

Does Work Release Work?*

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Abstract

United States prisons release more than 600,000 individuals each year. Within three years of release, 50 percent of released prisoners are back in prison. Work release has the potential to serve as a structured re-entry into the mainstream labor market. In an economic model of crime, an improvement in legal labor market opportunities should reduce criminal activity. After using a variety of econometric techniques to address the non-random selection of inmates into work release, I find that inmates who participate in work release have better post-prison employment outcomes. These positive effects are largest in the first year following release, but persist for at least three years. The earnings of work release participants are 30 to 40 percent higher than the earnings of non-participants, but the increased propensity to work explains much of this earnings differential. I also find that the labor market gains do not necessarily translate into reduced recidivism. The reduction in recidivism is dependent on the types of crimes offenders committed. Better labor market opportunities reduce recidivism for those convicted of income generating offenses, but have no impact on offenders who committed non-economically motivated crimes.

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1 Introduction

Mass incarceration has become an American reality. More than 600,000 individuals are released from prison each year. Yet 50 percent will return to prison within three years (Langan and Levin 2002). The poor labor market outcomes of ex-offenders may be an important contributing factor to the high rates of recidivism. Ex-offenders face many barriers that prevent successful labor market reintegration. They have low levels of education, little job experience, substantial mental health problems, poor social networks, and the stigma of prison time (Holzer, Raphael, and Stoll 2003).

Although interest and optimism in prison rehabilitative programming goes in waves, most types of training programs have been attempted in the prison setting. There are literacy and GED classes to increase the human capital of inmates. There are vocational training programs. There are also prison industries programs to simulate on the job training. The evidence on the effectiveness programs is certainly mixed (Wilson, Gallagher, and MacKenzie 2000). One limitation of these programs is that no prison work experience can simulate the free world. The transition from an in-house prison industry to the mainstream labor market would still be a jarring one.

Work release programs have the potential to serve as a structured reentry into the mainstream. In work release programs, inmates nearing the end of their sentence move into a less secure facility. From this center, they hold regular jobs in the community and return to the center during their non-work hours. Correctional officials ensure that inmates go to work everyday, arrive at work on time, and complete required work tasks. For inmates who have little steady job experience, this opportunity could be invaluable. Another important aspect of work release is that inmates arrive at their release date with a job and savings instead of a bus ticket home and \$100.

Unlike manpower programs in non-institutional settings, improving employment outcomes is not the ultimate goal of a prison program. A desire to reduce recidivism motivates attempts to increase the post-release earnings of inmates. If a program that increased earnings failed to reduce recidivism, the criminal justice world would likely view it as a failure. While the link between increasing legal earnings and decreasing crime is rather intuitive, the empirical evidence supporting the link has been mixed.

Using administrative data from the Florida Department of Corrections, I test the effect of work release participation on post-release employment outcomes and recidivism rates. Florida is the country's third largest state prison system and releases more than 30,000 individuals each year. In Florida, approximately one-sixth of released inmates participate in work release. Since inmates are not randomly assigned to participate in work release,

there may be substantial concerns of bias when comparing the outcomes of work release participants and non-participants.

I use a variety of econometric techniques to address the non-random selection of inmates into work release. I first estimate OLS models that include a detailed set of covariates including information on demographics, criminal offenses, and cognitive test scores. I then use information on pre-incarceration earnings to estimate difference-in-differences models. I next use propensity score techniques to see if the estimated treatment effect varies with the likelihood of participating in work release. I also estimate the average treatment effect as well as the effect of the treatment on the treated.

To preview the main results, I find that inmates who participate in work release have better post-prison employment outcomes. These positive effects are largest in the first year following release, but do persist for at least three years. Compared to other similar inmates, individuals who participate in work release have higher rates of employment and higher quarterly earnings. The earnings of work release participants are 40 percent higher than non-participants in the first year following release. By the third year, this gap falls to 30 percent. An increased propensity to work explains much of the earnings differential.

An economic model of crime predicts that an improvement in legal labor market opportunities will reduce criminal behavior. The evidence from Florida supports this conjecture. Yet while all criminal behavior may respond to incentives, one would expect that crimes committed for financial gain would be most affected by a change in legal earnings. When I divide the sample into offenders whose original crime was income-generating and offenders who committed nonmarket crimes, both groups appear to benefit from work release participation. All empirical specifications find gains in employment and earnings for both groups. Yet only the income-generating offenders exhibit a reduction in recidivism. This is strong evidence that offenders respond differently to prison programming, and that employment training programs are likely not the solution for all types of offenders.

2 Background on Work Release

North Carolina created the country's first modern work release program in 1957. The expansion of work release programs across states was slow until the passage of the Federal Prisoners Rehabilitation Act in 1965. This law, which only directly affected the federal prison system, included provisions for work release, short-term furloughs, and transfer of adult offenders to community treatment centers (Moeller 1969). The Federal act was very influential in altering state policy. While only three states had work release programs prior to the law's passage, 42 states created work release programs by 1973 (Waldo and Chiricos

1977).

While the details of work release programs vary from state to state, there are many common aspects. Work release allows inmates to work in the community during the day. Inmates spend all non-work time at a secure facility. It is important to distinguish work release programs from inmate work crews collecting trash along the highway or working on a state construction project. Inmates on work release hold regular jobs in private businesses. They might wash dishes, bag groceries, or garden. Inmates receive the same wages as other employees, but the state deducts inmates' room and board as well as other obligations like child support and victim restitution from inmate paychecks. Since individuals on work release are working in the community without constant supervision, inmates are only eligible to participate if they have the lowest custody class. Work release programs are generally reserved for inmates nearing their release date.

Florida initiated its work release program in 1970. In Florida, prison officials evaluate inmates' readiness for work release when 10 months remain on the sentence. Inmates are excluded from consideration if they have an escape history, have been terminated from work release, have four prior prison spells, have committed a sexual offense, or have a violent detainer. If inmates are approved for work release, they can express their preferences for two work release centers (WRC). Inmates usually request a facility in their county of origin. Unlike the larger state prisons that are predominantly in rural areas in Northern Florida, the WRC are distributed across counties proportionally to the county's contribution to the total Florida prison population.

In Florida, the inmates eligible for work release greatly exceed the number of beds at work release centers. Currently Florida has around 3,000 WRC beds and a waiting list of more than 1,000 inmates. While the beds at the WRC were not filled to capacity during the 1990s, this was not due to a shortage in eligible inmates. Officials at the Department of Corrections attribute this mismatch to a lack of coordination between officials overseeing major prisons and those running the WRC. An additional factor was the demand for prison labor to assist the prison construction boom of the 1990s (personal communication). Since wardens needed inmates to work on construction projects, fewer were transferred to WRC.

After a bed becomes available at one of the inmate's requested WRC, the inmate is transferred and completes a brief orientation. Then the inmate needs to find a job. The current system in Florida puts the onus for the job search completely on the inmate. The centers do not employ career counselors to assist in the job search. Some centers do have informal arrangements with employers who have a high willingness to employ inmates on work release. Most inmates find jobs through newspaper listings. Officials estimate that it takes inmates between two and four weeks to find a job. With this in mind, Florida does

not allow inmates to move to a WRC if they have fewer than 60 days remaining on their sentence. It takes time for inmates to orient, find a job, and start earning money.

Most inmates find jobs that pay little more than minimum wage. Currently work release inmates in Florida earn an average of \$7.75 per hour, about one dollar more than the state minimum wage. The state deducts 45 percent of after tax pay to cover room and board. The state also withholds 10 percent of net pay for restitution or court ordered payments and another 10 percent for family assistance including child support payments. Inmates are also required to save 10 percent of their net pay. Inmates are allowed \$65 per week for incidentals, and any remaining funds are deposited in the inmate's saving account. Even with relatively low wages and high garnish rates, inmates participating in work release accumulate an average of \$1000 in savings by their release date (personal communication with Zaccardi).

While inmates at WRC can work unsupervised in the community, their lives are still very structured. Inmates are only allowed out of the centers during their scheduled work hours. Inmates do not have the option to sleep late and miss work or leave work early and go out with friends. Both types of behavior would violate center rules and could result in a return to prison. Failing to return to the center at the scheduled time could actually be classified as an escape. Additionally, center staff visits the inmate's workplace twice a week to monitor the inmate's behavior.

Work release participation may improve an inmate's post-release employment outcomes through numerous channels. Inmates may acquire job-specific skills and improve their human capital. Given that most work release inmates have minimum wage jobs, this channel may be unlikely. More importantly perhaps is the time at a WRC teaches inmates how to hold a job. Since many inmates have not held a steady job prior to incarceration, this experience may play an important role in improving the non-cognitive skills of workers.

Another potentially important role of work release participation is the safety net it provides at release. When Florida releases inmates from prison, inmates receive a bus ticket home and 100 dollars. This period immediately following release is a very uncertain one for most ex-offenders (Nelson, Deess and Allen 1999). They have no job, no savings, and no place to live. For many, the temptation to return to illegal work may be too strong to resist. Work release participation may provide ex-offenders with an important safety net. Inmates are encouraged to continue their work release job after their release. They also have savings that could provide a cushion as they searched for a better position. Work release inmates also have recent job experience and perhaps more importantly, recent experience searching for a job.

While work release participation certainly has the potential to improve post-release

outcomes, the existing research is not definitive. The absence of reasonable control groups make it difficult to interpret previous studies. Most evaluations have focused on criminal justice outcomes and none had access to administrative data on post-release employment. Although Witte (1977) finds no significant differences in recidivism, the seriousness of the crimes has changed. Work release participants are more likely to be convicted for a new misdemeanor than a new felony. While Witte did not have access to administrative data, using self-reported employment data, she finds that work release does increase employment and wages. Participants attribute some of this differential to the work experience they gained in work release (25 percent) and to the value of using the work release employer as a reference for new positions (16 percent). Waldo and Chiricos (1977) are less optimistic in their conclusions. Although their study has the advantage of a random assignment design, the sample size is quite small. The treated group only includes 188 members and for important recidivism outcomes like reincarceration within 21 months, the sample size drops precipitously. Although the reincarceration rate is 6 percentage points lower for work release participants, this difference is not significant. The project design lacks the power to detect substantively important changes in recidivism.

In more recent work, Turner and Petersilia (1996) examine the work release program in Washington State. They judge the program to be a success because many inmates participate and few crimes are committed by inmates on work release. They do not find any significant effects on 12-month recidivism accounts. The short follow-up window may account for their failure to detect a change in criminal behavior. Turner and Petersilia have no access to post-release employment data, so they cannot measure the effect on program participation on labor market reintegration.

This paper contributes to the literature in a few important ways. The most basic addition is that the existing literature is quite old. Thirty years later, the prison population is different, the available data has improved, and we have new econometric techniques to bring to the data. I also can test an important implication of the economic model of crime - an improvement in labor market opportunities should have a greater effect on criminal behavior that is motivated by potential financial gain.

3 Work Release Program Selection

Work release participation is not randomly assigned. For prison officials to allow inmates to work unsupervised in the community, they need assurances that the inmates are not a public safety threat. Since we are never able to observe the same inmate in two states of the world - both as a work release participant and a non-participant, it is important to

carefully consider the implications of the work release assignment process.

In a typical evaluation of a labor market training program, researchers are most concerned about the individual decision to participate. Individuals are making decisions by comparing their gains from participation to the costs. Individual participate if

$$E\left[\sum_{j=1}^{T-k} \frac{Y_{1k+j}}{(1+r)^j} - c - \sum_{j=0}^{T-k} \frac{Y_{0k+j}}{(1+r)^j} | I_k\right] \geq 0 \quad (1)$$

Yet here there is little concern about the participant's decision. Work release is the first opportunity to leave the confines of prison. It provides inmates with a chance to return to their home community and earn some money. The opportunity costs of participating are very small if not zero. As Florida's sizeable work release waitlist suggests, inmates would choose to participate in work release almost regardless of their potential gains. The limiting factor is available bed space in work release centers and the decisions of prison officials.

While the individual choices of inmates may not generate selection bias, there is still reason to be concerned about the administrative selection process. The administrative selection process has a few components. The first piece are the regulations that determine who is eligible for work release. Inmates must have the lowest custody classification. Certain types of offenders, those convicted of sexual crimes and kidnapping for example, are prohibited from work release participation. All these regulations concern inmate characteristics that are observable to me as a research.

It would certainly be conceivable that the other choices made by prison officials represent the same optimization problem that individuals face in the free world. It is possible to imagine a world where prison officials chose the individuals that they thought were most likely to benefit from a work release assignment. Yet there is no institutional evidence to suggest this was the case. The Florida Department of Corrections received significant criticism in the 1990s for failing to prioritize inmates who were most likely to benefit from prison programming (OPPAGA 2000). The choices made by prison officials were motivated by administrative realities - space constraints and prison labor needs - not by the potential gain from participation.

After prison officials approve an inmate for transfer to a work release center, the inmate has the opportunity to request two facilities. Inmates only begin work release when a bed becomes available at one of their requested facilities. Some approved inmates never transfer to a work release center. If inmates have not been assigned when they are two months from release, they are removed from the queue.

Having a work release assignment, the only outcome I observe, is dependent on a two

stage process - first an inmate must be approved for work release and then the inmate needs a work release bed. The first stage is a decision made by prison officials. Since I have access to detailed administrative data, most of the information available to prison administrators is available to me. There may be other unobservable factors that affect the decision. I will use all available data and a variety of econometric techniques to control for selection on observables.

The probability that an approved inmate receives a work release assignment is affected by institutional constraints. The primary factor is space availability in an inmate's requested facilities. While I do not know the facilities that inmates requested, I can control for time and county of origin as proxies for availability of work release beds in an inmate's preferred location.

The likelihood of getting a work release assignment is also affected by space constraints at the sending prison facility as well as demands for prison labor. As discussed above, during the 1990s there was a demand for prison labor to assist the prison construction boom (personal communication with Zaccardi). Since wardens needed inmates to work on construction projects, fewer were transferred to work release centers. In Figure 1a, you can actually see a relationship between the distance an inmate's prison is from a construction project and the likelihood that inmate will receive a work release assignment. Inmates further from construction are not needed as construction labor and thus are more likely to participate in work release. These institutional constraints generate noise in the work release assignment process. They reduce my ability to predict work release participation with the observable administrative data, but these institutional constraints should be unrelated to inmate gains from program participation.

4 Data

I evaluate the Florida work release program using administrative data from the Florida Department of Corrections. This data, originally created for a Tyler and Kling (2007) study of the returns to a prison GED, include detailed information on all aspects of an offender's criminal history as well as the information from the prison intake record and prison administrative files. Essentially the dataset includes all the information in the Florida DOC mainframe. Using Social Security Numbers (SSNs), the DOC has matched these inmate files to earnings data from the Florida Unemployment Insurance (UI) System.¹

¹ All SSNs in the data were verified by programmers at the federal Social Security Administration based on standard verification algorithms used by the Employment Verification Service that matched DOC data

There are definite strengths and limitations of using UI wage records as the employment outcomes. The most significant advantage of UI wage records is that they allow you to follow a relatively large number of individuals for a low cost. Since the ex-offender population is going to be particularly difficult to track, the value of administrative records should not be understated. A definite limitation of UI wage records is that they do not include earnings from self-employment, out-of-state jobs, and informal work. The UI earnings likely understate an individual's total earnings. The discrepancies between total earnings and UI wage earnings may be particularly large for the ex-offender population (Kornfeld and Bloom 1999), many of whom may be working in the informal labor market.

This understatement of earnings should not affect the evaluation of work release unless the measurement error in UI earnings is correlated with treatment status. Here this might be a problem. Work release may be correlated with a shift away from the informal labor market and into UI covered employment. This is not particularly problematic since there are advantages to participating in the formal labor market. Jobs covered by the UI system may be more stable and provide access to other benefits.

Another weakness of UI data is that it only reports total quarterly earnings from each employer. This data limitation makes it difficult to distinguish whether a difference in average earnings is driven by changes in the extensive margin (employment), the intensive margin (hours worked), or the hourly wage.

From the administrative data, I construct a sample of males who entered prison after 1993 and are released by 1999. I exclude inmates who enter during an earlier period because they had a different sentencing regime. All inmates sentenced after 1993 are subject to the 85 percent law requiring all inmates to serve 85 percent of their sentence. This regulation alters the incentives of inmates while incarcerated. In the earlier period, inmates could reduce their time in prison by participating in prison programming. I also need to restrict the sample to those released by 1999 to ensure that I have enough post-release data to evaluate work release.

With this time-based sample restriction, the maximum incarceration spell in the sample is five years. While this sample clearly excludes an important right tail of individuals with very long sentences, the national median incarceration spell is 15 months (Austin 2001). Relatively short incarceration spells are the modal experience for today's ex-offenders. It would certainly be interesting to understand if work release had a different impact for offenders who had spent a decade in prison, but many of these inmates would never become

on the digits of the SSN, date of birth, sex, and name to SSA records with some tolerance for clerical error. As might be expected in data on criminal offenders, about 19 percent of the sample lacks a valid SSN. Individuals with non-valid SSNs are more likely to be minority, a non-Florida resident, and unemployed at the time of arrest.

eligible for a community-based program like work release.

For almost all of the analysis, I limit the sample to inmates who enter prison in minimum custody. Only a select, small group of the inmates who enter prison at higher custody levels (medium or close) have their custody level reduced quickly enough to be eligible for work release within a five year sentence. In trying to construct an appropriate counterfactual, it seems problematic to be drawing most of the treatment group from minimum custody inmates and a large portion of the comparison group from medium and close security inmates. Inmates in medium and close custody have different prison experiences and may have different propensities to recidivate regardless of work release participation.

The minimum security inmates account for forty percent of the potential sample of inmates. A quarter of minimum security inmates are incarcerated for drug sales. Property theft and burglary account for another forty percent of minimum custody inmates. While the sample does not include offenders serving time for murder, manslaughter, or sexual offense, 13 percent of minimum custody inmates are incarcerated for a violent offense.

5 Labor Market Outcomes

Figures 1 and 2 graph the quarterly earnings and employment rates of work release participants and non-participants. The zero quarter is the inmate's forecasted release quarter.² The figures are striking. In the period prior to release, the earnings of work release participants greatly exceed the earnings of non-participants. This is not surprising since the work release inmates are employed in UI covered positions during their stay at a work release center. What may be more surprising is that the quarterly earnings of non-participants do not equal zero. Part of this is due to discrepancies in the forecasted release date and actual release date. The other important factor is that inmates with relatively short spells may not yet be in prison four quarters prior to their release. This is captured by the sharp decline in employment rates for non-participants in the four quarters leading up to the release quarter.

In the first post-release quarter, ex-offenders who participated in work release have quarterly earnings that are more than double the earnings of non-participants. Although the earnings of work release participants are substantially larger than the earnings of non-participants, the first quarter earnings are lower than what the inmates were earning during their last quarter in prison. While the work release program is designed for inmates to keep their jobs after release, it is clear that not all inmates are maintaining their positions.

²I use the forecasted release date instead of the actual release date since the actual release date may be altered by an inmate's behavior in prison or on work release. All results are robust to using the actual release quarter

Over the course of the first year following release, the earnings of work release participants fall and the gap between the group declines. Although the gap does decline, the earnings of work release participants exceed the earnings of non-participants by at least \$400 in each quarter. While these figure suggest that work release participation has lasting positive effects on employment outcomes, these graphs are unconditional differences in earnings. If the inmates who participate in work release tend to be different from non-participants, the difference in earnings may not be causal.

Table 1 compares the characteristics of participants and non-participants. This table highlights that work release participation is not randomly assigned. A comparison of basic demographic characteristics indicates that while the two groups have similar education levels and ages at release, a greater share of white inmates participate in work release. Also, while the two groups have been convicted of similar primary offenses, work release participants have longer state prison spells. Although the gap in total time incarcerated is somewhat smaller (work release participants spend less time in jail), there is still a 5 months gap in total time in custody.

There are other important observable differences between the two groups. A higher percentage of work release participants are serving their first term in prison. Work release participants are also more likely to take advantage of other prison programming during their incarceration spell. A final difference worth highlighting is the gap in pre-prison earnings and employment rates. Inmates in the comparison group had significantly worse employment outcomes before prison entry.

While there are important differences between the treatment and comparison groups, accounting for selection on observables should substantially reduce the bias. As stated earlier, all inmates want a work release assignment. It is one of the only opportunities to earn money while incarcerated. It also allows inmates to spend their days working in the community and gives them more opportunities to see family. Consequently, the important selection issues are the decisions made by prison officials, not those made by the inmates.

Prison officials responsible for approving inmates for work release and assigning inmates to centers have access to the same administrative data that I am using. Their decisions are influenced by an inmate's sentence length, prior incarceration spells, and primary offense. While the observables do not fully predict exposure to treatment, they do account for 25 percent of the variation. Two other important determinants of work release participation are capacity constraints both at the inmate's initial prison facility and the requested work release facility. A final issue during the 1990s was the need for prison labor to help construct new prisons. If an inmate was initially assigned to a prison facility near a construction project, they would be less likely to be assigned to work release.

Recognizing the importance of selection on observables, I test the following specification.

$$Y_{it} = \beta_0 + \beta_1 WRA_i + X_i \beta_3 + CrimJust_i \beta_4 + AdmitVar_i \beta_5 + County_i + YrQtr_t + \varepsilon_{it} \quad (2)$$

where Y_{it} is the outcome variable for individual i in year-quarter t . I examine three different outcome variables - an individual's quarterly earnings in \$2002, whether an individual has positive earnings in that year-quarter, and whether an individual has committed an offense by that year-quarter that leads to prison, probation, or revocation. The coefficient of interest is β_1 .

X_i is a vector of basic demographic characteristics including race, education, age and age squared. The *CrimJust* vector includes controls for an individual's predicted length of incarceration, primary offense, prior incarceration spells, and disciplinary reports accumulated during those prior spells. The *AdmitVar* vector includes detailed information collected when an inmate is first admitted to the state prison system. This includes information on an individual's birthplace, place of residence, medical grade, psychological grade, marital status, immigration status, and recent employment status, as well as the results from a test of cognitive skills (TABE).

Since inmates usually select work release programs in their county of origin, I include a county fixed effect. This county fixed effect also controls for variation in local labor markets across the state of Florida. The specification also includes a year-quarter fixed effect.

I fit (2) on four stacked quarter of data so that I can estimate β_1 for the 1st, 2nd, and 3rd years after an individual's forecasted release date. In all regressions, I cluster the standard errors by the inmate's county of origin, allowing for an arbitrary correlation between error terms for inmates from the same county, either contemporaneously or over time. Standard errors are also robust to heteroskedasticity.

I also estimate a difference-in-differences specification taking advantage of information on pre-prison employment outcomes. The crucial identification assumption in the DD estimate is that the comparison group can capture the change in employment outcomes from the pre to post period in the absence of work release. This does not assume that the treatment and comparison groups would have had the same earnings in the absence of the program, only that the change in earnings would be the same for the two groups. The basic DD specification is below.

$$Y_{it} = \gamma_0 + \gamma_1 Post_{it} + \gamma_2 WRA_i + \gamma_3 PostWRA_{it} + \gamma_4 Age_{it} + YrQtr_t + \eta_{it} \quad (3)$$

In the DD specification, γ_1 measures the changes in the employment outcome from the pre to the post period. γ_2 is the estimate of the fixed difference between the treatment and the comparison group. If prison officials favor better inmates in the work release selection process, γ_2 will be positive. γ_3 is the coefficient of interest. It measures the effect of having a work release assignment. The DD specification does not include many control variables because they do not vary across time. Only age and year-quarter can be included.

Using a DD specification to estimate the effect of a manpower training program is usually problematic. As Ashenfelter highlighted, individuals who select into training programs frequently have a pre-program dip in earnings. If the treatment group has a pre-program dip in earnings, a DD specification will overestimate the effect of the training program. While a pre-incarceration dip in earnings is likely, this dip should occur for both the treatment and the comparison group.

Tables 2 and 3 display the results from estimating equations (2) and (3). In Table 2 the dependent variable is quarterly earnings, and in Table 3 the dependent variable is a binary indicator for positive earnings in that quarter. I first estimate the binary relationship between work release and the outcome variables and then progressively add control to illustrate how the estimated effect changes as I control for more of the observable differences between the treatment and comparison group.

One consistent finding in all models is that the difference between work release participants and non-participants is greatest in the first year following release. Work release participants are released from custody with a job and savings. Even if they choose not to keep their work release position, they still have the advantage of recent labor market experience as well as job search experience. While the comparison group does catch-up, the difference between the two groups remains large and significant in the third year post-release.

The second column contains the basic binary estimates for the minimum custody sample. In this specification, the quarterly earnings of work release participants are \$625 higher in the first year. While the estimates for the second and third years are lower, they still exceed \$450. These estimates are both substantively and statistically significant. If \$625 is the true work release treatment effect, work release increases earnings by 170 percent.

Since work release participation is not randomly assigned, the estimate of β_1 may be biased. I add a series of control vectors and examine changes in the treatment effect estimate.

The first vector controls for basic demographics including race, age, and education. In past prison program evaluations, these demographic controls are often the only data available to researchers. The inclusion of these controls decreases the estimated treatment by 13 percent.

Next, I control for a vector of criminal justice information including incarceration length, primary offense, and prior criminal history. It is unclear how the estimate should change when these coefficients are added. Inmates in work release have longer incarceration spells. If the incarceration penalty increases with the duration of incarceration, the work release estimate may have had a downward bias. At the same time, the inmates in work release have fewer prior prison terms. If inmates with multiple incarceration spells have worse labor market outcomes, the treatment estimate may have had an upward bias. The results in column four indicate an upward bias in the previous estimate. The inclusion of the criminal justice controls decrease the estimated treatment effect by 10-16 percent.

Column five includes controls for additional information collected at prison admission. This includes information on marital status, birthplace, employment history, and citizenship. I also control for the inmate's score on a basic literacy test. If the inmates assigned to work release have higher cognitive ability, including the TABE scores should reduce this source of bias. The estimated coefficients fall by 7-9 percent.

The allocation of prison inmates to work release assignments is clearly not random, yet the inclusion of a rich set of covariates does not eliminate the estimated treatment effect. Estimates from this specification indicate that in the first year, work release participants have mean quarterly earnings that are \$410 higher than other minimum security offenders with similar characteristics. The work release advantage is \$309 in the second year, \$319 in the third year, and all three estimates are statistically significant.

These estimates are robust to the inclusion of county fixed effects. Column 7 indicate these results are also robust to the inclusion of controls for prison programming. Prison programming was not included in early specifications since the decision to enroll in academic or vocational classes is clearly endogenous, but since the rates of program participation are very different between the two groups, I did want to test if the estimates would be affected by data on prison programming participation.

The entries in the final column of Table 2 are from the equation (3) DD specification. This specification uses the information from offenders' pre-incarceration earnings. It controls for fixed differences in the two groups across time. This specification does not include most of the controls variables because they are not time varying characteristics. Only age and year-quarter dummies are included in this model. The DD estimates in column 8 are very similar to the estimates from the full OLS model (2). Only the estimate from the third

year post-release decreases.

Both the OLS and the DD model suggest that work release participation has a large, significant effect on earnings. An increase in earnings could result from an increased propensity to work, an increase in the number of work hours, an increase in the hourly wage, or any combination of the above.

I estimate linear probability models to examine the effect of work release on the propensity to be observed with positive earnings. Table 3 displays the estimates from these models. Work release increases the probability of employment. This effect is particularly strong in the first year, but remains larger and significant in years two and three. In the preferred OLS specification (column 6), work release participation increases the probability of employment by 11 percentage points in the first year and 6 percentage points in the next two years. The DD estimates in column 7 are smaller, but still significant for all three years.

6 Propensity Score Methods

Another approach for correcting for selection into treatment is to use propensity score methods. Propensity score methods assume that selection into treatment is based on observable characteristics. This is the same assumptions underlying the OLS estimates. The advantage of calculating propensity scores is that you observe how the treatment effect varies with the propensity to be treatment. An estimated treatment effect that varies with the propensity score is not necessarily evidence of a heterogeneous treatment effect. It could also suggest that there is more selection along unobservables in certain areas of the propensity score distribution.

First I estimate a logit model for the probability of participating in work release. I include the full set of covariates allowing for polynomial terms and interactions. I also include information on pre-incarceration earnings. The full set of covariates explains 25 percent of the variation in observed work release participation. I then predict the probability of being treated and divide the sample into 100 equally sized bins. The covariates are balanced within these bins. Figure 3 shows reasonable overlap in the distribution of propensity scores for the treatment and comparison samples.

Figure 4 graphs the earnings of work release participants and non-participants in the year prior to prison entry by propensity score bin. Although the earnings of work release participants and non-participants are different on average, there is no significant difference when you compare earnings within propensity score bins.

In contrast, after prison release, there are clear differences between work release participants and non-participants (see Figures 5 and 6). In the first year after release, the

quarterly earnings of participants are higher than non-participants across the propensity score distribution. The employment graph tells a slightly different story. The treatment effect has a u-shape. The effect is largest for the groups with very low or very high estimated propensity scores. This could be evidence of a heterogeneous treatment effect or it could be evidence that selection on the unobservables is more dominant in the tails. Similar patterns are evident in the second and third year after release (Figures 7-10).

Using the estimated propensity scores, I can calculate the Average Treatment Effect (ATE) $E(Y_1 - Y_0|X)$ and the Treatment on the Treated (TOT) $E(Y_1 - Y_0|T = 1)$. The ATE estimate is the effect of picking a person at random and giving them the treatment. To estimate the ATE, the observations in the sample need to be reweighted. This procedure increases the weight on untreated observations with large propensity scores (weight = $1/(1 - pi)$) and decreases the weight on treated observations with large scores (weight = $1/pi$). Hirano, Imbens, and Ridder (2003) show this is efficient if estimated propensity scores $\hat{p}(xi)$ are used as weights. The results are in the first column of Table 4. The ATE estimates are larger than the earlier OLS estimates, but follow a similar pattern. The greatest improvement in earnings and employment is seen in the first year. In later years, the gap between participants and non-participants declines but remains statistically significant.

I can also estimate the TOT. This estimate is more comparable to the OLS estimate. Here I calculate the difference in the average outcomes of treated and untreated observations within each of the 100 propensity score bins. I then get a weighted average of these 100 differences, weighting each bin estimate by the share of treated observations within the bin. These estimates are in the second column of Table 4. In the TOT estimates, the earnings effect is relatively constant across the years, but the employment follows the same pattern seen in other estimates - the effect is largest in the first year, but remains significant in the second and third years.

Using propensity score methods confirm the earlier results from the OLS and DD analysis. Participating in work release does increase earnings and employment rates. Work release has the largest impact in the first year following release. Since this first year is crucial to the offender's successful reentry, the potential importance of this result should not be underestimated. I do find that the ATE exceeds the TOT. This suggests that the inmates who do participate in work release may not be the individuals with the most to gain from participation.

7 Changes in Employment

While UI earnings data prevent us from assessing the effect of work release participation on wages (prices) separate from hours (quantity), it is still interesting to consider what share of the total earnings effect is driven by changes along the extensive margin of employment. One approach to this question is to re-estimate the OLS earnings model conditioning on positive earnings. These results are displayed in the first column of Table 5. Conditional on employment, the earnings estimates are still positive but smaller than the unconditional effect on earnings.

These estimates provide some intuition, but are likely biased due to sample selection. Labor supply theory suggests a positive correlation between wages and the probability of employment. If individuals with higher earnings are more likely to work and program participation affects the probability of working, the estimate of the program's impact on earnings may be biased. Lee (2005) introduces a procedure for bounding average treatment effects in the presence of this type of sample selection. The intuition behind this procedure is to measure the differences in employment rates between the treatment and comparison groups. You can then construct worst-case scenario bounds by assuming that the individuals moved into employment were either at the top or the bottom of the treatment earnings distribution. This trimming procedure creates a bounds on the earnings effect for the population that would have worked irrespective of the work release treatment.

Since there is a large difference in the unconditional differences in the employment rates of work release participants and non-participants, the worst-case scenario bounds are quite large. The bound for the first year post-release is $[-601, 1533]$. As the differences in employment probabilities decline in years 2 and 3, the bounds become more narrow, but they still include zero. Using Lee's procedure, I cannot reject that work release had no effect on earnings conditional on employment.

8 Recidivism Outcomes

Although the immediate goal of most manpower programs is to improve employment outcomes, prison-based programs aim to reduce recidivism. The link between better employment outcomes and reduced recidivism has grounding in many theoretical approaches. The social ties and structure of a work environment may reduce criminal behavior (Sampson and Laub 1993). Another possibility is that work may serve as a substitute for crime. Since Becker (1968), economists have seen crime as responsive to incentives. While Becker emphasized the responsiveness to punishment, Ehrlich (1973) augmented the theory by considering

crime to be a form of work. In Ehrlich's formulation, criminal behavior is responsive to changes in legal employment opportunities.

A basic economic model of crime could take the following form. An individual engages in crime if and only if

$$(1 - p)U(W_c) - pU(s) > U(W) \tag{4}$$

where p is the probability of being caught, W_c is the gain from crime, s is the extent of the punishment, and W is the earnings from legal work. While this basic model may be too simplistic to capture many aspects of criminal behavior (many individuals engage in legal and illegal work concurrently (Fagan and Freeman, 1997)), it does generate the basic intuition that criminal behavior should decline if earnings from legal work increase.

Since the results above suggest that work release improves post-release employment outcomes, one would predict a decline in recidivism holding all else constant. Yet while theories linking work and crime are clear, the empirical evidence is more mixed (Witte 1980, Grogger 1998). To test this relationship using the Florida data, I re-estimate model (2) using recidivism as the dependent variable. I define recidivism as conviction for an offense that results in prison, probation, or revocation.³ I use three binary outcomes - recidivism within one year, two years, and three years. These measures are constructed using the date of the offense, not the date of the conviction.

The baseline recidivism rates are quite high. 25 percent recidivate within one year, 39 percent within two years, and 46 percent within three years. These rates are very similar to national estimates (Langan and Levin 2002). Table 6 presents the results from linear probability models with recidivism as the binary outcome measure. Among minimum custody inmates, the binary relationship shows that work release participation is associated with a 6 percentage point reduction in recidivism in the first year, a 7 percentage point reduction by year 2, and an 8 percentage point reduction by year 3.

This estimated relationship is substantially smaller when I include the control vectors. In the preferred OLS specification (column 6), the recidivism estimates are 3 percentage points reduction in year 1, 5 percentage points in year 2, and 6 percentage points in year 3. The estimated reduction in year 1 is marginally significant while the estimates for the later years are significant at the 1 percent level.⁴ The six percentage point difference in recidivism by year three is a 13 percent reduction in recidivism.

³Revocation is the end of a period of probation, parole, or conditional release caused by an offender's technical violation or new crime.

⁴I find similar results using propensity score methods.

These recidivism results provide some confirmation of the economic model of crime. Work release participation increases employment opportunities and raises earnings. This improvement in legal employment outcomes is accompanied by a reduction in crime (or at least a reduction in being caught). Yet it seems that certain types of criminal behavior would be more affected by a change in legal employment opportunities.

Not all crime is committed for financial gain. While a desire for income might motivate a robbery or a drug sale, it may not be the explanation for a drunken assault. In the earliest work on the economics of crime, Ehrlich (1973) suggests that it "may be appropriate to consider crimes against the person nonmarket activities, that is, activities that directly meet needs, as distinct from market or wealth-generating activities." Although Ehrlich does claim that a increase in hours of legal employment may reduce nonmarket crimes, a change in legal labor market opportunities should have the largest impact on individuals who commit income-generating crimes.

Implicit in this discussion is the idea of offender specialization. The concept of criminal specialization is a hotly debated one in the field of criminology. The competing theory is formalized by Gottfredson and Hirschi (1990) who propose a general theory of crime where the absence of self-control is the primary cause of criminal behavior. In this general model of crime, offenders are not weighing the costs and benefits of illegal activity, but instead fundamentally lack the ability to make the types of choices presumed by Becker and Ehrlich. Offenders may engage in rape one day and robbery the next. Farrington (1998) argues that it is "almost as though the most antisocial people commit different types of offenses at random."

In the economics literature, there is some empirical evidence that criminal specialization may be important. Bayer, Hjalmarsson, and Pozen (2007) investigate the importance of social interactions in criminal behavior. They find reinforcing peer effects - exposure to peers who have committed similar crimes increases the probability that an individual will recidivate in that crime category. Peer effects only matter when they reinforce an individual's criminal specialization. Even if offenders do show some diversity in criminal behavior, if motives are an important determinant of criminal behavior, it is reasonable to consider offenders who commit market crimes distinctly from offenders who committed nonmarket crimes.

My previous analysis suggests that work release participation increases the employment rate and raises earnings. When I aggregate all offenders, work release participation is associated with a significant reduction in recidivism in the second and third year following release. If motives are important, offenders who are motivated by financial gain should have a greater change in recidivism than offenders who are motivated by other concerns.

Using the Florida data, I divide offenders into two groups. The first group were originally sentenced for a income-producing crime. This category includes robbery, burglary, property theft, and drug sales. The other group of offenders committed crimes that could be thought of as consumption crimes. These offenses include violent crimes, weapons charges, drug possession, and other offenses. Among the sample of minimum custody offenders, 69 percent served sentences for income-producing crimes and the other 31 percent served sentences for consumption crimes.

Examining employment outcomes for these two groups suggests that both benefited economically from the work release opportunity (see Table 7). Interestingly, the consumption crime group seemed to have a larger return from work release participation. The two sets of estimates are statistically different.

The basic economic model of crime would anticipate a larger drop in criminal behavior among the group with the bigger improvement in employment outcomes, yet the results in Table 8 find the opposite. Only among the group originally convicted of income-generating offense do you see a drop in recidivism for work release participants. An improvement in legal work opportunities, even the very large improvement seen here, does not alter the recidivism rates for the consumption crime group.

My results suggest that the drug-dealing offender who has the opportunity to participate in work release is less likely to return to prison. While the popular image of the wealthy drug dealer makes it seem unlikely that an offender would exit the drug trade for an opportunity to wash dishes for little more than minimum wage, Levitt and Venkatesh (2000) find that the hourly returns from selling drugs in a gang barely exceed minimum wage, and the wage premium is offset by the riskiness of the activity. Ihlanfeldt (2007) also finds that the neighborhood availability of entry-level jobs affect the level of drug crime. Improvements in legal labor market opportunities generated by work release participation may be significant enough to shift some inmates away from income generating illegal activity.

A less optimistic possibility is that work release participants are do not reduce their criminal behavior, but are instead less likely to be caught or punished for their crime (Reuters et al. 1990). Employment in the legal labor market may be complementary with illegal activity - it provides a cover for illegal behavior and a less-risky source of new customers for drug-dealing offenders.

9 Conclusion

I find evidence that providing inmates with an opportunity to participate in work release can improve post-release labor market outcomes. Work release participants are more likely

to be employed. They also have higher earnings, but this difference in earnings may be fully explained by an increased propensity to work. Work release is not a vocational training program. Workers do gain job experience, but most work in low-skilled positions in the restaurant industry. It seems reasonable that a few months of employment as a dishwasher would not have a human capital effect and increase wages. If participation in work release increases employment, this is a significant outcome.

For prison programs, the bottom line is the recidivism effect. I find that work release participation does lower recidivism but that individuals who commit income-generating crimes are responsible for this change. Ex-offenders who commit non-income motivated crimes have improved employment outcomes after work release participation, but their probability of returning to prison does not change.

This research highlights the difficulty in designing effective prison programs. Here the immediate goal of improving labor market reintegration is achieved, but the ultimate goal of recidivism is only met for a subset of the offenders. Even if policy evaluators succeed in identifying the most effective prison training and work programs, it is unlikely that these interventions can alter the criminal behavior of all types of offenders.

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Figure 1a
Work Release Participation Rates by Distance to a Prison Construction Project

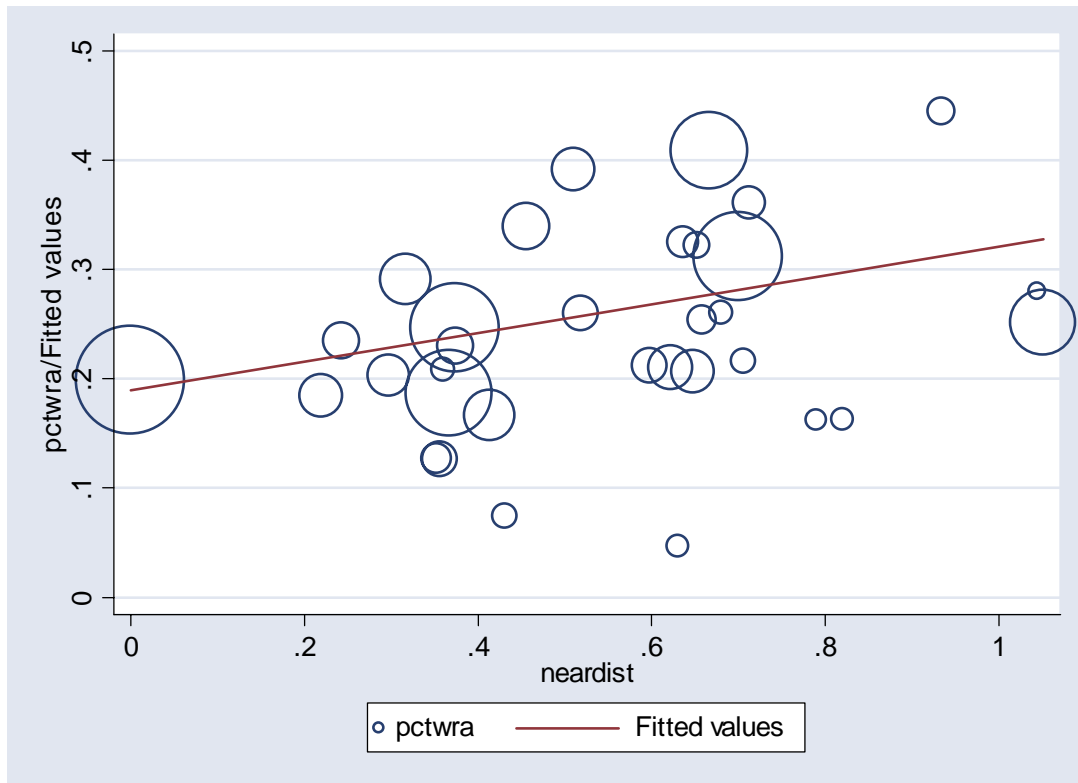


Figure 1
 Quarterly Earnings (\$2002) by Work Release Status
 Quarters Relative to Forecasted Release Date

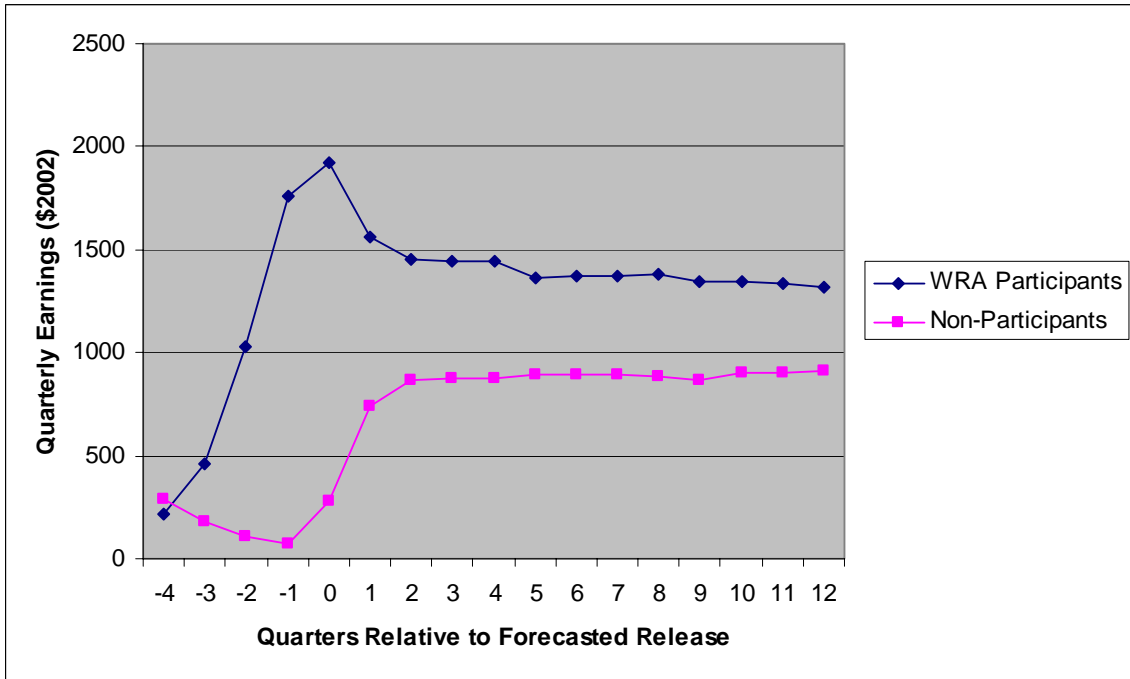


Figure 2
 Employment Rates by Work Release Status
 Quarters Relative to Forecasted Release Date

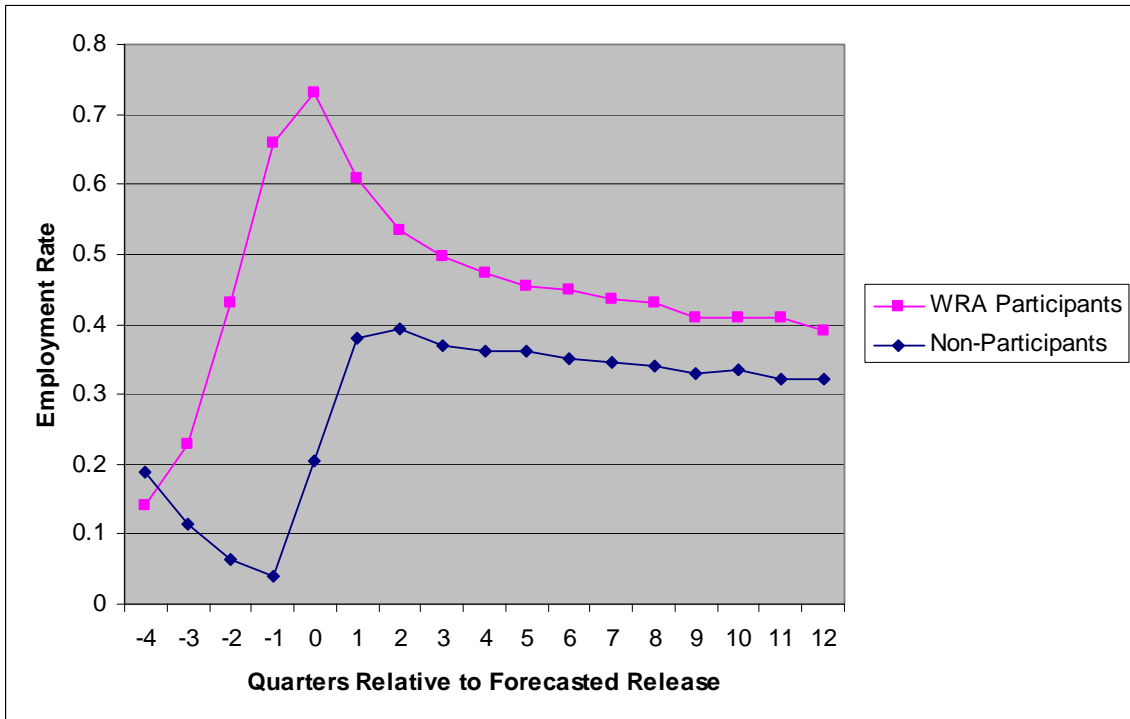


Figure 3

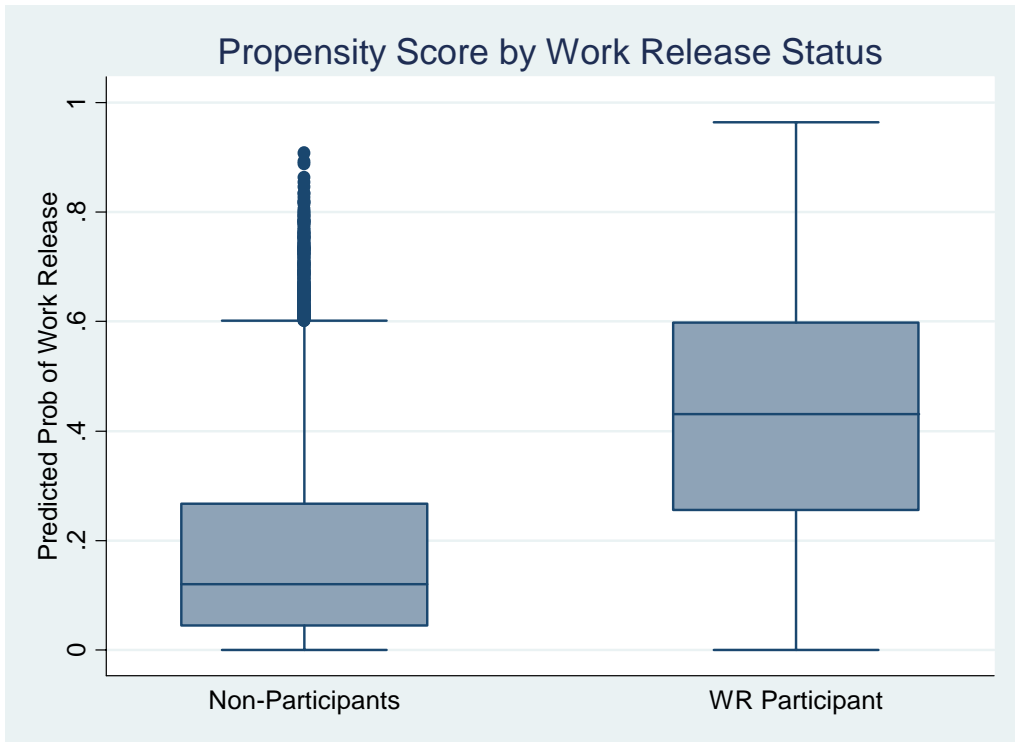


Figure 4

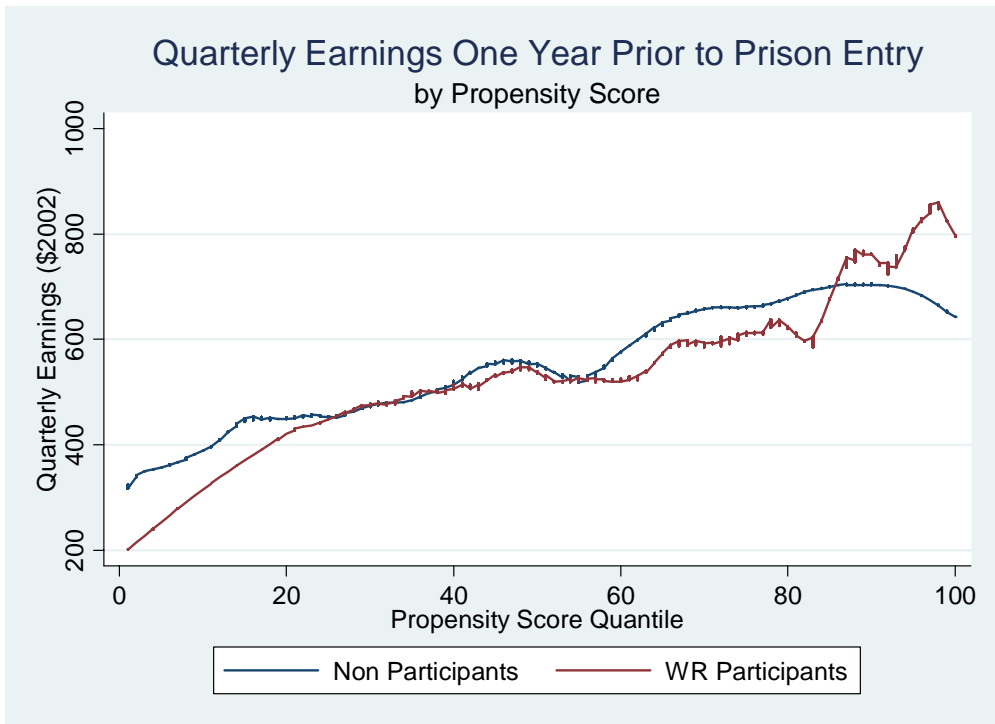


Figure 5

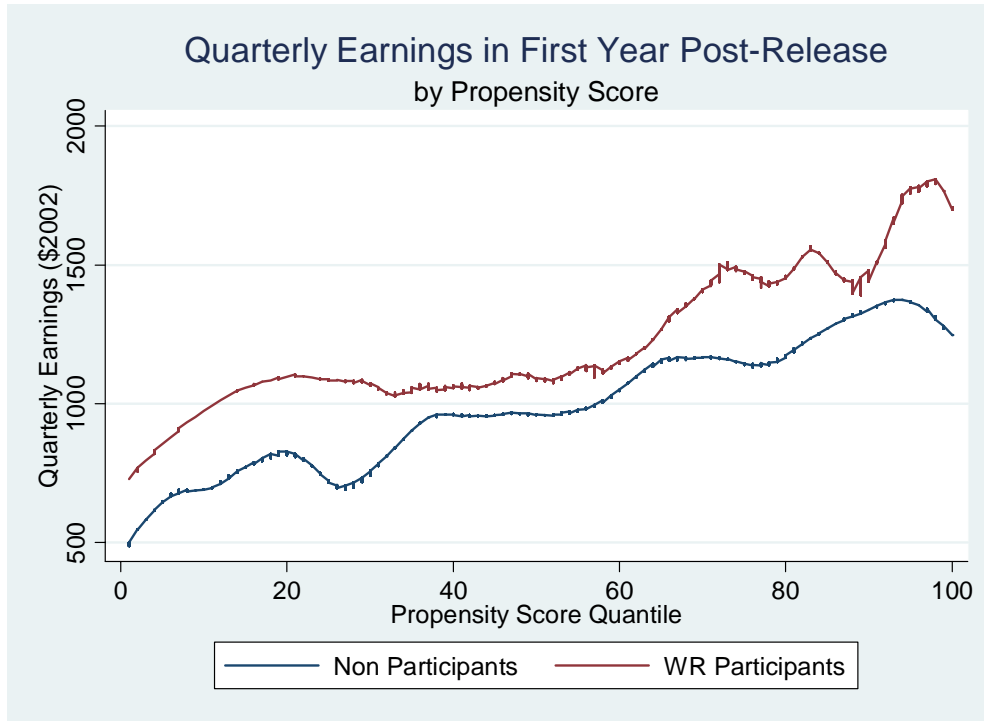


Figure 6

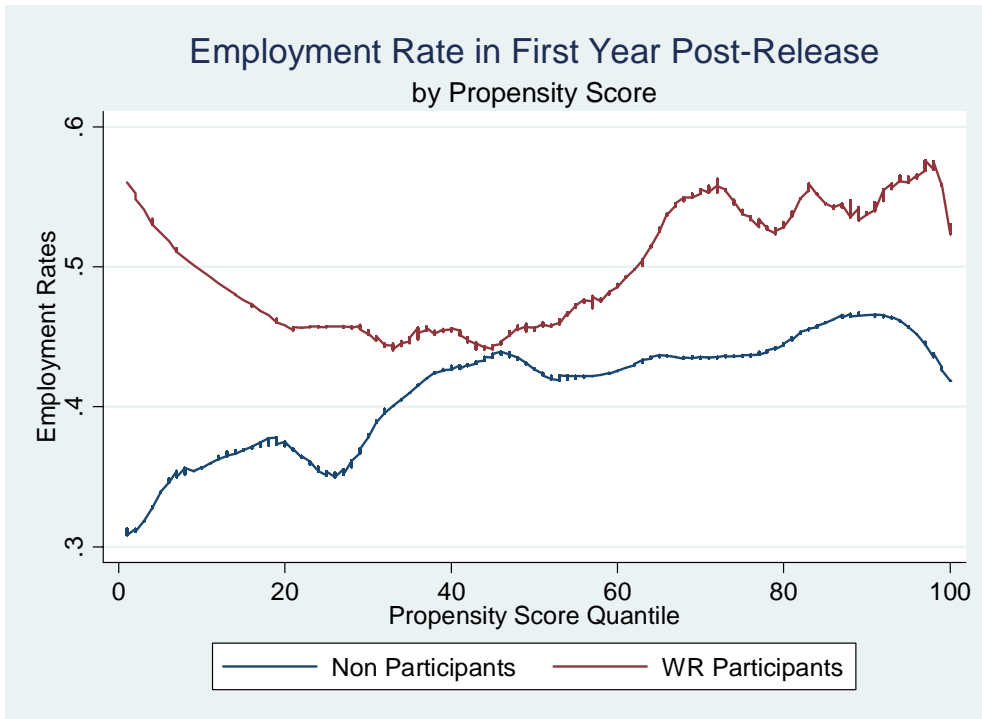


Figure 7

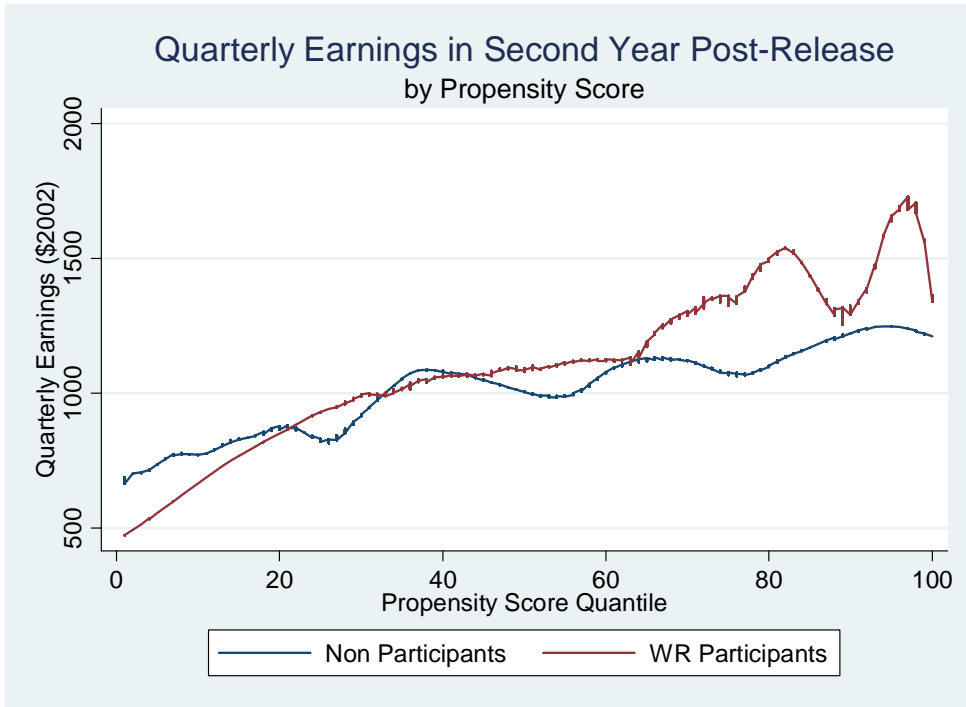


Figure 8

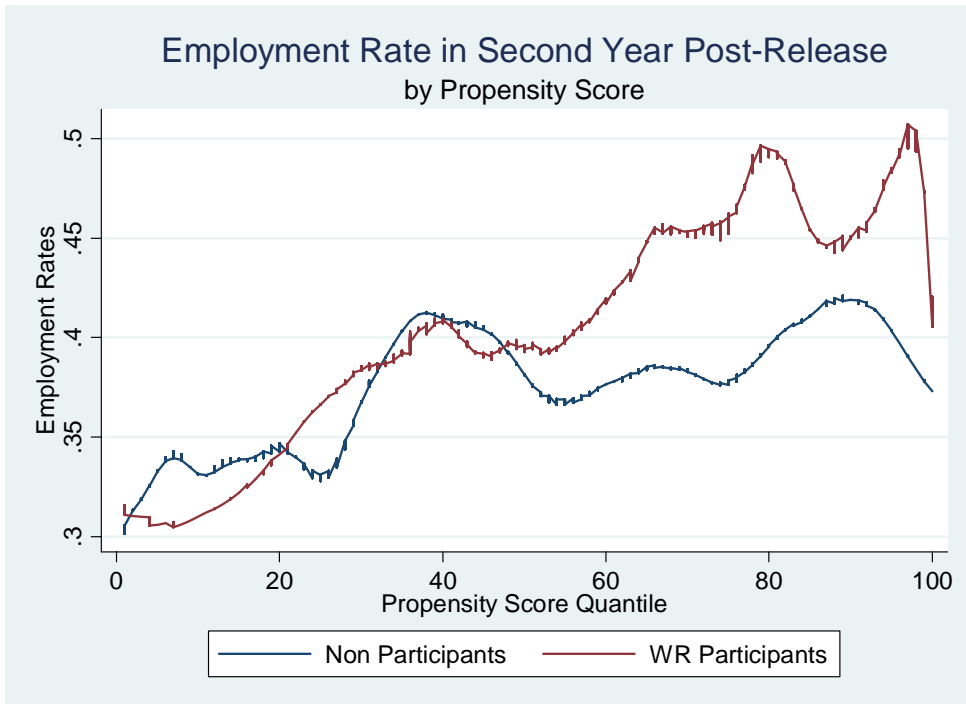


Figure 9

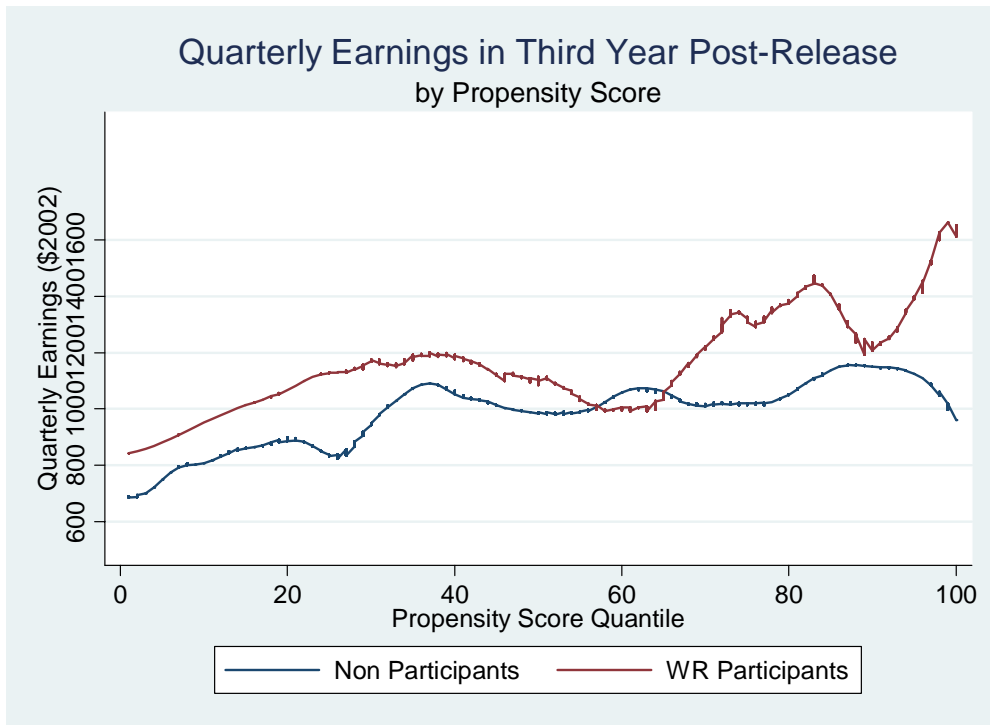


Figure 10

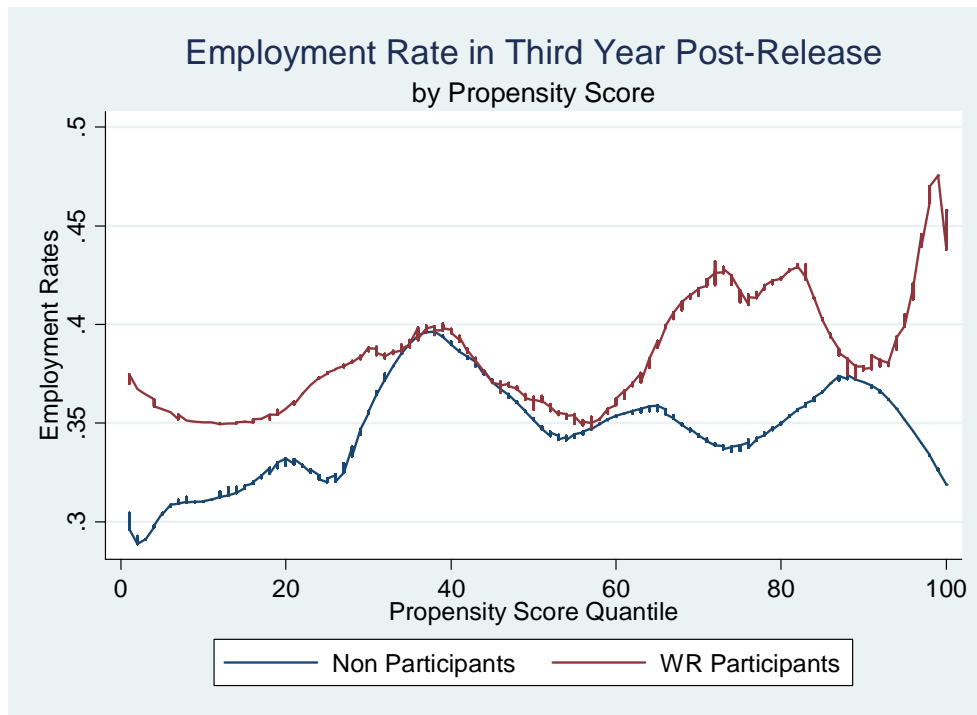


Table 1
A Comparison of Work Release Participants and Non-Participants

	All	No WRA	WRA
White	42.4%	41.4%	45.6%
Black	53.2%	54.3%	49.6%
Hispanic	4.2%	4.1%	4.5%
Age at Release	31.2	31.3	31.0
Yrs of Education	10.3	10.2	10.5
DOC Incarceration Length (in months)	13.3	12.0	17.5
Jail Time (in months)	3.7	3.8	3.2
Has Prior Prison Spell	38.5%	40.8%	31.1%
Number of Prior Spells	0.7	0.8	0.5
# Disciplinary Reports in Prior Spells	0.5	0.6	0.2
Primary Offense			
Murder, Manslaughter	0.5%	0.5%	0.8%
Sexual	0.0%	0.0%	0.0%
Robbery	4.3%	4.2%	4.6%
Violent, Other	12.6%	12.6%	12.3%
Burglary	22.0%	21.6%	23.4%
Property Theft/Fraud	17.1%	17.4%	16.1%
Drug Sales	25.1%	24.5%	26.7%
Weapons	5.3%	5.1%	5.9%
Other	3.4%	3.4%	3.3%
Drug Possession	9.7%	10.6%	7.0%
Marital Status at Prison Entry			
Married	11.6%	10.5%	15.3%
Single	43.6%	44.5%	40.8%
Separated, Divorced, Widowed	17.0%	17.1%	16.4%
Don't Know	27.8%	27.8%	27.5%
Prison Programming			
Adult Basic Education	19.5%	20.1%	17.5%
GED Prep	12.7%	11.9%	15.4%
Vocational Education	9.1%	9.7%	7.2%
Prison Industry	3.1%	2.9%	3.7%
Substance Abuse Treatment	42.5%	34.8%	67.3%
Earnings 1 Yr Prior to Incarceration	608	550	797
Pct Employed 1 Yr Prior to Incarceration	33.8%	32.5%	38.0%
Earnings 1 Yr Post Release	1007	872	1441
Pct Employed 1 Yr Post Release	38.9%	36.3%	47.3%
N	9221	7035	2186

The sample is limited to inmates who enter prison in minimum custody.

Table 2
Quarterly Earnings of Work Release Participants and Non-Participants

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1st Year	674 (27)	625 (36)	546 (36)	451 (33)	410 (35)	402 (34)	386 (38)	388 (32)
2nd Year	506 (35)	471 (36)	407 (34)	340 (30)	309 (33)	305 (33)	289 (35)	249 (38)
3rd Year	457 (31)	429 (32)	381 (33)	344 (33)	319 (38)	314 (36)	311 (36)	221 (40)
Limit to Minimum Custody	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Education, Age, & Time	No	No	Yes	Yes	Yes	Yes	Yes	No
Criminal Justice Controls ¹	No	No	No	Yes	Yes	Yes	Yes	No
Other Admission Variables ²	No	No	No	No	Yes	Yes	Yes	No
County Fixed Effects	No	No	No	No	No	Yes	Yes	No
Prison Programming Participation ³	No	No	No	No	No	No	Yes	No
DD Specification w/ Pre-Prison Earnings	No	No	No	No	No	No	No	Yes

Note: Standard errors, which appear in parentheses, are clustered at the county level and are robust to heteroskedasticity.

¹ Criminal justice variables includes months incarcerated, prior incarceration spells, offense type, and prison disciplinary reports.

² Other admission variables include information on marital status, citizenship, birthplace, and employment status at the time of the arrest.

The Admissions controls also include results from a literacy test administered at prison admission.

³ Prison programming variables include participation in adult basic education, GED prep, vocational education, prison industries, and substance abuse treatment.

Table 3
Employment Rates of Participants vs. Non-Participants

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1st Year	0.178 (0.009)	0.15 (0.011)	0.136 (0.011)	0.128 (0.011)	0.117 (0.012)	0.113 (0.011)	0.108 (0.011)	0.089 (0.008)
2nd Year	0.109 (0.008)	0.092 (0.009)	0.082 (0.009)	0.073 (0.010)	0.066 (0.009)	0.063 (0.009)	0.062 (0.009)	0.034 (0.011)
3rd Year	0.095 (0.007)	0.078 (0.009)	0.07 (0.010)	0.068 (0.011)	0.062 (0.011)	0.058 (0.010)	0.058 (0.011)	0.021 (0.012)
Limit to Minimum Custody	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Education, Age, & Time	No	No	Yes	Yes	Yes	Yes	Yes	No
Criminal Justice Controls ¹	No	No	No	Yes	Yes	Yes	Yes	No
Other Admission Variables ²	No	No	No	No	Yes	Yes	Yes	No
County Fixed Effects	No	No	No	No	No	Yes	Yes	No
Prison Programming Participation ³	No	No	No	No	No	No	Yes	No
DD Specification w/ Pre-Prison Employment	No	No	No	No	No	No	No	Yes

Note: Standard errors, which appear in parentheses, are clustered at the county level and are robust to heteroskedasticity.

¹ Criminal justice variables includes months incarcerated, prior incarceration spells, offense type, and prison disciplinary reports.

² Other admission variables include information on marital status, citizenship, birthplace, and employment status at the time of the arrest.

The Admissions controls also include results from a literacy test administered at prison admission.

³ Prison programming variables include participation in adult basic education, GED prep, vocational education, prison industries, and substance abuse treatment.

Table 4
Propensity Score Estimates

	ATE	TOT
Earnings		
1st Year	802 (62)	237
2nd Year	527 (79)	198
3rd Year	496 (83)	214
Employment		
1st Year	0.144 (0.026)	0.081
2nd Year	0.056 (0.027)	0.052
3rd Year	0.064 (0.040)	0.047

Table 5
Earnings Conditional on Employment
Work Release Participants vs. Non-Participants

	OLS
1st Year	196 (42)
2nd Year	268 (57)
3rd Year	321 (73)
Limit to Minimum Custody	Yes
Education, Age, & Time	Yes
Criminal Justice Controls ¹	Yes
Other Admission Variables ²	Yes
County Fixed Effects	Yes

Note: Standard errors, which appear in parentheses, are clustered at the county level and are robust to heteroskedasticity.

¹ Criminal justice variables includes months incarcerated, prior incarceration spells, offense type, and prison disciplinary reports.

² Other admission variables include information on marital status, citizenship, birthplace, and employment status at the time of the arrest.

The Admissions controls also include results from a literacy test administered at prison admission.

Table 6
Recidivism Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1st Year	-0.078 (0.013)	-0.059 (0.014)	-0.055 (0.014)	-0.029 (0.015)	-0.023 (0.016)	-0.027 (0.015)	-0.03 (0.015)
2nd Year	-0.084 (0.016)	-0.07 (0.015)	-0.066 (0.015)	-0.048 (0.016)	-0.041 (0.017)	-0.047 (0.016)	-0.05 (0.015)
3rd Year	-0.089 (0.017)	-0.079 (0.015)	-0.073 (0.016)	-0.058 (0.018)	-0.05 (0.018)	-0.056 (0.016)	-0.059 (0.016)
Limit to Minimum Custody	No	Yes	Yes	Yes	Yes	Yes	Yes
Education, Age, & Time	No	No	Yes	Yes	Yes	Yes	Yes
Criminal Justice Controls ¹	No	No	No	Yes	Yes	Yes	Yes
Other Admission Variables ²	No	No	No	No	Yes	Yes	Yes
County Fixed Effects	No	No	No	No	No	Yes	Yes
Prison Programming Participation ³	No	No	No	No	No	No	Yes
DD Specification w/ Pre-Prison Employment	No	No	No	No	No	No	No

Note: Standard errors, which appear in parentheses, are clustered at the county level and are robust to heteroskedasticity.

¹ Criminal justice variables includes months incarcerated, prior incarceration spells, offense type, and prison disciplinary reports.

² Other admission variables include information on marital status, citizenship, birthplace, and employment status at the time of the arrest.

The Admissions controls also include results from a literacy test administered at prison admission.

³ Prison programming variables include participation in adult basic education, GED prep, vocational education, prison industries, and substance abuse treatment.

Table 7
Quarterly Earnings and Employment of Work Release Participants and Non-Participants
By Offender Type

	Income Crimes		Consumption Crimes	
	Quarterly Wage	Employment	Quarterly Wage	Employment
1st Year	299 (45)	0.096 (0.011)	646 (76)	0.152 (0.019)
2nd Year	239 (46)	0.05 (0.013)	470 (63)	0.094 (0.016)
3rd Year	206 (41)	0.039 (0.010)	562 (79)	0.098 (0.022)
Limit to Minimum Custody	Yes	Yes	Yes	Yes
Education, Age, & Time	Yes	Yes	Yes	Yes
Criminal Justice Controls ¹	Yes	Yes	Yes	Yes
Other Admission Variables ²	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
Prison Programming Participation ³	Yes	Yes	Yes	Yes
DD Specification w/ Pre-Prison Employment	No	No	No	No

Note: Standard errors, which appear in parentheses, are clustered at the county level and are robust to heteroskedasticity.

¹ Criminal justice variables includes months incarcerated, prior incarceration spells, offense type, and prison disciplinary reports.

² Other admission variables include information on marital status, citizenship, birthplace, and employment status.

The Admissions controls also include results from a literacy test administered at prison admission.

³ Prison programming variables include participation in adult basic education, GED prep, vocational education, prison industries, and substance abuse treatment.

Table 8
Recidivism Estimates by Offender Type

	Income Crimes	Consumption Crimes
1st Year	-0.034 (0.016)	-0.008 (0.025)
2nd Year	-0.062 (0.017)	-0.008 (0.021)
3rd Year	-0.063 (0.017)	-0.038 (0.024)
Limit to Minimum Custody	Yes	Yes
Education, Age, & Time	Yes	Yes
Criminal Justice Controls ¹	Yes	Yes
Other Admission Variables ²	Yes	Yes
County Fixed Effects	Yes	Yes
Prison Programming Participation ³	Yes	Yes
DD Specification w/ Pre-Prison Employment	No	No

Note: Standard errors, which appear in parentheses, are clustered at the county level and are robust to heteroskedasticity.

¹ Criminal justice variables includes months incarcerated, prior incarceration spells, offense type, and prison disciplinary reports.

² Other admission variables include information on marital status, citizenship, birthplace, and employment status at the time of the arrest.

The Admissions controls also include results from a literacy test administered at prison admission.

³ Prison programming variables include participation in adult basic education, GED prep, vocational education, prison industries, and substance abuse treatment.