Training Inmates through Industrial Work Participation and Vocational and Apprenticeship Instruction

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Data on more than 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. Because the training effects may be subtle, a large sample was developed to evaluate the prison training programs. Furthermore, because inmates could not be randomly assigned to the training condition, selection bias was controlled for by a statistical matching procedure that modeled the training program selection process. The results demonstrate significant and substantive training effects both on in-prison and post-prison outcome measures.

Key words: *prison industries, prison infractions, recidivism, rehabilitation, survival, vocational training*

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 release to the community. The evaluation began in 1983, and data were collected through October 1987 on more than 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, the concept of 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 theoretical and empirical support for the proposition that unemployment is a predictor of

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criminal activity.[2-10] Furthermore, recent evidence by Nagin and Waldfogel shows that a prison term can reduce the lifetime earnings of the ex-offender.[11,12] 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.[13]

Prison systems have a very difficult agenda if they are to affect the cycle of criminality. Data from this project indicate that in the 5 years prior to their current incarceration, half of these offenders worked less than 50 percent of the time; 42 percent worked less than 2 years in that 5-year period.

In addition to the Nagin and Waldfogel studies, 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 and associates[14] 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, because ex-offenders could afford to live without seeking employment. Rossi and coworkers suggest that transitional aid for ex-offenders could work if it were coupled with an incentive to find a job.

Schmidt and Witte[15] reviewed the evidence regarding post-release employment among ex-offenders and reached the following conclusions:

- Job terminations are typically the ex-offender's choice rather than the employer's choice.
- Post-release supervision has competing influences on employment productivity—supervision results in maintaining a job, but at lower wages than unsupervised releasees.
- 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 finding is consistent with the TARP findings.

- Unlike most subpopulations of the labor force, an inmate's age and education have little impact on labor market success; jobs obtained by releasees are typically low wage and low skilled.
- Offenders exhibit instability in their post-release employment. Offenders who remain employed typically have jobs in the lowest skill categories, working mainly in large manufacturing industries.
- In Michigan, halfway house participation has contributed to higher post-release wage earnings.
- Relatively stable background characteristics of the offender population contribute to higher post-release wages—white, able-bodied, married men with dependents earn higher wages.
- 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 and skills, the industry of employment, and the economic climate of the local labor market.
- Citing Borus and associates, Schmidt and Witte conclude that prison programs designed to improve basic or vocational skills have failed to affect post-release employment.[15]

Similar to the findings of Borus and colleagues, Maguire and coworkers 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.[16] Maguire and associates 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, they controlled for prison environment effects, but it is likely that the approach also exaggerated the program effects (this phenomenon is referred to as selection bias and is discussed in more detail later). Their method potentially introduced bias in program effects because inmates working in prison industries are likely to be more "motivated," and this fact would have left a less motivated pool of inmates to be used as comparison subjects. Despite this potential bias toward favorable findings, Maguire and colleagues found no effect of prison industries.

In related research on the hard-core unemployed (HCU), Goldstein reviewed training literature on the problem of assisting the HCU into the labor market.[17] Goldstein argues that skill training alone does not solve the problems of the HCU. These individuals have developed expectations of job failure that are difficult to overcome. Although no published data on the overlap in the ex-offender and HCU populations could be found, there are theoretical reasons to believe these populations do overlap, especially in light of Nagin and Waldfogel's evaluation of expected lifetime earnings of ex-offenders.

In yet another related area of research, some economists (see especially Piore[18]) argue 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 that 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, data on the broad occupational groupings of industries in which ex-offenders find jobs will be examined and compared to the occupational groupings in which these individuals were employed prior to their most recent incarceration. This approach will yield insight into the extent to which ex-offenders enter the secondary labor market.

Thus, the evidence to date on the employment patterns of ex-offenders reveals that these individuals

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are faced with lowered expectations and extremely precarious labor market conditions. Many do not have skills or education and carry the additional burden of the stigma associated with a term of imprisonment. Under these conditions, it is questionable whether skills training in prison can be used to penetrate the difficult labor market barriers that these ex-offenders face upon their release.

The current study was undertaken with a different approach in mind. First, it explicitly tries to control for selection bias in prison training evaluations. Second, recognizing that the effects of training may be subtle and the size of the effect may be relatively small, it employs a larger sample than previous studies. Last, this study examines the impact of work and skills training on institutional adjustment, licit wages after release, and post-release recidivism.

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 6 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 vocational training, 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 halfway house, staff at these contract facilities 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 or her behavior, including verified employment.

It is difficult to measure the effectiveness of programs without representing a biased picture of the results due to two key methodological issues. These issues—selection bias and "strong" inference designs—are related to the measurement of program effectiveness and are often ignored in the research design of many program evaluations. PREP was designed to address both problems.

Selection Bias

Selection bias refers to unintended influences that 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 assign inmates to programs randomly. There are instances when random assignment has been employed; however, there are practical and ethical reasons why it is rare that random assignment is used in selecting inmates for programs. It is often impractical to assign inmates to programs randomly because: (1) researchers are not allowed to control the selection process, and (2) inmates will contaminate the random assignment process by dropping out of a program, by disrupting the program, or by transferring into a group other than the one to which they were assigned.

In addition to formidable practical problems, there are also important ethical considerations why inmates should not 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, then that motivation may be subverted in an irreparable way. Moreover, one must question what is achieved by randomly assigning an inmate to a program when he or she is not motivated and may even be hostile to program participation. Is that program being contaminated for other inmates? Could random assignment preclude an inmate's future interest in a program by assigning him or her at a time before he or she is willing to participate?

One final statement regarding the comparability of experimental and observational designs is in order. Heckman and Hotz[19] found that 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 more compelling if researchers always adopted a strong inference design. A strong inference design is one in which the 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, it was assumed that prison work would be related to the supervisor's ratings of work abilities, work habits, and the motivation to work. An attempt was made 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 enhance confidence in observed treatment effects; that is, effects are real and not an artifact of selection bias or some other contamination.

Estimating the Propensity Score

To overcome the problem of selection bias, the study employed a statistical matching procedure developed by Cochran and Rubin[20] and further refined by Rosenbaum and Rubin.[21-23] The procedure uses a two-step approach. In the first step, the researcher models the selection process, contrasting program participants and nonparticipants 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 he or she was 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 the results are not displayed here, it is empirically demonstrated that the two groups are statistically indistinguishable on the set of measures used to model the employment/training selection process.)

The authors had 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 percent of the inmates housed in Bureau facilities were employed by prison industries; however, the waiting list to become employed by prison industries was always lengthy. There were always far more inmates who desired a prison industries job than prison industries could accommodate.

The ultimate purpose of the propensity score is to select appropriate comparison subjects. Nevertheless, the results of the logistic regression that generates the propensity score yield insight into the selection process itself. The results of this analysis demonstrated that study group members were more likely to be released 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, was used in the procedure that matched each study observation with a comparison observation selected from the comparison reservoir of all other offenders released in the same calendar quarter. It was required that the matching algorithm first

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establish an exact match based on sex and race. Then, for each study group member, a matched comparison observation was selected based on his or her 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. Because every job was categorized using the U.S. Department of Labor's *Dictionary of Occupational Titles*,[24] occupations could be grouped into nine 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 with 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 pat-

terns, 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 appren-

ticeship training were primarily instructed in machine trades and structural work. Industries employees were working primarily 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

Table 1

OCCUPATIONAL CHANGES IN THE STUDY AND COMPARISON GROUPS

Occupational classification	Occupational changes in the study group							
	U.S. labor force, 1983	Pre-incarceration	Vocational training	Apprentice-ship 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	15.0	3.5	19.0	20.5	18.0	19.3
Service	13.7	15.4	5.3	16.7	3.0	13.6	13.8	11.9
Agricultural/ fishing	3.7	4.4	1.6	2.6	0	1.9	2.9	3.3
Processing	3.3	2.0	5.5	4.4	1.4	2.0	1.5	1.0
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	26.0	26.0
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 classification	Occupational changes in the comparison group				
	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
Agricultural/ 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

work, followed by clerical/sales, service jobs, and professional. Relative to the U.S. labor force, after getting out of prison, offenders were more likely to do structural work or miscellaneous jobs and less likely to work in clerical or professional jobs.

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 depicts job changes in the aggregate. Table 2 is a mobility table that depicts the transitions that occur for individuals prior to their incarceration and 12 months after their release from prison. This table, which collapses data across study and comparison groups, provides 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 present some interesting descriptive patterns of pre- and 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 first indicates the number of individuals who had a particular set of pre-incarceration and post-incarceration jobs, and then, 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 that sum to 100 percent 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 profes-

Table 2

OCCUPATIONAL MOBILITY FROM PRE-INCARCERATION TO POST-INCARCERATION JOB*

Frequency Row PCT	Profes- sional/ technical	Clerical/ sales	Service	Agri- cultural/ fishing	Processing	Machine trade	Bench work	Structural work	Miscella- neous	Row totals
Professional/	61	55	20	6	3	13	10	27	22	217
technical	28.11	25.35	9.22	2.76	1.38	5.99	4.61	12.44	10.14	100%
Clerical/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/	8	6	2	22	3	3	2	20	8	74
fishing	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 work	24	27	31	10	4	28	13	175	34	346
	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%

*Rows indicate pre-incarceration job; columns reflect 12-month follow-up job.

sional/technical occupations. Among these 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 seem to support the thesis that prior to incarceration offenders are 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 such 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 finding reflects 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 determines 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 more serious level of institutional misconduct within the last 2 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 prison 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, outcome data on their criminal recidivism and employment were collected. 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 percent and 8.4 percent, respectively. Because other dispositions were possible, 83.3 percent and 83.9 percent of the comparison and study groups successfully completed their halfway house stay. Thus, there was little difference in 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 1 year were 35 percent less likely to recidivate than comparison group members. Although the absolute difference may not appear large, 6.6 percent 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 a random sample of releasees had been taken and no adjustment made for the background differences between the study group and comparison reservoir members, the group differences would have been greatly exaggerated (6.6% study versus 20% comparison). Although there is no independent confirmation of the propensity score adjustment, theoretically 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 were controlled for.

Twelve-month post-release outcome— Employment

In each of the 12 months following release, study group members were more likely to be employed than comparison group members. By the 12th month, study group members were 14 percent more likely (71.7% versus 63.1%) 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 12-month period, the average wages were about $9,700. According to

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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, the automated Bureau of Prisons records were reviewed 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 underestimate 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, the study versus 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. Separate models were estimated 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 this figure was 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 finding 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 yielded significant results and is represented in Table 3. Aside from the program participation variables, the decile of the individual's propensity score (decile of propensity score), the natural log of time served for the commit-

Table 3

COX PROPORTIONAL HAZARDS MODEL FOR THE ANALYSIS OF DURATION TO RECOMMITMENT FOR A NEW OFFENSE FOR MALE OFFENDERS

Variable	Coefficient	Standard error	WALD TEST	DF	SIG	EXP(COEF)
Program participation						
Industrials (Ind)	−.2799	.1125	6.1878	1	.0129	.76**
Vocational training (VT) or apprenticeship training (App)	−.3952	.1623	5.9271	1	.0149	.67**
Ind/VT/App	−.2575	.1627	2.5028	1	.1136	.77
Deciles of propensity score						
1st	−.2101	.1709	1.5114	1	.2189	.81
2nd	−.3659	.1642	4.9664	1	.0258	.69**
3rd	.2276	.1282	3.1526	1	.0758	1.26*
4th	.0012	.1361	.0001	1	.9930	1.00
5th	.1065	.1322	.6484	1	.4207	1.11
6th	.1390	.1308	1.1285	1	.2881	1.15
7th	.2546	.1294	3.8713	1	.0491	1.29**
8th	−.2655	.1643	2.6106	1	.1062	.77
9th	−.1483	.1626	.8309	1	.3620	.86
10th	.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
African American	.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						
Elementary school or less	.1877	.1326	2.0017	1	.1571	1.21
9th–11th grade	−.0272	.1184	.0528	1	.8183	.97
12th grade	.0465	.1043	.1992	1	.6554	1.05
13th–15th	.1440	.1665	.7476	1	.3872	1.16
16th grade or beyond	−.5596	.3471	2.5993	1	.1069	.57

−2 log likelihood, 9262.706; covariates (−2LL), 262.491; df = 29; $p < .0001$.
*Significant, $p < .10$.
**Significant, $p < .05$.

ment during which these inmates were identified for this study (log time served), the year the inmate was released to the community (release cohort), race (African American), ethnicity (Hispanic), age at release (release age group), and education level (education group) were included. For propensity score, release cohort, release age group, and education group, missing data were treated as categorical values. 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 does not appear to be any coherent pattern of significant propensity score coefficients. This finding demonstrates that the two-stage selection method for identifying comparison observations yielded two groups that were balanced with respect to this proxy measure.

The coefficients for Hispanics, African Americans, younger inmates (ages 18 to 24), inmates with longer periods of time served, and inmates released in 1985 were statistically significant. These findings 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 have been 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.

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