PREP: Training Inmates through Industrial Work Participation, and Vocational and Apprenticeship

PREP: Training Inmates through Industrial Work Participation, and Vocational and Apprenticeship Instruction

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Abstract

Data on over 7,000 offenders were collected to evaluate the impact of industrial work experience and vocational and apprenticeship training on in-prison and post-release outcomes. Prior research on prison training outcomes failed to find any significant training effects. Related research on the economic conditions faced by ex-offenders has demonstrated the difficult conditions under which these offenders seek employment. Because the training effects may be subtle, we developed a large sample to evaluate the prison training programs. Furthermore, because we could not randomly assign inmates to the training conditions, we tried to control for selection bias by using a statistical matching procedure which modeled the training program selection process. The results of the evaluation demonstrated significant and substantively meaningful training effects both on in-prison and post-prison outcome measures.

Introduction

The Post-Release Employment Project (PREP) was designed to evaluate the impact of prison work experience, and vocational and apprenticeship training on an offender's behavior following his or her release to the community. The evaluation began in 1983 and data were collected through October 1987 on over 7,000 offenders. Although there are many perspectives on the purposes and goals of operating prison industries and employing inmate labor, an interesting historical perspective comes from the U.S. Congress. In support of the 1930 authorizing legislation for prison industries within the federal government, the Senate Judiciary Committee gave the following rationale:

It is unanimously conceded that idleness in prisons breeds disorder and aggravates criminal tendencies. If there is any hope for reformation and rehabilitation of those convicted of crimes, it will be founded upon the acquisition by the prisoner of the requisite skill and knowledge to pursue a useful occupation and the development of the habits of industry[1].

Thus, even at its inception, prison industries was contemplated to serve two masters. It was designed to minimize prison disorder and to prepare inmates for a successful life after release from prison.

Theoretical Background: The link between unemployment and crime

There is a great deal of theoretical and empirical support for the proposition that unemployment is a predictor of criminal activity[2]. Furthermore, recent evidence by Nagin and Waldfogel[3] shows that a prison term can reduce the lifetime earnings of the ex-offender. An unfortunate consequence of these findings may be that faced with lowered expectations of gainful employment in the licit economy, the ex-offender may return to illicit economic activities. All of this research converges on the proposition that it may be very difficult to break the reciprocal relationship between crime and unemployment, especially if the individual also has received a term of imprisonment[4].

Prison systems have a very difficult agenda if they are to impact on breaking the cycle of criminality. Data from this project indicate that in the five years prior to the offender's current incarceration, half of these offenders worked less than fifty percent of the time. Forty-two percent worked less than two years in that five-year period.

In addition to Nagin's study, there have been two major studies investigating the conditions of employment for ex-offenders. The Transitional Aid Research Project (TARP), which took place in Texas and Georgia, examined the influence of providing ex-offenders with monetary compensation during the first year after release from prison. Rossi, Berk, and Lenihan[5] concluded that this kind of unemployment insurance had two competing influences on the ex-offender's motivation to find a job. The money allowed ex-offenders an opportunity to find employment without resorting to crime and

without having to settle for a low-wage job. Unfortunately, the unemployment compensation was also a disincentive to find work, since ex-offenders could afford to live without seeking employment. Rossi et al. have suggested that transitional aid for ex-offenders can work if it is coupled with an incentive to find a job.

Schmidt and Witte[6] have reviewed the evidence regarding post-release employment among ex-offenders and have reached a number of conclusions:

C   job terminations are typically the ex-offender's choice rather than the employer's choice;

C   post-release supervision has competing influences on employment productivity--supervision results in maintaining a job, but at lower wages than unsupervised releasees;

C   when work programs allow offenders to accumulate money, inmates are more successful following release because they have more freedom to find a better paying job-- this is partially consistent with the TARP findings;

C   unlike most subpopulations of the labor force, inmates' age and education have little impact on their labor market success; jobs obtained by releasees are typically low wage and low skilled;

C   offenders exhibit a great deal of instability in their post-release employment; offenders who remain employed typically have jobs in the lowest skill categories, working mainly in large manufacturing industries;

C   in Michigan, halfway house participation has contributed to higher post release wage earnings;

C relatively stable background characteristics of the offender population contribute to higher post-release wages--white, able-bodied, married men with dependents earn higher wages upon release;

C the most compelling factors that determine post-release wages are those associated with the economic structure of the local labor market -- these factors include the ex-offender's occupation/skills, the industry of employment, and the economic climate of the local labor market;

C citing Borus, Hardin, and Terry[7], Schmidt and Witte concluded that prison programs designed to improve basic or vocational skills have failed to affect post-release employment.

Similar to the Borus, Hardin, and Terry results, Maguire, Flanagan, and Thornberry[8] found that there were no statistically significant differences in the hazard rates of post-release arrest between a prison industry study group and a comparison group of inmates chosen from the same New York State prisons. Maguire et al. controlled for time served, age at admission, prior felony arrests, grade completed, military service, marital status, occupation, race, commitment crime, employment status, prior drug use, and institution misconduct rate. By choosing comparison subjects from the same prisons as the study participants, Maguire et al. controlled for prison environment effects, but it is likely that the approach also exaggerated the program effects (this is referred to as selection bias and is discussed in more detail later in the paper). Their method potentially introduced bias in program effects because inmates working in prison industries are likely to be more "motivated" and this would have left a less

motivated pool of inmates to be used as comparison subjects. Despite this potential bias toward

favorable findings, Maguire et al. found no effect of prison industries.

In related research on the hard-core unemployed (HCU's), Goldstein[9] has reviewed the training

literature which addresses the problem of assisting HCU's into the labor market. Goldstein argues that

skill training alone does not solve the problems of the HCU's. HCU's have developed expectations of

job failure that are difficult to overcome. Although we are unaware of data on the overlap in the ex-

offender and HCU populations, there are theoretical reasons to believe these populations overlap quite

a bit, especially in light of Nagin's evaluation of expected lifetime earnings of ex-offenders.

In yet another related area of research, some economists (see especially Piore[10]) have argued for a

segmented labor market to explain differences in the unemployment patterns of the poor and the more

advantaged. The primary sector of the labor market is characterized by jobs which form a progression

from lower to higher pay. One's human capital (skills, experience, education) contributes to

promotional opportunities. In the secondary labor market, skill levels are relatively low and human labor

is more fungible. Thus, one's limited human capital is not strongly related to promotional opportunities.

The secondary labor market is characterized by high instability, low expectations for advancement, and

lower wages. If one's entry level is an occupation in the secondary labor market, then one's long-term

opportunities are severely limited.

Although there is no specific occupational definition of the secondary labor market, we will examine data on the broad occupational groupings of industries in which ex-offenders find jobs and compare this to the occupational groupings in which these individuals were employed prior to their most recent incarceration. This will give us some insight into the extent to which ex-offenders enter the secondary labor market.

In summary, the evidence to date on the employment patterns of ex-offenders is that these individuals are faced with lowered expectations and extremely precarious labor market conditions. Many do not have skills or education and their additional burden is that they carry the stigma of a term of imprisonment. Under these conditions, can skills training in prison be used to penetrate the difficult labor market barriers that these ex-offenders face upon their release? The evidence, to date, is not sanguine. We undertook this study with a different approach in mind. No study, to date, had explicitly tried to control for selection bias in prison training evaluations. Secondly, if the effects of training are subtle and the size of the effect is relatively small, a larger sample than those previously used might indicate small but substantively meaningful statistical results. Finally, we wanted to examine the impact of work and skills training on institutional adjustment, licit wages after release, and post-release recidivism. Thus, PREP was designed to meet these goals.

Study Design and Methodology

Unlike most studies of prison vocational training or work experience, PREP was designed as a prospective longitudinal evaluation. Inmates were selected as study group members if they had participated in industrial work within prison for at least six months prior to their release or had received in-prison vocational instruction or apprenticeship training. Based on these criteria, 57 percent of the study group participants worked exclusively in prison industries, 19 percent had a combination of work experience and vocational training, and the remaining 24 percent had received either vocational training, or apprenticeship training or a combination of the two.

A quasi-experimental design was used in which comparison subjects were chosen from the "reservoir" of all other inmates released in the same calendar quarter as study group members. When either a study or comparison group member was selected a data collection form was initiated and prison staff filled out the instrument. If an inmate went to a half-way house, staff at these contract facilities also completed a section of the data collection form. This information was then mailed to the Bureau's Office of Research. Post release information for the first year of release was collected by calling supervisory probation officers whose job was to meet with the ex-offender and monitor his/her behavior, including verified employment.[11]

It is a difficult matter to measure the effectiveness of programs without representing a biased picture of the results. This is because there are two key methodological issues related to the measurement of program effectiveness often ignored in the research design of many program evaluations: selection bias and "strong" inference designs. In PREP, we addressed both problems.

Selection Bias

Selection bias refers to unintended influences which control the selection of research observations and results from an inadequate research design. Such designs introduce a nonrandom process into the selection of study and comparison group members. Selection bias can produce a study group composed of members that show a more favorable outcome than "control" individuals, although the actual difference between these groups is attributable to observed and unobserved factors that predispose the study group to a more favorable outcome even in the absence of some program intervention. The simplest way to control for selection bias is to randomly assign inmates to programs. There are instances when this has been done; however, there are practical and ethical reasons why it is rare that random assignment is effectively used in selecting inmates for programs. It is often impractical to randomly assign inmates to programs because: 1) researchers are not allowed to control the selection process, and 2) inmates will contaminate the random assignment process by dropping out of programs, by disrupting the program, or transferring into a group other than the one to which they were assigned.

In addition to the formidable practical problems, there are also important ethical considerations why inmates ought not to be randomly assigned to prison programs. Inmates who express an interest in a specific program show a motivation to learn or to change. If an inmate who is motivated is assigned to a control (no program) condition, do we in any irreparable way subvert that motivation? The

complement to that question is: what do we achieve by randomly assigning an inmate to a program

when he or she is not motivated and may even be hostile to program participation? Are we

contaminating that program for other inmates? Could random assignment preclude an inmate's future

interest in a program by assigning him or her to that program before he or she is willing to participate?

One final statement regarding the comparability of experimental and observational designs, as Heckman

and Hotz[12] have shown, observational studies can yield the same estimates as experimental studies

when there is a theoretical reason to decide among the various observational estimators.

Strong Inference Designs

There are technical statistical solutions to selection bias. However, program evaluation designs would

be much more compelling if researchers always adopted a "strong" inference design. What we mean by

strong inference is that researchers explicitly state the theoretical mechanism through which they assume

the program intervention will be effective. Within the context of the research design, the mechanism is

measured, preferably before and after the intervention, and then the change in the mechanism is

analyzed in relation to the outcome variable. In the current study, although we will not report the results

here, we presumed that prison work would be related to the supervisor's ratings of work abilities, work

habits and the motivation to work. We made an attempt to measure these mechanisms through the

supervisor's ratings. Theoretically, the probability of recidivism for inmates who received prison work

experience should be related to their supervisor's ratings. Strong inference designs give us confidence

that if we observe treatment effects, those effects are not an artifact of selection bias or some other contamination.

Estimating the Propensity Score: The determinants of the selection process for work and vocational training

To overcome the problem of selection bias, we adopted a statistical matching procedure developed by William G. Cochran and Donald B. Rubin[13] and further refined by Paul R. Rosenbaum and Donald B. Rubin[14]. The procedure is a two-step approach. In the first step, the researcher models the selection process, contrasting program participants and non-participants on variables related to their participation. As a result of the modeling, a "propensity score" is generated, indicating the likelihood that an offender would be selected for participation in prison industry or vocational training, irrespective of whether they were in the study group or the comparison reservoir. Thus, individuals in the comparison reservoir who have high propensity scores should be similar to study group members who actually participate in work and training programs. In the second step, the propensity score is used in conjunction with other variables to select matched comparison subjects. Theoretically, the matched comparison subjects are equivalent to the study group participants in every respect except for their participation in the work or vocational training program. (Although we don't display the results here, we have empirically demonstrated that the two groups are statistically indistinguishable on the set of measures used to model the employment/training selection process.) Furthermore, we had good reason to believe that there were many individuals in the comparison reservoir who had an interest in working

in prison industries and would have, had the opportunity been available. Throughout the duration of the

PREP about 35% of the inmates housed in Bureau facilities were employed by prison industries,

however the waiting list to become employed by prison industries was always substantial. There were

always far more inmates who desired a prison industries job than prison industries could accommodate.

Although the ultimate purpose in using the propensity score was to select appropriate comparison

subjects, the results of the logistic regression that generates the propensity score yields insight into the

selection process itself.

The results of this analysis demonstrated that study group members were more likely to release to a

halfway house, were younger at the time of their current commitment, had more prior commitments,

were more likely to have committed an instant violent offense, were more likely to have been

incarcerated for longer periods of time, were more likely to have little or no violence in their past, were

more likely to be non-Hispanic, and white, and were more likely to have had a higher security level.

The propensity score (estimated log odds), along with the other variables used in the propensity score

estimation, were used in the procedure which matched each study observation with a comparison

observation selected from the comparison reservoir of all other offenders released in the same calendar

quarter. We required that the matching algorithm first establish an exact match based on sex and race.

Then for each study group member a matched comparison observation was selected based upon their

geometric similarity to the study group member. Following procedures outlined by Rubin and

Rosenbaum, potential comparisons of the same sex and race were first culled from the reservoir by using a proportion of the standard deviation of the estimated logit, selecting from the reservoir of comparison subjects those whose propensity scores were within 0.20 standard deviations of the study group member's propensity score. From that smaller pool, the comparison subject was chosen who had the smallest geometric distance from the study group member on the propensity score and all the other variables. Once a comparison observation was chosen, all data that were to be prospectively gathered on study group members were also gathered on comparison offenders.

Results

Occupational Changes in the Study and Comparison Groups

Table 1 shows the relationship among the distributions of a sample of study and comparison group participants in the major occupational groupings. Since every job was categorized using the Department of Labor's <u>Dictionary of Occupational Titles</u>[15], we could group occupations into 9 major groups: professional/ technical, clerical/sales, service, agricultural/fishing, processing (e.g. processing metal, ore, coal, gas, rubber, wood), machine trade (e.g. metal working, printing), bench work (e.g. fabrication, assembly, repair of metal products, electrical products), structural work (welding, painting, plastering, cementing, construction), and miscellaneous (e.g. transportation, amusement, recreation).

Compared to the distribution of the entire U.S. labor force in 1983, offenders in the study group were less likely to work in professional and clerical occupations, and more likely to work in machine trades, structural work, and miscellaneous occupations. Comparison group offenders had very similar patterns, although they also were more likely to work in service jobs as well, relative to the entire U.S. labor force.

Table 1 also represents the occupational categories of study group participants while they were employed or trained in prison. As Table 1 shows, individuals who were receiving vocational or apprenticeship training were primarily instructed in machine trades and structural work. Industries employees were primarily working in bench work activities and secondarily in clerical and machine trades.

After release from prison, both study group and comparison group offenders were working in similar occupations. They were primarily doing structural work, followed by clerical/sales, professional, and service jobs. Relative to the U.S. labor force, after getting out of prison, offenders were much more likely to do structural work or miscellaneous jobs and much less likely to work in clerical or professional jobs than the U.S. labor force.

In the aggregate, relative to their occupation groupings prior to prison, following prison, offenders were more likely to pursue clerical/sales jobs, somewhat more likely to pursue structural jobs and more likely to pursue miscellaneous jobs. They were slightly less likely to pursue professional jobs.

Table 1 merely depicts job changes in the aggregate. Table 2 is a mobility table which depicts the

transitions that occur for individuals prior to their incarceration and twelve months after their release

from prison. In this table, we collapse across study and comparison groups to gain further insight in the

mobility patterns. Some of the cells in this table were sparse and a statistical test of the patterns was not

done; however Table 2 does depict some interesting descriptive patterns of pre- to post-imprisonment

mobility. Table 2 is designed to be read row by row. For example, the first row shows individuals who

held a professional job prior to prison and the percentage of pre-incarceration professionals who

subsequently held a professional, clerical/sales, service, or other job. Thus, 28 percent of professionals

held a professional job after prison, 25 percent held clerical/sales jobs, and so forth. Each cell of the

table indicates, first, the number of individuals who had a particular set of pre-incarceration and post-

incarceration jobs, and second, for each pre-incarceration occupational group, the percentage of

individuals from that group who held a specific post-incarceration job. Thus, the percentages in Table 2

are row percentages and sum to 100 for each row.

The diagonal of Table 2 indicates the number and percentage of individuals who were employed in the

same occupational categories prior to and after prison. The structural trades (51 percent) and

clerical/sales (39 percent) occupations were the most stable. One of the largest transitions out of an

occupation group into a particular group was for professional/technical occupations. Among those

individuals who held these types of jobs prior to prison, 25 percent held a clerical/sales position after

prison.

The data in tables 1 and 2 do seem to support the thesis that offenders prior to their incarceration are much more likely than the general labor force to be employed in "secondary labor market" occupations, although there is insufficient detail to be precise about this conclusion. The primary post-incarceration jobs 12 months after release were clerical/sales, structural work, and miscellaneous occupations. The job emphasis in prison was bench work, machine trades and clerical /sales. With all of the resources devoted to bench work trades within prison industries, very few offenders find such jobs within 12 months of release. One of the reasons bench work is emphasized in prison is that these trades teach a skill and these types of occupations lend themselves to featherbedding, allowing industries to employ as many inmates as possible.

Type and Frequency of Disciplinary Reports within the Last Year of Prison

The data in this section were statistically analyzed using a chi-square statistic with degrees of freedom equivalent to the number of observations in the cross-classification. The analysis compared misconduct between the study and comparison groups. The data reported here reached conventional statistical significance, $p < .05$. An analysis of the frequency of disciplinary reports showed that 22.2 percent of study group participants and 26.2 percent of comparison group inmates received an incident report within the last year of commitment. This is a difference of 4 percent in the rate of incident reports, but in a relative context study group members were 15 percent less likely to receive an incident report than comparison group inmates. The Bureau of Prisons uses four levels of misconduct seriousness that

determine levels of sanctions commensurate to the misconduct. Comparison group members who received an incident report for the most serious types of institutional misconduct were 63 percent more likely to be convicted of that charge -- 2.6 percent (comparison) versus 1.6 percent (study), and were 46 percent more likely to be punished for the second most serious level of institutional misconduct within the last two years of their incarceration -- 3.5 percent (comparison) versus 2.4 percent (study).

Although the percentage differences reported here may appear small, because the quasi-experimental design controlled for background differences between the study and comparison groups, the differences are statistically and substantively meaningful. Furthermore, the larger relative percentages more accurately convey the differences in the rates of reported misconduct between the two groups. Misconduct is a serious problem faced by all prisons administrators. It threatens the orderly management of the institution and can threaten the lives of staff and inmates. Consequently, even an absolute difference of 4 percent in misconduct that can be attributed to prison work, and vocational and apprenticeship training is a very significant finding.

Halfway House Outcomes

For those offenders who were released to a halfway house prior to their release to the community, we collected outcome data on their criminal recidivism and employment. The data in this section were also analyzed using a chi-square statistic. The relevant variable was cross-classified by study versus

comparison group membership. Only significant results are reported in this section using conventional statistical significance levels, $p < .05$.

For comparison group members, 6.8 percent escaped from the halfway house during their stay, and 9.1 percent were returned to Bureau of Prisons custody for a new arrest or a technical violation. The percentages for study group members were 5.2 and 8.4 respectively. Because other dispositions were possible, 83.3 and 83.9 percent of the comparison and study groups successfully completed their halfway house stay. Thus, there was very little difference in the recidivism between the two groups while in a halfway house.

Study group members were more likely to obtain a full-time (86.5 percent) or day labor (9.0 percent) job while in the halfway house than were comparison subjects. Only 62.1 percent of comparison subjects obtained a full-time job and 1.3 percent obtained a day labor job.

Twelve Month Post-Release Outcome - Recidivism

Twelve months after release from prison, 6.6 percent of study group members and 10.1 percent of comparison group members had their supervision revoked either because of a technical violation of supervision or because they had been rearrested for a new offense. Thus, study group members at the end of one year were 35 percent less likely to recidivate than comparison group members. Although the

absolute difference may not appear large, 6.6 versus 10.1 percent, the relative difference was statistically significant and quite large -- 35 percent.

Previous recidivism studies conducted by the Office of Research within the Bureau of Prisons have consistently demonstrated that within the first year of release, about 20 percent of offenders are returned to prison for a new arrest or technical violation of their supervision. If we had simply taken a random sample of releasees and we had not adjusted for the background differences among the study group and comparison reservoir members, the group differences would have been greatly exaggerated (6.6 percent study versus 20 percent comparison). Although we have no independent confirmation of our propensity score adjustment, theoretically we have controlled for both potential differences in the background characteristics between study and comparison group offenders as well as their "propensity" or motivation to select themselves into work, vocational, and apprenticeship programs.

Twelve Month Post-Release Outcome - Employment

In each of the twelve months following release, study group members were more likely to be employed than comparison group members and by the twelfth month were 14 percent more likely (71.7 versus 63.1 percent) to be employed. These differences reached conventional levels of statistical significance using a chi-square test of the difference,

$p < .05$.

There were no statistical differences in the average wages earned between these two groups. For

individuals employed throughout the twelve month period, the average wages were about $9,700.

According to the U.S. Bureau of the Census, the poverty level for a family of two persons ranged from

$6,483 to $7,704 from 1983 to 1988, the years in which most of the PREP follow up data were

collected. For a family of four, the poverty level ranged from $10,178 to $12,092 in that same time

frame. Thus, the average wages of ex-offenders for the first year after release from prison were very

close to the poverty thresholds.

Long-Term Recidivism

In 1995, we culled the automated Bureau of Prisons records to determine whether the study or

comparison group members had been recommitted to a federal facility for a new offense or had been

returned for a technical violation of their supervision. The observations in this follow up had been

released for as long as 12 years or as few as 8 years. It was possible for offenders to be arrested,

convicted, or confined in jurisdictions other than the federal criminal justice system. Although the federal

recommitment data certainly underestimates total recommitment activity, there is no theoretical reason

to believe that study or comparison subjects would be more or less likely to be recommitted in non-

federal jurisdictions. Thus, we believe the study/comparison group contrast should be unbiased.

The analysis examined the amount of time an offender was in the community prior to his or her

commitment for a new federal offense. The data were analyzed using the Cox proportional hazards

model. The Cox proportional hazards model is a partially parametric technique that allows estimation of the effects of independent variables on the hazard of recidivating without estimating the precise base hazard rate. We estimated separate models for males and females, because it is well known that women are less likely to recidivate than men. Women who did fail in the study, however, failed much earlier, on average, than men. The average survival time for men who failed was 811 days, for women 647 days.

The study group participants were divided into three subgroups for the purpose of this analysis. There was a prison industries (Ind) group (57 percent), a vocational training (VT)/Apprenticeship training (App) group (24 percent), and a combination prison industries/training (Ind/VT/App) group (19 percent). Dummy variables were created that contrasted these groups to comparison group members.

There were no significant effects for the model of females. This was probably due to the fact that so few women recidivated in the time period. Only 52 of the 904 women were recommitted for a new offense over the entire period.

The model for the men was significant and is represented in Table 3. Aside from the program participation variables, we included race (BLACK), ethnicity (HISPANIC), age at release (RELEASE AGE GROUP), education level (EDUCATION GROUP), the year the inmate was released to the community (RELEASE COHORT), the natural log of time served for the commitment during which these inmates were identified for this study (LOG TIME SERVED), and the decile of the individual's

propensity score (DECILE OF PROPENSITY SCORE). For propensity score, release cohort,

release age group, and education group missing data were treated as a categorical value. For every

grouping variable other than program participation, the variables were coded as effects vectors. Thus,

the coefficients should be interpreted relative to the adjusted grand mean of the outcome measure.

Table 3 shows that the model with the covariates is statistically significant. The propensity score was

used in this analysis as a proxy for all of the background characteristics that were used to produce the

estimated logit for the selection process. Thus, inmates with high propensity scores were the most likely

to select into these programs given their background characteristics. There doesn't appear to be any

coherent pattern of significant propensity score coefficients. This demonstrates that our two-stage

selection method for identifying comparison observations yielded two groups that were balanced with

respect to this proxy measure.

The coefficients for hispanics, blacks, younger inmates (ages 18-24), inmates with longer periods of

time served, and inmates released in 1985 were statistically significant and indicate these groups were

more likely to recidivate throughout the observation period. These measures were included in the model

to provide statistical adjustments for any imbalance between the program and comparison groups not

accounted for by modeling the selection process (represented in the model by the propensity score)

and the matching algorithm.

Two of the program participation variables were statistically significant and the third approached

significance. Inmates who worked in prison industries were 24 percent less likely to recidivate

throughout the observation period while those who participated in either vocational or apprenticeship

training were 33 percent less likely to recidivate throughout the observation period. Inmates who

participated in all three programs were 23 percent less likely to recidivate, although the effect for that

group was not as significant. (For the Cox proportional hazards model these percentages are obtained

by subtracting the value 1 from the estimates in the column labeled "EXP(COEF.)". For example, for

the Industries estimate in the first row of Table 3, .76 minus 1 yields -.24, which when multiplied by

100, produces -24 percent.)

It appears that there was a long term impact of prison industries and vocational or apprenticeship

training on post-release recommitment rates.

Summary

Despite the stigma of imprisonment and the lowered expectations of an ex-offender, it appears that

prison programs can have an effect on post-release employment and post-release arrest in the short run

and recommitment in the long run. The failure to find these effects in the past may be due to either the

ineffectiveness of the particular programs that were evaluated or to an inadequate research design that,

among other things, provided insufficient sample sizes or failed to control for selection bias. While the

data reconfirm the notion of a secondary labor market for ex-offenders as well as extremely low wages

in the first year after release, inmates who participated in work and job skills programs were less likely

to be recommitted to federal prisons as much as 8 to 12 years after their release.

Table 1. Occupational changes in the study and comparison groups							
Occupational Changes in the Study Group							
Occupational Classification	U.S. Labor Force, 1983	Pre-Incarceration	Vocational Training	Apprenticeship Training	Prison Industries	Halfway House	Six Month Follow-Up	Twelve Month Follow-Up
Professional Technical	26.4	13.5	12.7	17.5	2.3	8.1	11.9	11.9
Clerical, Sales	28.0	16.7	15	3.5	19	20.5	18	19.3
Service	13.7	15.4	5.3	16.7	3	13.6	13.8	11.9
Agriculture, Fishing	3.7	4.4	1.6	2.6	0	1.9	2.9	3.3
Processing	3.3	2	5.5	4.4	1.4	2	1.5	1
Machine Trade	6.9	9.1	25.4	14.9	12.4	10.5	10.4	10.4
Bench work	3.6	4.3	4.2	7.9	47.9	3.9	3.3	3.8
Structural Work	7.7	23.5	23.8	29.8	3.9	30.5	26	26
Miscellaneous	6.7	11.1	6.4	2.6	10.1	9.1	12.2	12.3
	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
Number of Cases	100,922,000	2,837	1,357	114	2,024	2,538	2,312	1,624
Occupational Changes in the Comparison Group							
Occupational Classification	U.S. Labor Force, 1983	Pre-Incarceration	Halfway House	Six Month Follow-up	Twelve Month Follow-up			
Professional, Technical	26.4	12.5	11.8	12.9	12.5			
Clerical, Sales	28.0	15.9	17.6	19.8	20.0			

Service	13.7	20.6	11.2	12.4	11.1			
Agriculture, Fishing	3.7	4.0	5.9	4.5	5.2			
Processing	3.3	3.5	1.8	1.9	2.0			
Machine Trade	6.9	7.5	10.0	8.0	7.7			
Bench work	3.6	4.1	3.5	3.8	3.1			
Structural Work	7.7	20.3	30.6	26.9	26.2			
Miscellaneous	6.7	11.6	7.6	9.8	12.1			
	100.0	100.0	100.0	100.0	100.0			
Number of Cases	100,922,000	2,132	170	792	610			

Table 2. Occupational mobility from pre-incarceration to post-incarceration job.									

PRE-INCARCERATION JOB (ROWS) BY TWELVE MONTH FOLLOW-UP JOB (COLUMNS)

Frequency Row PCT	Profess & Tech	Clerical Sales	Service	Agricul & Fishing	Processing	Machine Trade	Bench Work	Structural Work	Miscellaneous	Row Totals
Professional and Technical	61	55	20	6	3	13	10	27	22	217
	28.11	25.35	9.22	2.76	1.38	5.99	4.61	12.44	10.14	100%
Clerical and Sales	43	105	32	9	1	14	12	32	20	268
	16.04	39.18	11.94	3.36	0.37	5.22	4.48	11.94	7.46	100%
Service	19	43	69	6	4	22	9	51	21	244
	7.79	17.62	28.28	2.46	1.64	9.02	3.69	20.9	8.61	100%
Agricultural and Fishing	8	6	2	22	3	3	2	20	8	74
	10.81	8.11	2.7	29.73	4.05	4.05	2.7	27.03	10.81	100%
Processing	4	5	2	2	0	5	0	13	3	34
	11.76	14.71	5.88	5.88	0	14.71	0	38.24	8.82	100%
Machine Trade	7	21	11	4	2	43	5	37	17	147
	4.76	14.29	7.48	2.72	1.36	29.25	3.4	25.17	11.56	100%
Bench work	6	8	7	1	2	8	9	26	2	69
	8.7	11.59	10.14	1.45	2.9	11.59	13.04	37.68	2.9	100%

Structural	24	27	31	10	4	28	13	175	34	346
Work	6.94	7.8	8.96	2.89	1.16	8.09	3.76	50.58	9.83	100%
Miscellaneous	21	34	10	6	1	19	6	29	58	184
	11.41	18.48	5.43	3.26	0.54	10.33	3.26	15.76	31.52	100%

Table 3. Cox proportional hazards model for the analysis of duration to recommitment for a new offense for male offenders.

VARIABLE	COEF.	STD. ERR.	WALD TEST	DF	SIG..	EXP(COEF.)
PROGRAM PARTICIPATION						
Industries (Ind.)	-.2799	.1125	6.1878	1	.0129	.76**
VT or Apprent.(App)	-.3952	.1623	5.9271	1	.0149	.67**
Ind./VT/App	-.2575	.1627	2.5028	1	.1136	.77
DECILES OF PROPENSITY SCORE						
1st Decile	-.2101	.1709	1.5114	1	.2189	.81
2nd Decile	-.3659	.1642	4.9664	1	.0258	.69**
3rd Decile	.2276	.1282	3.1526	1	.0758	1.26*
4th Decile	.0012	.1361	.0001	1	.9930	1.00
5th Decile	.1065	.1322	.6484	1	.4207	1.11
6th Decile	.1390	.1308	1.1285	1	.2881	1.15
7th Decile	.2546	.1294	3.8713	1	.0491	1.29**
8th Decile	-.2655	.1643	2.6106	1	.1062	.77
9th Decile	-.1483	.1626	.8309	1	.3620	.86
10th Decile	.1293	.1554	.6918	1	.4055	1.14
LOG TIME SERVED	.8123	.0652	155.3531	1	.0000	2.25**
RELEASE COHORT						
1985	.2395	.0804	8.8826	1	.0029	1.27**
1986	.0507	.0882	.3306	1	.5653	1.05

1987		.0233	.1379	.0285	1	.8661	1.02
BLACK	.1825	.0467	5.2893	1	.0001	1.20**	
HISPANIC		.2816	.0631	19.9345	1	.0000	1.33**

RELEASE AGE GROUP

18-24 years	.2700	.1427	3.5797	1	.0585	1.31*
25-34 years	.1163	.0883	1.7365	1	.1876	1.12
35-44 years	.0809	.0934	.7503	1	.3864	1.08
45-54 years	-.1381	.1345	1.0534	1	.3047	.87
>= 66 years	-.3630	.2319	2.4505	1	.1175	.70

EDUCATION GROUP

<=8th Grade	.1877	.1326	2.0017	1	.1571	1.21
9th to 11th	-.0272	.1184	.0528	1	.8183	.97
12th grade	.0465	.1043	.1992	1	.6554	1.05
13th to 15th	.1440	.1665	.7476	1	.3872	1.16
>= 16th grade	-.5596	.3471	2.5993	1	.1069	.57

- 2 Log Likelihood 9262.706, Covariates (-2LL) 262.491 df=29, p<.0001

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* Indicates the coefficient was significant, p < .10.

** Indicates the coefficient was significant, p < .05.

NOTES

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