. Guided by the CEO results, eligibility was limited to offenders who had been released from prison within 90 days. The treatment group was offered subsidized jobs with employers such as Goodwill Stores and recycling centers. Unlike with CEO, the offenders worked side by side with regular unsubsidized workers, rather than being put into teams with a trained supervisor. As with CEO, the program boosted employment rates early on, and once again this effect was limited to subsidized jobs, with no post-program effects on employment. By the end of the first year, only 28 % of participants were working, with little difference between control group and treatment group. Furthermore, there was no difference in recidivism rates, measured by arrests, convictions, and incarcerations. The results for the Milwaukee arm of this experiment were quite similar to the results reported below.

In reviewing the best evidence from the experiments of the 1970s and the last decade, the clearest conclusion is that employment and earnings of released offenders can be increased by providing them with subsidized jobs. But that temporary increase generally does not lead to more employment in unsubsidized jobs, despite several plausible mechanisms (for example, that work experience will develop “soft skills” that enhance future employability, or that employers would be more likely to hire an ex-con who had recent work experience). Furthermore, we can conclude that counseling and job-finding assistance by themselves are generally ineffective in increasing employment rates. Thus, there are some ex-inmates who will work if given the opportunity but struggle to get jobs in the regular labor market; apparently the enhanced resume is insufficient to overcome their prior criminal records or the employers’ judgment that their skills do not warrant the payment of even the minimum wage.

In sum, the evidence on whether temporary programs that improve employment opportunities have any effect on recidivism is mixed. There have been both null findings and somewhat encouraging findings. But given the comprehensively negative results of the large, multi-site, well-managed TJRD experiment, there are definite grounds for skepticism.

The Milwaukee Intervention

There remains the possibility that what is needed is not just improved job opportunities, but also help in dealing with the myriad other problems typically faced by released offenders, including drug addiction, family dysfunction, debts, gang connections, and lack of “soft skills” in dealing with other people on a day to day basis (Petersilia 2003). Moreover since recidivism rates are particularly high during the months shortly after release from prison, there may be value in providing these services to inmates while they are still in prison in order to prepare them for the challenges associated with re-entering mainstream society.

The Milwaukee Safe Streets PRI provided wrap-around services to address the various challenges facing prisoners. These services included 6 months of programming prior to release, and a variety of supports for post-release employment. Funding for this initiative began in 2007 through a grant from the US Department of Justice.³ The lead agencies were the Wisconsin Department of Corrections (WIDOC) in collaboration with the US Attorneys Office and local law enforcement.

Sample Selection and Random Assignment

Inmates were deemed eligible for PRI if they were aged 35 or under, male, scheduled for release to Milwaukee with at least 6 months of community supervision, and had a history of violence or gang involvement. Sex offenders were excluded.

The experimental sample was recruited by WIDOC through a “trickle in” process during the period January through August, 2009. Eligible inmates were informed by the prison social worker that if they consented to participate, there was a one-in-three chance that they would be included in the treatment group. Our research team carried out the random assignment.⁴

When it became clear that the total number of eligible prisoners was far less than predicted, we needed additional inmates in the treatment group to preserve power. Sixteen inmates were chosen at random from the control group and reassigned to the treatment group.⁵ The final sample included 106 in the treatment group, and 130 in the control group.

This modest sample size makes it unlikely to detect small treatment effects, although sufficiently powered to detect moderate and larger treatment effects. Applying the conventional two-tailed significance level of 0.05, the statistical power for detecting a change of 10 percentage points is 36 %; for 15 percentage points is 66 %; and for 20 percentage

³ In 2006, the US Department of Justice initiated the Comprehensive Anti-Gang Initiative (CAGI) as a follow-on to Project Safe Neighborhoods. In a competition among the US Attorneys’ offices, the Eastern District of Wisconsin, which includes Milwaukee, was selected as one of the first six sites. Each winning District received \$2.5 million dollars for the initiative, with \$1 million for *prevention*, \$1 million for *enforcement* and \$500K for *reentry* activities. The US Attorney’s Office, in conjunction with many of the Milwaukee Project Safe Neighborhoods (PSN) partners, developed the implementation plan for the CAGI funds; this overarching umbrella was called Milwaukee Safe Streets PRI. The Wisconsin Department of Corrections, from the Office of the Secretary, took the lead devising what has become PRI. Funds became available in early 2007. A more detailed history of this experiment is provided in a previous article by the current authors (Cook et al. 2012).

⁴ Each week WIDOC gave a member of our research team the list of inmates who had consented to participate. The WIDOC number was entered into a computer generated list randomizer. From the randomized list, beginning with the first number, every third number was selected for treatment, thus providing the initial treatment group.

⁵ It is an interesting question whether the reassignment influenced behavior or attitudes of subjects who were first told, in effect, that they had lost the coin toss, and then that they had won after all.

points is 88 %. In each case the baseline is assumed to be 70 % recidivism rate (See Lipsey 1990).

Program Details and Implementation

The multi-faceted treatment began with a transfer of inmates in the treatment group (hereafter referred to as Ts) to Racine 6 months prior to scheduled release. Racine is just 30 miles south of Milwaukee, which facilitated visits from family members as well as work release opportunities. The extent of compliance with planned treatment for Ts is suggested by the fact that all but nine (91 %) were transferred prior to release and hence available for all PRI services. Those nine were not transferred to Racine for various reasons, including custodial status (maximum security). In our evaluation of PRI we consider them to be part of the treatment group since dropping them would reduce comparability of the treatment and control groups.

In Racine, Ts were housed either in the Racine Correctional Institute, or in the Sturtevant Transitional Facility. In either case they met regularly with a social worker and were assessed using a number of standard protocols. The social worker was responsible for a case-management work-up (assessing needs, risk, child support and credit, and personal documentation issues, including driver's license and Social Security number). Ts were also given a vocational-skills assessment and access to soft-skills training and vocational training, and the chance to participate in restorative justice circles.⁶ All Ts were expected to participate in the Breaking Barriers cognitive-reality curriculum.⁷ This 12- to 16-week program is designed to change behavior, thinking, and attitudes known to contribute to criminality and to address the dynamic risk factors for criminal behavior. Previous research suggests that a great deal of criminal behavior may be driven by automatic (or what psychologists call "system one") behavior, which may be addressed by programming that helps people recognize and avoid these responses in high-stakes situations (Beck 2011; Heller et al. 2013; Cook 2014). Program participants are also given access to reach-in services of the Community Corrections Employment Program (CCEP), alcohol and drug treatment (through Wiser Choice), and remedial education. For minimum-security offenders who were placed in Sturtevant, work release was a possibility at the end of their term.

The coordinated-care team (including the social worker, job coach, and others) met with each T 30 days prior to release, with the intention of ensuring that suitable plans were in place for housing, transportation, documents, and job search. The care team continued to meet with him monthly following release. The Ts were eligible for substance-abuse treatment in the community, as well as CCEP services, which included assistance with finding jobs and, perhaps more important, job creation through subsidies to employers.

Table 2 provides a partial list of services delivered by the social workers for the Ts while they were incarcerated in Racine. The vast majority of the treatment group prisoners who made it to RCI or STF were assessed for post-release needs related to housing,

⁶ Restorative justice circles involving returning offenders have two parts: a pre-meeting and the circle itself. During the pre-meeting, law enforcement officials, police, and community prosecutors meet with the offenders as a group to tell them what is going to happen. The offenders then meet with victims, who are given a chance to discuss the suffering caused by the crime.

⁷ Breaking Barriers is a life skills and behavioral/cognitive change program developed by Gordon Graham based on cognitive psychology and social learning theory. The program is workshop-oriented and presented by trained facilitators using group and individual exercises. It is designed to increase self-efficacy, goal achievement, and personal accountability. http://www.idoc.idaho.gov/content/document/program_descriptions.

Table 2 PRI programming in racine ($n = 106$)

Program participation status	Rate of completion
Resident at treatment sites (STF and RCI)	0.92
Initial meeting with social worker	0.91
Special placement needed ^a	0.88
DOC-2266 completed ^b	0.87
Participated in restorative justice circles	0.59
Referred to Wiser choice ^c	0.62
Participated in breaking barriers	0.72

Rates computed from 106 offenders placed in the treatment group

^a Need for outpatient treatment of any kind after institution release

^b DOC-2266 is a standard reentry document that prison social workers fill out for all soon-to-be-released prisoners. Questions include housing and employment plans

^c Wiser Choice addresses offenders' unmet AODA needs

employment, education, health (mental and physical), parenting, open warrants and detainers, and other matters. Fully 62 % were referred to Wiser Choice for treatment with respect to alcohol and other drug abuse (AODA) problems. A majority participated in restorative justice circles, and 72 % participated in at least one session of the Breaking Barriers cognitive-reality programming. A prominent aspect of the initiative as originally conceived was a work-release option through the minimum-security wing of the Sturtevant Transitional Facility. As it turned out only 11 % of Ts went on work release. (Work release was also an option for inmates of some other correctional institutions, and 6 % of the Cs took advantage of this possibility.) The utilization of work release by the Ts may have been higher if the Milwaukee economy had not been in a deep recession. In our analysis below we include everyone assigned to treatment, regardless of whether they participated in services or not, to preserve the strength of the randomized experimental design.

The services to which the treatment group had access were already in existence, rather than designed specifically for the treatment group, but the Ts had guaranteed access (because of the service vouchers supported by the program grant) while the Cs could just get in line. In effect the planned treatment was to make available to Ts everything that WIDOC had to offer. Some of the inmates in the control group also undertook and completed in-prison treatment programs. WIDOC records indicate, for example, that 3 % completed a high school education, 25 % had counseling, and 13 % completed an AODA program. We do not have precisely comparable statistics for the Ts. It is clear, in any event, that the Ts had much more extensive contact with a social worker prior to release than was true for the controls. Following release the Ts continued to have access to AODA treatment services and other services designed to help with reintegration problems. The coordinated-care team continued to meet as they had before the inmate's release.

Perhaps most important was the role of CCEP in job creation. Over half (54 %) of Ts received assistance from CCEP; while the controls were technically eligible for CCEP services, none of them actually received any. CCEP offers work experience, training, and education vouchers for parolees. To encourage employers to hire its clients, CCEP also provides tax-credit certification of eligible offenders for employers, and bonding when necessary.

Prisoners were deemed eligible for PRI only if they were expected to be released to Milwaukee. As it turned out, 21 of the 236 participants were released to other

jurisdictions.⁸ The 6 who moved out of state were of course not subject to WIDOC supervision or eligible for its services, and those who were in state but away from Milwaukee had different opportunities and services than they would have found in the city. Nonetheless, in our evaluation of the experiment we do not drop these 21 subjects from the analysis just because of the release conditions, since that would subvert the validity of the experimental contrast, which might be confounded by systematic attrition.

Ts in Milwaukee and elsewhere in Wisconsin were subjected to enhanced supervision following release. The Milwaukee agents responsible for the Ts had a reduced case load (40, rather than 80 or more). The smaller load provided greater opportunity to assist the offender with the myriad problems of re-entry; closer supervision could also have resulted in a higher rate of observed violations of release conditions. It is possible that the closer relationship between agent and offender changed the likelihood of parole revocation, in either direction.

Data

To measure impacts of the PRI on service receipt and post-release behavioral outcomes, we rely on a variety of government data sources that cover the period from January 2009 (when enrollment began) through the end of calendar year 2011. We focus on behavioral outcomes of our study sample measured 12 months from the time of prison release. In principle “starting the clock” to measure post-prison behavior upon prison release could generate bias in our estimates if treatment assignment had some effect on prison release dates, given the strong age patterning to criminal behavior.⁹ But among those inmates who were randomly assigned to T and C conditions and were released from prison during the study period, the average release dates are similar across groups (May 6, 2010 for Cs vs. May 16, 2010 for Ts). The 17 who were not released are discussed below.

One general concern with RCTs that rely on administrative data is the possibility of mis-matches between the study sample and the outcome data. But that problem does not arise in the PRI because the study sample was recruited by WIDOC and is identified by an internal WIDOC inmate number that is used by other relevant data systems.

Data on employment and earnings were reported by the released offenders and recorded by the parole agents. Study subjects assigned to the treatment group were to see their parole agents once a week; those assigned to the control group were to see their parole agents every 1 or 2 weeks. Agents typically try to verify earnings by asking to see pay stubs and even checking with employers and doing work-site visits, since regular employment is a condition of parole.¹⁰ It is possible that there is still some misreporting. Given the incentives facing parolees, it is fair to assume that over-reporting of employment or earnings is more likely than under-reporting. There is no reason to believe that any mis-

⁸ In particular, 6 moved to another state (4 Cs, 2 Ts), and 15 remained in Wisconsin but not in Milwaukee (9 Cs, 6 Ts).

⁹ In the PRI sample, a multivariate analysis demonstrates that the likelihood of re-arrest in the 12 months following release declines strongly with time served and with age.

¹⁰ A potential alternative is the administrative records kept by the Wisconsin Unemployment Insurance. Unfortunately we were unable to obtain those records for this project. In principle the data collected by parole officers may be more comprehensive, since not all employment is covered by the Unemployment Insurance system.

reporting by parolees is systematically different between the randomized treatment and control groups in our experiment.

Post-prison recidivism was measured using administrative records from WIDOC. We examine arrest data that include information on the date of each arrest to any of our study subjects, as well as the criminal charges for which the person is arrested. WIDOC also maintains information on all inmates within the Wisconsin correctional system, which enables us to examine re-imprisonment rates. Re-imprisonment may be due to a new conviction or to revocation of early release. The revocation process in Wisconsin includes a hearing before a state administrative law judge, where the state is represented by the parole agent who testifies about the offender's rule violations. These violations do not necessarily entail new crimes—they can be violations of the conditions of release, such as regular reporting to the agent and working steadily—but presumably agents do not bring such revocation cases (and judges do not revoke) unless there is a reasonable belief that the offender has returned to crime. The judge can return the offender to prison to serve out some or all of the rest of his sentence.¹¹

Our two measures of recidivism are both indicators of a return to criminal activity, and both imperfect albeit in different ways. Most arrests are for misdemeanors, crimes that are typically insufficient to warrant revocation even if they do result in conviction. Revocations are much less common than arrests, and reflect the exercise of judicial discretion about whether such a serious disposition is warranted. Given the different actors (police, in the case of arrest, and parole officer and judge, in the case of revocation) and different standards that are likely to apply, the two types of outcomes may help bracket the truth of whether the offender has returned to serious criminal activity. If these disparate outcomes yield a qualitatively similar result about the effectiveness of the intervention, that result has additional credibility.¹²

Data on outcome measures are missing for some subjects. As mentioned above, six prisoners (4 Cs and 2 Ts) were released out of state, where data on employment and possible recidivism are largely unavailable. Seventeen prisoners (14 Cs and 3 Ts) were kept in prison through 2011; the next section explains how our analysis sought to accommodate that fact and preserve an unbiased estimate of the treatment effect.

Analytic Methods

Given the experimental design of PRI, our analysis plan is quite straightforward. The effect of being offered programming—the intention to treat effect (ITT)—is estimated using Eq. (1). Z_i is an indicator for whether the inmate is assigned to the treatment group ($=1$) or control group ($=0$), and X_i is a vector of covariates with values that are determined prior to random assignment. Y_i is some outcome of interest related to employment or recidivism. Our estimate of β represents the ITT effect. This estimate essentially compares the average outcomes for everyone randomly assigned to be offered treatment with everyone randomized to the control condition, *regardless* of whether the subject participated in

¹¹ https://media.law.wisc.edu/m/zkzwz/revocation_manual_2011.pdf.

¹² It is possible that parole revocation is subject to an “instrumentation” bias, since the parole agents are fully aware of whether their clients are in the treatment group or not, and furthermore, the parole officers in the treatment group see their clients more often and may be better informed about their true activities. Both of these features of the experiment may influence their tendency to recommend revocation, although perhaps in opposite directions. In any case, the “arrest” outcome, which reflects police discretion, is unlikely to be influenced by experimental status.

programming. The ITT is thus not susceptible to bias from the fact that within the treatment group, participants may differ from non-participants in ways that influence outcomes.

$$Y_i = \pi + \beta Z_i + \gamma X_i + \varepsilon_i \quad (1)$$

The ITT will understate the effects of actually participating in programming. In principle we could use random assignment to treatment as an instrumental variable to estimate the effects of program participation, which would reflect the effects of treatment on the treated if no Cs could access program services but in our application would instead be a local average treatment effect (LATE) (Angrist et al. 1996). But in the case of PRI, the treatment has so many elements, differing in their take-up rates, that it is unclear how to define who is actually “treated.” Moreover in practice the pre-release intervention was administered to all but nine of those randomly assigned in the PRI to treatment, so the difference between the ITT and LATE should be modest—at least if we define treatment generically as “receipt of reach-in services.”

We examine cumulative employment, earnings and recidivism data, where Eq. (1) is estimated using cross-section regression analysis (both ordinary least squares and logit) for outcomes measured for 1 year following prison release. We also analyze the temporal pattern of recidivism during the first year, applying a survival analysis to Eq. (1). Finally, logit regression results are reported as estimates of the intervention’s effects on the likelihood of having been arrested at least once by 3, 6, 9 and 12 months after release.

The main threat to valid inference in our experiment is selective sample attrition. Part of the issue here comes from data on employment and earnings that are unavoidably missing when someone is re-incarcerated and back in prison. We present quarterly results on labor market outcomes, conditional on being out of prison and hence available for work. (The cumulative percent re-incarcerated for the treatment and control groups by month since release is at the end of Q1, 4 and 7 %; at the end of Q2, 17 and 16 %; at the end of Q3, 20 and 22 %; and end of Q4, 22 and 26 %). We also have analyzed a joint outcome measure that is equal to one if the study subject is both working and not in prison, and equal to zero otherwise, an approach which keeps the incarcerated offenders in the analysis. The results are qualitatively similar to those reported here.

The most important source of differential attrition is that Cs are more likely to be retained in prison past their expected release date, due to the influence of PRI on the parole board’s release decisions.¹³ As it turns out, 14 of the 17 who were not yet released by December, 2011 (and hence not available for inclusion in the analysis of outcomes) are in the control group. All but one of these 17 were sentenced for crimes committed prior to 2000. The explanation for this pattern illustrates one pitfall of field experiments, namely that the experimental condition can influence decision making by authorities in ways that distort the experimental design. In the PRI, the inmates deemed eligible for PRI were sentenced either under the old indeterminate sentencing law (if sentenced for felonies committed before January 1, 2000), or the newer truth-in-sentencing system (commonly known as “Act 283”). Under indeterminate sentencing the inmate became eligible for parole after serving one-fourth of the sentence (Hammer 2002), and the parole board had broad authority to determine the actual release date. Under the newer truth-in-sentencing law, by contrast, the sentence imposed by the judge included a fixed term of confinement followed by a term of extended supervision in the community. For those inmates with an

¹³ While we do not have an explicit indication of “expected release date,” we do know that WIDOC’s procedure for identifying inmates as eligible for this experiment took account of when they were likely to be released.

indeterminate sentence, the fact that Ts were active in rehabilitation-oriented PRI programming and eligible for special treatment following release had a positive influence on the parole board. Indeed, we found explicit mention of PRI in release decisions for 16 % of the Ts. To take account of the parole board's exercise of discretion, our regression specification includes an indicator for whether the inmate was sentenced for a crime committed prior to 2000, and also includes an interaction with treatment assignment. The effects of selective attrition due to indeterminate sentencing are captured by the interaction term.

In the results below, the estimates are based on the specification in Eq. (1). The vector of covariates includes the two variables that are intended to correct for differential attrition. In addition, the specification includes a set of covariates measuring race, age, education, reading level, release location, juvenile record, risk level, and length of incarceration—all of which were measured prior to experimental assignment.

Whether analysts should control for such baseline covariates in the analysis of experimental data remains the topic of ongoing debate within applied statistics; the inclusion of pre-determined covariates that are known to influence the outcome tends to provide more precise estimates of the experimental effect (smaller standard errors) in exchange for a potential bias in the estimate of the treatment effect (Weisburd and Gill 2014; Berk et al. 2013). Given our small sample size, we opted for the multivariate analysis and the increased power that provides.

Empirical Results

Table 3 shows that the 106 PRI treatment cases and the 130 control cases are similar with respect to baseline characteristics such as race, age, marital status, and education. Minor differences can be readily explained by chance variation, as indicated by the p value. Most of the prisoners are black and unmarried. On average they read at the 9th grade level. The age at release averages 28 years. The two groups also have generally similar criminal histories. Only one of the 18 baseline characteristics we examine (number of prior felonies) is significantly different at the $p < 0.05$ cutoff using a pair-wise comparison error rate. The controls have a higher number of prior felony convictions than does our treatment group (2.7 vs. 2.2).

Effects on Employment and Earnings

Table 3 also highlights some of the challenges faced by ex-offenders trying to gain employment in the formal labor market following release from prison. Aside from their criminal records, over one-third are high school dropouts and their reading skills are on average at about a 9th grade level. Many have substance-abuse problems and limited work experience. They also have financial liabilities that average nearly \$2,000 stemming from a variety of sources: child support payments, outstanding fines and fees, court-ordered restitution payments, and other debts.¹⁴ Government efforts to collect on these liabilities can have the effect of raising the implicit marginal tax rate on earnings. On the other side of the

¹⁴ The growing importance of this phenomenon is documented in NPR's survey of state-mandated fees for indigent legal representation, room and board, and supervision in the community. All but two states increased fees between 2010 and 2014. <http://www.npr.org/2014/05/19/312455680/state-by-state-court-fees>. We do not have data on other liabilities. Some inmates managed to pay off part of their restitution debt before release, but the net restitution at release was not much smaller: including those with zero liability, average net values are \$1,484 for controls, and \$1,806 for PRIs.

Table 3 Comparison of control and treatment groups

Category	Value	Control	Treatment	<i>p</i> value
<i>Pre-release characteristics</i>				
				0.539
Race	White (%)	11.5	15.1	
	Black (%)	86.2	84	
	Other (%)	2.3	0.9	
Ethnicity	Hispanic (%)	8.5	10.4	0.616
Age at release	Years	27.5	28.3	0.207
Marital status	Married (%)	3.8	2.8	0.670
Education	<High school	33.8	35.8	0.749
Reading score	Grade	9.3	9.0	0.308
<i>Criminal background</i>				
Number of prior felonies	Count	2.7	2.2	0.018
Number of prior misdemeanors	Count	1.2	1.1	0.423
Age at first felony conviction	Years	19.6	19.9	0.406
Prior juvenile offense	(%)	31.5	35.8	0.487
Prior gang affiliation	(%)	66.2	58.5	0.228
<i>Prison and parole experience</i>				
Prison security classification				0.051
	Minimum (%)	1.5	7.5	
	Medium (%)	71.5	62.3	
	Maximum (%)	26.9	30.2	
<i>Financial assets and liabilities</i>				
Restitution owed (average)		\$1,667	\$1,902	0.767
% of Offenders with non-zero restitution		85 %	79 %	0.217
Restitution, conditional on non-zero value		\$1,952	\$2,400	0.640
Release funds		\$682	\$372	0.165
% of offenders with non-zero release funds		43 %	44 %	0.931
Release funds, conditional on non-zero value		\$1,581	\$851	0.128
Observations		130	106	

Information on reading score is missing for five offenders (one from control and four from treatment groups). Release fund information is available only for the offenders who were released (116 from control and 103 from treatment groups). The *F* test statistics for the joint significance of the 16 baseline covariates observed at the time of selection (i.e., excluding the age at release and release funds) is equal to 1.32 (*p* value = 0.193)

ledger is the release fund accumulated during the prison term, which could in principle help set ex-offenders up with an apartment or car or other necessary items to obtain or keep a job. As it turns out, only a minority of inmates had any release funds (about 44 %), and the average amount is far less than the restitution average (\$372 for T's and \$682 for C's).

The primary mechanism for reducing recidivism in the Milwaukee Safe Streets PRI was improving employment opportunity. That was to be accomplished through services provided both before and after release, as described above. Perhaps the most important post-

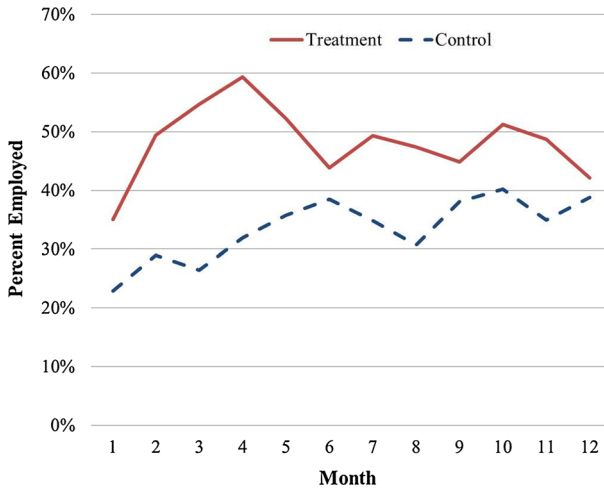


Fig. 1 Monthly prevalence of employment following release, Ts and Cs

release help was the ongoing connection with CCEP, which had funds to help create jobs for its clients through employer subsidies. Our experimental evidence makes clear that the PRI was effective in increasing employment. Using records kept by parole agents, we find that 59 % of Cs and 81 % of Ts had earnings at some point during the first 12 months following release. Figure 1 depicts monthly employment rates for Ts and Cs who had not recidivated. For Cs the trend is generally upward, from 23 to 40 %. The Ts' employment rate peaks at 60 % in month 4 and then drops to a level that fluctuates between 40 and 50 %. The likely explanation for the early success (relatively speaking) of the Ts is the concentration of CCEP subsidies (i.e., subsidized jobs) during that period. That service was generally no longer available after month 6.

Table 4 reports the estimated treatment effects for quarterly employment for those who have not recidivated. Each cell in the table is the result of a different regression. (The complete results for the regressions reported in the last row are in Appendix Table 7.) For the first two columns, the outcome measure is whether the released prisoner worked at least 1 h during a given quarter. The first column reports OLS estimates for a linear probability model, and the second reports logit estimates. The latter have somewhat smaller sample sizes because positive values for the indicator variables (reading score missing, and crime before 2000 X treatment) are associated with a 100 % employment rate, so those observations were necessarily dropped. As it turns out, however, the estimates of coefficients for the OLS regression are very similar to the logit estimates (after exponentiating the latter and converting to a probability at the mean). The result from the linear probability model is that Ts are about 20 percentage points more likely than Cs to be employed in any one of the four quarters, other things equal. The same result holds from the logit model at the mean values of the covariates. These estimated treatment effects are significantly different from zero by the usual standards.

The last two columns report estimated treatment effects for earnings. Note that earnings are the product of hours worked and hourly wage rate, although most of the variation in this case comes from hours worked. The third column estimates the effect of treatment on log earnings, where the estimation sample is restricted to those who worked at least 1 h

Table 4 Quarterly probability of employment for non-recidivists

	Worked during quarter (OLS)	Worked during quarter (Logit)	Log earnings during quarter if worked (OLS)	Earnings during quarter (OLS)
Q1	0.217*** (0.073)	0.213*** (0.068)	0.433** (0.213)	334** (135)
[Obs.]	[197]	[180]	[99]	[197]
Q2	0.175** (0.078)	0.164** (0.072)	0.262 (0.181)	470** (207)
[Obs.]	[173]	[169]	[100]	[173]
Q3	0.224*** (0.081)	0.225*** (0.076)	0.178 (0.225)	370 (251)
[Obs.]	[162]	[158]	[89]	[162]
Q4	0.168* (0.086)	0.171** (0.081)	0.175 (0.248)	449 (295)
[Obs.]	[156]	[153]	[81]	[156]
All Qs	0.198*** (0.057)	0.193*** (0.055)	0.230* (0.132)	408** (172)
[Obs.]	[688]	[673]	[369]	[688]

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The unit of observation is a quarter (3 months) during the year following release from prison. Quarters in which the subject was back in prison are excluded. Robust standard errors are presented in parentheses in quarter-specific employment measures. For the regressions based on the sample of all quarters, robust standard errors clustered at the offender level are reported. Sample size for each regression is shown in square brackets. Observations with missing reading score are dropped from the logit estimation (Four in Q1, Q2, and Q3, and Three in Q4), because they perfectly predict employment. 13 offenders in the treatment group whose crimes took place before 2000 are also dropped from Q1 employment logit regression because they perfectly predict employment

Average marginal effects are reported for the logit specification

Table 5 Earnings distributions during 12 months following release

Percentile	Earnings > 0		Entire sample	
	C	T	C	T
25	\$1,233	\$1,929	\$0	\$116
50	\$3,493	\$3,770	\$462	\$2,690
75	\$7,698	\$7,178	\$4,000	\$6,525
95	\$15,795	\$15,640	\$13,743	\$14,810
Max	\$25,925	\$18,785	\$25,925	\$18,785
Observations	61	77	110	99

during the quarter. Together with the logit results of the second column, the results can be viewed as part of a two-part estimate in the form recommended by Manning et al. (1987); they conclude that while this approach does not correct for selection into positive earnings, it often performs as well or better than selection models that impose strong assumptions on the data.

The final column reports the OLS estimates of the treatment effect on earnings based on the entire sample, including those who did not work. This estimation procedure may be seen as a linear approximation of an intrinsically nonlinear relationship, with an interpretation as an estimate of the treatment effect on average earnings including zeroes (Angrist and Pischke 2009, p. 39). As it turns out, columns three and four have similar patterns of results; all coefficients are positive, and the effects are significant in the first quarter and for all quarters combined. The linear results suggest that the treatment increased earnings an average of about \$400 per quarter.

Of course one reason why released offenders may not be employed is that they have been sent back to prison. The regression results are not much different if we count those in prison as not working and keep them in the sample, not surprising given that the reincarceration rate is similar across the randomized T and C groups.

Adequacy of Earnings

If we step back from the experimental setup, another fact comes into focus: the cumulative earnings for the entire 12 month period are low not only at the median (\$462 for Cs, \$2,690 for Ts), but even for those who were most successful. The 95th percentile annual earnings (about \$14,000) were about what someone would make working full time at the minimum wage. That income is insufficient to lift a household of two above the poverty line. The earnings of the most successful person in either sample were a bit <\$26,000 (a C) (Table 5).

These earnings are particularly low given how few other obvious sources of legal income are available to this sample. For a handful of offenders who qualified for disability payments, Supplemental Security Income provided a source of income following release, amounting to about \$650 per month. PRI succeeded in getting a higher share of inmates onto SSI following release compared to controls (6.8 vs. 1.7 %, $p = 0.06$). But for the great majority who were healthy, no government income-support programs are available. The expectation, realistic or not, is that they support themselves through employment. Yet reported earnings for this sample are far below subsistence levels. We return to this issue in the final section.

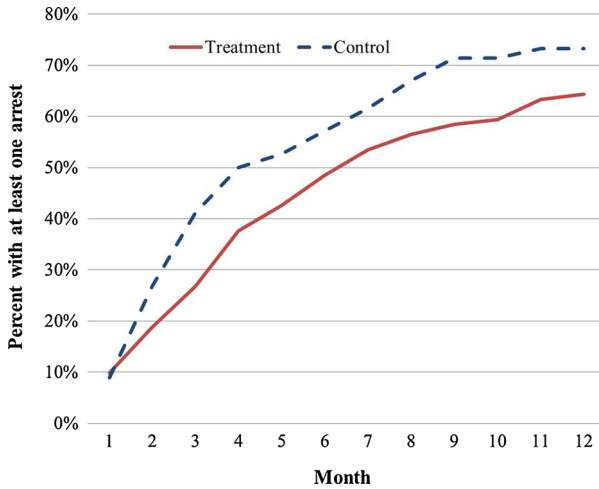


Fig. 2 Cumulative arrest rate by months from release

Effects on Recidivism

Our primary indicators of recidivism are arrest and re-imprisonment within the first year following release. We begin with an analysis of arrests.

Figure 2 shows the prevalence of arrest for the treatment and control group by month since prison release. Fully two-thirds of the offenders were arrested at least once during the first year, reflecting the fact that this sample was selected to be especially high risk. The prevalence of arrest after release is lower for Ts than Cs (63 vs. 72 %).

Table 6 reports the results of a survival analysis using the Weibull distribution. The coefficient on T provides an estimate of how the treatment affects the likelihood of “failing” in month $t + 1$ for those who have survived to period t . The point estimate indicates that the treatment reduces the probability of being arrested from one period to the next by about 39 %, which is statistically significantly different from zero at the $p < 0.01$ level. We also ran logit regressions for whether the subject had been arrested by 6, 9, and 12 months after release. These analyses provide consistent results, in the sense of indicating that the treatment reduced the rate at which offenders are arrested after release, and that the effect was highly significant. Among the strongest covariates in predicting re-arrest are age over 30 (–), juvenile record (+), and time served prior to release (–).

The arrest analysis can be expanded to take account of the number of arrests, or the seriousness of charge. Overall there were 1.24 arrests per offender; of those with at least one arrest, the average was 1.82 arrests. The difference between T and C by this measure is not significant. When we narrow our focus to just arrests for the most serious crimes—violence and weapons-related—there is no advantage for the treatment group. Indeed, 28 % of both the control and the treatment group were arrested for one or more of these serious crimes; the estimated treatment effect by that definition (with no regression adjustment) is zero.

Table 6 Arrests: survival analysis and logit estimates

Model	Survival (weibull)		Logit: new arrest within			
	Arrest	Imprisonment	6 Months	9 Months	12 Months	
Treatment	-0.494*** (0.187)	-0.420 (0.312)	-0.684** (0.343)	-1.138*** (0.361)	-0.928** (0.361)	
Crime before 2000	-0.091 (0.636)	0.144 (0.798)	0.352 (0.962)	-0.033 (0.992)	0.045 (1.015)	
T X crime before 2000	0.399 (0.742)	0.425 (0.992)	0.461 (1.172)	1.261 (1.274)	1.041 (1.314)	
Black	0.538* (0.304)	0.178 (0.461)	0.619 (0.525)	0.568 (0.495)	0.302 (0.489)	
Age (default < 24)						
24–26	-0.399 (0.272)	-0.889** (0.495)	-0.639 (0.525)	-0.834 (0.532)	-0.867 (0.542)	
27–29	0.290 (0.269)	-0.377 (0.483)	-0.120 (0.515)	0.134 (0.594)	0.010 (0.645)	
30+	-0.722*** (0.256)	-0.470 (0.434)	-1.114** (0.509)	-1.461*** (0.558)	-1.496*** (0.575)	
Less than HS	-0.135 (0.196)	0.047 (0.334)	-0.078 (0.379)	0.141 (0.431)	-0.059 (0.448)	
Juvenile record	1.020*** (0.196)	1.376*** (0.336)	1.387*** (0.391)	1.673*** (0.432)	1.638*** (0.468)	
Released to (default = other Milwaukee dist)						
District 2	0.849*** (0.305)	0.926** (0.438)	1.414** (0.566)	1.169* (0.615)	0.939 (0.615)	
District 5	-0.050 (0.221)	-0.403 (0.442)	-0.107 (0.426)	0.058 (0.486)	0.327 (0.512)	

Table 6 continued

Model	Survival (weibull)		Logit: new arrest within			
	Arrest	Incarceration	6 Months	9 Months	12 Months	
Outside Milwaukee	0.426 (0.365)	1.249** (0.495)	0.074 (0.632)	0.718 (0.738)	0.940 (0.721)	
<i>Reading level (default = 8th grade)</i>						
8–10th grade	-0.469** (0.201)	-0.733** (0.350)	-1.054*** (0.404)	-0.788* (0.419)	-1.048** (0.451)	
>10th grade	-0.361 (0.249)	-1.189** (0.477)	-1.211** (0.473)	-0.553 (0.541)	-0.657 (0.580)	
Reading level missing	-0.554 (0.636)	0.844 (0.830)	-1.564* (0.842)	-1.940** (0.841)	-1.726* (1.030)	
<i>502 risk level (default = low)</i>						
Moderate	0.682** (0.334)	0.819 (0.809)	0.751 (0.580)	1.482** (0.600)	1.591*** (0.577)	
High	0.311 (0.316)	1.526** (0.755)	0.174 (0.522)	0.389 (0.563)	0.251 (0.541)	
Log (incarc. length)	-0.593*** (0.144)	-0.157 (0.240)	-0.810*** (0.265)	-0.924*** (0.300)	-1.094*** (0.305)	
Constant	-2.172* (1.121)	-8.167*** (2.065)	5.943*** (1.933)	6.933*** (2.253)	8.794*** (2.284)	
Observations	213	213	213	213	213	

Regression coefficients and standard errors in parentheses are presented. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The coefficients in the first two columns represent regression coefficients which can be converted into a hazard rate. For example, placement in the treatment group decreases the hazard of new arrest by 39% ($e^{-0.494} = 0.61$) and the hazard of re-imprisonment by 34%. In the last three columns, logit coefficients and standard errors are reported

The second outcome measure is the re-imprisonment rate through the first year after prison release. Released prisoners were returned to prison either because they were convicted of a new crime and sentenced to prison, or because their parole or extended community supervision was revoked, or both. Everyone who was returned to prison during the first year was also arrested at least once, although in a few cases the arrest was for a technical violation (e.g., absconding). Most of the offenders (31 of 52) who were returned to prison during the first year also had a new conviction.¹⁵ The re-imprisonment at that point almost always (29 of 31 cases) included a formal order to revoke parole.¹⁶

The re-imprisonment rate is somewhat lower for the treatment group than for controls (22 vs. 26 %), but that unadjusted difference is not statistically significant, nor does the estimated treatment effect, while negative, rise to a level of statistical significance in the multivariate survival analysis in Table 6. It should be noted, however, that the point estimate in the survival analysis is almost as large as for the highly-significant treatment effect estimated in terms of arrest.

Discussion and Conclusions

Released prisoners enter the labor market with a number of handicaps, starting with a serious criminal record and usually including a lack of work experience, “soft skills,” and education. The gang members and violent criminals who were recruited into the PRI would seem to be a particularly unpromising group when it came to willingness and ability to work. PRI focused on this high-risk population partly because it is for this group that the societal benefits of reform would be greatest. A key innovation of the PRI intervention was to “reach in” and provide inmates with assessment and needed services before their release from prison, while also providing services and employer subsidies for the first 6 months following release.

The PRI was successful in one of its primary objectives, increasing employment, indicating that for at least a subset of even this high-risk population they are willing and able to work if jobs are available. The median increase in earnings was \$2,192 in the year. The hope was that the combination of reach-in services and of work experience and skills developed through subsidized employment would translate into gains in subsequent employment and earnings in unsubsidized jobs. That hope was realized, in the sense that Ts were more likely than Cs to be employed in the third and fourth quarters of the year following release (given that they had not been returned to prison).

There is also strong evidence that the likelihood of re-arrest during the first year is reduced by the PRI treatment. We find overall arrest rates and re-imprisonment rates for the treatment group by the end of the first year to be lower than for controls (by 9 and 4

¹⁵ Another released prisoner was the victim of a homicide during the first year after release. In our analysis we treat his case as equivalent to parole revocation, given a reasonable presumption that he was associating with criminals.

¹⁶ The most serious offense mentioned in connection with return was violent or weapons-related in 12 of the 21 revocations with no conviction, and 12/29 cases in which there was a conviction.

percentage points, respectively, equal to 13–15 % of the respective control means).¹⁷ A multivariate regression analysis that controls for baseline characteristics of the offenders yields highly significant estimated treatment effects based on arrest outcomes.

Whether the program passes a benefit-cost test is difficult to determine from our data, given the imprecise estimates. The average number of arrests during the first year released from prison was 1.32 for the treatment group and 1.45 for the control group, and the difference is not statistically significant. About one-fifth of Part 1 violent or property offenses known to police result in an arrest (US DoJ, FBI 2010). If the difference of 0.13 arrests (and hence 0.65 crimes) per person were accurate, given that we estimate the cost at \$5,000 per participant the program would pass a benefit-cost test if the average social costs of the crimes committed by this group were at least \$7,700.¹⁸

Recent studies have found that subsidized jobs by themselves do not translate into subsequent employment in unsubsidized jobs for ex-offenders. The TJRP experiments reviewed above did not reduce recidivism, and the CEO experiment had a mixture of null and positive results on recidivism (depending on the time frame and exact definition). Our experiment adds the new finding that “reach-in” services to help prepare inmates for productive lives, together with wrap around services following release, may in fact help sustain higher employment rates. That is a new and promising finding. The strong results on employment, coupled with the reduced likelihood of arrest, suggest that something “worked” in the PRI experiment. It is in the nature of the intervention that it is not possible to identify which of the various services helped account for that success. If the intervention had comprehensively failed, then that would have cast doubt on the entire approach of offering wraparound services before and after release to high risk prisoners. Because it did not fail, we are left wanting to know more about the mechanisms of success with an eye to refining this complex intervention.

Given the high stakes and continuing uncertainty about what works and what is worthwhile, we believe that further research is warranted. If that research is to build on what has been learned so far, then the lesson from the experiments with CEO and TJRP is that their approach (offering low-wage menial jobs and job-placement assistance without much help in other domains) is not promising for achieving the goals of recidivism reduction or a sustained increase in employment. Nonetheless, more information will be forthcoming to test this conclusion—the State of New York has launched a new large-scale RCT of the CEO job-creation intervention, which is funded by a social impact bond that pays investors only if there is a pre-specified reduction in days incarcerated for the treatment group (CEO 2014). Given our results for PRI, it would be of considerable interest to add an arm to the experiment that provides a pre-release component designed to

¹⁷ Interestingly, this point estimate is in the ball park of what informed opinion believes is feasible. For example, Christy Visher and Jeremy Travis (2011) observe: “According to best estimates of the research community, if we could implement effective programs for all returning prisoners with all the resources needed, we could expect recidivism reductions on the order of 15 to 20 % (p. 1,155)...”

¹⁸ The estimate of \$5,000 per offender is an approximation based on the assumption that the WIDOC spent the entire federal grant of \$500,000 on additional services for the 106 Ts. In terms of imprisonment, there is a statistically insignificant 4 percentage point difference in return rates to prison between the treatment and control groups. Given our estimate for the costs per participant of about \$5,000, the social costs per prison spell would need to be at least \$100,000 for the program to have passed a benefit-cost test.

prepare them as much as possible for the practical problems associated with housing, transportation, family reintegration, financial obligations, and exposure to alcohol and drugs—but that is not part of the planned intervention.

The Milwaukee PRI provides new evidence for a more fundamental challenge to the centrality of employment opportunities for influencing recidivism rates. Released prisoners as a group tend to have very low earnings, in part because of poor opportunities but in part because of a tenuous connection with the labor market (Apel and Sweeten 2010). The average offender in the PRI control group left prison with a debt of over \$1,000, and fewer than half of the Cs earned that much money during the entire first year. Only a handful of Ts earned enough to support themselves during the first year, even at the poverty level. Data from other recent studies with an employment focus (CEO, TJRP) have also established that earnings of most released prisoners are low or nil whether or not they are offered jobs. The inescapable conclusion is that for most of this group, employment is a minor source of funding for their day-to-day needs. The ethnographic work of Harding et al. (2014) suggest that they are making ends meet by taking handouts from family and partners, utilizing soup kitchens and cash assistance, and hustling.¹⁹ In retrospect it would have been very interesting to find out more about how the PRI men were getting by week to week, with an eye to designing a better targeted intervention. An employment-oriented intervention cannot go far for a group—perhaps a majority of released prisoners—for whom employment is scarcely relevant. To the extent that PRI was effective in reducing recidivism, the non-employment features of the intervention may turn out to deserve much of the credit.

Finally, one might ask whether the meager findings for employment-oriented programs are in some sense a challenge to the rational-choice model. One prediction of that model is that an improvement in legitimate opportunities will serve as a deterrent to crime (other things equal). It is important to understand that in this model the relevant valuations of legitimate and criminal opportunities are subjective, which raises the question of whether an offer of a temporary low-wage menial job is a strong test of the theory. Released prisoners who have never had a regular connection to the labor market may be largely indifferent to such an offer, and our results on earnings and those of other experiments seem to confirm that view as the norm rather than the exception. A stronger test would require a bigger “dose” of legitimate opportunity. What form that opportunity should take remains a vital question in the effort to reduce crime.

Appendix

See Table 7.

¹⁹ The struggles of the 15 men and 7 women in Michigan to find food and shelter were documented over a two-to three-year period following release (Harding et al. 2014). Especially in the first few months, subjects’ families and romantic partners bore most of the burden of meeting their needs. The handful who attained some level of economic security combined employment with public benefits, social services, and family support (p. 442).

Table 7 Estimation results for employment outcomes, all Qs

	Worked during quarter (OLS)	Worked during quarter (logit)	Log earnings during quarter if worked	Earnings during quarter
Treatment	0.198*** (0.057)	0.932*** (0.275)	0.230* (0.132)	408** (172)
Crime before 2000	0.040 (0.126)	0.199 (0.654)	0.353 (0.319)	368 (526)
T X crime before 2000	-0.177 (0.155)	-0.831 (0.818)	-0.768* (0.396)	-881 (649)
Black	0.069 (0.078)	0.346 (0.387)	-0.197 (0.188)	-7 (246)
Age (<i>default < 24</i>)				
24–26	0.071 (0.090)	0.289 (0.429)	0.234 (0.200)	183 (188)
27–29	0.006 (0.081)	-0.003 (0.375)	0.561** (0.229)	268 (224)
30+	0.133 (0.084)	0.600 (0.386)	0.763*** (0.217)	888*** (268)
Less than HS	-0.127** (0.063)	-0.588** (0.285)	-0.054 (0.175)	-95 (194)
Juvenile record	-0.156** (0.063)	-0.752** (0.300)	-0.258 (0.159)	-502*** (186)
<i>Released to (default = other Milwaukee dist.)</i>				
District 2	0.048 (0.107)	0.209 (0.476)	-0.388 (0.246)	-298 (300)
District 5	0.011 (0.065)	0.057 (0.319)	0.064 (0.172)	13 (216)

Table 7 continued

	Worked during quarter (OLS)	Worked during quarter (logit)	Log earnings during quarter if worked	Earnings during quarter
Outside Milwaukee	0.125 (0.084)	0.638 (0.449)	0.073 (0.240)	478 (318)
<i>Reading level (default = 8th grade)</i>				
8–10th grade	0.067 (0.059)	0.341 (0.276)	0.211 (0.170)	306* (18)
>10th grade	0.104 (0.076)	0.495 (0.352)	0.278 (0.177)	363 (238)
Reading level missing	0.514*** (0.105)	(omitted)	0.072 (0.415)	1,300 (794)
<i>502 risk level (default = low)</i>				
Moderate	-0.052 (0.083)	-0.304 (0.412)	-0.076 (0.192)	-231 (308)
High	-0.064 (0.072)	-0.344 (0.367)	0.011 (0.187)	-241 (261)
Log (incarc. length)	0.138*** (0.043)	0.649*** (0.216)	0.008 (0.092)	309** (127)
Constant	-0.585* (0.324)	-5.053*** (1.576)	6.647*** (0.830)	-1,601* (960)
Observations	688	673	369	688

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Robust standard errors clustered at the individual-level are in parentheses

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