More Than a Job

Final Results from the Evaluation of the Center for Employment Opportunities (CEO) Transitional Jobs Program

OPRE Report 2011-18

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Overview

This report presents the final results of the evaluation of the Center for Employment Opportunities (CEO). CEO is one of four sites in the Enhanced Services for the Hard-to-Employ Demonstration and Evaluation Project, sponsored by the Administration for Children and Families and the Office of the Assistant Secretary for Planning and Evaluation in the U.S. Department of Health and Human Services (HHS), with additional funding from the U.S. Department of Labor. MDRC, a nonprofit, nonpartisan social and education policy research organization, is leading the evaluation, in collaboration with the Urban Institute and other partners.

Based in New York City, CEO is a comprehensive employment program for former prisoners — a population confronting many obstacles to finding and maintaining work. CEO provides temporary, paid jobs and other services in an effort to improve participants' labor market prospects and reduce the odds that they will return to prison. The study uses a rigorous random assignment design: it compares outcomes for individuals assigned to the program group, who were given access to CEO's jobs and other services, with the outcomes for those assigned to the control group, who were offered basic job search assistance at CEO along with other services in the community.

The three-year evaluation found that CEO substantially increased employment early in the follow-up period but that the effects faded over time. The initial increase in employment was due to the temporary jobs provided by the program. After the first year, employment and earnings were similar for both the program group and the control group.

CEO significantly reduced recidivism, with the most promising impacts occurring among a subgroup of former prisoners who enrolled shortly after release from prison (the group that the program was designed to serve). Among the subgroup that enrolled within three months after release, program group members were less likely than their control group counterparts to be arrested, convicted of a new crime, and reincarcerated. The program's impacts on these outcomes represent reductions in recidivism of 16 percent to 22 percent. In general, CEO's impacts were stronger for those who were more disadvantaged or at higher risk of recidivism when they enrolled in the study.

The evaluation includes a benefit-cost analysis, which shows that CEO's financial benefits outweighed its costs under a wide range of assumptions. Financial benefits exceeded the costs for taxpayers, victims, and participants. The majority of CEO's benefits were the result of reduced criminal justice system expenditures.

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The Center for Employment Opportunities (CEO) site in the Enhanced Services for the Hard-to-Employ Demonstration and Evaluation Project has received strong support from CEO administrators and staff and from officials in several New York State and City agencies. While many people have supported the study, space does not permit us to mention everyone who contributed to it.

The study would not have been possible without the ongoing commitment of the funder, the U.S. Department of Health and Human Services. We are grateful to the many staff at CEO who have steadfastly supported the study and given generously of their time. We owe special thanks to Executive Director and Chief Executive Officer Mindy Tarlow and to Chief Operating Officer Brad Dudding. Thanks also go to Marta Nelson, Sam Schaeffer, Tani Mills, Mary Bedeau, and Patrick Yurgosky for their contributions. S. Rebecca Neusteter, formerly of CEO, played a key role in the study's early stages.

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The Authors

Executive Summary

Introduction

The Center for Employment Opportunities (CEO) is one of four sites in the Enhanced Services for the Hard-to-Employ Demonstration and Evaluation Project, sponsored by the Administration for Children and Families and the Office of the Assistant Secretary for Planning and Evaluation in the U.S. Department of Health and Human Services (HHS), with additional funding from the U.S. Department of Labor. The overall project is evaluating diverse strategies designed to improve employment and other outcomes for several hard-to-employ populations, using a rigorous random assignment research design. MDRC — a nonprofit, nonpartisan social and education policy research organization — is leading the evaluation, in collaboration with the Urban Institute and other partners.

Based in New York City, CEO is a comprehensive employment program for former prisoners, a population confronting many obstacles to finding and maintaining work. CEO provides temporary, paid jobs and other services in an effort to improve participants' labor market prospects and reduce the odds that they will return to prison. The study uses a rigorous random assignment design: it compares outcomes for individuals assigned to the program group, who were given access to CEO's jobs and other services, with the outcomes of those assigned to the control group, who were offered basic job search assistance at CEO along with other services in the community. This report presents the final results of the CEO evaluation.[1] The results presented below and in earlier reports show that:

- **CEO substantially increased employment early in the follow-up period, but those effects faded over time.** The initial increase in employment was due to the temporary jobs provided by the program. After the first year, when program group members had left these transitional jobs, their employment and earnings were similar to those of control group members.

- **CEO significantly reduced recidivism, with the largest reductions occurring among a subgroup of former prisoners who enrolled shortly after release from prison.** The CEO program was designed to serve individuals immediately after release, but the study sample includes both people who came to the program shortly after release and others who came later. Among those who enrolled within three months after release, program group members were less likely than their control group counterparts to be arrested, con-

[1]For early results, see Bloom, Redcross, Zweig, and Azurdia (2007) and Redcross et al. (2009).

victed of a new crime, and reincarcerated. In general, CEO's impacts were stronger for those who were more disadvantaged or at higher risk of recidivism when they enrolled in the study.

- **CEO's benefits to society outweighed its costs under a wide range of assumptions.** Financial benefits exceeded costs for taxpayers, victims, and participants. The majority of CEO's benefits came in the form of reduced criminal justice system expenditures.

The Prisoner Reentry Crisis

Over the past three decades, incarceration has increased dramatically in the United States. Consequently, unprecedented numbers of prisoners are released each year. Former prisoners face a range of challenges to successful reentry into the community, and rates of recidivism are high. Within three years of release, two-thirds are arrested, and more than half return to prison or jail.[2] Recidivism imposes huge costs on taxpayers, families, and communities.

Many researchers and practitioners working in the reentry field believe that employment is a key ingredient in determining the success or failure of former prisoners' transition back to society. Positive employment outcomes can help pave the way to better housing conditions and improved relations within the family and community. Moreover, employment may help former prisoners feel more connected to mainstream society and help deter them from criminal activity.

Unfortunately, finding a steady job on release from prison is a major challenge for this population. Many employers are reluctant to hire someone with a prison record.[3] Most people who are recently released also have other attributes, such as low educational attainment and limited work history, which make them less appealing to potential employers, and they may have competing demands from drug treatment programs and curfews or other restrictions on mobility that can exacerbate the problem of finding and keeping full-time employment.

Comprehensive employment services may be critical to ensuring better postrelease outcomes, but there is little rigorous evidence about effective employment strategies for former prisoners. Transitional jobs are seen by many as a promising employment model for former prisoners and other disadvantaged groups. When targeted to people coming out of prison, transitional jobs programs place participants into temporary, paid jobs shortly after release and provide various kinds of other supports and help with finding a permanent job once a person is

[2]Langan and Levin (2002).
[3]Pager (2003).

ready. CEO in New York City is one of the nation's largest and most highly regarded transitional jobs programs for formerly incarcerated people.

The Center for Employment Opportunities

CEO's model starts with a short preemployment class lasting five days. Once participants complete the class, they are placed immediately into a transitional job in one of CEO's work crews. Crews of about six participants work in city and state agencies throughout New York City and are supervised by a CEO staff person. During the study period, participants worked seven hours a day, four days per week, and were paid each day for the work performed that day. The type of work performed on the work crews is not designed to teach skills for a specific occupation but, instead, is geared toward teaching the soft skills that employers value, such as how to show up to work on time and how to behave on the job. On the fifth day of each week, participants reported to CEO's office and met with their job coaches (case managers) and job developers. They could also participate in other services, such as a fatherhood program and parenting classes. Once deemed job-ready, participants got help finding a permanent job.

CEO's model is based on the assumption that people recently released from prison have an immediate need for income and help finding a job. The program's transitional jobs are designed to serve two purposes: (1) the jobs provide stability and income, which may reduce the incentive to turn back to crime in the critical period just after release; (2) the experience of working in a transitional job may teach participants how to work. Specifically, the soft skills learned on the work sites may make participants more appealing to employers by demonstrating that the individuals were able to show up to work on time and could perform satisfactorily in the program jobs. Employers might then be more willing to overlook a criminal background and hire CEO participants. Participants might also be better equipped for the regular labor market, which, in turn, could make them more likely to hold a job. The model assumes that better employment outcomes will help deter future recidivism.

The Research Design and Methodology

The CEO evaluation aims to determine whether CEO's transitional jobs and other services are more effective than basic job search assistance. The evaluation was designed as a rigorous random assignment study whereby former prisoners who were referred to CEO by their parole officers and reported to the program were randomly assigned to one of two research groups:

- **Program group.** Individuals who were assigned to the program group were eligible for all of CEO's services, including the preemployment class, the transitional job, job coaching, job development, parenting classes, and post-placement services.

- **Control group.** Individuals who were assigned to the control group began with a shorter version of the preemployment class and were given access to a resource room with basic job search equipment, such as computers and fax machines. A staff person was available to assist them with aspects of the job search if needed. Control group members also had access to other services in the community.

Study enrollment was conducted between January 2004 and October 2005 and resulted in a sample of 977 former prisoners: 568 in the program group and 409 in the control group. (Due to the random assignment design, described in Chapter 1 of this report, the sizes of the program and control groups in the overall study sample are not the same.) The research team tracked all sample members for three years following random assignment, using a number of data sources. The CEO program provided information on sample members' participation in program activities. State, city, and federal agencies provided administrative data reporting on criminal justice involvement as well as employment in jobs covered by unemployment insurance (UI). A subset of sample members also participated in a survey.

Because the study's sample members were assigned at random to one group or the other, the two groups, on average, were similar on all personal characteristics at the start of the study. Therefore, one can be confident that any statistically significant differences in outcomes that emerge between the groups over time can be attributed to CEO's core components. These differences are known as *impacts.* All impacts are regression-adjusted, using ordinary least squares, controlling for characteristics of sample members prior to random assignment. Tests of statistical significance were performed on all impacts presented in the report, to determine whether an impact could confidently be attributed to the CEO program.[4] All analyses use an "intent to treat" framework that compares the outcomes for all program group members with the outcomes for all control group members. No one for whom data were available was excluded from the analysis.

The evaluation examines impacts for the full study sample and for subgroups of the sample. Subgroups were defined using pre-random assignment characteristics, including the time between release from prison and random assignment, age, educational attainment, criminal history, and risk of recidivism (based on a risk index) — all variables that are hypothesized to affect impacts. Because of small sample sizes, subgroup impact estimates are considered less precise than full-sample impacts and, therefore, should be interpreted cautiously. Subgroup

[4]An impact is considered statistically significant if there is less than a 10 percent probability that the estimated difference would have occurred by chance, in the absence of any effect of the program. The report's tables and figures use asterisks and daggers to indicate statistical significance at the 10 percent, 5 percent, and 1 percent levels, as explained in the notes for each exhibit showing impacts.

impacts also require an additional test of statistical significance to assess the magnitude of differences in impacts across subgroups. Whenever such differences are statistically significant, one can have greater confidence that the underlying impacts for the subgroups involved are actually different from one another.[5]

In addition to the impact analysis, the evaluation includes an implementation study (Chapter 2) and a benefit-cost analysis (Chapter 4).

Characteristics of the Research Sample

The research sample is similar in many ways to the parole population in New York City.[6] The vast majority of sample members are male, and most are African-American or Hispanic. On average, sample members were 34 years old when they enrolled in the study; 43 percent were age 30 or younger. Just over half the sample had completed a high school diploma or a General Educational Development (GED) certificate. About half the study sample had at least one child under age 18, but only a small number lived with any of their children at baseline. Most had worked in the past, but only three out of five had ever worked six consecutive months for a single employer. About one-fourth of sample members had worked in a UI-covered job in the year *before* random assignment.

The sample members had extensive histories with the criminal justice system, with an average of seven prior convictions and a total of five years in state prison. All were under parole supervision when they entered the study.

Most of the people served by CEO come to the program either immediately after release from prison or shortly thereafter. However, just 41 percent of sample members enrolled in the study within three months after release. This occurred because contractual obligations required that the evaluation target a subset of CEO's overall client base.[7] Because the CEO model was intentionally designed to serve ex-prisoners immediately after release, and because most of CEO's broader population fits this profile, the impact results presented in this report are examined separately for the subgroup of people who came to CEO soon after release and those

[5]A statistical test was used to test for statistically significant differences in impact estimates across subgroups. Statistical significance levels for differences in subgroup impacts are indicated in the impact tables using daggers, as follows: ††† = 1 percent; †† = 5 percent; † = 10 percent.

[6]New York State Department of Corrections and Community Supervision (2009).

[7]Graduates of New York's Shock Incarceration (boot camp) program and participants in some other special programs were excluded from the study for contractual reasons. Individuals in these special programs almost always come to CEO just after release. Those in the study sample came to CEO after referral by a parole officer who was not involved in special programs. Parole officers base their referral decisions on a wide variety of concerns and circumstances. Some ex-prisoners are recently released, and others are not.

who came later. As described above, because the subgroups are based on characteristics of sample members at baseline (prior to random assignment), the subgroup analysis is fully consistent with the experimental research design.

Program Implementation

- **CEO's program operated as intended during the study period, and most program group members received the core services.**

Program tracking and payroll data show that almost 76 percent of the program group completed the initial five-day preemployment class and that 71 percent worked in a CEO transitional job for at least one day. The average time spent in transitional employment was about nine weeks, which generally occurred over about four months of engagement with the program. About 91 percent of program group members who worked in a transitional job also met with CEO job coaches or job developers at least once. About 44 percent of those who worked in a transitional job were placed into permanent jobs, according to CEO's records.

- **The program group was more likely than the control group to receive specific kinds of employment services but, as designed, many control group members got help with job search at CEO or elsewhere.**

CEO offered some help with basic job search assistance to control group members. But CEO's core program components, including transitional jobs and job development services, were offered only to program group members. In addition to the services that CEO offered to research sample members, it was expected that members from both research groups might seek out assistance from other organizations in the community.

Responses from a client survey that was fielded an average of 20 months after study entry provide information on participation in programs and service receipt for both the program group and the control group. Not surprisingly, the program group was substantially more likely to receive some kinds of employment help, such as referrals to specific job openings — the kind of help provided by CEO job developers. About 32 percent of program group members reported that they were referred to a specific job opening, compared with about 18 percent of control group members. In other areas, however, such as advice about filling out job applications or résumé building, the differences between research groups were much smaller because, as intended, many control group members received these services from CEO or another organization. Very few control group members worked in a transitional job at CEO, but a small number reported that they worked in similar jobs at other organizations. As noted above, because the offer of a paid transitional job is a substantially more expensive service than more individual and commonly offered program strategies like job search assistance, the CEO evaluation was

designed to determine whether CEO's transitional jobs and related services were more effective than basic job search assistance alone.

Impacts of CEO on Employment and Recidivism

Impacts for the Full Sample

- **For the full study sample, CEO substantially increased employment early in the follow-up period, but the impact faded over time as program group members exited the transitional jobs.**

Figure ES.1 shows CEO's impacts on overall employment, which is defined as working at least one day in a quarter, in either a CEO transitional job or any other UI-covered employment in New York State. CEO's largest impacts on employment occurred early in the first year of the study period, when the increases in employment were driven entirely by the transitional jobs themselves, and the impact faded as program group members left the transitional jobs.[8] There were no impacts on employment for the remainder of the three-year period (the postprogram period). After the first year, employment rates for both research groups were low; only about 30 percent of sample members worked in a UI-covered job in each quarter.

- **CEO reduced convictions for new crimes and incarceration over the three-year follow-up period.**

Table ES.1 shows CEO's impacts on several measures of recidivism (arrest, conviction, incarceration). Over the three-year follow-up period, the program group was significantly less likely than the control group to be convicted of a crime and to be incarcerated. Rates of recidivism were high but are similar to what has been found in national studies and to the rates among parolees released from New York State prisons during the same time frame.[9] CEO reduced overall recidivism; during the three-year follow-up period, 70 percent of the control group experienced some form of recidivism, compared with 65 percent of the program group.

[8]The data from the evaluation suggest that program group members left the transitional jobs for numerous reasons, including placement in unsubsidized employment or reincarceration. Many disengaged from program activities for unknown reasons.

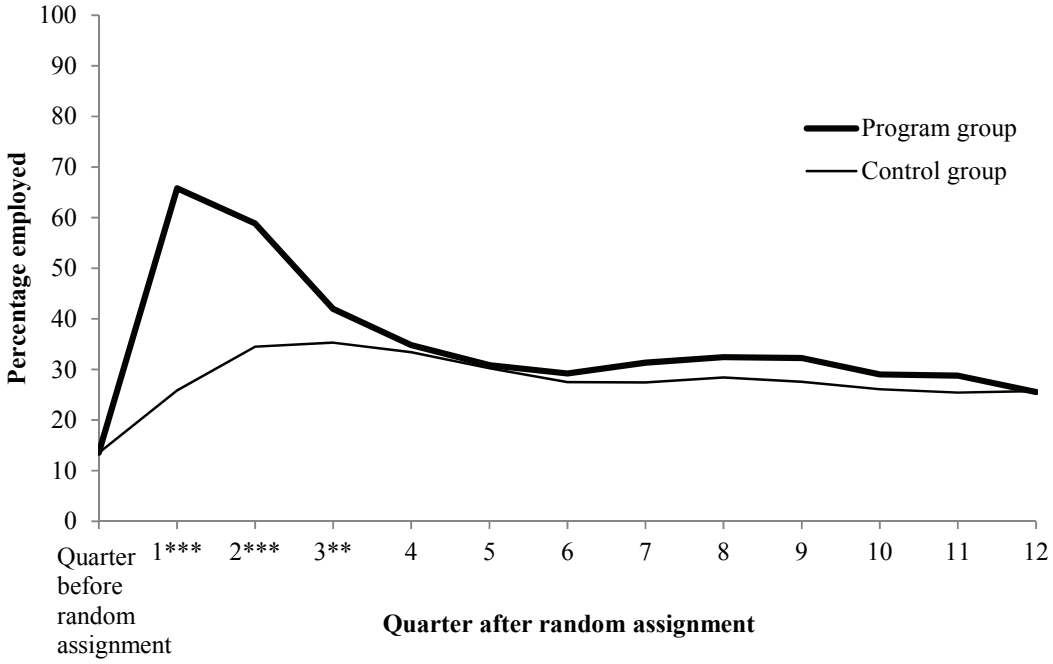
[9]Among the recently released subgroup, 50 percent were convicted of a new crime; this is similar to the proportion found among a national sample of prisoners released in 1994, in which 47 percent were convicted of a new crime within three years of release (Langan and Levin, 2002). Among the 24,520 offenders released from New York's state prisons in 2006, 11 percent were reincarcerated for a new felony within three years, which is similar to the recently released subgroup's average of 12 percent (New York State Division of Criminal Justice Services, 2010).

The Enhanced Services for the Hard-to-Employ Demonstration

Figure ES.1

Quarterly Impacts on Overall Employment

Center for Employment Opportunities



SOURCE: MDRC calculations from unemployment insurance (UI) wage records from New York State.

NOTES: Results in this figure are weighted by week of random assignment and adjusted for pre-random assignment characteristics.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

The sample size is 973. Four sample members are missing Social Security numbers and therefore could not be matched to employment data.

Impacts for the Subgroups Defined by Time Since Release from Prison

- **There is some evidence that CEO increased unsubsidized employment for sample members who came to the program shortly after release; this pattern was not evident for sample members who came to the program later.**

The Enhanced Services for the Hard-to-Employ Demonstration

Table ES.1

Three-Year Impacts on Recidivism

Center for Employment Opportunities

Outcome	Program Group	Control Group	Difference (Impact)	P-Value
Ever arrested[a] (%)	48.1	52.8	-4.7	0.147
Ever convicted of a crime[b] (%)	43.1	48.8	-5.6 *	0.078
Convicted of a felony	10.0	11.7	-1.6	0.419
Convicted of a misdemeanor	34.0	39.3	-5.4 *	0.083
Ever incarcerated[c] (%)	58.1	65.0	-6.9 **	0.027
Prison	33.7	35.2	-1.5	0.626
Jail	56.6	63.0	-6.4 **	0.041
Ever incarcerated for a new crime (%)	23.7	28.0	-4.3	0.128
Prison	7.8	9.9	-2.1	0.249
Jail	16.9	19.9	-3.0	0.229
Ever incarcerated for a technical parole violation (%)	37.5	35.1	2.4	0.435
Prison	21.9	19.6	2.2	0.394
Jail	35.4	31.6	3.8	0.216
Total days incarcerated	173	187	-14	0.392
Prison	92	104	-13	0.273
Jail	81	82	-1	0.917
Ever arrested, convicted, or incarcerated[d] (%)	64.9	70.6	-5.7 *	0.060
Incarcerated at end of Year 3[e] (%)	25.4	30.0	-4.6	0.114
Sample size (total = 977)	568	409		

SOURCES: MDRC calculations using data from the New York State Division of Criminal Justice Services (DCJS) and the New York City Department of Correction (DOC).

NOTES: Results in this table are weighted by week of random assignment and adjusted for pre-random assignment characteristics.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

[a]Each arrest date is counted only as a single event. If there are multiple crimes or charges on the same date, only the most serious charge is recorded in the analysis.

[b]A total of 23 convictions were found to be associated with an arrest that occurred prior to random assignment. These convictions are counted in the analysis as occurring after random assignment.

[c]Includes all reasons for incarceration, such as sentences for new crimes, technical violations of parole, detainee (jail), and other admission reasons. A sample member may have multiple admissions; therefore, incarcerations for new crimes and parole violations do not sum to the percentage incarcerated.

[d]This composite measure was created by combining three measures that are not mutually exclusive: arrest, conviction, and incarceration. Participants who were arrested and/or convicted, for example, were also incarcerated. The composite measure represents people who experienced one or more of these recidivism measures.

[e]Incarceration status based on Quarter 12 after random assignment; includes both prison and jail.

Table ES.2 shows impacts on measures of employment and earnings for subgroups defined at baseline and before random assignment occurred, analyzed by the elapsed time between release from prison and random assignment, with a focus on postprogram unsubsidized employment.[10] Similar to what was found for the full sample, recently released program group members had much higher rates of overall employment than recently released control group members, and the difference was driven by CEO's transitional jobs. However, in a pattern that differs from the full sample's, CEO had positive impacts on some measures of postprogram employment for the recently released subgroup.

On average, recently released program group members had more quarters with unsubsidized employment during the postprogram period than their control group counterparts. There were no significant impacts on postprogram earnings.[11] Without impacts on earnings, it is difficult to say with certainty that CEO had an impact on employment stability; one would expect that if the program group was employed in more quarters, there would have been a corresponding impact on earnings during the same time period. Among those who were further from release when they were randomly assigned, there were no impacts on postprogram unsubsidized employment outcomes (shown on the right-hand side of Table ES.2). The daggers in the rightmost column of the table identify impacts that are statistically different for the two subgroups. Again, these impact results should be interpreted with caution because the sample sizes of subgroups are smaller.

Given that CEO's impacts on unsubsidized employment for the recently released subgroup appeared relatively late in the follow-up period — long after most participants had left the program — it seems unlikely that they are a *direct* result of the program's services.[12] One hypothesis is that the same behavioral changes that led to CEO's effects on recidivism (discussed further below) may also have led to better employment retention for some people. It is also possible that the employment impacts are a secondary effect of the program's impacts on recidivism. Specifically, recently released program group members were more available for work than their control group counterparts because they were less likely to be incarcerated or otherwise involved with the criminal justice system, making it more likely that they would be employed later in the follow-up period.

[10]The overwhelming majority of program group members had no participation in CEO in Years 2 and 3 after random assignment, so this period of time is considered postprogram.

[11]The difference of about $1,100 is not statistically significant. Notably, the weighted average of the impacts for the subgroups is not equal to that for the full sample. This pattern occurs as a result of regression adjusting and has no effect on the basic impact finding for the earnings outcome. Even when impacts are run unadjusted, the differences do not rise to the level of statistical significance, and the main finding of no impact is unchanged.

[12]For the program to have had a direct effect, one would have expected to find effects on continuous employment as individuals transitioned from subsidized to unsubsidized employment.

The Enhanced Services for the Hard-to-Employ Demonstration

Table ES.2

Impacts on Employment and Earnings, by Time Between Prison Release and Random Assignment

Center for Employment Opportunities

Outcome	Length of Time Between Prison Release and Random Assignment: 3 Months or Less — Program Group	Control Group	Difference (Impact)	P-Value[a]	More Than 3 Months — Program Group	Control Group	Difference (Impact)	P-Value[a]	Difference Between Subgroup Impacts[b]
Employment (Years 1-3) (%)									
Ever any employment[c]	87.3	72.3	15.1 ***	0.000	82.0	69.1	12.9 ***	0.000	
Ever worked in a CEO transitional job[d]	73.5	-0.9	74.4 ***	0.000	68.3	5.2	63.1 ***	0.000	††
Ever worked in an unsubsidized job	68.9	71.4	-2.5	0.612	59.5	67.9	-8.4 **	0.037	
Postprogram unsubsidized employment outcomes (Years 2-3) (%)									
Ever worked in an unsubsidized job	58.3	54.6	3.7	0.472	47.9	51.7	-3.8	0.374	
Average quarterly unsubsidized employment	33.8	27.5	6.2 *	0.074	24.9	27.1	-2.2	0.444	†
Number of quarters with unsubsidized employment									
None	41.7	45.4	-3.7	0.472	52.1	48.3	3.8	0.374	
1 to 3	23.9	25.0	-1.1	0.818	21.6	23.1	-1.5	0.679	
4 to 6	15.5	18.2	-2.7	0.489	14.0	16.1	-2.1	0.502	
7 to 8	18.9	11.3	7.5 **	0.038	12.3	12.5	-0.2	0.954	†
Worked 6 or more consecutive quarters	17.9	12.0	5.9 *	0.086	14.3	10.6	3.7	0.199	
Earnings (Years 2-3)[e] ($)									
Total UI-covered earnings[c]	12,385	11,185	1,200	0.582	9,820	8,252	1,568	0.356	
CEO transitional job earnings	61	-6	67	0.121	159	18	141 ***	0.000	
Unsubsidized earnings	12,323	11,192	1,132	0.603	9,662	8,235	1,427	0.401	
Sample size (total = 926)[f]	223	160			310	233			

(continued)

Table ES.2 (continued)

SOURCES: MDRC earnings calculations from the National Directory of New Hires (NDNH) database and employment calculations from the unemployment insurance (UI) wage records from New York State.

NOTES: Results in this table are weighted by week of random assignment and adjusted for pre-random assignment characteristics.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

A small number of control group members accessed CEO's transitional jobs. Due to weighting and regression adjustments, CEO employment and earnings outcomes for the recently released control group are less than zero.

[a]Standard errors are presented in this report for all impacts with a p-value of 0.000. Following are the standard errors for all impacts with a p-value of 0.000 (presented in the order in which they appear in the table and beginning with the "3 Months or Less" subgroup): Employment: 4.131, 3.691, 3.571, and 3.301. Earnings: 37.772.

[b]When comparing impacts between two subgroups, an H-statistic is generated. The H-statistic is used to assess whether the difference in impacts between the subgroups is statistically significant. It is interpretable in much the same way as the t-statistic and the F-statistic from analysis of variance (ANOVA) tests are interpreted. Statistically significant differences across subgroups are indicated as: ††† = 1 percent; †† = 5 percent; † = 10 percent.

[c]These outcomes reflect only UI-covered employment and earnings.

[d]CEO transitional employment is estimated using data from NDNH and CEO's management information system (MIS).

[e]Due to missing earnings data for Year 1, this report includes impacts for only Years 2 and 3. The earnings difference of around $1,100 is not statistically significant. Notably, the weighted average of the impacts for the subgroups is not equal to that of the full sample. This pattern occurs as a result of regression adjusting and has no effect on the basic impact finding for the earnings outcome. Even when impacts are run unadjusted, the differences do not rise to the level of statistical significance and the main finding of no impact is unchanged.

[f]A total of 48 sample members are missing the latest prison release date prior to random assignment and are therefore missing from estimates in this table. An additional three sample members are missing Social Security numbers and therefore could not be matched to employment data.

- **CEO's impacts on recidivism were strongest among those who were recently released from prison. For that subgroup, CEO reduced arrests, convictions, and incarceration during the three-year follow-up period.**

Table ES.3 presents CEO's impacts on recidivism for subgroups defined by time between prison release and random assignment. The impacts on recidivism were largest for those who came to the program shortly after release. Among that subgroup, program group members were significantly less likely than control group members to be arrested (49 percent, compared with 59 percent); convicted of a crime (44 percent, compared with 57 percent); or incarcerated (60 percent, compared with 71 percent). These impacts represent a reduction in recidivism of 16 percent to 22 percent across the three outcomes.[13] There were no statistically significant impacts on recidivism among those who entered the study more than three months after release from prison.

Figure ES.2 is helpful in understanding how CEO's impacts on recidivism unfolded over the three-year follow-up period. The figure shows CEO's impacts on the first occurrence of an arrest, conviction, or admission to prison or jail after an individual's date of random assignment.[14] The upper panel of the figure shows that CEO's impacts on returns to crime were concentrated in Year 1 of the follow-up period, when program group members were active in the program or shortly thereafter. (For the recently released subgroup, this time period corresponds closely to the first year after release from prison.)[15] Rates of recidivism in the first year were 12 percentage points lower for the program group than for the control group (35 percent, compared with 47 percent); this impact represents a 26 percent reduction in recidivism. In Years 2 and 3, by contrast, there was no significant difference between the program and control groups in *initial* recidivism.

Once CEO initially prevented someone from returning to crime, some of those same people continued to have lower recidivism rates in future years. Prior reports from this evaluation and tables in Appendix D of this report show that CEO had impacts on some measures of recidivism in the second and third years of the follow-up period. In combination with the

[13]The percentage change in recidivism is estimated by dividing CEO's impact on recidivism by the mean of the control group for each outcome. For convictions, the calculation would be –12.7 divided by 56.7.

[14]There is an important distinction between the *first* recidivism event and the recidivism events presented above in Tables ES.1 and ES.3. The *first* recidivism event for an individual can occur only once during the study follow-up, while the overall recidivism outcomes, shown in the preceding tables, include recidivism that occurred in each of the years, without regard for whether it was the first, second, or third event for an individual. Measures of impacts on the *first* recidivism event are identical to *any* recidivism event for Year 1 of the follow-up. These two measures differ in Years 2 and 3.

[15]For the recently released subgroup, it is likely that the first event *after random assignment* is the same as the first event *after release* because of the short amount of time that had passed between release from prison and being randomly assigned.

The Enhanced Services for the Hard-to-Employ Demonstration

Table ES.3

Three-Year Impacts on Recidivism, by Time Between Prison Release and Random Assignment

Center for Employment Opportunities

Outcome	Length of Time Between Prison Release and Random Assignment: 3 Months or Less — Program Group	Control Group	Difference (Impact)	P-Value	More Than 3 Months — Program Group	Control Group	Difference (Impact)	P-Value	Difference Between Subgroup Impacts[a]
Ever arrested[b] (%)	49.1	59.1	-10.0 *	0.056	47.0	50.5	-3.5	0.420	
Ever convicted of a crime[c] (%)	44.0	56.7	-12.7 **	0.014	42.7	45.7	-3.0	0.493	
Convicted of a felony	15.6	14.6	1.0	0.789	6.8	10.2	-3.4	0.156	
Convicted of a misdemeanor	31.9	46.1	-14.3 ***	0.005	35.5	36.8	-1.3	0.764	††
Ever incarcerated[d] (%)	60.2	71.3	-11.2 **	0.027	57.1	63.2	-6.1	0.147	
Prison	38.9	43.4	-4.5	0.387	33.1	31.6	1.4	0.722	
Jail	56.7	71.4	-14.7 ***	0.004	56.7	60.0	-3.4	0.428	†
Ever incarcerated for a new crime (%)	26.5	35.4	-8.9 *	0.061	22.7	24.7	-2.0	0.593	
Prison	11.2	12.5	-1.4	0.685	5.9	8.8	-2.8	0.209	
Jail	16.4	25.6	-9.1 **	0.030	17.9	17.1	0.7	0.822	†
Ever incarcerated for a technical parole violation (%	38.5	39.8	-1.3	0.801	36.8	34.2	2.6	0.528	
Prison	23.8	25.5	-1.7	0.717	22.3	16.8	5.5	0.110	
Jail	34.2	35.1	-0.9	0.856	36.1	31.2	4.9	0.228	
Total days incarcerated	213	247	-34	0.234	154	151	3	0.872	
Prison	118	138	-20	0.345	82	85	-3	0.852	
Jail	95	109	-14	0.334	72	66	6	0.560	
Ever arrested, convicted, or incarcerated[e] (%)	66.8	75.8	-9.0 *	0.063	64.3	70.2	-5.9	0.148	
Incarcerated at the end of Year 3[f] (%)	30.1	36.1	-6.0	0.221	22.5	27.4	-4.9	0.195	
Sample size (total = 929)[g]	225	160			311	233			

(continued)

Table ES.3 (continued)

SOURCES: MDRC calculations using data from the New York State Division of Criminal Justice Services (DCJS) and the New York City Department of Correction (DOC).

NOTES: Results in this table are weighted by week of random assignment and adjusted for pre-random assignment characteristics.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

[a]When comparing impacts between two subgroups, an H-statistic is generated. The H-statistic is used to assess whether the difference in impacts between the subgroups is statistically significant. It is interpretable in much the same way as the t-statistic and the F-statistic from analysis of variance (ANOVA) tests are interpreted. Statistically significant differences across subgroups are indicated as: ††† = 1 percent; †† = 5 percent; † = 10 percent.

[b]Each arrest date is counted only as a single event. If there are multiple crimes or charges on the same date, only one the most serious charge is recorded in the analysis.

[c]A total of 23 convictions were found to be associated with an arrest that occurred prior to random assignment. These convictions are counted in the analysis as occurring after random assignment.

[d]Includes all reasons for incarceration, such as sentences for new crimes, technical violations of parole, detainee (jail), and other admission reasons. A sample member may have multiple admissions; therefore, incarcerations for new crimes and parole violations do not sum to the percentage incarcerated.

[e]This composite measure was created by combining three measures that are not mutually exclusive: arrest, conviction, and incarceration. Participants who were arrested and/or convicted, for example, were also incarcerated. The composite measure represents people who experienced one or more of these recidivism measures.

[f]Incarceration status based on Quarter 12 after random assignment. Incarceration includes both prison and jail.

[g]A total of 48 sample members are missing the last prison release date and are therefore missing from all outcomes in this table.

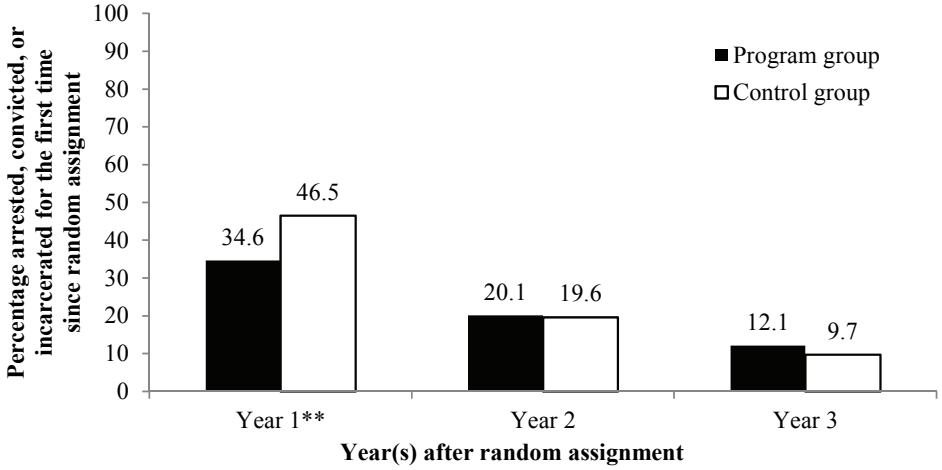
The Enhanced Services for the Hard-to-Employ Demonstration

Figure ES.2

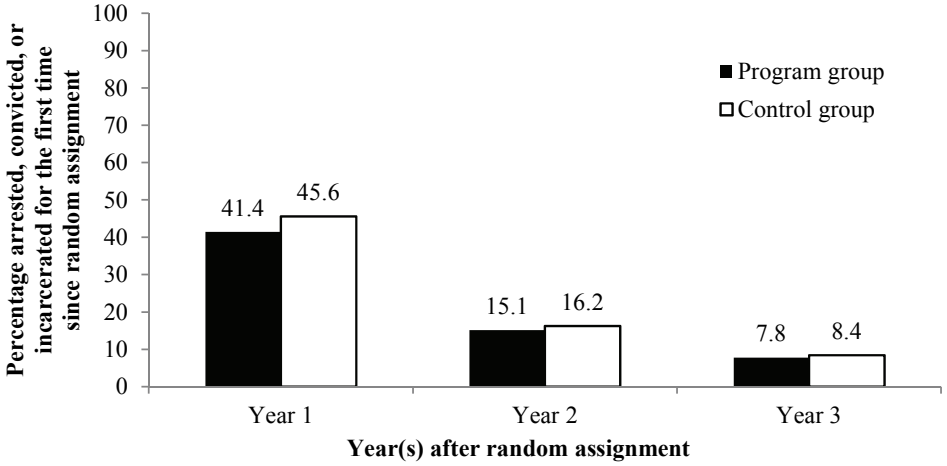
Impacts on First Incident of Recidivism After Random Assignment, by Time Between Prison Release and Random Assignment

Center for Employment Opportunities

Recently released subgroup



Not recently released subgroup



SOURCES: MDRC calculations using data from the New York State Division of Criminal Justice Services (DCJS) and the New York City Department of Correction (DOC).

NOTES: Results in this figure are weighted by week of random assignment and adjusted for pre-random assignment characteristics.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

Sample members may have multiple arrests, convictions, or incarcerations in the follow-up period, but only the first incident of recidivism was used in the calculations for this table.

The sample size is 929. A total of 48 sample members are missing the last prison release date and are therefore missing from all outcomes in this figure.

findings shown in Figure ES.2, there is evidence that CEO's impacts on recidivism in Years 2 and 3 of the study's follow-up period may have been driven by its initial impacts on crime, which occurred during Year 1. This pattern is consistent with other studies, which have shown that the risk of returning to crime is highest in the first year after release and declines steadily with time.[16]

Impacts for Other Subgroups

- **CEO's impacts were stronger for those who were more disadvantaged and those at highest risk of recidivism.**

Impact results were also examined for other subgroups that were formed based on sample members' characteristics before random assignment. The pattern of findings across several subgroups suggests that CEO's impacts were strongest for those who were more disadvantaged and at higher risk of recidivism. The subgroups with the largest impacts on employment and recidivism include those with four or more prior convictions, those without a high school diploma or GED, and those with a high risk of recidivism (based on a risk index determined by age, number of prior convictions, and other static factors) at the time of random assignment.[17] For example, among the subgroup with four or more prior convictions at the time of study entry, CEO reduced convictions for new crimes by 12.8 percentage points. Among the subgroup with fewer prior convictions at study entry, no statistically significant difference in new convictions was found between program and control group members. Notably, there is some overlap among these subgroups. For instance, many of those with four or more prior convictions are also categorized as having a high risk of recidivism.

Benefits and Costs of CEO

The results described above show that CEO generated positive impacts on employment and reductions in recidivism and that these impacts were strongest for the recently released subgroup. If the cost to operate the CEO program is less than the savings associated with those impacts, taxpayers and other stakeholders may realize monetary benefits. The CEO evaluation includes a benefit-cost analysis to assess the benefits and costs associated with the CEO program and to answer questions about whether CEO is cost-effective from the perspective of

[16]Blumstein and Nakamura (2010). The highest risk of recidivism occurs in the first year after release. Research has shown that recidivism declines steadily with time clean. After 3.8 to 7.7 years (depending on the type of crime), the likelihood of a former prisoner's committing a new crime is equal to that of people of the same age in the general population.

[17]A working paper from this evaluation describes the method used to assess a sample member's level of risk and shows that CEO's impacts on recidivism were larger for those at "high" risk of recidivism when they entered the study (Zweig, Yahner, and Redcross, 2010).

taxpayers, victims, participants, and society as a whole. The societal perspective sums the taxpayer, victim, and participant perspectives and represents the combined net benefit from all three perspectives.

The analysis of CEO's financial benefits expands on the impact analysis and considers the value of earnings (and associated taxes and credits), transitional jobs labor, and recidivism outcomes. The CEO benefit-cost analysis places a dollar value on each *incident* of recidivism (that is, the number of arrests, convictions, and admissions to prison), unlike the main impact analysis, which focuses primarily on the *prevalence* of recidivism in the research sample (that is, the proportions of the sample who experienced each outcome).

- **For the full sample, the estimated benefits generated by CEO outweighed the program's costs. For the recently released subgroup, benefits outweighed costs by a larger margin than for the full sample.**

CEO's impacts on recidivism and employment translated into economic benefits that outweighed program costs. The benefit-cost analysis estimated that the total net benefit of CEO was about $4,100 per program group member from a taxpayer perspective; the total net benefit to society was estimated to be about $4,900 per program group member. As discussed above, CEO's impacts on recidivism were larger for the subgroup that was recently released from prison; therefore, the total net benefit of CEO was also larger for this subgroup than for the full sample. The estimated net benefit of CEO is about $8,300 per *recently released* program group member, from a taxpayer perspective. The majority of benefits to taxpayers came in the form of reduced criminal justice system expenditures and the value of services that CEO participants provided to government agencies in the transitional job work sites.[18]

As with any benefit-cost analysis of this type, the CEO analysis is based on estimated impacts, which have varying levels of certainty. In order to estimate the financial value of CEO's effects, the actual differences observed in outcomes were used, whether or not they reach a level of statistical significance because they are nonetheless the best estimate of the actual impact of the program. If the true impact of the program is larger or smaller than the differences in outcomes observed in this study, the net value of the CEO program will correspondingly increase or decrease.

The estimates of the benefits and costs of CEO also depend greatly on assumptions about the number of people that the program is likely to serve (its operating scale) and about the

[18]Victim costs contribute minimally to the net benefit-cost results in this study because the conviction charges are primarily for drug crimes, which are generally considered "victimless" in the research literature.

number of prison beds that might be eliminated due to reduced recidivism.[19] Changes in the current operating scale of the CEO program and adjustments to other underlying assumptions would correspondingly increase or reduce the estimated net benefits generated by the program. In order to illustrate how different assumptions change the results, a number of sensitivity adjustments are presented in the benefit-cost chapter of this report. Even when the most conservative assumptions are applied simultaneously, CEO appears to be a cost-effective option. Under a wide range of assumptions, the program generates between $1.26 and $3.85 in benefits per $1.00 of cost.[20]

Conclusion and Policy Implications

CEO generated large impacts on initial employment because of the transitional jobs, but the effect faded quickly as program group members attempted to transition to unsubsidized jobs. CEO also generated significant reductions in key measures of recidivism. The program's impacts on recidivism were especially promising for the recently released subgroup (the group that the program was designed to serve). CEO's impacts on initial returns to crime were concentrated in the first year of the follow-up period, when program group members were active in the program or shortly after they left it. The evaluation produced strong evidence that CEO prevented the first recidivism event after release for some program group members. By using employment as an immediate engagement strategy after release, CEO intervened early on and placed those people on a different trajectory, deterring future criminal activity.[21] Reductions in recidivism are difficult to achieve and have rarely been seen in rigorous evaluations such as this one. One of the primary goals of the program was to produce sustained and consistent impacts on unsubsidized employment. The results in that area are less encouraging, though the program may have improved employment stability later in the follow-up for some participants.

One factor that complicates the interpretation of the CEO findings is the fact that another rigorous random assignment study of transitional jobs programs for former prisoners — the Transitional Jobs Reentry Demonstration (TJRD), which was conducted in four programs in the Midwest — did not find similar impacts on recidivism in the first year. The TJRD programs operated transitional jobs models that differed from CEO in a number of ways; notably, none of them operated a work crew model. Yet all of the programs were successful in placing program

[19]Underlying assumptions most directly affect the marginal cost of incarceration in prison and jail, where costs vary greatly depending on the scale of the program and the number of beds eliminated.

[20]The low-end value of $1.26 was estimated for the full sample using all of the most conservative underlying assumptions. The high-end value of $3.85 represents the high-end assumptions for the recently released subgroup. See the discussion in Chapter 4 for details about the benefit-cost methodology and sensitivity analyses.

[21]See Appendix Table D.3 and Redcross et al. (2009).

group members into transitional jobs and, as a result, increased employment initially. Taken together, these evaluations show that although it is possible for a transitional jobs program to reduce recidivism, such results are not typical. The results also underscore the complexity of the relationship between employment and recidivism.

One hypothesis for why the CEO model produced stronger impacts on recidivism than the TJRD models is that the CEO model — particularly its small work crews — encouraged a mentoring type of relationship to develop between participants and CEO staff, particularly worksite supervisors. Indeed, survey results show that program group members were more likely than control group members to feel connected with staff.[22] The work crew model also gives participants the opportunity to interact with peers in a positive environment, which may have affected their attitudes and behaviors.

One thing is clear and consistent across the findings from the CEO and TJRD evaluations: rates of employment are very low among those returning from prison. Less than 30 percent of the control group in each study were employed in any given quarter. In both studies, results show that transitional jobs were successful in generating higher rates of employment than would otherwise be found for returning prisoners, demonstrating that when former prisoners are offered an immediate paid job, they are willing to work.

With the current fiscal crisis in most states, policymakers are looking for ways to reduce criminal justice expenditures while maintaining public safety. From a policy perspective, the CEO program as operated appears to be a cost-effective reentry option. Under a wide range of assumptions, the monetary benefits generated by the CEO program exceed its costs to taxpayers. It is important to confirm these findings in additional studies.

In designing future transitional jobs evaluations, it will be important for policymakers and program operators to consider enhancements to existing models. Recommendations for enhancements depend largely on the reasons that one believes the programs studied thus far have not produced consistent employment impacts. One hypothesis is that the transitional jobs programs generally did not train participants for specific occupations. Another hypothesis is that the transitional jobs were too short and should be extended to allow more time to build a participant's employability before a transition to the regular labor market is considered. All of the programs experienced difficulties in identifying job opportunities in the private sector and in helping participants make the transition to regular employment. Therefore, programs may consider boosting job development and placement services, perhaps by offering incentives to employers or by putting more emphasis on identifying employment opportunities, cultivating partnerships with private employers, and helping participants stay employed once they obtain

[22]Redcross et al. (2009).

unsubsidized jobs. Financial incentives for keeping an unsubsidized job have shown promising effects in TJRD and other studies and could be part of a comprehensive employment strategy.[23]

Future evaluations of programs serving former prisoners should also consider looking closely at program components that address criminal thinking and behaviors. Some criminologists believe that cognitive-behavioral approaches may be key to reducing recidivism. Indeed, the findings from the CEO evaluation suggest that the program's promising impacts on recidivism may have been driven by the positive peer and staff influences that extended beyond the basic provision of employment, though paid employment may be needed to engage participants in activities designed to influence behaviors.

The U.S. Department of Labor and the U.S. Department of Health and Human Services are both mounting multisite random assignment studies of enhanced transitional jobs models for various populations, including former prisoners, noncustodial parents, and welfare recipients. Both evaluations are building on the body of evidence produced by this and other studies of transitional jobs programs.

Partly in response to evidence from this evaluation, CEO has refined and enhanced the model that was implemented in this study. Thus, the results of this evaluation might be different if the study took place today. CEO is currently operating replication programs in several locations in the United States; a random assignment evaluation of those programs is planned in the coming years.

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Chapter 1

Introduction

This report presents final results from a rigorous evaluation of the New York City-based Center for Employment Opportunities (CEO), one the nation's largest and most highly regarded employment programs for former prisoners. CEO is one of four sites in the Enhanced Services for the Hard-to-Employ Demonstration and Evaluation Project, which is testing innovative employment strategies for groups facing serious obstacles to steady work. The Hard-to-Employ project is sponsored by the Administration for Children and Families and the Office of the Assistant Secretary for Planning and Evaluation in the U.S. Department and Health and Human Services (HHS), with additional funding from the U.S. Department of Labor. It is being conducted under contract to HHS by MDRC, a nonprofit, nonpartisan research organization, along with the Urban Institute and other partners.

Prisoner Reentry and Transitional Work

The number of people incarcerated in the United States has increased more than fourfold since the 1970s.[1] Today, more than 2 million people are incarcerated in federal and state prisons and local jails,[2] and almost 700,000 people are released from state prisons each year.[3] State governments alone spend over $50 billion per year on corrections costs.[4]

Ex-prisoners face daunting obstacles to successful reentry — for example, difficulties finding jobs, housing, and services for substance abuse or mental health problems. Moreover, they tend to return home from prison to a relatively small number of urban neighborhoods that also experience high rates of poverty and other social problems. Not surprisingly, rates of recidivism are very high.[5] The most recent national statistics show that two-thirds of ex-prisoners are rearrested and that half are reincarcerated within three years of release.[6] In New York State, of the 24,500 people released from state prisons in 2006, about 40 percent returned

[1]On a per capita basis, the number of people in prison in the United States remained roughly constant — about 110 per 100,000 residents — from the 1920s to the 1970s. By 2004, there were 484 prisoners per 100,000 residents (Raphael and Stoll, 2007).

[2]Bureau of Justice Statistics (2011b).

[3]Sabol, West, and Cooper (2010).

[4]National Association of State Budget Officers (2010).

[5]Lynch and Sabol (2001).

[6]Langan and Levin (2002).

to prison within three years — 10 percent for a new felony conviction and 30 percent for a "rule violation," such as a failure to comply with parole conditions.[7]

Many believe that stable employment is critical to a successful transition back into the community, but ex-prisoners tend to have characteristics that make them hard to employ — for example, low levels of education and little previous work experience.[8] Moreover, there is strong evidence that incarceration further hinders their employability. Ex-prisoners are legally barred from working in certain occupations, and many employers are reluctant to hire people who have criminal records.[9] At the end of 2009, only 13 percent of New York State parolees who were available for work were employed full time and earning above minimum wage; 65 percent were not employed at all.[10]

Very little is known about effective employment strategies for ex-prisoners, but many people see transitional jobs as a promising model.[11] This is the latest in a long line of subsidized employment models that have been targeted to hard-to-employ individuals.[12] Transitional jobs programs rapidly place participants into temporary, paid jobs, usually in nonprofit or government agencies; provide various kinds of support (for example, case management, job coaching workshops, or referrals for social services); and then help participants find permanent jobs. When targeted to ex-prisoners, transitional jobs give participants a source of legitimate income during the critical postrelease period,[13] and they also provide staff with an opportunity to identify and address workplace problems before participants move to the regular labor market.[14] Thus, if there is indeed a causal relationship between employment and recidivism, transitional jobs programs may lead to immediate reductions in recidivism, in addition to preparing participants for longer-term success in the labor market.

As an example of the ongoing interest in transitional jobs as a potentially successful model for people coming out of prison, both foundations and the federal government have funded additional transitional job evaluations. One evaluation that drew from the experiences of

[7]New York State Division of Criminal Justice Services (2010).

[8]Harer (1994); Uggen (2000).

[9]Holzer, Raphael, and Stoll (2003); Pager (2003).

[10]New York State Division of Criminal Justice Services (2010). These figures are not directly comparable to this report's outcome data for study participants.

[11]For a review of research on employment-focused programs for ex-prisoners, see Bloom (2006).

[12]The term "subsidized job" is defined as employment where wages are subsidized by an employment program such as CEO.

[13]Recidivism is higher during the first year after release (Langan and Levin, 2002; Blumstein and Nakamura, 2010). Two-thirds of all the recidivism over the three-year follow-up period occurred during the first year.

[14]For more information on the transitional job model, see Bloom (2010) and the National Transitional Jobs Network Web site: http://www.heartlandalliance.org/ntjn/.

CEO is the Transitional Jobs Reentry Demonstration (TJRD). Funded by the Chicago-based Joyce Foundation and others, TJRD evaluates transitional jobs programs for formerly incarcerated people in four Midwestern cities. Early findings from TJRD are discussed below, in the context of the CEO findings described in this report. Evaluations of enhanced transitional job models — sponsored by the U.S. Department of Health and Human Services and the U.S. Department of Labor — are currently in the design phase.

The Center for Employment Opportunities

The Center for Employment Opportunities (CEO) currently serves nearly 2,500 parolees each year and provides immediate, paid work to individuals returning to New York City after being released from prison. The CEO model is designed on the assumption, as discussed above, that helping ex-prisoners find jobs — particularly during the immediate postrelease period — will ultimately lead to lower recidivism. In the short-term, the program raises rates of employment directly through the transitional jobs. Over the long-term, the transitional job experience is designed to help participants develop "world of work" or "soft" skills, such as punctuality and the ability to work in a team — and to build a track record of successful performance on the job — that will help them get and keep permanent employment.

Individuals who are referred to CEO by their parole officer are placed in paid jobs immediately after completing a five-day preemployment class. Participants work in crews, performing maintenance and repair work for city and state agencies at two dozen sites around New York City. Participants generally work four days a week and are paid minimum wage.[15] They are paid each afternoon, at their work sites.[16] The fifth day each week is spent in CEO's office in lower Manhattan, where participants meet with staff and can participate in supplementary activities, such as assistance with child support orders and parenting classes.

Participants are continuously evaluated by their work site supervisors, who are CEO staff members. Both supervisors and office-based job coaches (employment-focused case managers) seek to identify and address workplace problems. Although CEO offers some supplementary services, it maintains a sharp focus on employment. Job coaches may occasionally work with parole officers to refer participants to other organizations for help with housing, substance abuse, or other issues. Once participants demonstrate good performance on the job, they are deemed "job-start-ready" and begin working with a job developer to find permanent employment. CEO continues to provide support after placement, though, as discussed further in Chapter 2, the nature and intensity of those postemployment services changed during the period of the study.

[15]New York State's minimum wage at the time of publication is $7.25 per hour. When study participants were enrolled in CEO, the minimum wage ranged from $5.15 to $6.75 per hour.

[16]Participants who work on CEO's night crews are paid for their work the following day.

As Figure 1.1 illustrates, the services provided by CEO may lead to employment and much-needed income for participants as a result of the transitional jobs. In addition, work site supervisors and job coaches address and help improve participants' soft skills so that they can function well in a non-CEO workplace. The daily routine of CEO's schedule, along with the network of supervision and support, can provide work readiness and stability during the critical period that follows a person's release from prison. The income and stability gained during the course of a person's involvement with CEO may lead to lower rates of recidivism. Through these experiences, CEO participants may be better equipped to gain and retain employment beyond the transitional jobs. If employed, participants may be less likely to return to — or resort to — criminal activities. In turn, reduced recidivism may result in better employment outcomes; in the most straightforward way, incarceration in jail or prison would keep a participant from being employed.

Methodology of the CEO Evaluation

The impacts of CEO's program model are being assessed using a random assignment research design. Between January 2004 and October 2005, a total of 977 ex-prisoners who were referred to CEO by their parole officer, and who reported to the program, and who met the study's eligibility criteria, were randomly assigned to one of two groups:

- **Program group (total = 568).** Individuals who were randomly assigned to this group were eligible for all of CEO's program services, including the preemployment class, placement in a transitional job, job coaching, additional services (such as parenting classes, job development services, and post-placement services.
- **Control group (total = 409).** Individuals who were randomly assigned to this group started with a revised version of the preemployment class that lasted one and a half days rather than four days, and they were given access to a resource room and a staff person who could assist them with many aspects of job search. Control group members also had access to other services in the community.

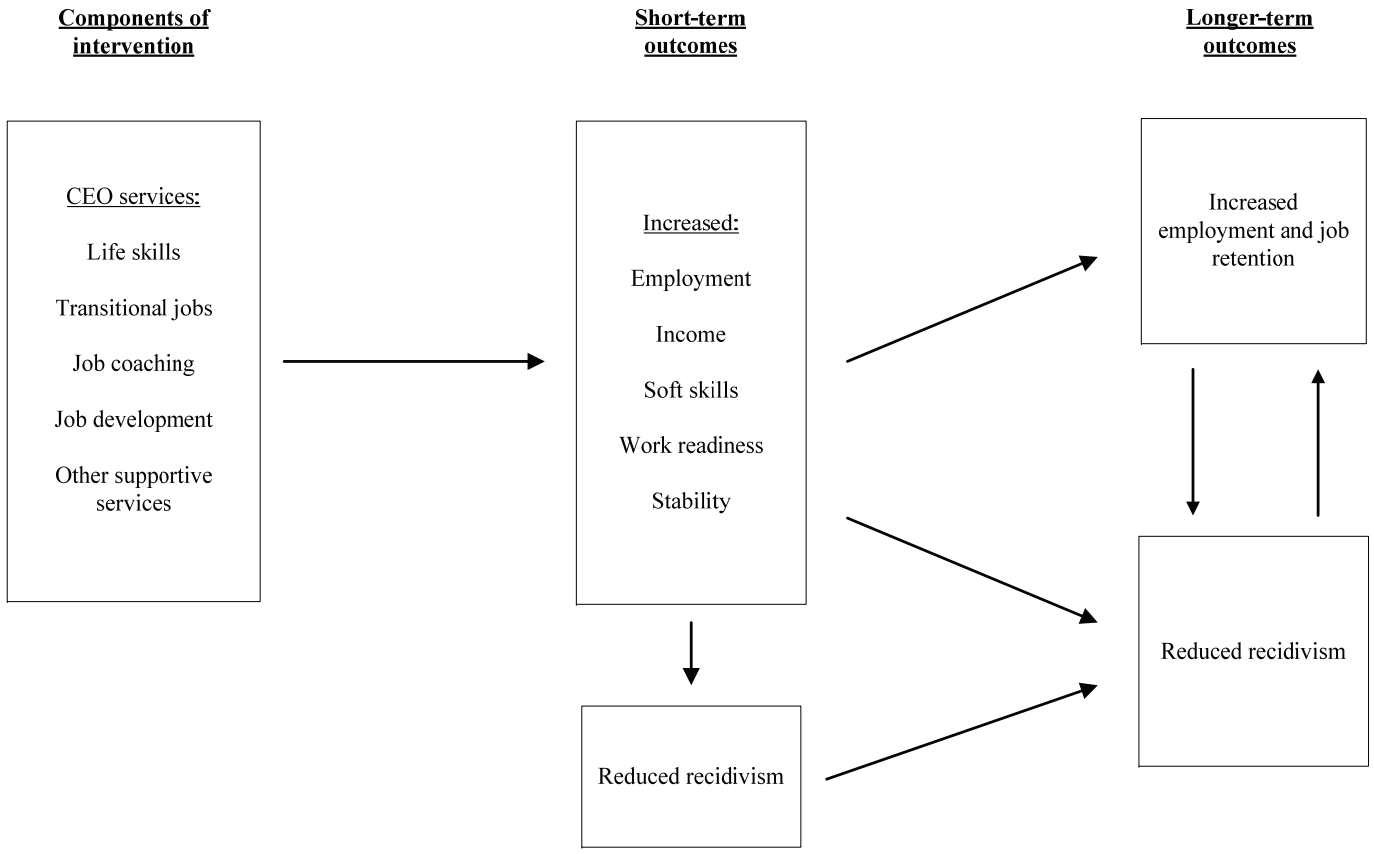
The research team tracked both groups for three years, using a number of data sources (described below). Because the study's participants were assigned to one group or the other through a random process, one can be confident that any significant differences that emerge

The Enhanced Services for the Hard-to-Employ Demonstration

Figure 1.1

Conceptual Model of CEO Program Outcomes

Center for Employment Opportunities



between the groups over time can be attributed to CEO. These differences are described as CEO's "impacts," or "effects."[17]

See Appendix A for further information on the research design and methodology.

The evaluation team worked with CEO to design a random assignment process that ensured that the study did not decrease the number of people who received transitional job slots and that ensured that CEO had enough participants to fulfill its contractual obligations on the transitional job work sites. Random assignment was conducted only during weeks when the number of new enrollees exceeded the number of available slots on the work sites. Since the proportion of people randomly assigned to the program and control groups varied from week to week, weights were used in the impact analysis to ensure that one group did not disproportionately affect the results. The varying sample sizes each week also meant that ultimately the sample sizes are not the same in the program and control groups in the overall study sample.

All impact estimates presented in this report are regression-adjusted using ordinary least squares, controlling for characteristics of sample members prior to random assignment. The impacts of CEO are examined for the full study sample and for key subgroups. Subgroups are based on characteristics of sample members at baseline — prior to random assignment — and are therefore consistent with the experimental research design of the study. Because the CEO model was intentionally designed to serve ex-prisoners immediately after release, and because most of CEO's broader population fit this profile, the impact results presented in this report are examined separately for the subgroup of people who came to CEO soon after release and those who came later.

Time Line and Data Sources Used in This Report

Data on each sample member were collected for three years after the date on which an individual was randomly assigned. Thus, the exact dates of the three-year follow-up period are different for every person. Figure 1.2 illustrates the random assignment and follow-up periods for the impact analysis of employment measures. As shown in the figure, Sample Member A was randomly assigned in February (Quarter 1) 2004; his follow-up period continues through the next 11 quarters and ends in Quarter 4 of 2006. The first year of the follow-up period indicates when he would have been directly involved in CEO's services, if he was a member of the program group. The final two years of follow-up are known as the postprogram period,

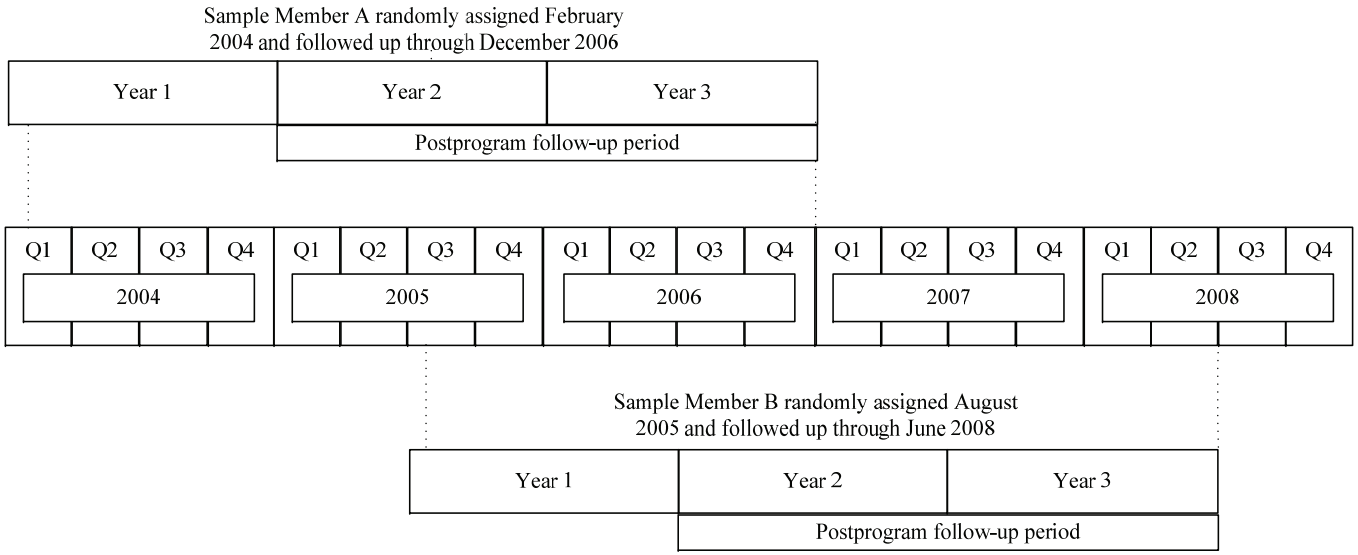
[17] Although not all members of the program group used CEO's services, the analyses presented throughout the report include the full program group in calculating the impacts.

The Enhanced Services for the Hard-to-Employ Demonstration

Figure 1.2

Random Assignment and Follow-Up Periods for Two Hypothetical Sample Members in the Impact Analysis

Center for Employment Opportunities



NOTE: This figure covers employment measures only, for which data are available quarterly. Thus, while random assignment occurs in a particular month, follow-up employment data are collected quarterly. The month in which each sample member is randomly assigned determines his first quarter of follow-up, whether it is the first, second, or third month in that quarter.

since participation in CEO services would have ended in the vast majority of cases. Sample Member B was randomly assigned in August (Quarter 3) 2005, and so his postprogram period began in August 2006, and his three-year follow-up period ended in Quarter 2 of 2008.[18]

The analysis in this report uses data on the study sample participants from a wide variety of sources:

- **Baseline data.** Participants completed a short baseline information sheet when they entered the study. Some additional baseline data were also obtained from CEO's management information system. These data provide a "snapshot" of each sample member at the point of random assignment.

- **State criminal justice data.** These data were provided by the New York State Division of Criminal Justice Services (DCJS), which compiles data from many city and state agencies. The data provide information on a range of outcomes — including arrests and convictions in New York State and incarceration in state prison — for each member of the study sample, both before and after study entry.

- **Local criminal justice data.** These data, provided by the New York City Department of Correction, show admissions and releases from New York City jails.

- **Employment data.** Data from the National Directory of New Hires (NDNH) and the New York State Department of Labor show employment in jobs covered by unemployment insurance (UI), including CEO transitional jobs.[19] These data cover the period from three years prior to study entry to three years after study entry for all sample members.[20]

[18]Because unemployment insurance (UI) data are available only as quarterly measures, employment measures include the quarter of random assignment plus the next 11 quarters. For the criminal justice measures, exact dates of each event are reported, so three-year outcomes include the month of random assignment plus the next 35 months. Therefore, for individuals who were randomly assigned in the second or third month of a quarter, the two periods do not exactly overlap. For example, if an individual was randomly assigned in March 2004, the first year of follow-up includes Quarter 1, 2004, through Quarter 4, 2004, while the Year 1 recidivism measures include March 2004 through February 2005.

[19]The NDNH database is maintained by the federal Office of Child Support Enforcement. It includes earnings data from all states.

[20]Due to NDNH data archiving rules, earnings data are not available for the first three quarters following random assignment; therefore, this report presents annual average earnings impacts for Years 2 and 3 of the follow-up period. Employment data are available for the full three-year period.

- **Program data.** These data, from CEO's management information and payroll systems, provide information on each individual's participation in the CEO components for which he was eligible, including transitional jobs and job coach and job developer appointments.

- **Survey data.** A survey was administered to 531 members of the program and control groups, approximately 16 to 23 months after people entered the study. The survey asked questions about service receipt, employment, housing, drug treatment, family relationships, and other issues. The response rate for the survey was about 68 percent.

- **Field research data.** Members of the research team visited CEO and its work sites on several occasions between 2004 and 2006, conducting structured interviews with staff and observing program activities. In addition, in-depth interviews were conducted with 19 members of the program group.

Following earlier publications on the study's interim results, this report presents the final results of the evaluation.[21]

Characteristics of the Study Participants

Table 1.1 presents the baseline characteristics of the program group, the control group, and both groups combined (the full sample). As expected in a random assignment design, there are very few significant differences in background characteristics between the two research groups. In addition, the characteristics of the CEO study sample are similar to the national population of individuals released from state prison to parole and similar to the parole population in New York City.[22]

The vast majority of sample members are male (93 percent).[23] Almost all are black or Hispanic. (Nationally, about one-third of those released to parole are white; in New York City, less than 10 percent of parolees are white.) Slightly more than 43 percent of the sample members were age 30 or younger at the time of random assignment, and the average age was 34. About one-third of New York City parolees are under age 30; nationally, the average age of prisoners being released in recent years is 34 years.

[21]Redcross et al. (2009); Bloom, Redcross, Zweig, and Azurdia (2007).

[22]National data are from the Bureau of Justice Statistics (2011a); state data are from the New York State Department of Corrections and Community Supervision (2009).

[23]There is a small but statistically significant difference between the two groups in the percentage of sample members who are male. The impact calculations presented in Chapters 2 and 3 are adjusted to account for this difference. Also, all results were examined for males separately, and the results are similar to those presented.

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Table 1.1

Selected Characteristics of Sample Members at the Time of Random Assignment, by Research Group

Center for Employment Opportunities

Characteristic	Program Group	Control Group	Full Sample	
Age (%)				
18 to 24 years	19.0	20.3	19.6	
25 to 30 years	23.8	23.7	23.8	
31 to 40 years	31.4	30.3	30.9	
41 years or older	25.7	25.7	25.7	
Average age (years)	33.7	33.7	33.7	
U.S. citizen (%)	74.6	73.6	74.2	
Race/ethnicity (%)				
White, non-Hispanic	1.4	2.2	1.8	
Black, non-Hispanic	64.3	64.5	64.4	
Hispanic	31.2	29.8	30.6	
Other	3.0	3.4	3.2	
Male (%)	91.4	95.3	93.0	**
Has any children under age 18 (%)	48.1	47.9	48.0	
Lives with any children under age 18 (%)	16.3	15.2	15.8	
Ordered to provide child support to a child under age 18 (%)	18.9	19.9	19.3	
Education (%)				
High school diploma	9.5	11.4	10.3	
General Educational Development (GED) certificate	42.6	43.9	43.1	
Beyond high school	4.8	3.5	4.3	
None of the above	43.1	41.2	42.3	
Housing status (%)				
Rents or owns home	16.6	19.9	18.0	
Lives with friends or relatives	59.1	55.1	57.4	
Lives in transitional housing	12.4	11.2	11.9	
Lives in emergency housing or is homeless	3.7	5.4	4.4	
Other	8.1	8.4	8.3	
Marital status (%)				
Married, living with spouse	8.1	9.3	8.6	
Married, living away from spouse	7.4	7.7	7.5	
Unmarried, living with partner	21.8	20.1	21.1	
Single	62.6	63.0	62.8	

(continued)

Table 1.1 (continued)

Characteristic	Program Group	Control Group	Full Sample
Ever any employment (%)	81.1	81.2	81.2
Employed 6 consecutive months for one employer (%)	59.9	62.7	61.1
UI-covered employment in the quarter prior to random assignment[a] (%)	14.9	11.7	13.6
UI-covered employment in the year prior to random assignment[a] (%)	24.1	24.0	24.0
Sample size	568	409	977

SOURCES: MDRC calculations using data from the CEO Baseline Information Form and unemployment insurance (UI) wage records from New York State.

NOTES: Data in this table are unweighted, but the results for the statistical significance test are weighted by week of random assignment.

In order to assess differences in characteristics across research groups, chi-square tests were used for categorical variables, and t-tests were used for continuous variables.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

[a]This measure was created using data from unemployment insurance (UI) wage records from New York State.

Before random assignment, about 16 percent of the sample were married, and 9 percent were living with their spouse. Another 21 percent were unmarried but living with a partner. About half of the study sample (48 percent) had at least one child under age 18, although only 16 percent were living with any of their children under that age. Only 19 percent reported having a formal child support order in place. Many sample members were living with friends or relatives (57 percent) or in some type of transitional housing (12 percent); 4 percent reported living in emergency housing or were homeless.

Just over half the sample had completed a high school diploma or a General Educational Development (GED) certificate, and a very small proportion (4 percent) had any postsecondary education. Most had at least some employment history. More than 80 percent reported that they had worked, and about 61 percent reported that they had worked for a single employer for six consecutive months.

In the year before study entry, 24 percent of the sample were employed in a UI-covered job. This means that at least some study participants did not come to CEO immediately after release from prison — an issue that is discussed further below.

Table 1.2 shows the criminal histories of study participants for both the program group and the control group before random assignment and including the offense for which they were recently released from prison.[24] Again, there are few differences between the groups. As expected, all the study participants had a history of arrest, conviction, and incarceration. The average lifetime total time spent in state prison for study participants was about five years.

Before random assignment, study participants had been arrested an average of eight times — similar to the 1994 national release cohort[25] — with about five felony arrests and three misdemeanor arrests. Study participants had been convicted an average of seven times, with about three of those being felony convictions. Nearly 70 percent of the sample members had prior arrests for violent offenses, and 51 percent were convicted of a violent offense. Nearly three-quarters of the sample had prior convictions for drug-related offenses.

Table 1.2 also shows the time between the last prison release and the date of random assignment. Toward the bottom of the table, the data show that only 41 percent of the study sample came to CEO within three months after release from prison; on average, sample members joined the study one year following their release. This is not typical of the full CEO population, about three-fourths of whom come to the program either immediately on release or shortly thereafter.[26] Because the CEO model was designed to serve ex-prisoners just after release, and because most of CEO's broader population fit this profile, the results presented in this report are examined separately for sample members who came to CEO soon after release and those who came later. As mentioned above, these groups were defined based on their characteristics before random assignment.

Appendix Table A.1 compares the demographic characteristics, employment history, and criminal records of these two groups.

[24]These data include incarceration in state prison but not local jails.

[25]Langan and Levin (2002).

[26]Parolees who were referred to CEO by their parole officer represent a subset of the parolees that CEO serves. See Appendix A for further details about the evaluation's eligibility criteria. However, CEO provides similar services to all parolees.

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Table 1.2

Criminal History of Sample Members at the Time of Random Assignment, by Research Group

Center for Employment Opportunities

Characteristic	Program Group	Control Group	Full Sample	
Arrest history				
Any prior arrests (%)	100.0	100.0	100.0	
Average number of arrests[a]	7.4	7.7	7.5	
Number of prior felony arrests	4.5	4.6	4.5	
Number of prior misdemeanor arrests	2.8	3.1	2.9	
Ever arrested for a violent crime (%)	67.5	67.5	67.5	
Number of prior arrests for a violent crime	1.4	1.4	1.4	
Conviction history				
Any prior conviction[b] (%)	100.0	100.0	100.0	
Average number of prior convictions[c]	6.6	6.9	6.7	
Number of prior felony convictions	2.7	2.5	2.6	***
Number of prior misdemeanor convictions	3.6	4.1	3.8	
Convicted of a violent crime (%)	51.7	50.9	51.4	
Convicted of a drug-related crime (%)	73.1	73.9	73.4	
State prison history				
Lifetime number of months in state prison[d]	60.6	59.1	60.0	
Months between latest state prison release and random assignment[e] (%)				
1-3 months	41.4	39.4	40.6	
4-6 months	14.7	13.5	14.2	
7-9 months	10.8	11.7	11.2	
More than 9 months	33.0	35.4	34.0	
Parole				
Months remaining on parole	34.2	32.9	33.6	**
Sample size[f]	561	409	970	

(continued)

Table 1.2 (continued)

SOURCE: MDRC calculations using data from the New York State Division of Criminal Justice Services (DCJS).

NOTES: T-tests were used to assess differences in characteristics across research groups.
Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.
Measures categorized as violent are based on an indicator provided by the DCJS.
[a]Each arrest date is counted only as a single event. If there are multiple crimes or charges on the same date, only the most serious charge is recorded in the analysis.
[b]This outcome excludes convictions where a final disposition was not found.
[c]Each conviction date is counted only as a single event. If there are multiple convictions on the same date, only the most serious charge is recorded in the analysis.
[d]"Lifetime" includes historical data as early as 1970.
[e]A total of 48 sample members are missing the latest prison release date and are excluded from this measure.
[f]Due to missing data, seven sample members are missing prior criminal histories.

Roadmap of the Report

The remainder of the report focuses on the three-year impacts of the CEO program and is organized as follows:

Chapter 2 describes the implementation of CEO's program during the study period.[27]

Chapter 3 describes the program's impacts on employment and recidivism.

Chapter 4 presents a benefit-cost analysis of CEO.

Chapter 5 discusses the policy implications of the study's findings and the next steps, based on this study and others in the field. Finally, the chapter gives an update on how the CEO program looks today.

[27]Chapter 2 summarizes findings from an earlier report on the implementation, two-year impacts, and costs of the CEO program (Redcross et al., 2009).

Chapter 2

CEO Program Implementation and Service Receipt

The New York City-based Center for Employment Opportunities (CEO) offers one the largest and most highly regarded employment programs for former prisoners who are returning to the community. This chapter describes the implementation of CEO's program model as it operated during the time frame of the study. Although the follow-up period for the evaluation covers roughly 2004 through 2008, most participants had stopped participating in CEO's services by the end of 2006. Throughout the time frame of the study, the program's service model was continuously refined and enhanced, but its core components remained the same and are described here.

CEO Program Flow

Typically, all CEO clients follow a single program path starting with intake sessions and leading to transitional employment and other services. Intake occurred every Friday during the study period. Participants came to the program with a referral from a parole officer, although most of the parolees in this study were not mandated through parole orders to participate in the CEO program. (All parolees, however, are expected to seek, obtain, and maintain employment as a condition of their parole.) Once participants signed in and completed the necessary paperwork, CEO staff accessed an MDRC database specifically designed to conduct random assignment for this study. Participants were randomly assigned either to the program group — representing CEO's usual service offerings — or to the control group.[1]

Immediately after random assignment, program and control group members started a preemployment life skills class specifically tailored for them. For program group members, the preemployment life skills class lasted five days, and, on completion, participants were placed in transitional job work sites and began working the following day. Once in transitional jobs, participants worked four days a week and, on the fifth day — considered their "appointment day" — came to the CEO program office to meet with job coaches and job developers and take part in specialized programming, such as the Responsible Fatherhood Program. Participants also had access to a staffed job search resource room that they could use during scheduled hours. Once a participant was considered "job ready," he or she began working with a job

[1]The random assignment system was designed to ensure that CEO could meet its obligations to work site sponsors and to ensure that the study did not reduce the number of people who received CEO's core services. Thus, random assignment was conducted only during weeks in which more people showed up than CEO could accommodate.

developer to find a permanent job. Participants could use their appointment day to schedule interviews for permanent employment. After the participant was placed into a permanent position, CEO kept in touch with the person for six months to one year, during which time participants were eligible for the Rapid Rewards incentive program (described below).

Control Group Services

The control group received a limited service package at CEO. This group also participated in a preemployment life skills class, which was of shorter duration (one and a half days) and did not culminate in a transitional job placement. The curriculum included topics related to securing identification documents needed for employment, job search strategies, and interview concepts, such as addressing how to answer questions related to criminal history. About 37 percent of control group members completed the class[2] — at which point they were given access to a job search resource room equipped with computers, fax machines, and phones with voice mail accounts for use in their job search. Control group participants had access to the room at different times during the day than program group members. During the initial months of the study, the control group was assigned a staff person who provided supports similar to case management. Later, the resource room was staffed by a rotating job coach who served as the "librarian" and was available to answer questions about the equipment and computers. Very few control group members visited the resource room more than two or three times.

Program Group Services

The Preemployment Life Skills Class

As noted above, typical participation in CEO begins with the preemployment life skills class. The class covers what to expect from the CEO program and its services. Specifically, participants hear about the program's rules and regulations, the nature of the work done at work sites, how to behave at the work sites, and what types of behaviors (such as unexcused absences) could lead to suspensions and discharges from the program. Clients sign forms agreeing to abide by CEO program rules. Class instructors also introduce CEO's Company Principles of Success, or "CEO CPs" — an abbreviation that is formed by the first letters of the five workplace behaviors described in the company philosophy: Cooperation with supervisors, Effort at work, On time, Cooperation with co-workers, and Personal presentation. The CEO CPs provide the framework for teaching participants soft skills that are thought to be helpful in making

[2]Some control group clients were disappointed about not being offered a transitional job; it is not clear whether this disappointment affected the group's dropout rate from the life skills class.

clients more employable and in helping them secure and retain permanent employment. The framework is introduced during class and is also emphasized at transitional job work sites.

The life skills class also addresses other common barriers to employment. For instance, former prisoners often do not have the proper identification needed to secure employment, such as Social Security cards. Additionally, participants spend some time in class learning job searching skills and how best to deal with employers' questions about criminal history on job applications and during interviews.

Transitional Jobs

CEO participants begin their transitional jobs after they finish the life skills class. They are assigned to small work crews, each with a supervisor who is employed by CEO. Participants work on work sites at city and state agencies, which obtain CEO's services through work orders.[3] The work is mostly maintenance, repair, and janitorial in nature. On a typical day, more than 200 CEO participants work at 30 or more work sites across New York City. Staff do not attempt to match participants with work in their field of interest but, instead, focus on fulfilling the contractual obligations to work sites. The two-year interim report from this evaluation provides more detail about the work sites.[4]

In addition to providing traditional supervision, CEO work site supervisors play an important coaching role at the sites by identifying and helping to address inappropriate workplace behavior. They also identify individuals who may require disciplinary action. At the beginning of the study period, supervisors were filling out paper evaluation forms in an effort to communicate issues to the office-based staff — such as job coaches — about participants' work site performance. Partway through the study period, in May 2005, CEO implemented the "Passport to Success." This evaluation system created a portable Passport booklet that participants are required to carry with them every day on the work sites. Work site supervisors complete evaluations directly in the Passport booklets. On days that participants are in the CEO office, the job coaches review the completed booklets with participants during their weekly meetings and provide feedback as appropriate, including discussing any issues or concerns.

Participants are paid each afternoon for their work that day. CEO provides daily pay as part of a day-labor model of transitional work. Daily pay is seen as a way of putting much-

[3]Agencies contract with parole for CEO's work crews. CEO operates as a managing agent for its work crews.

[4]See Redcross et al. (2009). In addition, a detailed work site report is available from the authors. Nine work sites were observed for this report. The work site analysis was led by Demetra Nightingale of the Johns Hopkins Institute for Policy Studies and John Trutko of Capital Research Corporation.

needed cash in hand during the critical period after release from prison. It is seen as a way to ease the day-to-day life of people recently released, because they do not have to wait several weeks for a paycheck after they start working. This, in turn, may offset the appeal of getting "quick money" through illegal activities.

Job Coaching

Hands-on job coaching is provided on-site to the transitional job work crew by work site supervisors (described above) and through office-based job coaches. Job coaches are assigned once participants complete the life skills class and begin working the transitional job. Generally, participants meet weekly with job coaches, on their appointment day.

During these one-on-one meetings, job coaches review individual participants' job performance using the Passport to Success booklet as a guide. They address any barriers to work that require additional support and offer referrals to other programs and services. Job coaches sometimes visit work sites to meet with participants. Job coaches also work with participants to develop a résumé, to build job-searching skills, and to prepare for job interviews (such as how to answer potential employers' questions related to criminal history). Job coaches typically began assessing participants' "job readiness" after two weeks in a transitional job, with the goal of moving people into permanent employment as soon as possible. Job readiness is based primarily on work site performance and presentation skills demonstrated during meetings with staff. However, participants who have a significant employment history may be considered job ready right away.

Job Development

CEO employs job developers to work with both CEO participants and employers to move people from transitional jobs into permanent jobs. Once participants are considered job ready, job developers begin meeting with them one-on-one. At the first meeting, the job developer completes an assessment in order to learn about the participant's interests, job skills, and work history. Although job developers work with participants directly, the majority of their time is spent developing employer accounts and identifying job opportunities for participants. As selling points to potential employers, job developers sometimes use the tax credits associated with hiring former prisoners along with bonding insurance, which protects employers from risks involved with such hiring. Under the federal bonding program, CEO pays for up to $10,000 of bonding insurance for the first six months of employment, which covers any financial losses related to employee dishonesty that might occur as a result of the hire. Staff report that small employers are sometimes interested in the tax credits and bonding but that larger employers are not as responsive to these inducements.

Once the job developers identify employer accounts and job openings, they try to match the positions with appropriate clients in their caseloads. If they cannot find a match from their own caseload, they consult with other developers to find a suitable job candidate. The job developers schedule interviews for those clients who seem appropriate for the position. Each job developer is responsible for specific employer accounts, but all the developers are expected to continually add new employers to their set of accounts. Each job developer is expected to meet a job placement quota each month.

The Responsible Fatherhood Program

CEO provides assistance with child support orders and offers other parenting programming for its participants. This service, known as the Responsible Fatherhood Program, is voluntary. The program offers assistance managing child support orders by working directly with the New York Child Support Enforcement agency. CEO staff arrange meetings with Child Support staff and are typically able to have support orders modified to $25 per month while fathers are in the program. These order changes are temporary, and there are no adjustments to arrears, which can be quite large.[5] The Responsible Fatherhood Program also offers parenting classes, which clients attend on their appointment day.

Postplacement Services

Early in the study's follow-up period, CEO's postplacement services focused mainly on keeping track of whether clients remained employed in the jobs into which they were placed. Initially, CEO's job coaches were responsible for contacting clients to confirm their employment. CEO also has a unit that is dedicated to verifying employment directly with employers. Partway through the study's enrollment period, CEO started an incentive program called "Rapid Rewards."[6] Through Rapid Rewards, clients who are placed in positions by CEO, as well as those who find jobs on their own, are eligible for incentives if they present their paystubs every month. At various retention milestones, people receive such rewards as store gift cards and mass transit passes. After one year of continuous employment, clients receive an additional $250 in Rapid Rewards payments. CEO also holds retention events twice per year for staff and clients to come together and celebrate employment successes. Late in the study period, specific staff, known as

[5]"Arrears" refers to back child support that is owed by the participant to the state or custodial parent. In many cases, noncustodial parents continue to accrue arrears while they are incarcerated, and the debt can be very large.

[6]Although 56 percent of the sample members were enrolled in the study after this change occurred, it affected only the 22 percent of participants who enrolled and who were placed in permanent employment positions.

retention specialists, began handling the Rapid Rewards program.[7] Most of CEO's enhanced postplacement services were not implemented in time to affect the research sample in this study.

Program Participation and Service Receipt

- **CEO's program operated smoothly during the study period, and most program group members received the core services.**

Table 2.1 presents data on program group members' participation in CEO's core activities. The participation data in the table, drawn from CEO's management information systems, cover all participation that occurred within three years of random assignment. Almost all program participation occurred in the months immediately after random assignment, though a small number of participants may have left the program and returned at a later time. Program tracking and payroll data show that 76 percent of the program group completed the initial five-day preemployment class and that about 71 percent worked in a CEO transitional job for at least one day. The average time spent in transitional employment was about nine weeks, which generally occurred over about four months of engagement with the program.[8] Among program group members who worked in a transitional job, about 91 percent also met with CEO job coaches or job developers at least once (not shown in the table). On average, participants who worked in a transitional job met about four times with each of these staff. About 44 percent of those who worked in a transitional job (31 percent of the full program group) were placed into permanent jobs, according to CEO's records.

- **The program group was more likely than the control group to receive specific kinds of employment services, but many control group members got help with job search at CEO or elsewhere; some control group members worked in transitional jobs at other organizations.**

As described above, CEO offered some help with basic job search assistance to control group members, but CEO's core program components — including transitional jobs and job development services — were offered only to program group members. In addition to the services that CEO offered to research sample members, it was expected that members from both

[7]Very few study sample members had any contact with a CEO retention specialist because this aspect of the program was implemented so late in the study period.

[8]It is important to note that "weeks worked" may not be consecutive and is not measured by calendar time. The variable is created by summing the total number of days worked in a CEO job after random assignment and dividing that by 4 because participants worked four days per week in CEO's transitional jobs at the time of the study.

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Table 2.1

Program Group's Participation in CEO Activities

Center for Employment Opportunities

Outcome	Program Group
Completed life skills/preemployment class (%)	76.0
Ever worked a CEO transitional job (%)	70.5
Weeks worked in a CEO transitional job[a] (%)	
Never worked in a CEO transitional job	29.5
Less than 1 week	5.2
1-4 weeks	18.0
5-12 weeks	28.0
13-24 weeks	15.1
More than 24 weeks	4.3
Days between random assignment date and start of CEO transitional job (%)	
Never worked in a CEO transitional job	29.5
0-7 days	47.5
8-14 days	15.3
More than 14 days	7.8
Met with a job coach (%)	59.4
Met with a job developer (%)	56.5
Placed in an unsubsidized job[b] (%)	31.3
Among those who worked in a CEO transitional job[c]	
Weeks worked in a CEO transtitional job[a] (%)	
Less than 1 week	7.3
1-4 weeks	25.6
5-12 weeks	39.7
13-24 weeks	21.4
More than 24 weeks	6.1
Average weeks worked in a CEO transitional job	8.9
Average number of meetings with job coach	4.4
Average number of meetings with job developer	4.0
Placed in an unsubsidized job[b] (%)	43.6
Program placement	34.8
Self-placement	11.7
Participated in fatherhood activity (among fathers) (%)	41.7
Sample size	568

(continued)

Table 2.1 (continued)

SOURCE: MDRC calculations from CEO's management information system (MIS).

NOTES: This table reflects program participation and CEO employment between January 2004 and October 2010. There were nine control group members (2.2 percent) who worked in CEO during the follow-up period. CEO outcomes for these control group members are not shown in the table.

[a]It is important to note that weeks worked may not be consecutive but includes a total of weeks worked after an individual's random assignment date. This variable is created by taking total days worked in CEO and dividing by 4 because participants work four days per week in CEO and attend job coaching or other CEO services on the fifth day.

[b]This includes unsubsidized employment placements by CEO staff and self-placement employment that CEO was made aware of or that the client reported to CEO.

[c]A total of 412 program group participants worked in a CEO transitional job.

research groups might seek out assistance from other organizations in the community. Thus, one would not necessarily expect to see large differences between groups in the receipt of basic job search assistance.

The client survey, which was administered to 531 sample members an average of 20 months after study entry, included many questions about the services that sample members received through CEO or other organizations. Table 2.2 shows the impacts on program participation and service receipt based on responses to the client survey. Not surprisingly, the program group was substantially more likely to receive some kinds of employment help, such as referrals to specific job openings — the kind of help provided by CEO job developers; 32 percent of the program group and 19 percent of the control group reported receiving such a referral. In other areas, however, such as advice about filling out job applications or résumé building, the differences between groups are much smaller because many control group members received these services from CEO or another organization. Although very few control group members worked in a transitional job at CEO, some worked in similar jobs at other organizations. It is difficult to determine precisely how many control group members worked in such jobs, however, because survey responses on this topic do not appear to be accurate.

In addition to providing employment services and transitional jobs to program group members, CEO staff provided support and guidance to address issues that could prevent clients from successfully reentering the community and the workforce. As shown in the bottom portion of Table 2.2, program group members were significantly more likely than the control group to report that they had received advice and support about things like how to behave on the job and

The Enhanced Services for the Hard-to-Employ Demonstration

Table 2.2

Impacts on Participation and Service Receipt

Center for Employment Opportunities

Outcome (%)	Program Group	Control Group	Difference (Impact)	P-Value
Participated in any job search, education, or training activity	67.9	64.3	3.6	0.400
Participated in a job search activity	60.6	58.9	1.6	0.709
Group job search/readiness	50.7	51.8	-1.2	0.798
Individual job search	31.2	21.3	9.9 **	0.012
Participated in an education/training activity	26.5	29.1	-2.6	0.515
Adult basic education/GED/ESL classes	10.4	10.2	0.1	0.962
College courses	4.4	4.7	-0.3	0.883
Vocational training	10.6	10.6	-0.1	0.977
Other	13.4	13.9	-0.5	0.867
Received employment services	82.5	78.9	3.6	0.295
Referral to specific job opening	32.3	18.8	13.5 ***	0.001
Advice about job interviews	64.6	56.3	8.4 *	0.054
Advice about discussing criminal history with potential employers	69.1	60.7	8.4 **	0.044
Advice about how to behave on a job	66.4	55.9	10.6 **	0.013
Names of people to contact about jobs	29.7	22.6	7.1 *	0.076
Help putting a résumé together	57.7	60.0	-2.3	0.611
Advice about filling out job applications	58.5	53.8	4.7	0.285
Can turn to someone on staff for advice and support with personal or family issues	40.5	30.8	9.7 **	0.024
Sample size (total = 531)	316	215		

SOURCE: MDRC calculations based on responses to the client survey. For more information about the survey and response rates, refer to the interim report (Redcross et al. 2009).

NOTES: Results in this table are weighted by week of random assignment and adjusted for pre-random assignment characteristics.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

GED = General Educational Development.

ESL = English as a Second Language.

about personal and family issues. This kind of support could have come from any type of CEO staff, but it likely occurred as part of the hands-on job coaching provided by work site supervisors while participants were on the work crew.

For additional results from the client survey, see the two-year report from this study.[9]

[9]Redcross et al. (2009).

Chapter 3

Impacts of CEO on Employment and Recidivism

Many former prisoners have low levels of education and little work experience, and the added burden of a criminal record can make finding employment even more challenging. The New York City-based Center for Employment Opportunities (CEO) program seeks to place participants in immediate, wage-paying transitional jobs that provide legitimate income during the critical period after release from prison.

This chapter discusses the CEO program's impacts on employment, earnings, and recidivism for the three years following random assignment, which occurred between January 2004 and October 2005. Through a combination of their work experiences at the program and the assistance provided by staff, participants may be better equipped to gain and retain employment. A steady income, daily routines, and connections to more positive social networks could help ease the transition into the community from prison and reduce the incentive to commit crimes.[1] The focus here is on CEO's impacts for the full study sample and for subgroups defined by the time between release from prison and reporting to CEO. (Box 3.1 explains how to read the impact tables in this report.)

Employment and earnings outcomes were estimated using unemployment insurance (UI) data from New York State and the National Directory of New Hires (NDNH). Using these data, it is possible to accurately estimate the proportion of this study's sample members who were employed in a UI-covered job for at least one day in each quarter during the three years following random assignment. CEO's transitional jobs are included in UI wage records, so overall employment measures include both transitional and unsubsidized employment.[2] Quarterly earnings data are from the NDNH and are available only for the second and third years of follow-up.

The recidivism analysis uses administrative data from the New York State Division of Criminal Justice Services (DCJS) to measure arrests, convictions, prison incarceration, and

[1]Bloom (2006).

[2]CEO jobs were identified in the UI data using its unique Federal Tax Identification Number, or FEIN. For the purposes of this analysis, any job that is not the CEO transitional job is considered "unsubsidized" in the analysis. It is possible that some jobs were transitional or subsidized jobs at other programs but are considered unsubsidized in this analysis. In New York City, there are several transitional jobs programs operating at a relatively large scale (for example, Ready, Willing and Able and Wildcat). Findings from the client survey also suggest that some control group members accessed transitional jobs elsewhere; about 16 percent of survey respondents reported working in a transitional job (Redcross et al., 2009).

Box 3.1

How to Read the Impact Tables in This Report

Most tables in this report use a similar format, illustrated below. Several recidivism outcomes are shown for the program group and for the control group. For example, the table shows that 48.1 percent of the program group and 52.8 percent of the control group were arrested in the three years following random assignment.

The "Difference" column in the table shows the differences between the two research groups' arrest rates — that is, the program's estimated impact on arrest. For example, the estimated impact on arrest can be calculated by subtracting 52.8 percent from 48.1 percent, yielding a difference of –4.7 percentage points.

Differences marked with asterisks are "statistically significant," meaning that it is quite unlikely that the differences arose by chance. The number of asterisks indicates whether the estimated impact is statistically significant at the 10 percent, 5 percent, or 1 percent level (the lower the level, the less likely that the impact is due to chance). One asterisk corresponds to the 10 percent level; two asterisks, the 5 percent level; and three asterisks, the 1 percent level. For example, as shown in the second row of data, the program group model had a statistically significant impact of –5.6 percentage points on convictions in the three years following random assignment. The p-value shows the exact levels of significance. This impact is statistically significant at the 10 percent level.

An examination of subgroup impacts requires an additional test of statistical significance to assess the magnitude of differences in impacts across subgroups. Therefore, an additional column, "Difference Between Subgroup Impacts," is included in the tables showing subgroup impacts. Similar to the asterisks in the table below, the daggers in this column correspond to the level of statistical significance. One dagger corresponds to the 10 percent level; two daggers, the 5 percent level; and three daggers, the 1 percent level. Whenever such differences are statistically significant, one can have greater confidence that the underlying impacts for the subgroups involved are actually different.

Three-Year Impacts on Recidivism

Outcome	Program Group	Control Group	Difference (Impact)	P-Value
Ever arrested (%)	48.1	52.8	-4.7	0.147
Ever convicted of a crime (%)	43.1	48.8	-5.6 *	0.078
Convicted of a felony	10.0	11.7	-1.6	0.419
Convicted of a misdemeanor	34.0	39.3	-5.4 *	0.083

SOURCE: MDRC calculations using data from the New York State Division of Criminal Justice Services (DCJS).

parole outcomes. Data from the New York City Department of Correction are used to measure jail incarceration.

Impacts for the Full Sample

Employment

- **CEO substantially increased employment early in the follow-up period, but the impact faded over time as program group members left the transitional jobs. CEO had no positive impact on unsubsidized employment for the full sample.**

Figure 3.1 shows CEO's impacts on overall employment, which includes both CEO's transitional jobs and all other UI-covered employment in New York State. The figure shows the employment rates for the program and control groups in each quarter following random assignment — that is, the proportion of each research group who worked for at least one day in a given quarter. CEO substantially increased employment early in the follow-up period, but the impact faded as program group members left the transitional job. There were no impacts on employment for the remainder of the three-year period. After the first year, employment rates for both research groups were low; only about 30 percent of sample members worked in each quarter.

Ultimately, CEO aimed to move participants out of transitional jobs and into regular, unsubsidized, employment. By differentiating between CEO and non-CEO jobs within the UI employment records, it was possible to estimate CEO's impacts on unsubsidized employment separate from CEO transitional jobs.[3] Figure 3.2 presents rates of unsubsidized employment for program and control group members in each quarter of the follow-up period. The figure shows that, overall, CEO had no impact on unsubsidized employment for the full study sample. In the first two quarters, control group members had slightly higher rates of unsubsidized employment than program group members. This substitution of the transitional job for regular employment occurred because some program group members accepted the offer of a transitional job when they could have found regular employment on their own. Working in the transitional job may have delayed their search for regular employment, whereas their control group counterparts quickly found employment on their own when they were not given access to the transitional job.

Table 3.1 presents CEO's impacts on additional measures of employment and earnings. The top panel of the table shows that most sample members worked at some point during the

[3]CEO's transitional jobs are reported in state UI records and in the NDNH federal data.

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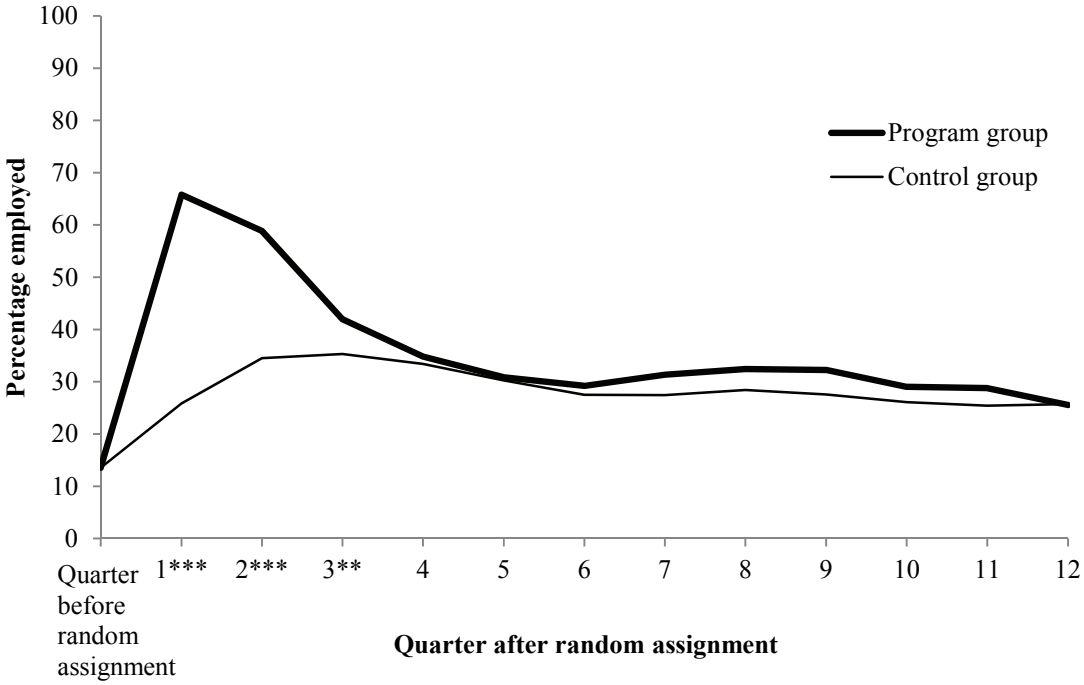
Figure 3.1

Quarterly Impacts on Overall Employment, Full Sample

Center for Employment Opportunities



SOURCE: MDRC calculations from unemployment insurance (UI) wage records from New York State.

NOTES: Results in this figure are weighted by week of random assignment and adjusted for pre-random assignment characteristics.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

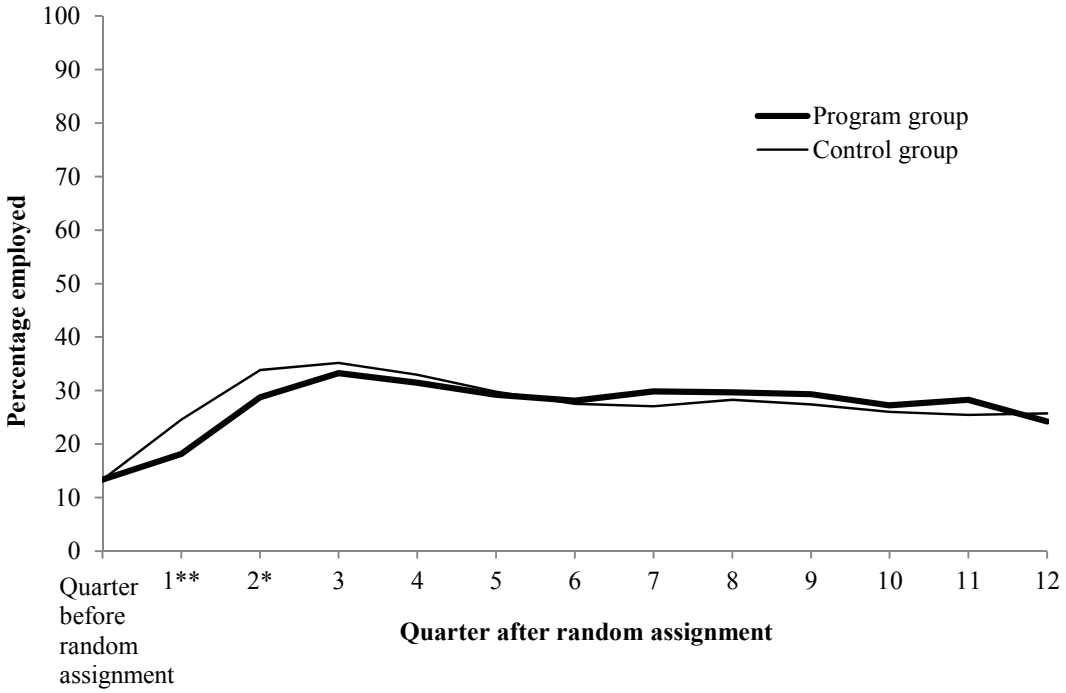
The sample size is 973. Four sample members are missing Social Security numbers and therefore could not be matched to employment data.

The Enhanced Services for the Hard-to-Employ Demonstration

Figure 3.2

Quarterly Impacts on Unsubsidized Employment, Full Sample

Center for Employment Opportunities



SOURCE: MDRC calculations from unemployment insurance (UI) wage records from New York State.

NOTES: Results in this figure are weighted by week of random assignment and adjusted for pre-random assignment characteristics.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

The sample size is 973. Four sample members are missing Social Security numbers and therefore could not be matched to employment data.

The Enhanced Services for the Hard-to-Employ Demonstration

Table 3.1

Impacts on Employment and Earnings

Center for Employment Opportunities

Outcome	Program Group	Control Group	Difference (Impact)		P-Value[a]
Employment (Years 1-3) (%)					
Ever any employment[b]	83.8	70.4	13.4	***	0.000
Ever worked in a CEO transitional job[c]	70.1	3.5	66.6	***	0.000
Ever worked in an unsubsidized job	63.7	69.0	-5.3	*	0.078
Postprogram unsubsidized employment outcomes (Years 2-3) (%)					
Ever worked in an unsubsidized job	53.3	52.1	1.2		0.713
Average quarterly unsubsidized employment	28.2	27.2	1.1		0.618
Number of quarters with unsubsidized employment					
None	46.7	47.9	-1.2		0.713
1 to 3	24.4	23.4	1.1		0.705
4 to 6	14.8	16.5	-1.7		0.470
7 to 8	14.0	12.2	1.8		0.404
Worked 6 or more consecutive quarters	14.7	11.9	2.8		0.195
Earnings (Years 2-3)[d] ($)					
Total UI-covered earnings[b]	10,435	9,846	589		0.658
CEO transitional job earnings	115	12	102	***	0.000
Unsubsidized earnings	10,320	9,834	486		0.715
Sample size (total = 973)[e]	564	409			

SOURCES: MDRC earnings calculations from the National Directory of New Hires (NDNH) database and employment calculations from the unemployment insurance (UI) wage records from New York State.

NOTES: Results in this table are weighted by week of random assignment and adjusted for pre-random assignment characteristics.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

[a]Standard errors are presented in this report for all impacts with a p-value of 0.000. Following are the standard errors for all impacts with a p-value of 0.000 (presented in the order in which they appear in the table): Employment: 2.615 and 2.394. Earnings: 27.297.

[b]These outcomes reflect only UI-covered employment and earnings.

[c]CEO transitional employment is estimated using data from NDNH and CEO's management information system (MIS).

[d]Due to missing earnings data for Year 1, this report includes impacts for only Years 2 and 3.

[e]Four sample members are missing Social Security numbers and therefore could not be matched to employment data.

three-year follow-up period. About 83 percent of program group members ever worked in a UI-covered job, compared with about 70 percent of control group members, for an impact of about 13 percentage points. This impact was driven by employment in CEO's transitional jobs.

The outcomes presented in the second panel of Table 3.1 show measures of unsubsidized employment, with a focus on measures of employment and employment stability after program group members left the CEO program, or the postprogram period. ("Postprogram" refers to Years 2 and 3 of the follow-up period and excludes the first year, when many program group members worked transitional jobs.) Only about 28 percent of each research group worked in an unsubsidized job in an average quarter, highlighting the extremely low levels of employment for the study sample in general. A distribution of the number of quarters with unsubsidized employment shows that the majority of the sample worked in no more than three of the eight postprogram quarters. Nearly half did not work at all in a UI-covered job during this time period. During the last two years of follow-up, only a small proportion of the sample worked in an unsubsidized job for six or more consecutive quarters, and that did not differ by research group.

The bottom panel of Table 3.1 shows CEO's impacts on postprogram earnings (covering Years 2 and 3 after random assignment).[4] Sample members earned about $10,000 over the two-year period. (This amount includes zeros for those who did not work.) CEO had no significant impact on UI-covered earnings in Years 2 and 3; the difference of $589 is not statistically significant. Because earnings data are not available for the first year of follow-up — when program group members had much higher rates of employment, owing to the transitional jobs — it is unclear whether CEO had any impact on earnings within that time period.

Recidivism

Table 3.2 presents CEO's impacts on measures of recidivism for the full study sample of 977 participants, all of whom were under parole supervision at the time they entered the study. The data provide a complete picture of convictions and incarcerations in both New York State prisons and New York City jails; arrests include only unsealed events.[5]

[4]This measure includes UI-covered earnings only for Quarters 5 through 12.

[5]In New York, arrests may be "sealed" — removed from a person's public record — for a number of reasons, including some lower-level misdemeanor convictions, arrests that do not end in a conviction, or some events for which "youthful offender status" is granted. The two-year interim report from this evaluation (Redcross et al., 2009) includes impacts on sealed and unsealed events. There were no impacts on overall arrests at the two-year follow-up.

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Table 3.2

Three-Year Impacts on Recidivism

Center for Employment Opportunities

Outcome	Program Group	Control Group	Difference (Impact)	P-Value
Ever arrested[a] (%)	48.1	52.8	-4.7	0.147
Ever convicted of a crime[b] (%)	43.1	48.8	-5.6 *	0.078
Convicted of a felony	10.0	11.7	-1.6	0.419
Convicted of a misdemeanor	34.0	39.3	-5.4 *	0.083
Ever incarcerated[c] (%)	58.1	65.0	-6.9 **	0.027
Prison	33.7	35.2	-1.5	0.626
Jail	56.6	63.0	-6.4 **	0.041
Ever incarcerated for a new crime (%)	23.7	28.0	-4.3	0.128
Prison	7.8	9.9	-2.1	0.249
Jail	16.9	19.9	-3.0	0.229
Ever incarcerated for a technical parole violation (%)	37.5	35.1	2.4	0.435
Prison	21.9	19.6	2.2	0.394
Jail	35.4	31.6	3.8	0.216
Total days incarcerated	173	187	-14	0.392
Prison	92	104	-13	0.273
Jail	81	82	-1	0.917
Ever arrested, convicted, or incarcerated[d] (%)	64.9	70.6	-5.7 *	0.060
Incarcerated at end of Year 3[e] (%)	25.4	30.0	-4.6	0.114
Sample size (total = 977)	568	409		

SOURCES: MDRC calculations using data from the New York State Division of Criminal Justice Services (DCJS) and the New York City Department of Correction (DOC).

NOTES: Results in this table are weighted by week of random assignment and adjusted for pre-random assignment characteristics.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

[a]Each arrest date is counted only as a single event. If there are multiple crimes or charges on the same date, only the most serious charge is recorded in the analysis.

[b]A total of 23 convictions were found to be associated with an arrest that occurred prior to random assignment. These convictions are counted in the analysis as occurring after random assignment.

[c]Includes all reasons for incarceration, such as sentences for new crimes, technical violations of parole, detainee (jail), and other admission reasons. A sample member may have multiple admissions; therefore, incarcerations for new crimes and parole violations do not sum to the percentage incarcerated.

[d]This composite measure was created by combining three measures that are not mutually exclusive: arrest, conviction, and incarceration. Participants who were arrested and/or convicted, for example, were also incarcerated. The composite measure represents people who experienced one or more of these recidivism measures.

[e]Incarceration status based on Quarter 12 after random assignment; includes both prison and jail.

The first row in Table 3.2 shows that about half the study sample were arrested one or more times within the three-year follow-up period; the difference of around 5 percentage points between the program and control groups is not quite statistically significant. The program group was 6 percentage points less likely than the control group to be convicted of a new crime (43 percent, compared with 49 percent). The majority of convictions found within the research sample were for misdemeanor-level offenses, and CEO's impacts on conviction were driven by reductions in misdemeanors. Most of the new convictions were for drug and property crimes (Appendix Table D.1).

Table 3.2 also shows that program group members were significantly less likely than control group members to be incarcerated in the three years following random assignment (58 percent, compared with 65 percent). Incarceration rates were markedly higher in jail than prison, and CEO's impact was driven mainly by reductions in jail incarceration.[6] More than a third of the sample were reincarcerated for a technical parole violation; there were no differences between the two research groups on this measure. There were no impacts on the number of days incarcerated over the follow-up period.

A summary measure of recidivism that combines arrests, convictions, and incarcerations shows that CEO reduced overall recidivism by approximately 6 percentage points. More than 71 percent of the control group and 65 percent of the program group experienced some form of recidivism in the three years following random assignment. The high rates of recidivism shown in Table 3.2 are consistent with what has been found in prior studies.

There is evidence that CEO's impacts on recidivism were largest in the first year after random assignment — in other words, while program group members were active in the program or shortly thereafter. The black bars in Figure 3.3 represent the proportions of program group members who experienced their first arrest, conviction, or incarceration after random assignment, in each given year.[7] The white bars represent the same rates for the control group. Rates of new recidivism were highest in the first year following random assignment: 45 percent of control group members experienced their first recidivism within one year after random assignment, compared with 39 percent of program group members. This finding is consistent

[6]Because those convicted of a misdemeanor usually receive a sentence to a jail or no additional incarceration, there is likely a close link between the impacts on misdemeanor convictions and jail incarcerations.

[7]The first recidivism event after random assignment was created by identifying the first occurrence of an arrest, conviction, or admission to prison or jail after the random assignment date. For example, an individual who experienced recidivism within the first year was coded as "yes" in the first year after random assignment but as "no" on this outcome for the second and third years. Similarly, an individual who experienced a first incident of recidivism in Year 2 was coded as "no" in Year 1 and "no" in Year 3 but as "yes" in Year 2.

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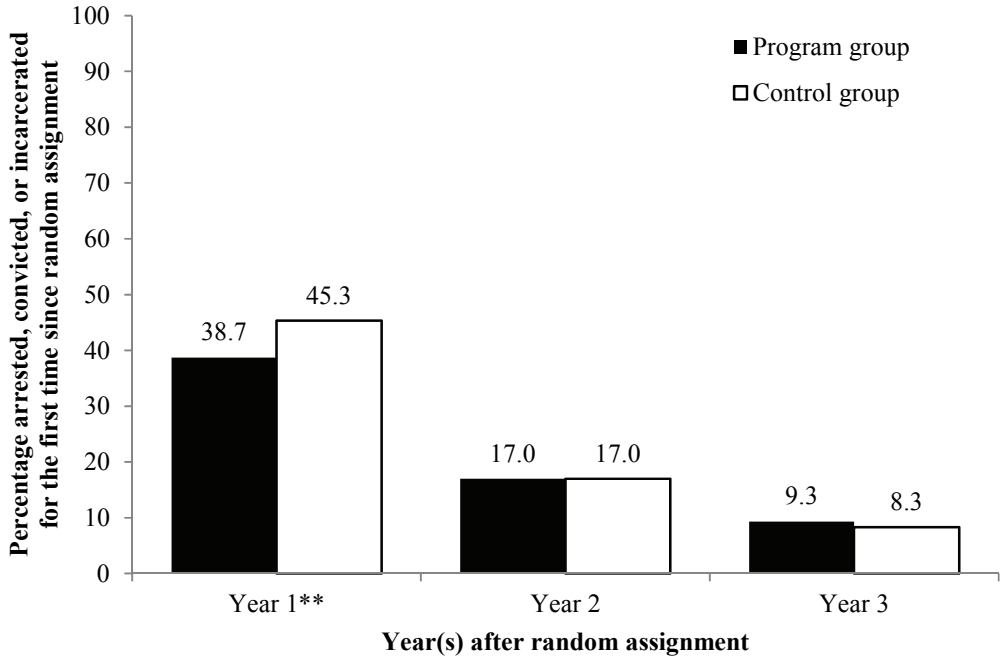
Figure 3.3

Impacts on First Incident of Recidivism After Random Assignment

Center for Employment Opportunities



SOURCES: MDRC calculations using data from the New York State Division of Criminal Justice Services (DCJS) and the New York City Department of Correction (DOC).

NOTES: Results in this figure are weighted by week of random assignment and adjusted for pre-random assignment characteristics.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

Sample members may have multiple arrests, convictions, or incarcerations in the follow-up period, but only the first incident of recidivism was used in the calculations for this table.

The sample size is 977.

with prior research, which has shown that rates of new recidivism are highest in the first year after release and that they decline steadily with each year clean.[8]

[8]Blumstein and Nakamura (2010). A good proportion of the full sample had been released from prison up to a year before random assignment; thus, the first year after random assignment does not correspond directly to the first year after release. Nonetheless, research has shown that rates of recidivism decline steadily over time, which is consistent with the findings from this evaluation.

In each of the two years that followed, similar percentages of the two research groups experienced their first recidivism (Figure 3.3). In Year 2, 17 percent (of each research group) had their first recidivism. By the last year of the follow-up period, less than 10 percent of the sample had been arrested, convicted of a new crime, or incarcerated for the first time since random assignment.

Additional recidivism outcomes and impacts for the full sample, by year, are presented in Appendix D.

Impacts for Subgroups Defined by Time Since Prison Release

CEO's program model is designed to provide immediate employment and income to ex-prisoners in the critical weeks following release. A large majority of CEO's overall client population comes to the program either immediately on release or shortly thereafter. As discussed in Chapter 1, however, a large proportion of the study sample members had been out of prison for some time before coming to CEO. Because the CEO model was intentionally designed to serve ex-prisoners immediately after release, and because most of CEO's broader population fits this profile, impact results are examined separately for the subgroup of people who came to CEO soon after release and those who came later. The "recently released" subgroup impact results may be more representative of CEO's impacts for its overall service population. More broadly, the "recently released" group better fits the usual definition of a "reentry" population, and the results for that group may help to test the widely held assumption that reentry programs are more effective if they begin working with ex-prisoners immediately after release.

There are several important considerations about interpreting subgroup impact findings in a random assignment study. First, the smaller sample sizes of the subgroups mean that impacts are less precise than they are for the full sample. Second, an examination of subgroup impacts requires an additional test of statistical significance to assess the magnitude of differences in impacts across subgroups. Third, some of the baseline data that define the subgroups were not available for small portions of the sample. For example, 48 people could not be placed into a subgroup category based on time since release from prison because they were missing the latest prison release date. One consequence of missing data is that the weighted average of the impacts for that subgroup is not always equal to the full-sample impacts.

Employment

The two line graphs presented in Figure 3.4 show that, among those who were recently released from prison (the top graph), CEO had positive impacts on unsubsidized employment in

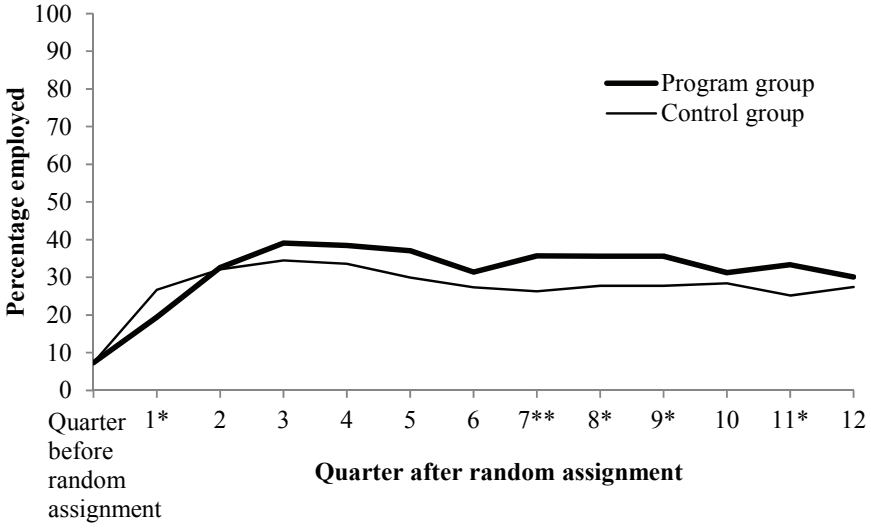
The Enhanced Services for the Hard-to-Employ Demonstration

Figure 3.4

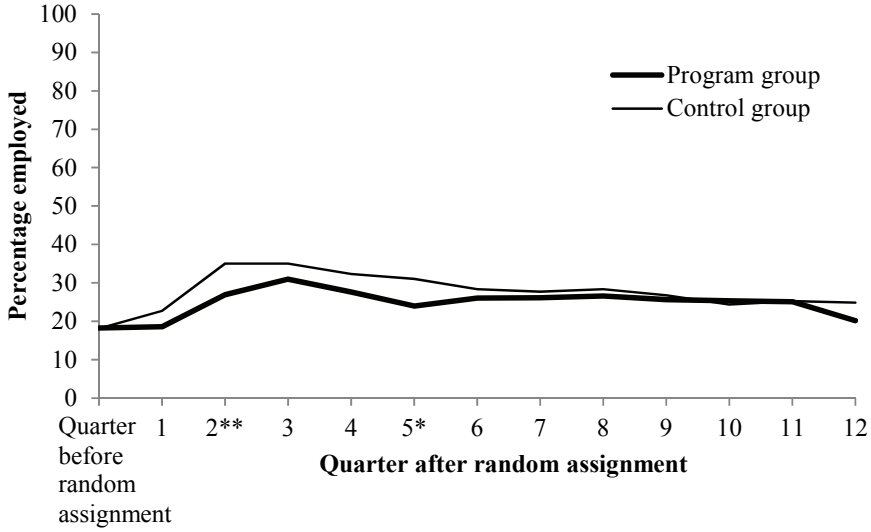
Quarterly Impacts on Unsubsidized Employment, by Time Between Prison Release and Random Assignment

Center for Employment Opportunities

Recently released subgroup



Not recently released subgroup



SOURCE: MDRC calculations from unemployment insurance (UI) wage records from New York State.
NOTES: Results in this figure are weighted by week of random assignment and adjusted for pre-random assignment characteristics.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

The sample size is 926. A total of 48 sample members are missing the latest prison release date prior to random assignment and are therefore missing from estimates in this table. An additional three sample members are missing Social Security numbers and therefore could not be matched to employment data.

several quarters late in the follow-up period. Among the subgroup that was further from release (the bottom graph), there are few differences between the research groups in working in an unsubsidized job throughout the follow-up period. These subgroup results should be interpreted with caution because the sample sizes are quite small and the impacts are not statistically different between the subgroups in some quarters. (Figure 3.4 does not show the significance levels of differences in impacts between the subgroups.)

Table 3.3 shows that CEO's effects on unsubsidized employment are somewhat different for the two subgroups. The second panel of the table shows that, among those who were recently released (the left-hand side of the table), CEO had positive impacts on some measures of postprogram unsubsidized employment. For example, on average, recently released program group members had more quarters with unsubsidized employment during the postprogram period than their control group counterparts (19 percent of program group members worked seven or eight quarters, compared with 11 percent of the control group). Program group members were also more likely than control group members to have six or more consecutive quarters with unsubsidized employment in the two years after they left the program.[9] It is unclear whether these jobs were for the same employer in every quarter. Among those who were further from release (the right-hand side of the table), CEO had no impacts on postprogram employment outcomes. The last row of the table shows that CEO had no statistically significant impact on unsubsidized earnings during the postprogram period.[10]

For the first year and a half following random assignment, CEO had little effect on unsubsidized employment outcomes. Yet, late in the second year and into the third year, statistically significant impacts on unsubsidized employment began to emerge among those who were recently released from prison. It is difficult to determine whether these impacts on unsubsidized employment were directly caused by the transitional job and CEO's other program services. Given the pattern of findings, two additional hypotheses seem plausible. First, evidence from this evaluation shows that CEO reduced recidivism and that these reductions were more consistent and pronounced for the recently released subgroup. Thus, CEO's impacts on unsubsidized employment during later quarters of the follow-up period may, in fact, be a secondary effect of the program's impact on recidivism. Specifically, program group members were more available for work than control group members because they were less

[9]The overwhelming majority of program group members had no participation in CEO in Years 2 and 3 after random assignment, so this period of time is considered postprogram.

[10]The difference of $1,100 is not statistically significant. Notably, the weighted average of the impacts for the subgroups is not equal to that for the full sample. This pattern occurs as a result of regression adjusting and has no effect on the basic impact finding for the earnings outcome. Even when impacts are run unadjusted, the differences do not rise to the level of statistical significance, and the main finding of no impact is unchanged.

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Table 3.3

Impacts on Employment and Earnings, by Time Between Prison Release and Random Assignment

Center for Employment Opportunities

Outcome	Length of Time Between Prison Release and Random Assignment: 3 Months or Less — Program Group	Control Group	Difference (Impact)	P-Value[a]	More Than 3 Months — Program Group	Control Group	Difference (Impact)	P-Value[a]	Difference Between Subgroup Impacts[b]
Employment (Years 1-3) (%)									
Ever any employment[c]	87.3	72.3	15.1 ***	0.000	82.0	69.1	12.9 ***	0.000	
Ever worked in a CEO transitional job[d]	73.5	-0.9	74.4 ***	0.000	68.3	5.2	63.1 ***	0.000	††
Ever worked in an unsubsidized job	68.9	71.4	-2.5	0.612	59.5	67.9	-8.4 **	0.037	
Postprogram unsubsidized employment outcomes (Years 2-3) (%)									
Ever worked in an unsubsidized job	58.3	54.6	3.7	0.472	47.9	51.7	-3.8	0.374	
Average quarterly unsubsidized employment	33.8	27.5	6.2 *	0.074	24.9	27.1	-2.2	0.444	†
Number of quarters with unsubsidized employment									
None	41.7	45.4	-3.7	0.472	52.1	48.3	3.8	0.374	
1 to 3	23.9	25.0	-1.1	0.818	21.6	23.1	-1.5	0.679	
4 to 6	15.5	18.2	-2.7	0.489	14.0	16.1	-2.1	0.502	
7 to 8	18.9	11.3	7.5 **	0.038	12.3	12.5	-0.2	0.954	†
Worked 6 or more consecutive quarters	17.9	12.0	5.9 *	0.086	14.3	10.6	3.7	0.199	
Earnings (Years 2-3)[e] ($)									
Total UI-covered earnings[c]	12,385	11,185	1,200	0.582	9,820	8,252	1,568	0.356	
CEO transitional job earnings	61	-6	67	0.121	159	18	141 ***	0.000	
Unsubsidized earnings	12,323	11,192	1,132	0.603	9,662	8,235	1,427	0.401	
Sample size (total = 926)[f]	223	160			310	233			

(continued)

Table 3.3 (continued)

SOURCES: MDRC earnings calculations from the National Directory of New Hires (NDNH) database and employment calculations from the unemployment insurance (UI) wage records from New York State.

NOTES: Results in this table are weighted by week of random assignment and adjusted for pre-random assignment characteristics.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

A small number of control group members accessed CEO's transitional jobs. Due to weighting and regression adjustments, CEO employment and earnings for the recently released control group are less than zero.

[a]Standard errors are presented in this report for all impacts with a p-value of 0.000. Following are the standard errors for all impacts with a p-value of 0.000 (presented in the order in which they appear in the table and beginning with the "3 Months or Less" subgroup): Employment: 4.131, 3.691, 3.571, and 3.301. Earnings: 37.772.

[b]When comparing impacts between two subgroups, an H-statistic is generated. The H-statistic is used to assess whether the difference in impacts between the subgroups is statistically significant. It is interpretable in much the same way as the t-statistic and the F-statistic from analysis of variance (ANOVA) tests are interpreted. Statistically significant differences across subgroups are indicated as: ††† = 1 percent; †† = 5 percent; † = 10 percent.

[c]These outcomes reflect only UI-covered employment and earnings.

[d]CEO transitional employment is estimated using data from NDNH and CEO's management information system (MIS).

[e]Due to missing earnings data for Year 1, this report includes impacts for only Years 2 and 3. The difference of around $1,100 is not statistically significant. Notably, the weighted average of the impacts for the subgroups is not equal to that of the full sample. This pattern occurs as a result of regression adjusting and has no effect on the basic impact finding for the earnings outcome. Even when impacts are run unadjusted, the differences do not rise to the level of statistical significance and the main finding of no impact is unchanged.

[f]A total of 48 sample members are missing the latest prison release date prior to random assignment and are therefore missing from estimates in this table. An additional three sample members are missing Social Security numbers and therefore could not be matched to employment data.

likely to be incarcerated or otherwise involved with the criminal justice system. A second hypothesis is that the same attitudinal and behavioral changes that led to CEO's effects on recidivism may also have led to better employment retention for some people. CEO's recidivism effects are discussed in more detail below.

Notably, there are no systematic differences in measured background characteristics or in CEO program participation between the subgroups defined by time since release from prison (Appendix Tables A.1 and B.1). Therefore, the differences in impacts that are observed for the subgroups cannot be explained by these factors.

Recidivism

- **CEO's impacts on recidivism are larger among those who enrolled in the program shortly after release from state prison. For that subgroup, CEO reduced arrests, convictions, and incarceration.**

The right-hand side of Table 3.4 illustrates a pattern suggesting that CEO's impacts on recidivism are concentrated among those recently released from prison. Within this group, program group members were 10 percentage points less likely than control group members to be arrested (49 percent, compared with 59 percent). Program group members were also significantly less likely to be convicted of a crime and were less likely to be incarcerated for a crime, with reductions of 13 and 11 percentage points, respectively (60 percent of program group members were incarcerated, compared with 71 percent of control group members). CEO reduced convictions for a violent crime, though few convictions after random assignment were for violent crimes (Appendix Table D.3). There were no statistically significant impacts on recidivism among those who entered the study more than three months after their release from prison. As shown in the rightmost column of the table, only some of the impacts are statistically different for the two subgroups. The sample sizes are small for the subgroups, making it difficult to detect a significant impact.

Similar to the results found in the full study sample (Figure 3.3 above), the impact of CEO was largest during the first year after random assignment. Among the recently released subgroup (the top graph in Figure 3.5), in Year 1 the program group was 12 percentage points less likely than the control group (47 percent, compared with 35 percent) to be arrested, convicted, or incarcerated for the first time since random assignment. The proportion of sample members who experienced their *first* recidivism event after random assignment in Years 2 or 3 is similar for the two subgroups.

As discussed above, this pattern suggests that CEO had its biggest impact on returning to criminal behavior while program group members were active at the CEO program or shortly after they left. Since impacts are concentrated among those recently released from prison, the

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Table 3.4

Three-Year Impacts on Recidivism, by Time Between Prison Release and Random Assignment

Center for Employment Opportunities

	Length of Time Between Prison Release and Random Assignment								Difference Between Subgroup Impacts[a]
	3 Months or Less				More Than 3 Months				
Outcome	Program Group	Control Group	Difference (Impact)	P-Value	Program Group	Control Group	Difference (Impact)	P-Value	
Ever arrested[b] (%)	49.1	59.1	-10.0 *	0.056	47.0	50.5	-3.5	0.420	
Ever convicted of a crime[c] (%)	44.0	56.7	-12.7 **	0.014	42.7	45.7	-3.0	0.493	
Convicted of a felony	15.6	14.6	1.0	0.789	6.8	10.2	-3.4	0.156	
Convicted of a misdemeanor	31.9	46.1	-14.3 ***	0.005	35.5	36.8	-1.3	0.764	††
Ever incarcerated[d] (%)	60.2	71.3	-11.2 **	0.027	57.1	63.2	-6.1	0.147	
Prison	38.9	43.4	-4.5	0.387	33.1	31.6	1.4	0.722	
Jail	56.7	71.4	-14.7 ***	0.004	56.7	60.0	-3.4	0.428	†
Ever incarcerated for a new crime (%)	26.5	35.4	-8.9 *	0.061	22.7	24.7	-2.0	0.593	
Prison	11.2	12.5	-1.4	0.685	5.9	8.8	-2.8	0.209	
Jail	16.4	25.6	-9.1 **	0.030	17.9	17.1	0.7	0.822	†
Ever incarcerated for a technical parole violation (%	38.5	39.8	-1.3	0.801	36.8	34.2	2.6	0.528	
Prison	23.8	25.5	-1.7	0.717	22.3	16.8	5.5	0.110	
Jail	34.2	35.1	-0.9	0.856	36.1	31.2	4.9	0.228	
Total days incarcerated	213	247	-34	0.234	154	151	3	0.872	
Prison	118	138	-20	0.345	82	85	-3	0.852	
Jail	95	109	-14	0.334	72	66	6	0.560	
Ever arrested, convicted, or incarcerated[e] (%)	66.8	75.8	-9.0 *	0.063	64.3	70.2	-5.9	0.148	
Incarcerated at the end of Year 3[f] (%)	30.1	36.1	-6.0	0.221	22.5	27.4	-4.9	0.195	
Sample size (total = 929)[g]	225	160			311	233			

(continued)

Table 3.4 (continued)

SOURCES: MDRC calculations using data from the New York State Division of Criminal Justice Services (DCJS) and the New York City Department of Correction (DOC).

NOTES: Results in this table are weighted by week of random assignment and adjusted for pre-random assignment characteristics.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

[a]When comparing impacts between two subgroups, an H-statistic is generated. The H-statistic is used to assess whether the difference in impacts between the subgroups is statistically significant. It is interpretable in much the same way as the t-statistic and the F-statistic from analysis of variance (ANOVA) tests are interpreted. Statistically significant differences across subgroups are indicated as: ††† = 1 percent; †† = 5 percent; † = 10 percent.

[b]Each arrest date is counted only as a single event. If there are multiple crimes or charges on the same date, only one the most serious charge is recorded in the analysis.

[c]A total of 23 convictions were found to be associated with an arrest that occurred prior to random assignment. These convictions are counted in the analysis as occurring after random assignment.

[d]Includes all reasons for incarceration, such as sentences for new crimes, technical violations of parole, detainee (jail), and other admission reasons. A sample member may have multiple admissions; therefore, incarcerations for new crimes and parole violations do not sum to the percentage incarcerated.

[e]This composite measure was created by combining three measures that are not mutually exclusive: arrest, conviction, and incarceration. Participants who were arrested and/or convicted, for example, were also incarcerated. The composite measure represents people who experienced one or more of these recidivism measures.

[f]Incarceration status based on Quarter 12 after random assignment. Incarceration includes both prison and jail.

[g]A total of 48 sample members are missing the last prison release date and are therefore missing from all outcomes in this table.

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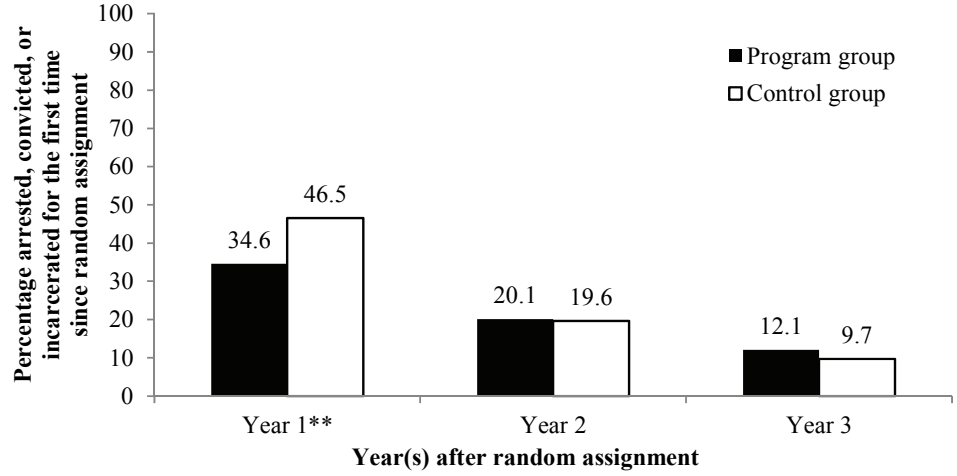
Figure 3.5

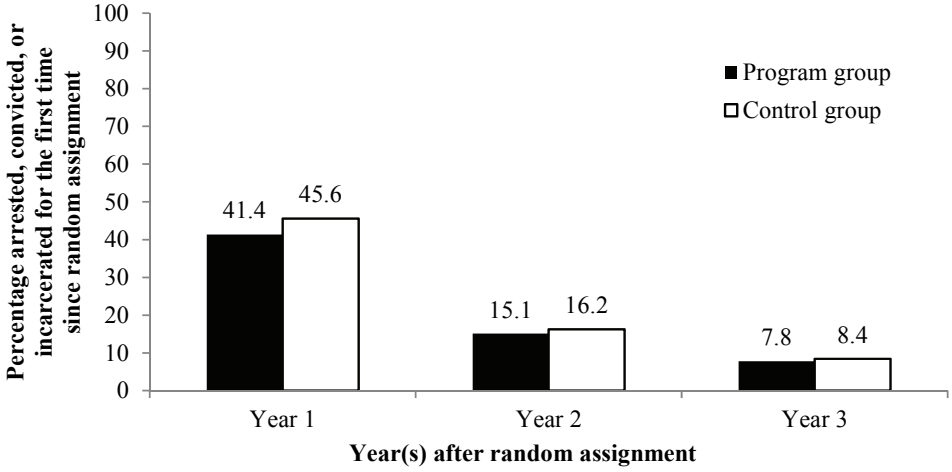
Impacts on First Incident of Recidivism After Random Assignment, by Time Between Prison Release and Random Assignment

Center for Employment Opportunities

Recently released subgroup



Not recently released subgroup



SOURCES: MDRC calculations using data from the New York State Division of Criminal Justice Services (DCJS) and the New York City Department of Correction (DOC).

NOTES: Results in this figure are weighted by week of random assignment and adjusted for pre-random assignment characteristics.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

Sample members may have multiple arrests, convictions, or incarcerations in the follow-up period, but only the first incident of recidivism was used in the calculations for this table.

The sample size is 929. A total of 48 sample members are missing the last prison release date and are therefore missing from all outcomes in this figure.

findings also reinforce the fact that the CEO program had its biggest impact during the critical period shortly after release. For the recently released subgroup, Year 1 after release corresponds closely to Year 1 after random assignment. This finding offers a possible explanation for CEO's persistent impacts on recidivism. (Appendix D presents impact results by year.) Prior research suggests that once someone avoids returning to crime in the first year after release from prison, it becomes much less likely that the person will commit future crimes.[11] It may be that CEO initially prevented some people from returning to crime while they were active in the program and that these same people continued to have lower recidivism in future years.

Impacts for Other Subgroups

The research team examined results for other subgroups based on literature suggesting that transitional jobs programs may affect certain subgroups differently. (These subgroups were defined on the basis of sample members' characteristics before random assignment.) For example, prior research has shown that employment programs may be more successful for older men coming out of prison.[12] To explore this question, subgroup impacts were measured for those who were younger than 29 years old and those who were 29 or older. Impact results were also examined separately for groups that may be more disadvantaged, such as those without a high school diploma or a General Educational Development (GED) certificate, those with a lengthier criminal history, and those at higher risk of recidivism. The risk index is based on age, number of prior convictions, and other factors.[13] The hypothesis for analyzing these subgroups is that those who are more disadvantaged or more at risk for recidivism may benefit more from other interventions.[14]

- **CEO's impacts were stronger for those who were more disadvantaged and those at highest risk of recidivism.**

CEO's impacts on employment, and especially recidivism, are generally more positive for individuals at higher risk or those who are more disadvantaged, such as those who have a lengthier criminal history, those without a high school credential, and those who are at higher risk of recidivism. There is some overlap between the subgroups. For example, many of those with four or more prior convictions are also categorized as having a high risk of recidivism. Table 3.5 compares the difference in impacts for those with three or fewer convictions prior to random assignment (the left-hand side of the table) and those with four or more prior convictions (the right-hand side). CEO's impacts on convictions were stronger for those with four or more prior convictions. Among those with a lengthier criminal history, program group members were significantly less likely than control group members to be convicted of a crime (49 percent, compared with 62 percent). There were no significant impacts on convictions for those with fewer prior convictions. Results for the remaining subgroups are shown in Appendix E.

[11]Blumstein and Nakamura (2010).

[12]Uggen (2000).

[13]A working paper from this evaluation (Zweig, Yahner, and Redcross, 2010) describes the method used to assess a sample member's level of risk. That analysis shows that CEO's impacts on recidivism were larger for those at "high" risk of recidivism when they entered the study.

[14]Bonta and Andrews (2007).

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Table 3.5

Three-Year Impacts on Employment and Recidivism, by Number of Prior Convictions at Baseline

Center for Employment Opportunities

Outcome	Prior Convictions at Baseline: 1-3 Convictions Program Group	1-3 Convictions Control Group	1-3 Convictions Difference (Impact)	1-3 Convictions P-Value[a]	4 or More Convictions Program Group	4 or More Convictions Control Group	4 or More Convictions Difference (Impact)	4 or More Convictions P-Value[a]	Difference Between Subgroup Impacts[b]
Employment outcomes (%)									
Ever any employment[c]	80.0	75.5	4.5	0.270	86.9	65.6	21.3 ***	0.000	†††
Ever worked in a CEO transitional job[d]	58.8	4.7	54.2 ***	0.000	74.3	2.4	71.9 ***	0.000	†††
Ever worked in an unsubsidized job	63.9	73.4	-9.6 **	0.036	64.2	65.0	-0.8	0.839	
Postprogram employment and earnings outcomes (Years 2-3)									
Average quarterly unsubsidized employment (%)	27.6	29.7	-2.1	0.540	28.8	24.7	4.1	0.155	
Total UI-covered earnings[e] ($)	11,576	10,135	1,441	0.546	9,770	9,581	188	0.908	
Recidivism outcomes									
Ever arrested[f] (%)	38.6	41.2	-2.6	0.608	54.5	62.9	-8.4 **	0.050	
Ever convicted of a crime[g] (%)	35.2	32.5	2.8	0.575	49.1	61.9	-12.8 ***	0.003	††
Convicted of a felony	11.3	8.3	3.0	0.323	9.2	15.1	-5.9 **	0.036	††
Convicted of a misdemeanor	23.3	22.8	0.5	0.912	42.0	52.0	-10.0 **	0.021	†
Incarcerated[h] (%)	47.7	61.0	-13.2 ***	0.006	65.0	71.0	-6.0	0.134	
Total days incarcerated	142	144	-2	0.914	196	227	-31	0.169	
Sample size (total = 951)	216	172			337	226			

(continued)

Table 3.5 (continued)

SOURCES: MDRC employment calculations using data from unemployment insurance (UI) wage records from New York State and earnings data from the National Directory of New Hires (NDNH) database. Incarceration calculations use data from the New York State Division of Criminal Justice Services (DCJS) and the New York City Department of Correction (DOC).

NOTES: Results in this table are weighted by week of random assignment and adjusted for pre-random assignment characteristics.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

[a]Standard errors are presented in this report for all impacts with a p-value of 0.000. Following are the standard errors for all impacts with a p-value of 0.000 (presented in the order in which they appear in the table and beginning with the "1-3 Convictions" subgroup): Employment: 3.951, 3.482, and 3.140.

[b]When comparing impacts between two subgroups, an H-statistic is generated. The H-statistic is used to assess whether the difference in impacts between the subgroups is statistically significant. It is interpretable in much the same way as the t-statistic and the F-statistic from analysis of variance (ANOVA) tests are interpreted. Statistically significant differences across subgroups are indicated as: ††† = 1 percent; †† = 5 percent; † = 10 percent.

[c]These outcomes reflect only UI-covered employment and earnings.

[d]CEO transitional employment is estimated from NDNH and CEO's management information system (MIS).

[e]Due to missing earnings data for Year 1, this report includes impacts for only Years 2 and 3.

[f]Each arrest date is counted only as a single event. If there are multiple crimes or charges on the same date, only one the most serious charge is recorded in the analysis.

[g]A total of 23 convictions were found to be associated with an arrest that occurred prior to random assignment. These convictions are counted in the analysis as occurring after random assignment.

[h]Includes all reasons for incarceration, such as sentences for new crimes, technical violations of parole, detainee (jail), and other admission reasons. A sample member may have multiple admissions; therefore, incarcerations for new crimes and parole violations do not sum to the percentage incarcerated.

Chapter 4

Benefit-Cost Analysis of the CEO Program

The preceding chapters discuss the implementation and impacts of the New York-based Center for Employment Opportunities (CEO), which places former prisoners who are returning to the community in immediate, wage-paying transitional jobs during the uncertain period after release. The evaluation results show that the CEO program was well implemented and that it increased employment early in the study's follow-up period and reduced recidivism during the three years following random assignment.[1]

This chapter presents the benefits and costs of those effects. It begins by discussing CEO's net costs, which were estimated by comparing the costs of services for program group members with the costs of providing services to the control group both within CEO and through outside organizations. It then presents the economic benefits of reduced recidivism and increased employment. It concludes with a comparison of the net benefits and costs of CEO to determine the cost-effectiveness of the program.

Summary of the Findings

- **For the full sample, the benefits of CEO outweighed the costs.** CEO's total benefits — from reduced criminal justice expenditures, reduced victimization costs, and increased employment — outweighed program costs by over $4,900 per program group member. Benefits for the full sample outweighed costs 2.1 to 1 from the taxpayer perspective and 2.4 to 1 from the combined perspectives of taxpayers, victims, and participants.

- **For the recently released subgroup, the benefits of CEO outweighed the costs by a larger margin than for the full sample.** CEO's total benefits outweighed program costs by about $10,300 per recently released program group member. Benefits for the recently released sample outweighed costs 3.3 to 1 from the taxpayer perspective and 3.9 to 1 from the combined perspectives of taxpayers, victims, and participants.

The above findings illustrate the cost-effectiveness of the CEO program. As with all benefit-cost analyses, these findings depend on a number of critical assumptions, such as the marginal cost values used for various components of the criminal justice system and uncertainty

[1]Random assignment occurred between January 2004 and October 2005.

around impact estimates. However, even when very conservative assumptions are used, the program remains cost-effective. (Under the presented scenarios, the overall benefit-cost ratio remains above 1.)

Methodology of the Benefit-Cost Analysis

The two-year interim report from this study presents the costs of operating CEO.[2] The analysis presented in this chapter expands on that earlier work by examining CEO's cost-effectiveness from the perspectives of taxpayers, victims, and participants. From the taxpayers' viewpoint, the key question is: If the public invests in CEO, will the program reduce crime and increase employment rates enough to exceed the cost of the investment? In other words, for every taxpayer dollar allocated to CEO now, will more than one taxpayer dollar be generated in the years ahead? The victims' perspective is also considered because CEO reduces recidivism rates, which means that there will be fewer crime victims. Finally, the economic benefits to participants are estimated.

The program's benefits and costs are presented per program group member — that is, the benefits and costs of CEO, over and above the benefits and costs that would have been incurred in the absence of the program. Benefits associated with impacts and all program costs are estimated by year for the three-year follow-up period of this analysis.[3] Impacts are monetized by year so that discount adjustments can be made to future benefits. As in Chapters 2 and 3 describing the program's impacts, all program and control group members — not just those who participated in CEO program activities — were included in calculating the benefits and costs of the program.

This analysis focuses on benefits and costs incurred for the full sample and for the subgroup of participants who entered CEO within three months of their release from prison. As mentioned above, this subgroup represents the majority of CEO's overall client population and also better fits the usual definition of a reentry population. Tables with detailed benefits and costs for the not recently released subgroup are provided in Appendix Table F.4.[4]

[2]See Redcross et al. (2009), Chapter 7.

[3]The analysis assumes no difference in recidivism beyond the three years because data are not available to accurately project the recidivism impact beyond that time period.

[4]Participation in CEO's transitional jobs and other services did not vary for the subgroups based on time between prison release and random assignment (Appendix Table B.1). Therefore, program costs do not differ for the subgroups and the full sample.

Estimating Program Costs

The CEO cost analysis measures the differences in expenditures for serving the program group and the control group. It compares the costs of CEO's services with the typical costs of employment services that control group members received from CEO and other organizations.[5]

The total cost of CEO for each program group member is made up of expenditures for CEO's program services, including (1) intake and recruitment, (2) expenses for transitional jobs, (3) the cost of other CEO services (job coaching, job development, and fatherhood services), and (4) the management information system and payroll functions.

The cost components for the control group include expenses for (1) CEO's limited job coaching and life skills services and (2) expenses incurred by outside agencies for providing employment and training services — and in some cases, transitional jobs — to control group members.[6]

The benefit-cost analysis incorporates the net cost of CEO — that is, the cost per program group member over and above the cost per control group member.[7]

Estimating the Benefits of Reduced Recidivism

The benefits of reduced recidivism have been estimated by comparing the program and control group costs related to the criminal justice system and to victims. Criminal justice costs include expenditures for operating New York City's police and jail services and New York State's prison and court systems.[8] The victim costs represent the monetary value of losses

[5]The direct cost of services provided by CEO and other organizations was estimated for 20 months, on average, following random assignment. There should be no difference between research groups in the direct cost of services beyond this period. This differs from the benefits, which were calculated based on three years of impacts because the impacts on key outcomes — or benefits — continued to accrue beyond the time when the cost of services accrued.

[6]A client survey provided information about the services received outside CEO.

[7]For the full cost analysis, see Redcross et al. (2009).

[8]Other components of the criminal justice system include parole and probation. This benefit-cost analysis does not include parole costs because of CEO's complex impact on parole costs. On the one hand, CEO reduces parole costs by reducing the number of people who go to prison. Because most prisoners are released on parole, CEO reduced the number of parolees, thus reducing parole costs. On the other hand, CEO increases parole costs by reducing the number of people who return to prison, thus increasing the number of people who stay on parole. Data are not available to measure how much longer CEO participants who are sent to prison would have stayed on parole. Therefore, this benefit-cost analysis assumes that CEO's positive and negative impacts on parole costs cancel each other out, and the parole costs are excluded from the analysis. In addition, probation costs are not estimated because all CEO participants are on parole, and if they commit a crime, they either stay on parole or return to jail/prison, but they are not placed on probation.

incurred by victims as a result of crimes committed by study participants. The cost difference between the program and control groups represents savings associated with participation in CEO.

To estimate criminal justice costs, the research team used marginal rather than average costs for each part of the system. Average costs equal the total cost of operation divided by the quantity of goods or services produced. This approach tends to overstate savings, as it fails to recognize that some costs are fixed and will not change as workload increases or decreases. In contrast, marginal costs describe how the cost of an operation changes as workload levels change.[9] At the most basic level, marginal cost is the change in total cost that arises when the quantity produced changes by one unit. As opposed to average cost, marginal cost recognizes that some costs are fixed. However, as workload changes become bigger in magnitude (for example, a 100-unit change instead of a 1-unit change), the appropriate marginal cost can change as costs that were once fixed start to change.

For example, if one fewer person is sent to jail, the corrections department operating the facility would be able to save on marginal costs for things like food, clothing, and some medical expenses, but staffing costs would remain the same. However, if 100 fewer people are sent to jail, the corrections department would save on food, clothing, medical, and *staffing associated with an entire housing area.* At this level (a 100-unit change), many costs still remain fixed — like rent, utilities, and executive management salaries — but the staffing associated with a housing area has become a marginal cost. This type of marginal cost may be more effectively described as a "step cost." Given a large enough change in workload, nearly every fixed cost will become a marginal cost; some steps are simply bigger than others.

This analysis assumes that CEO served nearly 2,500 clients — which is equal to the number of clients CEO serves per year. Changes in the operating scale of the CEO program would change the program benefits and costs appreciably. In order to determine the appropriate marginal cost of incarceration for this analysis, program impacts for the recently released subgroup were applied to 75 percent of the entire CEO program, and the program impacts for the not recently released subgroup were applied to 25 percent of the entire CEO program.[10] Extrapolating impacts from the study sample to the entire CEO program provided an estimate of CEO's overall impact. This calculation allowed the research team to select the most appropriate marginal cost assumptions for various criminal justice services. These marginal cost assumptions were maintained throughout the analysis; the assumed marginal costs change only in the sensitivity analysis at the end of this chapter.

[9] Aos, Miller, and Drake (2006).

[10] This breakdown was based on CEO data suggesting that 75 percent of all CEO clients — including clients who were not part of the study sample during the period of concern — enter the program within three months of release.

In order to determine the number of people who avoid involvement in each part of the criminal justice system due to CEO, key outcomes for the program and control groups were utilized. These outcomes included incidence-based measures, rather than the prevalence-based measures described in preceding chapters. Incidence-based measures indicate the number of arrests, convictions, and incarcerations, whereas prevalence results indicate the percentage of participants who were arrested or incarcerated. See Appendix Tables D.4 and D.5 for incidence impact results. To calculate CEO program benefits, the incidence-based outcome measures were multiplied by the appropriate marginal cost assumption. For example, arrest costs were multiplied by the number of prevented arrests; court costs were multiplied by the number of prevented court cases; jail costs were multiplied by the estimated number of jail days prevented; and prison costs were multiplied by the estimated number of prison days prevented. See Box 4.1 for further details on how outcomes and marginal costs were used to estimate CEO's net value.

It is important to note that savings are not calculated using actual time spent in jail or prison, as shown in the preceding impact tables. Looking at "total days incarcerated" over a three-year period fails to capture the full cost of many crimes committed during the three-year observation period. For example, a crime committed in the third year that is associated with 1,000 days in prison would fail to capture nearly two-thirds of the true cost. To overcome this shortfall in calculating jail and prison costs for program and control group members, conviction data, by type, are combined with statistics on the average length of stay, by charge.[11]

In addition to imposing costs on the criminal justice system, crime also inflicts costs on victims. Some victims incur direct out-of-pocket expenses, like medical costs and the value of stolen property. Others suffer physical injuries, endure psychological pain, or even lose their lives. As crime decreases, fewer people incur the costs associated with crime. Over the past few decades, researchers have developed methods to place a dollar value on the monetary and the nonmonetary costs of crime. One recent study by McCollister et al. uses the most current data available to estimate the victimization costs of 13 crime categories. To estimate the monetary costs, the study's authors apply the cost-of-illness approach, which measures medical expenses, cash loss, property theft or damage, and lost earnings that result from injury and other victimization-related consequences.[12]

However, like most other studies that estimate victimization costs, McCollister's study does not provide estimates for drug-related and some lower-level offenses. Yet crimes like simple assault and fraud often impose victim costs. Because the incidence of these crimes was high among the members of this study's program and control groups and because there was an

[11]From New York State Department of Correctional Services (2009).

[12]McCollister, French, and Fang (2010); also see Miller, Cohen, and Wiersema (1996).

Box 4.1

How to Convert Impacts to Costs

The table to the right shows the values that are used to conduct the benefit-cost analysis. Column A presents the four parts of the criminal justice system and the eight associated outcome measures that are monetized in the benefit-cost analysis. Columns B and C list the program and control group values for each of the eight outcome measures. Column D shows the marginal cost per outcome. A marginal cost is estimated for each component of the criminal justice system, and these costs remain the same for members of both the program and the control group. Columns E and F list the average lengths of stay in jail and prison per program and control group member. Column G shows the cost associated with each of the measures.

The average length of stay columns are not applicable (NA) to the cost of police or courts, so the following formula can be used to estimate the net present value (NPV) of these costs:

$$\text{NPV (Column G)} = (\text{Column B} \times \text{Column D}) - (\text{Column C} \times \text{Column D})$$

For incarceration measures, like jail and prison, the average length of stay columns are applicable. For some measures, such as a parole violation that results in jail time, the average length of stay is the same for program and control group members. However, for new crimes that result in jail and new crimes that result in prison, the average length of stay is different for program and control group members because of differences in the types of offenses that they committed and, therefore, differences in their sentences. The following formula is used to estimate the reduced cost of the six incarceration measures listed:

$$\text{NPV (Column G)} = (\text{Column B} \times \text{Column D} \times \text{Column E}) - (\text{Column C} \times \text{Column D} \times \text{Column F})$$

Following these formulas produces values similar to those shown in Column G, the net present values of the impacts. However, due to rounding and to 3 percent annual discounting, these formulas do not produce the exact values shown. Appendix Table F.2 presents the three-year program impacts for the recently released subgroup and the cost savings for the criminal justice system.

(continued)

Box 4.1 (continued)

Three-Year Program Impacts and Criminal Justice System Cost-Savings (in 2009 Dollars), Full Sample

A	B	C	D	E	F	G
				Average Length of Stay (Days)*		
Part of the System	Program Group	Control Group	Marginal Cost ($)	Program Group	Control Group	Net Present Value of Impact ($)†
Police (number of arrests)	1.058	1.177	359	NA	NA	42
Court (number of plea bargains)	0.764	0.861	884	NA	NA	84
Jail (number of admissions for a new crime)	0.297	0.312	72/day	38	40	75
Jail (number of admissions as a detainee)	0.586	0.726	72/day	50	50	478
Jail (number of admissions for a parole violation)	0.483	0.432	72/day	29	29	-99
Prisons (number of admissions for a new crime)	0.078	0.099	129/day	1,032	978	2,279
Prisons (number of admissions for a parole violation)	0.281	0.268	129/day	222	222	-304
Prisons (number of admissions for other reasons)	0.073	0.086	129/day	222	222	357
Total criminal justice cost savings						2,912

SOURCES: Marginal costs were estimated using information provided by New York City and state criminal justice agencies, New York State executive budget agencies, and Taifa (1995). See Appendix Table F.1 for more detail. For additional criminal justice data sources, see Table 3.2.

NOTES *The average length of stay in jail for a new crime is estimated by looking at the types of crimes sample members were charged with, then tying each crime type with an observed average length of stay derived from an analysis conducted by the Vera Institute's Substance Abuse and Mental Health Program using data from the New York City Department of Correction (DOC).

The average length of stay in prison for a new crime is estimated by looking at the types of crimes sample members were charged with, then tying each crime type with the average length of stay presented in "Statistical Overview: Year 2008 Discharges" published by the New York State Department of Correctional Services (DCJS).

†The net present value of the impacts is calculated using yearly impacts that are adjusted with a 3 percent annual discount rate. Due to rounding and discounting, it is not possible to derive the exact net present value of each impact with only this table.

impact on these types of crimes, it was necessary to estimate the victim costs of these crimes. This study assigned these crimes a cost estimate of $480, which is the amount McCollister's study provides for the least expensive crime category, theft/larceny. Victim costs for drug crimes are also not estimated because they are considered victimless due to their consensual nature: offenders typically sell unlawful substances to willing buyers.[13] Given that drug crimes often impose medical and other costs on victims, the results of the analysis represent conservative estimates of CEO's impact on victimization costs.

The authors computed the victim benefits of CEO using the reconviction rates from the impact analysis and the monetary victim costs estimated in McCollister's study. The general approach was to multiply the victim costs associated with different offenses by the number of crimes for those offenses that CEO prevented.[14]

Estimating the Benefits of Increased Employment

The benefits of increased employment were estimated by comparing employment rates, earnings, and the value of transitional jobs for the program group with the employment and earnings of the control group. Earnings estimates were based on administrative data from the National Directory of New Hires that were collected for the impact analysis. Earnings data are available only for the second and third years of follow-up for all sample members and, thus, exclude most earnings from CEO's transitional jobs.

Because CEO led to higher earnings for the program group compared with the control group, taxpayers may have benefited from increases in federal and state income taxes paid by participants.[15] The benefit-cost analysis includes estimates of income taxes paid by participants net of the Earned Income Tax Credit (EITC). Income taxes and the federal and state EITC are based on tax rules for filing year 2009. The analysis assumes single filing status for everyone (few participants lived with their children); therefore, no deductions or child tax credits were applied. All estimates are discounted and inflation-adjusted to 2009 dollars.

The value of the services that CEO participants provided to government agencies was estimated using the compensation that these agencies would have had to pay other workers in the absence of CEO — in other words, the market value. Estimates of the market value of the

[13]See Rubin (1999).

[14]To estimate the number of crimes that the program avoided, the number of convictions (which is generally fewer than the actual number of crimes) was multiplied by the proportion of crime reports filed in New York State compared with the number of convictions in New York State.

[15]Earnings impacts are not statistically significant; however, they represent the best available estimate of these outcomes.

transitional job labor were based on information provided by a large New York City agency that uses CEO work crews. According to this agency, the quality of the CEO participants' labor was comparable to, if not better than, the quality of other workers' labor, possibly because of the intensive supervision that CEO participants receive. In other words, although CEO's participants are likely to be less experienced than other workers, it is also likely that they receive much more intensive supervision from CEO's work site supervisors than other workers who would perform similar tasks. Thus, it is reasonable in this analysis to take the approach of using the wages of other workers to infer the value of CEO participants' work.[16]

Comparing Program Benefits and Costs

In order to demonstrate the cost-effectiveness of the CEO program, this analysis calculates the net present value (NPV) of program benefits and costs and benefit-cost ratios for the full sample and for the recently released subgroup. The NPV is presented per CEO program group member. Both NPV and benefit-cost ratios are calculated from each perspective: taxpayer, victim, participant, and total.

What is net present value?

"Net" means that the amounts represent differences between the program and the control groups, just as impacts do. "Present value" is an accounting method for estimating the worth today of dollar effects that occur in the future. In a program such as CEO, most costs are incurred early on, while many benefits (for example, avoided criminal justice and victim costs) are realized in later years. However, simply comparing the nominal dollar value of program benefits and costs would be problematic. The value of a dollar is greater in the present than in the future: a dollar available today can be invested and can produce income over time, making it worth more than a dollar available in the future. Thus, to make a fair comparison between benefits and costs, it is essential to focus on their value at a common point in time — that is, in the present. This benefit-cost analysis discounts all future benefits and costs using a 3 percent discount rate, and it presents all findings in 2009 dollars.[17]

What is a benefit-cost ratio?

A benefit-cost ratio divides the net program benefits by the net program costs. This can be done for the full sample or at a subgroup level. The ratio indicates how much value CEO

[16]The market value approach has also been used in similar studies as a way to assign a value to the labor output of program participants (Kemper, Long, and Thornton, 1981).

[17]Because future benefits occur within several years and current inflation is minimal, most benefits are not adjusted for inflation.

generates for every dollar invested in the program. Ratios above 1 are desired, as they indicate that the program is providing a positive return on investment, meaning that the program is cost-effective.

To conduct this analysis and compute the NPV and benefit-cost ratios, the authors developed an Excel model that combines recidivism and employment data from the impact analysis; marginal costs, by component of the criminal justice system; length-of-stay information, by type of crime; and discount rates.

Data Sources

This benefit-cost analysis draws on a wide variety of sources, including CEO's financial documents, budget documents from New York City and New York State criminal justice agencies, and research literature on victimization costs. The cost of CEO was estimated using CEO's detailed financial expenditure reports and in-depth interviews with program fiscal staff. Marginal costs of criminal justice agencies were estimated using information provided by the city and state criminal justice agencies, most of which measure marginal costs in order to forecast and allocate funding. The authors also contacted staff at the city and state budget agencies to ensure the accuracy of the estimates. The average lengths of stay in jail and prisons for various offense categories were calculated using data provided by New York City and New York State correctional agencies. Victim costs were estimated using the recent study cited above.[18] Earnings estimates were based on administrative data collected from the National Directory of New Hires for the impact analysis. Income taxes and the federal and state EITCs were estimated based on tax rules for filing year 2009. (Appendix Table F.1 provides a detailed description of the data sources used in the benefit-cost analysis.)

Limitations of the Analysis

Some limitations of the benefit-cost analysis should be recognized. First, the criminal justice cost savings presented in this report describe the potential savings that government agencies can realize if they downsize their capacities in response to decreased workload. This is particularly true in the case of New York State prisons, which are already operating under capacity: the maximum savings from CEO will be generated only if the state closes some prisons.[19]

[18]McCollister, French, and Fang (2010).
[19]New York State Department of Corrections and Community Supervision (2008).

Second, while this analysis incorporates the best available estimates of victim costs for several offense categories, it does not include victim costs for drug offenses, for which CEO has had the most impact. To date, victim costs for drug offenses have not been estimated. As a result, this analysis may underestimate the magnitude of CEO's impact on victim costs.

Third, this benefit-cost analysis measures only the program effects that are easily monetized. Not included in the analysis are such effects as the possible displacement of other workers in the full-time labor market, losses of leisure time, changes in quality of life, increased satisfaction on the part of the general public as a result of more employment among formerly incarcerated individuals, enhanced communities where program group members live, and any other effects that are not easily monetized.[20] Excluding these factors could increase or decrease the net gains from CEO.

Finally, the benefit-cost analysis uses the impact — the average difference between program and control groups — of CEO on key outcomes, whether or not that difference is statistically significant in the impact analysis. These values are used because they are nonetheless the best estimate of the actual value of these outcomes available. If one assumes that an impact that is not statistically significant is actually zero (meaning no difference between program and control groups), then the corresponding monetary benefits would also be zero, thereby reducing the value of the benefit (or cost) in this analysis. Box 4.2 provides a more detailed explanation.

[20]Because CEO increased employment, program group members correspondingly had less time for nonmarket activities, or "leisure time." Leisure time is difficult to value because many factors can affect the value that an individual places on it. Some of these factors include the number of work hours that an individual desires, the value of the "reservation wage" (the wage that an individual requires before being willing to work at all), and the wage available to an individual in the labor market — with and without CEO program services. Losses of leisure time have no effect on government budgets, and yet they are a cost to program group members. Thus, if the value of leisure time were taken into account, it is certain that the net benefit of CEO would be reduced, because lost leisure time would reduce the benefit to program group members and would have no effect on the government budget. For further discussion of estimating the value of leisure time, see Greenberg and Robins (2008).

Box 4.2

How Uncertainty in Impact Results Affects the Benefit-Cost Analysis

The benefit-cost results presented in this chapter are based on the impact evaluation described in the preceding chapters, and any uncertainty in the impact estimates leads to uncertainty in the benefit-cost results. The incidence-based outcomes and impacts that are monetized in this benefit-cost analysis are presented in Appendix Tables D.4 and D.5. The abbreviated table below shows the incidence of incarceration in prison for a new crime for program and control group members in the recently released subgroup.

The row labeled "Year 1" shows that the program's estimated impact on prison incarceration for new crimes during the first year is –0.044. The three asterisks indicate that this difference is statistically significant at the 1 percent level, meaning that there is less than a 1 percent chance of observing a difference this large if the program's true impact is zero; that is, it is very unlikely to observe an impact this large if the program's true impact is zero.

The statistical significance is calculated using the standard error of the impact estimate, shown in the rightmost column. The standard error is a measure of uncertainty or variability around the impact estimate. For example, in the row labeled "Year 1" below, there is roughly a 99 percent chance that the program's impact on the average total number of prison incarcerations for new crimes lies between –0.003 and –0.085, calculated as $-0.044 \pm (2.58 \times 0.016)$. The impacts in Years 2 and 3 are not statistically significant, meaning that there is a greater level of uncertainty in these outcomes.

The impact estimate of each outcome has a corresponding standard error. The larger the standard error, the greater the amount of uncertainty around the impact estimate. Standard errors for all impact estimates used in the benefit-cost calculations are shown in Appendix Tables D.4 and D.5. Because the benefit-cost analysis is based on outcomes with varying levels of uncertainty, the benefit-cost results involve some uncertainty as well. In other words, the actual benefits and costs may be higher or lower than the estimates.

Outcome	Program Group	Control Group	Difference (Impact)	Standard Error
Prison incarceration measures				
Number of admissions for a new crime	0.112	0.125	-0.014	0.034
Year 1	0.006	0.049	-0.044 ***	0.016
Year 2	0.067	0.032	0.034	0.024
Year 3	0.039	0.044	-0.004	0.022

SOURCE: MDRC calculations using data from the New York State Division of Criminal Justice Services (DCJS).

Benefits and Costs of CEO

Program Group Costs

CEO's operating costs cover expenditures for the average program group member and have been estimated for all the main program activities or functions. These costs include all staff salaries, fringe benefits, overhead, and administration costs for the activity. The costs of providing CEO services for the subgroups, defined by time since release from prison, did not differ from the average cost for the full sample because program participation did not differ for those groups (Appendix Table B.1).

- **The net cost of providing CEO's services was approximately $4,800 per program group member, including $1,000 in direct payments to participants.[21]**

As Table 4.1 indicates, the single largest expense ($1,500) was for field and site supervision of the participants on the transitional job work sites. Field and work site supervisors make up the largest proportion of CEO's staff and play a critical role in managing the work of participants. The second-largest expense was for participants' wages, which averaged $1,000 per program group member.

Taken together, other services provided to participants were estimated to cost nearly $1,600 per program group member. Most of this cost was for job coaching ($630) and job development activities (about $550). Program group members were given about $80 worth of supportive service payments and incentives (for example, transportation and clothing). Some program members also participated in CEO's fatherhood program, which cost about $290 per program member.[22]

Finally, the management information system was estimated to cost $410 per program group member. This system supports detailed tracking of participation in the various activities as well as an efficient payroll system that enables CEO to pay its participants on a daily basis for their work in the transitional jobs.

[21]Redcross et al. (2009) states that the total cost of CEO was $4,263 per participant. These costs were inflation-adjusted and discounted using a 3 percent annual interest (3 percent discount rate and zero percent inflation) compounded monthly. Therefore, the adjusted cost per participant in this study is $4,807.

[22]As with all CEO costs, program group member costs are presented as an average, spread across participants and nonparticipants. For more information about program cost estimates, see Redcross et al. (2009).

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Table 4.1

Estimated Costs of CEO (in 2009 Dollars)

Center for Employment Opportunities

Cost Component	Gross Cost per Program Group Member ($)	Gross Cost per Control Group Member ($)	Net Cost per Program Group Member ($)
CEO services			
Recruitment/intake	180	180	0
Expenses for transitional jobs			
Transitional job wages	1,000	0	1,000
Transitional job fringe benefits[a]	167	0	167
Field/site supervision	1,495	0	1,495
Expenses for other services			
Job coaching/life skills	630	432	200
Job development	551	0	551
Bonding	6	0	6
Fatherhood component	291	0	291
Supportive services	79	0	79
MIS and payroll	408	0	408
Total CEO costs	4,807	612	4,195
Services obtained outside CEO[b]			
Expenses for transitional jobs[c]			
Transitional job wages	0	226	-226
Transitional job fringe benefits	0	37	-37
Field/site supervision	0	242	-242
Expenses for other services			
Employment/job search	178	363	-185
Adult basic education / GED	7	6	1
College courses	239	142	97
Vocational training	723	723	0
Total non-CEO costs	1,147	1,739	-592
Total CEO and non-CEO costs	5,954	2,351	3,603

SOURCES: MDRC calculations from CEO's financial expenditure reports for Fiscal Year 2005 and CEO's management information system (MIS), adjusted for inflation.

NOTES:

[a]CEO incurs fringe benefit costs of 16.7 percent of day labor wages resulting from employer-paid payroll taxes.

[b]Estimates of services outside CEO are based on survey reports from 16 percent of control group respondents.

[c]Costs for non-CEO transitional jobs were assumed to be the same as CEO transitional jobs.

Control Group Costs

The control group in this evaluation was provided basic job search assistance and a life skills class by CEO. All control group costs are outlined in Table 4.1. Because CEO provided only limited job search assistance to control group members, it was expected that many would seek out additional services within the community. As the bottom panel of the table shows, many control group members — and some program group members — received employment and training services from other programs. The single largest expense ($700) for control group members was associated with vocational training. The second-largest expense was for job coaching and life skills services, which averaged over $400 per program group member.

Net Cost of CEO

The net cost of CEO is the difference between the cost of employment, education, and training services for the program group and the cost of these services for the control group.

- **The cost of all employment and training services was $3,600 more per program group member than the cost of services for the average control group member.**

The top panel of Table 4.1 shows CEO's expenditures for services for the program and control groups. As noted above, CEO provided members of the control group a limited set of services, including a shorter version of the life skills class and access to a resource room for job search activities. It was estimated that the cost to serve a control group member in CEO was $610.

The bottom panel of Table 4.1 presents estimates of expenditures by outside organizations to provide job search and other education and training services to both research groups. It was expected that the members of the control group would be more likely to seek out services provided by other organizations. A client survey provided information about the services that program and control group members participated in outside CEO. The expenditures for outside services were $590 more for the control group than for the program group.

Benefits of Reduced Recidivism

The benefits of CEO's impact on recidivism were estimated by measuring the difference between the total costs of crimes committed by program group members and by control group members. These costs include criminal justice expenditures and victimization costs. Criminal justice expenditures consist of police costs of arrest, court costs of plea bargaining,

jail costs of incarceration, and prison costs of incarceration.23 Victimization costs consist of medical expenses, cash loss, property theft or damage, and lost earnings. The discussion of criminal justice savings in this section includes full-sample results and results for the recently released subgroup.

For the full sample, CEO participants committed fewer crimes than the control group, saving about $2,900 in criminal justice expenditures and about $430 in victim costs per program group member. (Table 4.2 presents the detailed economic benefits from reduced recidivism for the full sample.) CEO's impact on the participants who entered the program more than three months after release is smaller than the impact on participants who entered the program within three months of release. This reduced CEO's impact on the average program group member in the full sample. As noted above, the estimated economic benefits for the full sample were smaller per program group member in the full sample than in the recently released subgroup. Prison and jail incarceration for parole revocation was higher for the program group in the full sample, which lowered savings from reduced incarceration.

For the recently released group, CEO's overall benefits from reduced recidivism total about $8,100 per program group member. Table 4.3 presents the estimated benefits to taxpayers from CEO's impact on recidivism rates among the recently released group. These benefits represent the differences between program and control group costs. The costs are presented for each part of the system, including police, courts, jails, and prisons. For jails and prisons, the costs are presented by reason for incarceration, that is, conviction for a new crime, parole revocation, and pretrial detention.

The largest taxpayer benefit from reduced recidivism stems from the difference in prison costs (about $6,000 per program group member), which is based on CEO's impacts on prison incarceration for new crimes and on parole revocation. Although CEO's impact on prison incarceration is smaller than its impact on jail incarceration and other outcomes, the cost savings from avoided prison stays are more substantial for two reasons. First, shorter sentences (fewer than six months) are served in jail, whereas longer sentences (more than six months) are served in prison; this means that a typical stay in prison is longer and more costly than a typical stay in jail. Second, the marginal cost used in this analysis for prison ($47,000 per bed per year), which assumes that entire prisons can be closed, is higher than the marginal cost for jail ($26,000 per bed per year), which assumes that only housing areas can be closed.

[23]The cost of plea bargains was used to estimate the court costs because over 99 percent of all convictions in this study resulted from plea bargains, rather than trials.

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Table 4.2

Estimated Benefits from Reduced Recidivism (in 2009 Dollars), Full Sample

Center for Employment Opportunities

Part of the System	Program Group Cost ($)	Control Group Cost ($)	Difference: Per Person Cost Savings ($)
Police (arrest)	367	409	42
Court (plea bargain)	651	735	84
Jail (new crime)	783	858	75
Jail (detention)	2,057	2,535	478
Jail (parole violation)	987	888	-99
Prisons (new crime)	10,006	12,285	2,279
Prisons (parole violation)	7,805	7,501	-304
Prisons (other)	2,032	2,388	357
Total criminal justice benefits	24,688	27,600	2,912
Victim benefits			432
Total benefits			3,344

SOURCES: Marginal costs were estimated using information provided by New York City and state criminal justice agencies, New York State executive budget agencies, and Taifa (1995). See Appendix Table F.1 for more detail.

NOTE: Prison cost savings are based on the assumption that New York State would close prisons in response to the reduced number of inmates.

The cost savings from prison incarceration were calculated on the assumption that CEO has contributed to the decline in New York State's inmate population, which has resulted in prison closures and has prompted the state's Department of Correctional Services (DOCS) to announce further prison closures in 2011.[24] The analysis also assumes that CEO operated at its current scale. By reducing incarceration rates among program group members, CEO has eliminated the need for approximately 200 prison beds a year, which approximates the capacity

[24]New York State Department of Correctional Services (2010).

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Table 4.3

Estimated Benefits from Reduced Recidivism (in 2009 Dollars), Recently Released Subgroup

Center for Employment Opportunities

Part of the System	Program Group Cost ($)	Control Group Cost ($)	Difference: per Person Cost Savings ($)
Police (arrest)	394	477	83
Court (plea bargain)	693	865	172
Jail (new crime)	870	1,122	252
Jail (detention)	2,488	3,219	731
Jail (parole violation)	912	949	38
Prison (new crime)	13,188	17,214	4,026
Prison (parole violation)	8,273	9,624	1,351
Prison (other)	2,076	2,653	577
Total criminal justice benefits	28,893	36,123	7,230
Victim benefits			882
Total benefits			8,112

SOURCES: Marginal costs were estimated using information provided by New York City and state criminal justice agencies, New York State executive budget agencies, and Taifa (1995). See Appendix Table F.1 for more detail.

NOTE: Prison cost savings are based on the assumption that New York State would close prisons in response to the reduced number of inmates.

of several housing areas of a prison. DOCS estimates that further prison closures will save about $47,000 per inmate per year, and this amount was used in the analysis as the marginal cost, or cost savings.[25] This *marginal* cost is lower than the *average* cost of $63,000 per inmate per year,

[25]The marginal cost estimate of about $47,000 per inmate per year is based on the assumption that New York State can close some of its underutilized prisons. In other words, if the state operated efficiently by closing prisons when the inmate population declines, the savings for every prison bed closed would be about $47,000. Closing prisons maximizes savings, because most prison operating costs — including staff salaries, medical care, utilities, building maintenance, food, and so on — can be eliminated. The marginal cost of $47,000 is used in this benefit-cost analysis because it represents the cost savings that CEO can help achieve if the state responds to the inmate population's decline by closing prisons.

(continued)

which includes overhead expenses like the cost of operating the DOCS central office and is higher than the "low-end marginal cost" of $10,000, which applies when only about 60 prison beds are eliminated. Because the benefit-cost results are particularly sensitive to the prison marginal cost, an alternative benefit-cost analysis was conducted using the lower marginal cost of $10,000. (See "Sensitivity Analysis" below.) Average lengths of stay were estimated for the program and control groups using averages associated with various charge types. As a result, the average length of stay in prison is different for program and control group members. Among the recently released subgroup, program group members spent 946 days incarcerated in prison, compared with 1,095 days for the control group.[26]

The second-largest taxpayer benefit from reduced recidivism stems from the difference in jail costs for the program and control groups. As Table 4.3 indicates, CEO reduced jail expenditures by $250 per program group member through prevented convictions for new crimes, by $730 per program group member through prevented detention, and by $40 per program group member through prevented parole revocations. These cost savings are driven by a few factors. The first factor is CEO's impact on jail incarceration. The percentage of study participants who were incarcerated in a jail was 14.7 percentage points lower for the program group (56.7 percent) than for the control group (71.4 percent) (Chapter 3, Table 3.4). The second factor is the marginal jail cost savings of $26,000 per inmate per year. And the third factor is the average length of stay at Rikers Island (New York City's jail complex) for both the program group and the control group, which was 50 days for detainees, 44 days for those convicted of new crimes,[27] and 29 days for inmates incarcerated for a parole violation.

Other benefits resulted from reduced arrests and convictions. Arrest costs were estimated using police overtime costs associated with arrests, based on the assumption that when arrests in New York City decrease, the New York Police Department can reduce overtime spending associated with arrests. Court costs were estimated using plea bargaining costs, since over 98 percent of the study cases were resolved through a plea bargain. Table 4.3 shows that, by preventing arrests and convictions for new crimes, CEO saved $80 in police costs and $170 in court costs per program group member.

New York State, however, has failed to close some of its underused prisons. In the absence of additional prison closures, the cost savings from CEO's impact on recidivism are only about $10,000 per inmate for each avoided year of incarceration. This amount reflects the cost savings that prisons achieve by eliminating some food, clothing, and staff expenses, but it excludes such fixed costs as leases and wardens' salaries.

[26]The average length of stay for a new crime among the full sample was 1,032 days for the program group and 978 days for the control group.

[27]The length of stay at Rikers Island for those convicted of a new crime was 44 days for program group members and 43 days for control group members.

Victim benefits were estimated using the difference in victim costs for program and control groups. As Table 4.3 shows, CEO reduced victimization costs by $880 per program group member in the recently released group. These benefits are relatively small because most of the convictions in the study sample were for drug crimes, property crimes, and other crimes that do not impose significant victim costs. (Appendix Table F.3 lists convictions by type of offense.) As mentioned above ("Methodology of the Benefit-Cost Analysis"), the victim costs of drug crimes, which CEO reduced, are not included in the analysis.

Benefits of Increased Employment

By providing CEO program group members with employment training and placement in transitional jobs, CEO provided benefits to participants and taxpayers. Participants benefited from increased earnings, and taxpayers benefited from increased tax contributions and the value of services provided by CEO participants.

Chapter 3 shows that CEO produced gains in employment and earnings for program group members (compared with control group members) during the follow-up period for the impact analysis. The value of gains in earnings over the observation period was, on average, $1,200 per program group member in the recently released group (in 2009 dollars).[28]

Because CEO increased taxable income through increased earnings, the program also increased tax contributions from the participants. State and federal income taxes, net of the Earned Income Tax Credit, were estimated using tax rules for filing year 2009.

Table 4.4 shows that, for the full sample, the economic benefit from increased earnings is smaller than for the recently released group, mostly due to lower impacts on earnings. Program group members in the full sample earned an average of $590 more than the control group. They contributed an additional $190 in taxes. Among the recently released subgroup, program group members earned an average of $1,200 more than the control group and contributed $160 in taxes.[29]

CEO also benefited taxpayers by providing services to government agencies. The market value of the services that CEO participants provide to these agencies is estimated using the

[28]Differences in earnings between the program group and the control group are not statistically significant but are used in the benefit-cost analysis because they are the best estimates available.

[29]Notably, the weighted average of the impacts for the subgroups is not equal to that for the full sample. This pattern occurs as a result of regression adjusting and has no effect on the basic impact finding for the earnings outcome. Even when impacts are run unadjusted, the differences do not rise to the level of statistical significance, and the main finding of no impact is unchanged.

The Enhanced Services for the Hard-to-Employ Demonstration

Table 4.4

Net Benefits and Costs (in 2009 Dollars), Full Sample

Center for Employment Opportunities

	Taxpayer ($)	Victim ($)	Participant ($)	Total ($)
Benefits				
Criminal justice	2,912	432	0	3,344
Employment				
Earnings	0	0	590	590
Tax payments and credits[a]	190	0	-190	0
Value of output from CEO transitional jobs[b]	4,576	0	0	4,576
Costs				
CEO program costs[c]	-3,603	0	0	-3,603
Net benefits (per person)	4,075	432	400	4,907
Benefit-cost ratio	2.13	NA	NA	2.36

SOURCES: Marginal costs were estimated using information provided by New York City and state criminal justice agencies, New York State executive budget agencies, and Taifa (1995). See Appendix Table F.1 for more detail. For additional employment data sources, see Table 3.1; for additional criminal justice data sources, see Table 3.2.

NOTES:

[a]Income taxes and the federal and state Earned Income Tax Credit were based on tax rules for filing year 2009.

[b]The value of output from CEO transitional jobs was calculated using information from the Department of Citywide Administrative Services.

[c]CEO program costs were calculated using CEO's financial expenditure reports for Fiscal Year 2005 and CEO's management information system (MIS), adjusted for inflation.

compensation that government agencies would have to pay in the absence of CEO.[30] CEO participants provided government agencies services valued at about $4,600 per program group member. An alternative value of services provided by CEO participants is presented in the section below, "Sensitivity Analysis."

Net Benefits (Net Present Value) of CEO

Table 4.4 presents all the benefits and costs for the full sample, by perspective: taxpayers, victims, and program participants. The benefits represent the monetary value of CEO's

[30]Compensation was estimated as the amount of wages and fringe benefits that government agencies would have to pay other workers in the full-time labor market.

impact on recidivism and employment outcomes. The costs include the net operating costs of CEO, over and above the costs of services provided to the control group. The differences between program and control groups were defined as net gains. A value of zero is not considered a gain or a loss for the perspective to which it applies. The results were then added to obtain an estimate of the overall net gain of CEO from each perspective.

- **For the full sample, from the combined perspectives of taxpayers, victims, and participants, CEO's benefits outweighed the costs by $4,900 per program group member, resulting in a 2.4 benefit-cost ratio.**

The first column of Table 4.4 presents the benefits and costs of CEO from the perspective of taxpayers. The column presents differences in benefits from reduced recidivism, increased tax contributions, the value of CEO's output, and the cost of CEO for the program group compared with the control group. As the table shows, the total taxpayer benefit for the full sample is about $7,700, most of which comes from the value of services from CEO participants ($4,600) and avoided criminal justice costs ($2,900). The net taxpayer benefit is approximately $4,100, and the net benefit from the combined perspectives of taxpayers, victims, and participants is $4,900.

Table 4.5 presents all the benefits and costs for the recently released subgroup, by perspective. Over the three-year period, taxpayers derived approximately $11,900 per program group member in benefits. The majority of this gain came through decreased criminal justice expenditures from reduced recidivism ($7,200) and the value of labor from CEO program group members ($4,600). As a result of increased earnings among program group members, taxpayers benefited by approximately $160 in additional tax contributions. The net benefits were higher than the program cost of $3,600. In other words, CEO was cost-effective from the taxpayer perspective. The taxpayer benefits of CEO outweighed the costs by about $8,300 per program group member in the recently released group, resulting in a 3.3 benefit-cost ratio.

The second column of Table 4.5 shows the gains of CEO from the perspective of victims. As the table indicates, CEO decreased victim costs by $880 by reducing arrest and convictions for new crimes among program group members.

The third column of Table 4.5 displays the benefits and costs of CEO from the participants' perspective. As the table indicates, the value of gains in earnings over the observation period was on average $1,200 per program group member in the recently released subgroup (in 2009 dollars). Participants paid an additional $160 in personal taxes on these additional earnings. The net gain, therefore, was approximately $1,000 per program group member.

The Enhanced Services for the Hard-to-Employ Demonstration

Table 4.5

Net Benefits and Costs (in 2009 Dollars), Recently Released Subgroup

Center for Employment Opportunities

	Taxpayer ($)	Victim ($)	Participant ($)	Total ($)
Benefits				
Criminal justice	7,230	882	0	8,112
Employment				
Earnings	0	0	1,200	1,200
Tax payments and credits[a]	157	0	-157	0
Value of output from CEO transitional jobs[b]	4,551	0	0	4,551
Costs				
CEO program costs[c]	-3,603	0	0	-3,603
Net benefits (per person)	8,336	882	1,043	10,260
Benefit-cost ratio	3.31	NA	NA	3.85

SOURCES: Marginal costs were estimated using information provided by New York City and state criminal justice agencies, New York State executive budget agencies, and Taifa (1995). See Appendix Table F.1 for more detail. For additional employment data sources, see Table 3.1; for additional criminal justice data sources, see Table 3.2.

NOTES:
[a]Income taxes and the federal and state Earned Income Tax Credit were based on tax rules for filing year 2009.
[b]The value of output from CEO transitional jobs was calculated using information from the Department of Citywide Administrative Services.
[c]CEO program costs were calculated using CEO's financial expenditure reports for Fiscal Year 2005 and CEO's management information system (MIS), adjusted for inflation.

- **From the combined perspectives of taxpayers, victims, and participants, CEO's benefits outweighed the costs by $10,300 per program group member in the recently released group, resulting in a benefit-cost ratio of 3.9.**

The rightmost column of Table 4.5 presents the benefits and costs of CEO from the combined perspectives of taxpayers, victims, and participants. CEO was cost-effective from the combined perspectives of the main stakeholders. Participants, victims, and taxpayer gains outweighed the taxpayer costs of the program.

Sensitivity Analysis

Sensitivity analysis provides information about the degree to which results of a benefit-cost analysis are sensitive to changes in the underlying assumptions. Conducting a sensitivity analysis involves varying important or uncertain assumptions and then examining the impact that these changes have on the results. This sensitivity analysis uses data from the recently released group and focuses on four variables: marginal prison cost savings, marginal jail cost savings, the value assigned to transitional jobs, and the value assigned to victimization costs.[31]

As mentioned above, the benefits from reduced recidivism are sensitive to the marginal cost estimates used in the analysis. For example, if New York State is unable to close prison facilities due to decreased recidivism, then the marginal cost savings could decrease from $47,000 to approximately $10,000 per bed per year. As a result, the overall benefit-cost ratio would decrease from 3.9 to 2.6. Similarly, if New York City is unable to close jail facilities due to decreased recidivism, then the marginal cost savings could decrease from approximately $26,000 to $5,000. As a result, the overall benefit-cost ratio would decrease from 3.9 to 3.6.

Additionally, benefits associated with transitional jobs are sensitive to the value that is assigned to the work done by program participants. In this analysis, the value of transitional jobs is defined as the compensation that would have been paid to complete this work if the program had not been operating, which is considered the market value. However, if the value of transitional jobs is defined as the wages paid to participants and to CEO work site supervisors to complete the work as part of the program, then the value of a transitional job would decrease from approximately $4,500 to $2,800. As a result, the overall benefit-cost ratio could decrease from 3.9 to 3.4.

Under an extreme sensitivity analysis in which all three factors listed above — prison marginal costs, jail marginal costs, and the value of labor — use low-end values simultaneously, the benefit-cost ratio remains positive at 1.8 from the combined perspective of government, participants, and victims. Appendix Tables F.5 and F.6 show the extreme low-end benefit-cost results for the full sample and the recently released subgroup. In this scenario, the benefits of CEO would be reduced substantially, but the program would still be cost-effective from the perspective of taxpayers, victims, and participants.

One circumstance that would simultaneously decrease the marginal cost values of prison and jail (similar to the extreme sensitivity analysis above) is a reduction in the number of participants served by CEO. Currently, when impacts are spread across the entire CEO popula-

[31]The text discusses results for the recently released subgroup. Sensitivity analysis results for the full sample are included in Appendix Table F.5.

tion (approximately 2,500 participants), they produce workload changes substantial enough to close housing areas in jails and to close entire prisons. If similar impacts were spread across a smaller population (possibly half the size of the current CEO program), they would substantially lower the marginal cost savings of jail and prison. Decreasing the capacity of the CEO program would substantially decrease overall cost-effectiveness. In order to achieve substantial marginal savings, it is important to maintain current program capacity. Conversely, if CEO serves a larger population, the marginal cost savings could be greater.

This analysis does not include the intangible victim costs of pain and suffering. Including these costs in the victim costs of robbery and assault increases the victim benefit from nearly $900 to $1,200 for the recently released group. As a result, the overall benefit-cost ratio would change very little, from 3.9 (3.85) to 3.9 (3.94).

The above examples illustrate that the cost-effectiveness of the CEO program depends more on prison marginal costs than the value assigned to transitional jobs. However, the value assigned to transitional jobs affects program cost-effectiveness significantly more than jail marginal cost and/or the cost of victimization. Examining these sensitivities helps illustrate that making adjustments to multiple factors does not alter results significantly enough to change the cost-effectiveness of this program. CEO remains a viable investment even when multiple factors simultaneously use low-end values. Therefore, while determining cost-benefit ratios is not a precise art, it seems reasonable to conclude that investing in this program will provide positive returns; likely $2 to $3 of value will be produced for each $1 invested in the program.

Summary

CEO's impact on recidivism and employment has translated into economic benefits that outweigh program costs. The total net benefit to taxpayers of the three-year impacts is over $4,000 per program group member in the full sample and over $8,300 per program group member in the recently released group. CEO serves about 2,500 people per year, and so its potential to produce budget savings is considerable. The majority of the benefits to taxpayers came in the form of reduced criminal justice expenditures and the value of services that CEO participants provided to government agencies. Further, these benefits outweighed the program costs, making CEO a cost-effective program. For each $1 invested in the program, taxpayers derived $2.13 in savings for the full sample and $3.30 in savings for the recently released group.

CEO also provided benefits to victims and participants. Victims incurred fewer costs associated with victimization, and program participants received greater earnings than members of the control group. For the recently released group, the total net benefit from the combined perspectives of taxpayers, victims, and participants is about $10,300 per recently released program group member.

Because the recidivism impact is more pronounced for the recently released group than for the full sample, the benefits outweigh the costs by a greater margin for this group than for the full sample. Given that three-fourths of CEO's full population enters the program shortly after prison release, the net benefits of the recently released group members are considered to be more representative of the net benefits of the average CEO participant.

This analysis shows that CEO's benefits outweigh its costs, suggesting that it is cost-effective for city and state governments. The amount that CEO (and other effective reentry programs) can save in taxpayer dollars depends, however, on whether prisons are closed in response to decreasing inmate population. In the absence of prison closures, correctional systems can only close housing units within prisons. This generates savings, but the savings are a fraction of those realized when entire facilities are closed. To some extent, most cost savings can be realized only when agencies take steps to downsize. Additionally, given some uncertainty of the impact estimates, the actual program benefits could be lower or higher than estimated.

The benefit-cost methodology that was used to evaluate the cost-effectiveness of CEO has several strengths. First, it is based on a random assignment evaluation, which is widely regarded as the gold standard for research. Second, the analysis incorporates cost data that are specific to the New York City and New York State criminal justice system. And, third, it projects economic benefits using marginal costs, which provides a realistic measure of potential cost savings.

Ultimately, the benefit-cost analysis contributes to the body of knowledge about cost-effectiveness of employment programs for the formerly incarcerated. It shows that investment in such programs pays off, particularly when participants enter the program shortly after release.

Chapter 5

Conclusion

The evaluation of the Center for Employment Opportunities (CEO) transitional job program is one of the most rigorous tests of an employment program for former prisoners in recent years. The findings presented in this report and earlier publications on CEO show that the program operated as intended and that most of the program group worked in a transitional job and received other key services, such as job coaching and job placement assistance. At the same time, few control group members accessed a transitional job (though many received employment services at either CEO or another program in the community). The evaluation thus provides a good test of CEO's transitional job model and offers important evidence about the effectiveness of transitional jobs for former prisoners.

The results of the study are promising in some key respects. CEO reduced recidivism among those recently released from prison — the group for whom the program was designed. Reductions in recidivism are difficult to achieve and have rarely been seen in rigorous evaluations like this one. Although the program did not increase unsubsidized employment overall, there is some evidence that CEO increased the number of quarters with unsubsidized employment during the postprogram period, among the recently released subgroup. But it is difficult to say with certainty that these employment impacts were a result of program group members' working more steadily, because there were no corresponding impacts on earnings.

Although CEO dramatically raised employment for the full sample in the first year, the impacts on recidivism are concentrated in the subgroup of parolees who were recently released from prison. If there were a straightforward causal relationship between employment and recidivism, one would expect to see corresponding impacts on arrests and other forms of recidivism in the first year for all sample members and not just for a subgroup.[1] As mentioned above, CEO is designed for those recently released from prison. In that regard, the fact that the impacts are concentrated in that subgroup is not surprising. However, these findings show that simply providing temporary jobs to parolees will not necessarily result in lower recidivism. The pattern of effects suggests that other aspects of the program model, not just the employment itself, are contributing to the impacts on recidivism.

One hypothesis is that the CEO model — particularly its small work crews — encouraged a mentoring type of relationship to develop between participants and CEO staff, particularly work site supervisors. It is plausible that participants connected in some way to staff at the

[1]Employment impacts in Year 1 were similar for the full sample and for those who were recently released.

program and that these positive influences, in turn, affected participants' attitudes and behaviors. Indeed, survey results show that program group members were more likely than control group members to feel connected with staff.[2] In addition, the work crew model gives participants the opportunity to interact in a positive environment. Some believe that peer support can be crucial for people to rebuild their sense of community when they have had a disconnecting experience.[3]

Regardless of the mechanism involved, it seems apparent that something about participating in CEO during the critical period just after prison release changed the attitudes and behaviors of participants, placing them on a different trajectory and deterring criminal activity. There is evidence that CEO's impacts were strongest during the first year, while program group members were active in the program or shortly thereafter; some of those program group members continued to avoid recidivism in subsequent years.[4]

From a policy perspective, CEO is a cost-effective reentry option. The monetary benefits generated by the program are higher than its costs to taxpayers. When benefits to victims and participants are included, CEO's net benefit may be as high as $10,300 per person for those recently released from prison. Given the current fiscal crisis in most states, policymakers are looking for ways to cut spending on corrections, while maintaining public safety. As demonstrated by CEO, even small impacts on recidivism can lead to monetary savings for government budgets, because incarceration is so costly.

Findings from Other Evaluations of Transitional Jobs Programs

The CEO evaluation offers much-needed evidence about the effectiveness of transitional jobs for those reentering society from prison. But CEO is just one program in one location. Policymakers must consider the broader body of evidence that is currently emerging from similar rigorous evaluations of transitional jobs for former prisoners.

One such evaluation is the Transitional Jobs Reentry Demonstration (TJRD), which was developed by the Chicago-based Joyce Foundation.[5] Led by MDRC and research partners from the Urban Institute and the University of Michigan, TJRD is testing transitional jobs programs for former prisoners in four Midwest states. Some of the early lessons from the CEO evaluation fed into the design of TJRD. For example, the TJRD programs limited eligibility to

[2]See Chapter 2 of this report and Redcross et al. (2009).

[3]Mead and MacNeil (2006).

[4]Blumstein and Nakamura (2010). The highest risk of recidivism occurs in the first year after release. Research has shown that recidivism declines steadily with time clean. After 3.8 to 7.7 years (depending on the type of crime), the likelihood of a former prisoner's committing a new crime is equal to that of people of the same age in the general population.

[5]For more information about TJRD, see Redcross et al. (2010).

men released from prison within the prior three months. The TJRD programs were not meant to be a replication of the CEO model, but the findings from TJRD, in conjunction with the CEO results, offer important lessons about the effectiveness of transitional jobs for former prisoners.

Similar to CEO's program, the four TJRD programs offered temporary, wage-paying jobs and job placement services to former prisoners shortly after their release from prison. Two of the programs provided more intensive placement services that included financial incentives for staying employed.[6] The transitional jobs programs were compared with a specific job search program in each of the study sites that specialized in providing employment services to former prisoners in the form of job readiness and job placement assistance.

The one-year results of TJRD were largely disappointing. Similar to CEO, the programs all generated large increases in employment early in the follow-up period as a direct result of the transitional jobs. Also like CEO, the programs generally did not increase unsubsidized employment. Unlike CEO, the TJRD programs, for the most part, did not have impacts on recidivism, although one site that served a large proportion of individuals on an intensive form of parole supervision led to reductions in technical parole violations during the first six months while program group members worked in the transitional jobs.[7]

One promising result from the TJRD study suggests that the financial incentives — offered in one of the sites to participants who obtained and retained regular employment — led to stronger unsubsidized employment and earnings effects. The impact results also suggest that the TJRD transitional jobs programs had somewhat better impacts during the economic downturn and for those who were more disadvantaged, but it is still early to tell; the TJRD study has one year of follow-up at the time of this writing.

Implications for Policy

CEO's transitional job model shows promising results, particularly in the area of reducing recidivism. But the overall evaluation evidence is complicated, in that the exact mechanism through which CEO achieved success in reducing recidivism is not entirely clear. CEO's impacts on recidivism are largely concentrated in a subgroup, even though the early employment impacts were found, in equal magnitude, for the full sample. Further complicating the interpretation of the CEO findings is the fact that the TJRD programs did not produce similar

[6]One of the TJRD programs also offered a small vocational training component to some sample members, but this aspect of the program was not evaluated separately.

[7]This result may have occurred because of changes in participants' behavior while they worked in the transitional job or because parole officers consider employment to be a mitigating factor when deciding whether to incarcerate someone for a minor technical infraction. Because of their transitional jobs, program group members were more likely to be employed.

impacts on recidivism, suggesting that transitional jobs, per se, do not guarantee reductions in recidivism. Taken together, the findings from both of these evaluations highlight the complex relationships between employment and crime.

It is unclear exactly why the TJRD transitional jobs programs did not produce the same impacts on recidivism as CEO, but it may help to look at the similarities and differences between the TJRD and CEO programs for clues. The TJRD and CEO programs were similar in that all successfully placed most participants in a paid transitional job and all offered help with finding a permanent job once a participant was considered ready.

So what was different? None of the TJRD programs operated the work crew model of transitional employment that CEO uses. One site used a scattered-site model, whereby participants were placed individually in jobs with a private (usually nonprofit) employer. Two of the sites were Goodwill Industries affiliates, and participants worked at the program site or in retail locations. The fourth site — the Safer Foundation in Chicago — operated transitional jobs through a contract with a waste management company, and participants all worked at a garbage recycling plant. Another difference between TJRD and CEO is that the control groups in some of the TJRD sites were offered more intensive job search services by programs that specialized in employment services for former prisoners. CEO's control group members were offered only limited assistance with job search, though they could access help in the community.

CEO was the only one of the five programs evaluated that had extensive experience with both transitional jobs and providing services to former prisoners. In fact, some of the TJRD programs had only recently begun serving the population, and staff were new to the nuances of working with corrections and parole agencies and coping with the special needs of those returning home from prison. Further, some of the TJRD programs had little experience with operating transitional jobs. These sites had the added burden of developing work sites that were suitable for former prisoners.

One thing is clear and consistent across the findings from these and other studies: rates of employment are very low among those returning from prison; less than 30 percent of the control group were employed in an average quarter. In both studies, results show that transitional jobs were successful in generating higher rates of employment than would otherwise be found for returning prisoners, demonstrating that when former prisoners are offered an immediate paid job, they are willing to work.

However, since the goal of transitional jobs programs usually moves beyond short-term employment, enhancements to existing transitional job models should be considered. Although the evaluation evidence provides little guidance on what works to increase longer-term employment, it may help to learn from and build on the strategies that were less successful in these programs.

One hypothesis for the lack of consistent employment impacts in these studies is that the transitional jobs generally did not train the participants for specific occupations. Another hypothesis is that the transitional jobs were too short and should be extended to allow more time to build a participant's employability before a transition to the regular labor market is considered. However, the evidence from these current studies does not suggest that many participants were terminated from the program because they had hit a time limit. All the programs experienced difficulties in identifying job opportunities in the private sector and in helping participants make the transition to regular employment. Therefore, programs may consider boosting job development and placement services, perhaps by offering incentives to employers or by putting more emphasis on identifying employment opportunities, cultivating partnerships with private employers, and helping participants stay employed once they obtain unsubsidized jobs. Financial incentives for keeping an unsubsidized job have shown promising effects in these and other studies and could be part of a comprehensive employment strategy.[8]

CEO Today

CEO continues to improve on its model and has implemented several of the enhancements discussed above in an effort to increase its impact on longer-term employment.

Though the follow-up data for this evaluation extend through 2008, the summer of 2006 marked the end of program services for the vast majority of the study sample. During the study's time frame, CEO made several service enhancements. (For example, Chapter 2 describes the Passport to Success evaluation system, Rapid Rewards, and the addition of retention workers.) Over time, CEO continued to refine and enhance its model. Today, while the core components remain the same, the structure of the program is somewhat different.

CEO participants who are on parole are still enrolled in the program every Friday and begin with an initial preemployment life skills class lasting five days. After finishing the life skills class, they are placed in a transitional job work site, and they work three days per week instead of the four-day workweek that was in place during the study period. This temporary change was made by CEO in order to balance the need to accommodate more transitional job participants with reductions in the number of work sites available during the economic downturn.

Among the biggest changes in CEO's operations is that staff now operate in a team structure. Each team includes job coaches, job developers, and retention specialists. Participants are assigned to a job coach and job developer from the same team on the day that they complete the life skills class. Job readiness is assessed by job developers during the last day of

[8]Michalopoulos (2005); Bloom (2010); Holzer and Martinson (2005); Loprest and Martinson (2008); Berlin (2000); Martinson and Hendra (2006); and Redcross et al. (2010).

the life skills class or on the participant's first appointment day. Under the team structure, each team is expected to meet corporate-wide as well as team-specific quotas for employment placement and retention.

The program has also enhanced its postplacement services to include additional retention staff. Although not everyone is assigned a retention specialist, all CEO participants are eligible for the Rapid Rewards incentive payments once they begin working a regular job and bring paystubs to CEO each month. Some participants are offered additional reemployment services if they lose their job.

CEO has also added pre- and postplacement vocational or hard-skills programming. Some participants are eligible to participate in customer service, construction, or warehouse training programs as part of CEO's preemployment services. Postplacement vocational services are offered through the CEO Academy. These services are targeted to individuals who remain employed in an unsubsidized job for three months and who are interested in working in a building trade; they need not have gone through CEO's transitional job program. The CEO Academy provides participants with contextualized math instruction designed to bring their skills up to the levels required for enrollment in vocational training programs at community colleges that partner with CEO. Once participants complete the CEO Academy preparation, they enroll in one of the vocational training programs offered at a partner college. CEO fully covers the cost of the training. At the end, participants receive a certificate from the community college in their specific field of study (electrical, plumbing, or refrigeration). CEO then provides job placement services to its graduates.

Although some of the changes in CEO's program structure are different from the perspective of participants, the core elements of service described in Chapter 2 remain largely the same. Yet CEO believes that the team structure, for example, will allow for better communication and collaboration among staff about participants' needs —which, it is hoped, will translate into better outcomes for clients.

What's Next?

CEO is currently operating several models that replicate the New York City program in upstate New York and other locations in the United States. The U.S. Department of Labor and the U.S. Department of Health and Human Services are both in the early stages of designing multisite random assignment studies of enhanced transitional job models for various populations, including former prisoners, noncustodial parents, and long-term welfare recipients. All the upcoming evaluations are building on the body of evidence produced by this and other studies of transitional jobs programs.

Appendix A

Additional Information About the Research Design and Participants' Characteristics at Baseline

Eligibility Criteria

For both ethical and methodological reasons, individuals who had worked in a Center for Employment Opportunities (CEO) transitional job in the year prior to the baseline ("recycles") were excluded from the study and were assigned to the regular CEO program. Also, only individuals who signed an Informed Consent form were included in the study sample.

At the time of the study, all graduates of New York State's Shock Incarceration (boot camp) program who return to New York City are required to participate in CEO's program. Because all boot camp graduates are mandated to CEO, it was not possible to include them in a random assignment evaluation. Similarly, for contractual reasons, participants in some other smaller, special programs were also excluded.

The Random Assignment Process

The MDRC team worked with CEO and the New York State Department of Corrections and Community Supervision to design a random assignment process that ensured that the study did not decrease the number of people who received transitional job slots, and that ensured that CEO had enough participants to fill its contractual obligations to work site sponsors. Random assignment was conducted only during weeks when the number of new enrollees exceeded the number of available transitional jobs at the work sites.

Control Group Resources

In designing the study, MDRC, CEO, and the funders decided to offer basic job search assistance to the control group, rather than denying them CEO services altogether. This decision was made in part for ethical reasons but also because the study sought to isolate and test the impacts of the core elements of CEO's model — transitional jobs with job coaching and assistance from CEO job developers — rather than assessing whether CEO was more effective than other programs that control group members might find in the community if CEO simply sent them away. They were given access to a resource room that includes computers (with job search software), phones, voice mail, a printer, a fax machine, and other job search tools, including publications. When clients came into the resource room, a staff person was available, if needed, to assist them with many aspects of job search, including the use of the equipment, help developing a résumé, and assistance with setting up a voice mail account so that potential employers could leave messages for participants. Nevertheless, sample members in the control group were free to seek other services in the community, and many did so. In interpreting the study results, it is critical to understand the level and type of services that the control group received.

A control group member who worked diligently in the resource room for three months but was unable to find employment was offered CEO's job development services (but not a transitional job), but this happened rarely. Of course, control group members could — and did — seek assistance from other employment programs in the community.

Analysis Strategy

Since the proportion of people who were randomly assigned to the program group varied from week to week, weights were used so that the impact results would not be dominated by a disproportionate assignment to one group or the other in any given week. In particular, each program group member received a smaller weight in the impact calculation if that individual entered the study in a week when a higher proportion of people were assigned to the program group, and vice versa. The same was true for control group members.

In addition, following standard practice in studies like this, the estimates were regression-adjusted using ordinary least squares, controlling for characteristics of sample members *before* random assignment. These include age, gender, race/ethnicity, number of quarters employed in the three years prior to random assignment, number of prior felony convictions, and whether the following was true for a sample member: mandated to attend CEO, employed in the year prior to random assignment, employed in the quarter prior to random assignment, employed for six consecutive months prior to random assignment, violated parole prior to random assignment, had a prior drug conviction, and whether it was three months or less between release from prison and random assignment.

The Enhanced Services for the Hard-to-Employ Demonstration

Appendix Table A.1

Selected Characteristics of Sample Members at Random Assignment, by Time Between Prison Release and Random Assignment

Center for Employment Opportunities

Characteristic	Time Between Prison Release and Random Assignment		Full Sample
	3 Months or Less	More Than 3 Months	
Age (%)			
18 to 24 years	21.9	16.0	19.6
25 to 30 years	21.6	24.1	23.8
31 to 40 years	31.5	31.8	30.9
41 years or older	25.0	28.1	25.7
Average age (years)	33.6	34.3	33.7
U.S. citizen (%)	75.1	73.9	74.2
Race/ethnicity (%)			
White, non-Hispanic	1.8	1.5	1.8
Black, non-Hispanic	64.2	65.5	64.4
Hispanic	31.3	30.1	30.6
Other	2.6	3.0	3.2
Male (%)	94.5	91.9	93.0
Has any children under age 18 (%)	46.7	50.2	48.0
Lives with any children under age 18 (%)	14.5	17.5	15.8
Ordered to provide child support to a child under age 18 (%)	15.2	22.5	19.3
Education (%)			
High school diploma	10.9	10.2	10.3
General Educational Development (GED) certificate	44.1	43.1	43.1
Beyond high school	5.9	3.1	4.3
None of the above	39.1	43.6	42.3
Housing status (%)			
Rents or owns home	17.0	19.5	18.0
Lives with friends or relatives	62.8	51.8	57.4
Lives in transitional housing	8.4	15.4	11.9
Lives in emergency housing or is homeless	5.1	3.7	4.4
Other	6.7	9.7	8.3
Marital status (%)			
Married, living with spouse	9.9	8.0	8.6
Married, living away from spouse	6.4	8.4	7.5
Unmarried, living with partner	17.4	24.3	21.1
Single	66.3	59.2	62.8

(continued)

Appendix Table A.1 (continued)

Characteristic	Time Between Prison Release and Random Assignment: 3 Months or Less	Time Between Prison Release and Random Assignment: More Than 3 Months	Full Sample	
Ever any employment (%)	79.1	84.4	81.2	
Ever employed 6 consecutive months for one employer (%)	58.4	64.5	61.1	
UI-covered employment in the quarter prior to random assignment[a] (%)	7.3	18.6	13.6	***
UI-covered employment in the year prior to random assignment[a] (%)	11.8	33.6	24.0	***
Arrest history				
Average number of arrests[b]	7.9	7.6	7.5	
Number of prior felony arrests	4.8	4.6	4.5	
Number of prior misdemeanor arrests	3.0	3.0	2.9	
Ever arrested for a violent crime[c]	69.1	68.4	67.5	
Conviction history				
Average number of prior convictions[d]	7.0	6.6	6.7	
Number of prior felony convictions	2.7	2.6	2.6	
Number of prior misdemeanor convictions	4.0	3.7	3.8	
Convicted of a violent crime (%)	51.4	52.3	51.4	
Convicted of a drug-related crime (%)	71.4	75.3	73.4	*
State prison history				
Lifetime number of months in state prison[e]	62.7	58.1	60.0	**
Sample size[f]	385	544	977	

SOURCES: MDRC calculations using data from the CEO Baseline Information Form and the New York State Division of Criminal Justice Services (DCJS).

NOTES: Data in this table are unweighted, but the results for the statistical significance test are weighted by week of random assignment.

In order to assess differences in characteristics across research groups, chi-square tests were used for categorical variables, and t-tests were used for continuous variables.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

[a]This measure was created using data from unemployment insurance (UI) wage records from New York State.

[b]Each arrest date is counted only as a single event. If there are multiple crimes or charges on the same date, only the most serious charge is recorded in the analysis.

[c]This categorization is based on a definition provided by the DCJS.

[d]Each conviction date is counted only as a single event. If there are multiple convictions on the same date, only the most serious charge is recorded in the analysis.

[e]"Lifetime" includes historical data as early as 1970.

[f]Due to missing data, seven sample members are missing prior criminal history characteristics, and 48 sample members are missing the latest prison release date and thus are excluded from the subgroup analysis in this table.

Appendix B

Additional Information About the Program Group's Participation

The Enhanced Services for the Hard-to-Employ Demonstration

Appendix Table B.1

Program Group's Participation in CEO Activities, by Time Between Prison Release and Random Assignment

Center for Employment Opportunities

Participation	Time Between Prison Release and Random Assignment: 3 Months or Less	Time Between Prison Release and Random Assignment: More Than 3 Months	Total
Completed life skills/preemployment class (%)	78.2	74.0	76.0
Ever worked in a CEO transitional job (%)	72.2	68.2	70.5
Weeks worked in a CEO transitional job[a] (%)			
Never worked in a CEO transitional job	27.8	31.8	29.5
Less than 1 week	5.1	4.6	5.2
1-4 weeks	16.9	17.2	18.0
5-12 weeks	31.1	25.7	28.0
13-24 weeks	14.3	16.5	15.1
More than 24 weeks	4.9	4.1	4.3
Days between random assignment date and start of CEO transitional job (%)			
Never worked in a CEO transitional job	27.8	31.8	29.5
0-7 days	50.3	46.2	47.5
8-14 days	17.2	13.3	15.3
More than 14 days	4.7	8.6	7.8 *
Met with a job coach (%)	58.5	60.4	59.4
Met with a job developer (%)	58.3	54.6	56.5
Placed in an unsubsidized job[b] (%)	34.1	29.3	31.3
Among those who worked in a CEO transitional job[b]			
Weeks worked in a CEO transtitional job[a] (%)			
Less than 1 week	7.1	6.8	7.3
1-4 weeks	23.4	25.3	25.6
5-12 weeks	43.1	37.7	39.7
13-24 weeks	19.7	24.2	21.4
More than 24 weeks	6.7	6.0	6.1
Average number of weeks worked in a CEO transitional job	8.8	9.4	8.9
Average number of meetings with job coach (%)	4.6	4.3	4.4
Average number of meetings with job developer (%)	4.2	3.9	4.0
Placed in an unsubsidized job[c] (%)	46.8	41.6	43.6
Program placement	38.4	32.0	34.8
Self-placement	12.8	11.5	11.7
Participated in fatherhood activity (among fathers) (%)	42.1	40.3	41.7
Sample size[d]	225	311	568

(continued)

Appendix Table B.1 (continued)

SOURCE: MDRC calculations from CEO's management information system (MIS).

NOTES: This table reflects program participation and CEO employment between January 2004 and October 2010. There were nine control group members (2.2 percent) who worked in CEO during the follow-up period. CEO outcomes for these control group members are not shown in the table.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

[a]It is important to note that weeks worked may not be consecutive but includes a total of weeks worked after an individual's random assignment date. This variable is created by taking total days worked in CEO and dividing by 4 because participants work four days per week in CEO and attend job coaching or other CEO services on the fifth day.

[b]A total of 412 program group participants worked in a CEO transitional job.

[c]This includes unsubsidized employment placements by CEO staff and self-placement employment that CEO was made aware of or that the client reported to CEO.

[d]Due to missing data, 32 program group members are missing the latest prison release date and thus are excluded from the subgroup analysis in this table.

Appendix C

Additional Impacts on Employment and Earnings

The Enhanced Services for the Hard-to-Employ Demonstration

Appendix Table C.1

Yearly Impacts on Employment and Earnings

Center for Employment Opportunities

Outcome	Program Group	Control Group	Difference (Impact)	P-Value[a]
Employment (%)				
Ever any employment[b]				
Year 1 (Q1-Q4)	80.0	55.5	24.5 ***	0.000
Year 2 (Q5-Q8)	47.3	43.4	3.9	0.228
Year 3 (Q9-Q12)	41.8	40.3	1.5	0.630
Ever worked in a CEO transitional job[c]				
Year 1 (Q1-Q4)	69.2	2.4	66.8 ***	0.000
Year 2 (Q5-Q8)	6.3	1.4	4.9 ***	0.000
Year 3 (Q9-Q12)	5.5	0.3	5.3 ***	0.000
Ever worked in an unsubsidized job				
Year 1 (Q1-Q4)	49.5	54.6	-5.1	0.106
Year 2 (Q5-Q8)	44.4	42.8	1.7	0.600
Year 3 (Q9-Q12)	39.0	40.1	-1.1	0.721
Earnings[d] ($)				
Total UI-covered earnings[b]				
Year 1 (Q1-Q4)	---	---	---	---
Year 2 (Q5-Q8)	5,213	4,683	530	0.458
Year 3 (Q9-Q12)	5,223	5,163	59	0.933
CEO transitional job earnings				
Year 1 (Q1-Q4)	535	17	518 ***	0.000
Year 2 (Q5-Q8)	59	8	51 ***	0.009
Year 3 (Q9-Q12)	56	4	51 ***	0.003
Unsubsidized earnings				
Year 1 (Q1-Q4)	---	---	---	---
Year 2 (Q5-Q8)	5,153	4,675	478	0.503
Year 3 (Q9-Q12)	5,167	5,159	8	0.991
Sample size (total = 973)[e]	564	409		

(continued)

Appendix Table C.1 (continued)

SOURCES: MDRC earnings calculations from the National Directory of New Hires (NDNH) database and employment calculations from the unemployment insurance (UI) wage records from New York State.

NOTES: Results in this table are weighted by week of random assignment and adjusted for pre-random assignment characteristics.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

[a]Standard errors are presented in this report for all impacts with a p-value of 0.000. Following are the standard errors for all impacts with a p-value of 0.000 (presented in the order in which they appear in the table): Employment: 2.853, 2.365, 1.304, and 1.156. Earnings: 44.088.

[b]These outcomes reflect only UI-covered employment and earnings.

[c]CEO transitional employment is estimated from NDNH and CEO's management information system (MIS).

[d]Due to missing earnings data for Year 1, this report includes impacts for only Years 2 and 3.

[e]Four sample members are missing Social Security numbers and therefore could not be matched to employment data.

The Enhanced Services for the Hard-to-Employ Demonstration

Appendix Table C.2

Yearly Impacts on Employment and Earnings, by Time Between Prison Release and Random Assignment

Center for Employment Opportunities

Outcome (%)	Length of Time Between Prison Release and Random Assignment: 3 Months or Less — Program Group	Control Group	Difference (Impact)	P-Value[a]	More Than 3 Months — Program Group	Control Group	Difference (Impact)	P-Value[a]	Difference Between Subgroup Impacts[b]
Employment (%)									
Ever any employment[c]									
Year 1 (Q1-Q4)	84.9	54.1	30.7 ***	0.000	78.4	55.2	23.2 ***	0.000	
Year 2 (Q5-Q8)	51.6	42.2	9.4 *	0.069	43.9	45.5	-1.6	0.713	
Year 3(Q9-Q12)	45.6	43.3	2.3	0.648	38.3	38.2	0.1	0.983	
Ever worked in a CEO transitional job[d]									
Year 1 (Q1-Q4)	72.9	-0.7	73.6 ***	0.000	67.4	3.2	64.2 ***	0.000	†
Year 2 (Q5-Q8)	2.7	0.1	2.6 *	0.060	8.5	2.3	6.2 ***	0.003	
Year 3(Q9-Q12)	2.9	-0.3	3.3 **	0.019	7.9	0.2	7.6 ***	0.000	†
Ever worked in an unsubsidized job									
Year 1 (Q1-Q4)	55.8	53.3	2.5	0.631	45.9	54.7	-8.7 **	0.039	†
Year 2 (Q5-Q8)	50.2	42.3	7.9	0.125	40.1	44.2	-4.1	0.335	†
Year 3(Q9-Q12)	43.5	43.5	0.1	0.991	34.8	38.2	-3.4	0.401	
Earnings[e] ($)									
Total UI-covered earnings[c]									
Year 1 (Q1-Q4)	---	---	---	---	---	---	---	---	
Year 2 (Q5-Q8)	6,239	5,550	689	0.549	4,877	3,842	1,035	0.273	
Year 3 (Q9-Q12)	6,145	5,635	511	0.665	4,944	4,411	533	0.542	
CEO transitional job earnings									
Year 1 (Q1-Q4)	711	-18	729 ***	0.000	436	23	413 ***	0.000	†††
Year 2 (Q5-Q8)	40	-5	45	0.259	73	12	61 ***	0.004	
Year 3 (Q9-Q12)	21	-1	22	0.196	85	6	80 ***	0.004	†
Unsubsidized earnings									
Year 1 (Q1-Q4)	---	---	---	---	---	---	---	---	
Year 2 (Q5-Q8)	6,199	5,556	643	0.576	4,804	3,830	974	0.303	
Year 3 (Q9-Q12)	6,124	5,636	488	0.679	4,858	4,405	453	0.604	
Sample size (total = 926)[f]	223	160			310	233			

(continued)

Appendix Table C.2 (continued)

SOURCES: MDRC earnings calculations from the National Directory of New Hires (NDNH) database and employment calculations from the unemployment insurance (UI) wage records from New York State.

NOTES: Results in this table are weighted by week of random assignment and adjusted for pre-random assignment characteristics.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

A small number of control group members accessed CEO's transitional jobs. Due to weighting and regression adjustments, CEO employment and earnings outcomes for the recently released control group are less than zero.

[a]Standard errors are presented in this report for all impacts with a p-value of 0.000. Following are the standard errors for all impacts with a p-value of 0.000 (presented in the order in which they appear in the table and beginning with the "3 Months or Less" subgroup): Employment: 4.555, 3.734, 3.874, 3.211, and 1.794. Earnings: 80.080 and 53.225

[b]When comparing impacts between two subgroups, an H-statistic is generated. The H-statistic is used to assess whether the difference in impacts between the subgroups is statistically significant. It is interpretable in much the same way as the t-statistic and the F-statistic from analysis of variance (ANOVA) tests are interpreted. Statistically significant differences across subgroups are indicated as: ††† = 1 percent; †† = 5 percent; † = 10 percent.

[c]These outcomes reflect only UI-covered employment and earnings.

[d]CEO transitional employment is estimated from NDNH and CEO's management information system (MIS).

[e]Due to missing earnings data for Year 1, this report includes impacts for only Years 2 and 3. The difference of about $1,100 is not statistically significant. Notably, the weighted average of the impacts for the subgroups is not equal to that of the full sample. This pattern occurs as a result of regression adjusting and has no effect on the basic impact finding for the earnings outcome. Even when impacts are run unadjusted, the differences do not rise to the level of statistical significance and the main finding of no impact is unchanged.

[f]A total of 48 sample members are missing the latest prison release date prior to random assignment and are therefore missing from estimates in this table. An additional three sample members are missing Social Security numbers and therefore could not be matched to employment data.

Appendix D

Additional Impacts on Recidivism

The Enhanced Services for the Hard-to-Employ Demonstration

Appendix Table D.1

Three-Year Impacts on Additional Recidivism Outcomes

Center for Employment Opportunities

Outcome	Program Group	Control Group	Difference (Impact)	P-Value
Additional arrest measures				
Average number of arrests[a]	1.1	1.2	-0.1	0.313
Number of months between random assignment and first arrest (%)				
Never arrested	51.9	47.2	4.7	0.147
3 months or less	5.3	5.2	0.1	0.959
4-6 months	4.6	5.5	-0.9	0.533
7-12 months	11.1	11.9	-0.9	0.678
13-18 months	9.5	11.0	-1.5	0.452
19-24 months	6.6	7.7	-1.1	0.507
25-30 months	5.8	7.4	-1.6	0.312
31-36 months	5.4	4.1	1.2	0.383
Additional conviction measures				
Average number of convictions[a]	0.8	0.9	-0.1	0.268
Conviction type[b] (%)				
Violent crime	7.0	9.5	-2.4	0.176
Drug crime	22.8	25.7	-2.9	0.299
Property crime	22.7	24.9	-2.2	0.418
Public order crime	4.0	5.5	-1.5	0.276
Other	1.7	1.8	-0.1	0.939
Additional incarceration measures (%)				
Ever incarcerated in state prison[c]	33.7	35.2	-1.5	0.626
Incarcerated for a new crime	7.8	9.9	-2.1	0.249
Incarcerated for a technical parole violation	21.9	19.6	2.2	0.394
Incarcerated for all other/unknown reasons[d]	5.9	7.9	-1.9	0.230
Ever incarcerated in jail[c]	56.6	63.0	-6.4 **	0.041
Incarcerated for a new crime	16.9	19.9	-3.0	0.229
Incarcerated for a technical parole violation	35.4	31.6	3.8	0.216
Incarcerated as a "detainee"	35.9	44.6	-8.7 ***	0.005
Months until first arrest, conviction, or incarceration[e]	20.0	18.1	1.9 **	0.035
Parole outcome (%)				
Ever absconded/revoked from parole	35.5	36.8	-1.2	0.692
Sample size (total = 977)	568	409		

(continued)

Appendix Table D.1 (continued)

SOURCES: MDRC calculations using data from the New York State Division of Criminal Justice Services (DCJS) and the New York City Department of Correction (DOC).

NOTES: Results in this table are weighted by week of random assignment and adjusted for pre-random assignment characteristics.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

[a]All sample members are included in this measure. Sample members who were never arrested or never convicted are counted as zero.

[b]Categories of conviction charges are based on definitions from Langan and Levin (2002). Categories may sum to more than the percentage convicted because a person may be convicted of more than one crime during the follow-up period.

[c]Due to multiple incarcerations, subcategories are not mutually exclusive and may sum to a total greater than the "Incarcerated" measure.

[d]All other incarcerations, including those for technical violations while not on parole and for other unknown/unspecified reasons.

[e]A total of 23 convictions were found to be associated with an arrest that occurred prior to random assignment. These convictions are counted in the analysis as occurring after random assignment. The total includes convictions for "other" reasons, felonies, and misdemeanor crimes.

The Enhanced Services for the Hard-to-Employ Demonstration

Appendix Table D.2

Yearly Impacts on Recidivism

Center for Employment Opportunities

Outcome	Program Group	Control Group	Difference (Impact)	P-Value
Ever arrested[a] (%)	48.1	52.8	-4.7	0.147
Year 1	21.0	22.6	-1.7	0.532
Year 2	22.0	27.3	-5.3 *	0.056
Year 3	23.3	24.3	-1.0	0.726
Ever convicted of a crime[b] (%)	43.1	48.8	-5.6 *	0.078
Year 1	15.0	17.0	-2.0	0.399
Year 2	19.8	26.2	-6.3 **	0.019
Year 3	22.9	23.1	-0.2	0.933
Ever convicted of a violent crime[c] (%)	7.0	9.5	-2.4	0.176
Year 1	1.9	1.4	0.5	0.543
Year 2	2.5	4.2	-1.7	0.142
Year 3	2.9	4.3	-1.5	0.231
Ever incarcerated in prison or jail for a new crime (%)	23.7	28.0	-4.3	0.128
Year 1	7.7	9.6	-2.0	0.276
Year 2	8.8	11.7	-2.9	0.141
Year 3	12.6	13.0	-0.4	0.871
New admission to prison or jail[d] (%)	58.1	65.0	-6.9 **	0.027
Year 1	35.0	39.9	-4.9	0.117
Year 2	30.5	33.2	-2.7	0.364
Year 3	27.3	28.8	-1.6	0.591
Ever incarcerated for a technical parole violation (%)	37.5	35.1	2.4	0.435
Year 1	19.9	21.7	-1.8	0.506
Year 2	19.2	15.7	3.5	0.162
Year 3	13.6	11.3	2.3	0.301
Total days incarcerated	173	187	-14	0.392
Year 1	37	39	-2	0.737
Year 2	65	70	-5	0.473
Year 3	71	78	-7	0.370
Ever arrested, convicted, or incarcerated[e] (%)	64.9	70.6	-5.7 *	0.060
Year 1	38.7	45.3	-6.7 **	0.037
Year 2	46.7	52.0	-5.3 *	0.095
Year 3	46.0	50.4	-4.5	0.163
Sample size (total = 977)	568	409		

(continued)

Appendix Table D.2 (continued)

SOURCES: MDRC calculations using data from the New York State Division of Criminal Justice Services (DCJS) and the New York City Department of Correction (DOC).

NOTES: Results in this table are weighted by week of random assignment and adjusted for pre-random assignment characteristics.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

[a]Each arrest date is counted only as a single event. If there are multiple crimes or charges on the same date, only the most serious charge is recorded in the analysis.

[b]The total of 23 convictions were found to be associated with an arrest that occurred prior to random assignment. These convictions are counted in the analysis as occurring after random assignment.

[c]Violent crimes are based on conviction charges defined by Langan and Levin (2002).

[d]Defined as a new incarceration in a given year. For example, if a sample member was incarcerated in jail in Year 1 and transferred directly to prison in Year 2, his new admission would be captured in the year in which he entered jail, that is, in Year 1.

[e]This composite measure was created by combining three measures that are not mutually exclusive: arrest, conviction, and incarceration. Participants who were arrested and/or convicted, for example, were also incarcerated. The composite measure represents people who experienced one or more of these recidivism measures.

The Enhanced Services for the Hard-to-Employ Demonstration

Appendix Table D.3

Yearly Impacts on Recidivism, by Time Between Prison Release and Random Assignment

Center for Employment Opportunities

Outcome	Length of Time Between Prison Release and Random Assignment: 3 Months or Less — Program Group	Control Group	Difference (Impact)	P-Value	More Than 3 Months — Program Group	Control Group	Difference (Impact)	P-Value	Difference Between Subgroup Impacts[a]
Ever arrested[b] (%)	49.1	59.1	-10.0 *	0.056	47.0	50.5	-3.5	0.420	
Year 1	19.9	23.8	-3.9	0.365	23.1	21.7	1.4	0.707	
Year 2	25.9	32.8	-6.9	0.153	18.9	24.7	-5.8	0.105	
Year 3	22.0	26.9	-4.9	0.284	23.4	23.9	-0.5	0.888	
Ever convicted of a felony	15.6	14.6	1.0	0.789	6.8	10.2	-3.4	0.156	
Year 1	1.5	4.9	-3.4 *	0.053	1.2	2.3	-1.1	0.349	
Year 2	10.5	4.5	6.0 **	0.044	1.6	4.5	-2.9 **	0.046	†††
Year 3	3.6	6.2	-2.7	0.244	4.0	3.4	0.6	0.726	
Ever convicted of a misdemeanor	31.9	46.1	-14.3 ***	0.005	35.5	36.8	-1.3	0.764	††
Year 1	9.6	11.9	-2.4	0.470	14.3	12.5	1.8	0.547	
Year 2	13.4	25.6	-12.2 ***	0.003	16.8	20.0	-3.3	0.333	†
Year 3	17.2	21.6	-4.4	0.296	18.1	17.6	0.5	0.893	
Ever convicted of a violent crime[c] (%)	5.4	14.3	-8.9 ***	0.004	7.5	6.7	0.8	0.713	††
Year 1	2.5	1.3	1.2	0.423	1.5	1.4	0.1	0.945	
Year 2	1.9	6.6	-4.8 **	0.025	2.9	2.8	0.1	0.943	†
Year 3	1.0	7.1	-6.1 ***	0.002	3.5	2.7	0.9	0.585	†††
Ever incarcerated in prison or jail for a new crime (%	26.5	35.4	-8.9 *	0.061	22.7	24.7	-2.0	0.593	
Year 1	8.0	13.5	-5.5 *	0.084	8.2	7.6	0.6	0.800	
Year 2	11.5	12.0	-0.5	0.891	8.0	11.4	-3.4	0.190	
Year 3	12.9	16.3	-3.4	0.356	12.3	11.9	0.3	0.907	

(continued)

Appendix Table D.3 (continued)

Outcome	Length of Time Between Prison Release and Random Assignment: 3 Months or Less — Program Group	Control Group	Difference (Impact)	P-Value	More Than 3 Months — Program Group	Control Group	Difference (Impact)	P-Value	Difference Between Subgroup Impacts[a]
New admission to prison or jail[d] (%)	60.2	71.3	-11.2 **	0.027	57.1	63.2	-6.1	0.147	
Year 1	32.8	42.9	-10.2 **	0.044	36.5	39.0	-2.5	0.556	
Year 2	34.4	37.9	-3.6	0.489	28.8	30.0	-1.3	0.751	
Year 3	28.5	29.4	-0.9	0.851	25.1	30.6	-5.5	0.155	
Total days incarcerated	213	247	-34	0.234	154	151	3	0.872	
Year 1	39	56	-16 *	0.058	36	29	7	0.194	††
Year 2	83	95	-12	0.338	59	54	4	0.606	
Year 3	90	96	-6	0.679	59	68	-9	0.354	
Ever arrested, convicted, or incarcerated[e] (%)	66.8	75.8	-9.0 *	0.063	64.3	70.2	-5.9	0.148	
Year 1	34.6	46.5	-11.9 **	0.020	41.4	45.6	-4.2	0.333	
Year 2	50.9	59.0	-8.1	0.118	45.1	49.2	-4.2	0.339	
Year 3	50.9	56.8	-5.9	0.264	42.0	48.1	-6.2	0.147	
Sample size (total = 929)[f]	225	160			311	233			

SOURCES: MDRC calculations using data from the New York State Division of Criminal Justice Services (DCJS) and the New York City Department of Correction (DOC).

NOTES: Results in this table are weighted by week of random assignment and adjusted for pre-random assignment characteristics.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

[a]When comparing impacts between two subgroups, an H-statistic is generated. The H-statistic is used to assess whether the difference in impacts between the subgroups is statistically significant. It is interpretable in much the same way as the t-statistic and the F-statistic from analysis of variance (ANOVA) tests are interpreted. Statistically significant differences across subgroups are indicated as: ††† = 1 percent; †† = 5 percent; † = 10 percent.

[b]Each arrest date is counted only as a single event. If there are multiple crimes or charges on the same date, only one the most serious charge is recorded in the analysis.

[c]Violent crimes are based on conviction charges defined by Langan and Levin (2002).

[d]Defined as a new incarceration in a given year. For example, if a sample member was incarcerated in jail in Year 1 and transferred directly to prison in Year 2, his new admission would be captured in the year in which he entered jail, that is, in Year 1.

[e]This composite measure was created by combining three measures that are not mutually exclusive: arrest, conviction, and incarceration. Participants who were arrested and/or convicted, for example, were also incarcerated. The composite measure represents people who experienced one or more of these recidivism measures.

[f]A total of 48 sample members are missing the last prison release date and are therefore missing from all outcomes in this table.

The Enhanced Services for the Hard-to-Employ Demonstration

Appendix Table D.4

Recidivism Impacts Used in the Benefit-Cost Analysis, by Year, Full Sample

Center for Employment Opportunities

Outcome	Program Group	Control Group	Difference (Impact)	P-Value	Standard Error
Police measures					
Number of arrests	1.058	1.177	-0.119	0.313	0.117
Year 1	0.276	0.297	-0.020	0.632	0.042
Year 2	0.338	0.453	-0.114 **	0.047	0.058
Year 3	0.443	0.427	0.016	0.812	0.067
Court measures					
Number of plea bargains[a]	0.764	0.861	-0.097	0.268	0.087
Year 1	0.163	0.180	-0.018	0.564	0.030
Year 2	0.253	0.355	-0.102 **	0.018	0.043
Year 3	0.348	0.326	0.023	0.662	0.052
Jail incarceration measures					
Number of admissions for a new crime	0.297	0.312	-0.015	0.796	0.058
Year 1	0.074	0.079	-0.005	0.812	0.019
Year 2	0.082	0.104	-0.022	0.422	0.028
Year 3	0.141	0.129	0.012	0.734	0.035
Number of admissions for a technical parole violation	0.483	0.432	0.050	0.295	0.048
Year 1	0.190	0.216	-0.026	0.358	0.028
Year 2	0.168	0.114	0.055 **	0.027	0.025
Year 3	0.124	0.102	0.022	0.358	0.024
Number of admissions as detainee[b]	0.586	0.726	-0.140 **	0.029	0.064
Year 1	0.230	0.230	-0.001	0.984	0.033
Year 2	0.190	0.253	-0.063 *	0.067	0.034
Year 3	0.167	0.243	-0.077 **	0.014	0.031
Prison incarceration measures					
Number of admissions for a new crime	0.078	0.099	-0.021	0.249	0.018
Year 1	0.009	0.031	-0.022 **	0.012	0.009
Year 2	0.034	0.038	-0.004	0.742	0.012
Year 3	0.035	0.031	0.005	0.675	0.012
Number of admissions for a technical parole violation	0.281	0.268	0.013	0.748	0.039
Year 1	0.080	0.098	-0.017	0.352	0.019
Year 2	0.116	0.105	0.011	0.621	0.022
Year 3	0.084	0.065	0.019	0.298	0.018
Number of admissions for other/unknown reasons[c]	0.073	0.086	-0.013	0.508	0.020
Year 1	0.022	0.014	0.007	0.411	0.009
Year 2	0.024	0.046	-0.022 *	0.065	0.012
Year 3	0.027	0.026	0.001	0.927	0.011
Sample size (total = 977)	568	409			

(continued)

Appendix Table D.4 (continued)

SOURCES: MDRC calculations using data from the New York State Division of Criminal Justice Services (DCJS) and the New York City Department of Correction (DOC).

NOTES: Results in this table are weighted by week of random assignment and adjusted for pre-random assignment characteristics.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

All sample members are included in each of these measures. For example, sample members who were never arrested or never convicted are counted as zero for those outcomes.

[a]Almost all convictions were disposed of through plea bargains.

[b]Individuals were incarcerated as a detainee for a variety of reasons both before and after adjudication. They were released for the following reasons: transferred to prison, in custody of the New York City Police Department, transferred to a mental hospital, paid bail, expired city sentence, dismissed case, and released on own recognizance.

[c]All other incarcerations, including those for technical violations while not on parole and for other unknown/unspecified reasons.

The Enhanced Services for the Hard-to-Employ Demonstration

Appendix Table D.5

Recidivism Impacts Used in the Benefit-Cost Analysis, by Year, Recently Released Subgroup

Center for Employment Opportunities

Outcome	Program Group	Control Group	Difference (Impact)	P-Value	Standard Error
Police measures					
Number of arrests	1.135	1.373	-0.238	0.219	0.193
Year 1	0.283	0.301	-0.018	0.786	0.066
Year 2	0.406	0.577	-0.170	0.115	0.108
Year 3	0.445	0.495	-0.050	0.649	0.109
Court measures					
Number of plea bargains[a]	0.812	1.013	-0.201	0.172	0.147
Year 1	0.150	0.195	-0.045	0.350	0.049
Year 2	0.301	0.423	-0.122	0.130	0.081
Year 3	0.362	0.395	-0.033	0.720	0.092
Jail incarceration measures					
Number of admissions for a new crime	0.287	0.375	-0.088	0.365	0.097
Year 1	0.079	0.096	-0.017	0.602	0.032
Year 2	0.071	0.136	-0.065	0.195	0.050
Year 3	0.138	0.143	-0.006	0.916	0.054
Number of admissions for a technical parole violation	0.447	0.460	-0.014	0.857	0.076
Year 1	0.156	0.229	-0.072	0.111	0.045
Year 2	0.162	0.146	0.016	0.691	0.041
Year 3	0.128	0.086	0.042	0.292	0.040
Number of admissions as detainee[b]	0.710	0.922	-0.212 *	0.085	0.123
Year 1	0.268	0.286	-0.019	0.756	0.060
Year 2	0.253	0.369	-0.116 *	0.094	0.069
Year 3	0.189	0.266	-0.077	0.167	0.056
Prison incarceration measures					
Number of admissions for a new crime	0.112	0.125	-0.014	0.685	0.034
Year 1	0.006	0.049	-0.044 ***	0.006	0.016
Year 2	0.067	0.032	0.034	0.160	0.024
Year 3	0.039	0.044	-0.004	0.850	0.022
Number of admissions for a technical parole violation	0.296	0.344	-0.048	0.476	0.067
Year 1	0.073	0.128	-0.055 *	0.090	0.032
Year 2	0.140	0.126	0.013	0.738	0.040
Year 3	0.084	0.090	-0.006	0.852	0.032
Number of admissions for other/unknown reasons[c]	0.075	0.094	-0.019	0.562	0.033
Year 1	0.017	0.025	-0.008	0.594	0.015
Year 2	0.022	0.050	-0.027	0.150	0.019
Year 3	0.036	0.020	0.016	0.378	0.018
Sample size (total = 385)[d]	225	160			

(continued)

Appendix Table D.5 (continued)

SOURCES: MDRC calculations using data from the New York State Division of Criminal Justice Services (DCJS) and the New York City Department of Correction (DOC).

NOTES: Results in this table are weighted by week of random assignment and adjusted for pre-random assignment characteristics.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

All sample members are included in each of these measures. For example, sample members who were never arrested or never convicted are counted as zero for those outcomes.

[a]Almost all convictions were disposed of through plea bargains.

[b]Individuals were incarcerated as a detainee for a variety of reasons both before and after adjudication. They were released for the following reasons: transferred to prison, in custody of the New York City Police Department, transferred to a mental hospital, paid bail, expired city sentence, dismissed case, and released on own recognizance.

[c]All other incarcerations, including those for technical violations while not on parole and for other unknown/unspecified reasons.

[d]A total of 48 sample members are missing the last prison release date and are therefore missing from all outcomes in this table.

Appendix E

Additional Impacts for Subgroups Defined by Educational Attainment, Age, and Recidivism Risk Index

The Enhanced Services for the Hard-to-Employ Demonstration

Appendix Table E.1

Three-Year Impacts on Employment and Recidivism, by Educational Attainment

Center for Employment Opportunities

Outcome	Educational Attainment: Does Not Have High School Diploma — Program Group	Control Group	Difference (Impact)	P-Value[a]	Has High School Diploma — Program Group	Control Group	Difference (Impact)	P-Value[a]	Difference Between Subgroup Impacts[b]
Employment outcomes (%)									
Ever any employment[c]	78.4	63.6	14.8 ***	0.002	88.0	76.5	11.5 ***	0.000	
Ever worked in a CEO transitional job[d]	65.8	2.3	63.4 ***	0.000	72.2	4.6	67.6 ***	0.000	
Ever worked in an unsubsidized job	58.6	62.7	-4.2	0.413	69.8	74.9	-5.0	0.196	
Postprogram employment and earnings outcomes (Years 2-3)[e]									
Average quarterly unsubsidized employment (%)	25.6	24.6	1.0	0.747	31.8	30.5	1.3	0.692	
Total UI-covered earnings[c] ($)	11,212	8,789	2,423	0.324	10,728	11,212	-484	0.765	
Recidivism outcomes									
Arrested[f] (%)	43.7	52.5	-8.7 *	0.093	49.9	50.2	-0.4	0.932	
Convicted of a crime[g] (%)	40.5	49.1	-8.6 *	0.097	44.2	46.3	-2.1	0.631	
Convicted of a felony	8.2	11.4	-3.2	0.288	11.2	12.1	-0.9	0.756	
Convicted of a misdemeanor	32.2	40.7	-8.5 *	0.091	34.0	36.3	-2.3	0.578	
Convicted of a violent crime[h] (%)	8.5	9.6	-1.1	0.700	5.7	8.7	-3.0	0.191	
Incarcerated[i] (%)	58.1	69.3	-11.2 **	0.024	56.0	60.1	-4.0	0.357	
Total days incarcerated	155	205	-50 **	0.044	178	167	10	0.638	†
Sample size (total = 893)	223	155			294	221			

(continued)

Appendix Table E.1 (continued)

SOURCES: MDRC employment calculations using data from unemployment insurance (UI) wage records from New York State and earnings data from the National Directory of New Hires (NDNH) database. Incarceration calculations use data from the New York State Division of Criminal Justice Services (DCJS) and the New York City Department of Correction (DOC).

NOTES: Results in this table are weighted by week of random assignment and adjusted for pre-random assignment characteristics.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

[a]Standard errors are presented in this report for all impacts with a p-value of 0.000. Following are the standard errors for all impacts with a p-value of 0.000 (presented in the order in which they appear in the table and beginning with the "Does Not Have High School Diploma" subgroup): Employment: 3.929, 3.277, and 3.337.

[b]When comparing impacts between two subgroups, an H-statistic is generated. The H-statistic is used to assess whether the difference in impacts between the subgroups is statistically significant. It is interpretable in much the same way as the t-statistic and the F-statistic from analysis of variance (ANOVA) tests are interpreted. Statistically significant differences across subgroups are indicated as: ††† = 1 percent; †† = 5 percent; † = 10 percent.

[c]These outcomes reflect only UI-covered employment and earnings.

[d]CEO transitional employment is estimated from NDNH and CEO's management information system (MIS).

[e]Due to missing earnings data for Year 1, this report includes impacts for only Years 2 and 3.

[f]Each arrest date is counted only as a single event. If there are multiple crimes or charges on the same date, only one the most serious charge is recorded in the analysis.

[g]A total of 23 convictions were found to be associated with an arrest that occurred prior to random assignment. These convictions are counted in the analysis as occurring after random assignment.

[h]Violent crimes are based on conviction charges defined by Langan and Levin (2002).

[i]Includes all reasons for incarceration, such as sentences for new crimes, technical violations of parole, detainee (jail), and other admission reasons. A sample member may have multiple admissions; therefore, incarcerations for new crimes and parole violations do not sum to the percentage incarcerated.

The Enhanced Services for the Hard-to-Employ Demonstration

Appendix Table E.2

Three-Year Impacts on Employment and Recidivism, by Age at Baseline

Center for Employment Opportunities

Outcome	Age at Baseline: Less Than 29 Years Old — Program Group	Control Group	Difference (Impact)	P-Value[a]	Age at Baseline: 29 Years Old or Older — Program Group	Control Group	Difference (Impact)	P-Value[a]	Difference Between Subgroup Impacts[b]
Employment outcomes (%)									
Ever any employment[c]	81.2	72.4	8.9 **	0.032	86.3	68.3	18.0 ***	0.000	†
Ever worked in a CEO transitional job[d]	64.0	4.8	59.1 ***	0.000	74.8	1.7	73.1 ***	0.000	†††
Ever worked in an unsubsidized job	62.2	70.7	-8.5 *	0.065	65.1	67.1	-2.0	0.612	
Postprogram employment and earnings outcomes (Years 2-3)[e]									
Average quarterly unsubsidized employment (%)	25.5	28.6	-3.1	0.363	30.1	25.9	4.2	0.139	†
Total UI-covered earnings[c] ($)	10,571	9,900	671	0.780	10,096	9,855	240	0.877	
Recidivism outcomes									
Arrested[f] (%)	45.7	50.9	-5.2	0.335	49.8	54.2	-4.3	0.285	
Convicted of a crime[g] (%)	40.3	42.2	-1.9	0.724	45.2	52.7	-7.6 *	0.060	
Convicted of a felony	14.0	11.7	2.3	0.531	7.8	11.4	-3.6	0.136	
Convicted of a misdemeanor	26.3	31.4	-5.1	0.293	39.0	43.9	-4.9	0.223	
Convicted of a violent crime[h] (%)	10.6	6.6	4.0	0.197	5.0	11.1	-6.1 ***	0.006	†††
Incarcerated[i] (%)	56.2	66.9	-10.8 **	0.037	60.0	64.3	-4.2	0.275	
Total days incarcerated	178	191	-13	0.632	172	184	-12	0.542	
Sample size (total = 976)	207	153			360	256			

(continued)

Appendix Table E.2 (continued)

SOURCES: MDRC employment calculations using data from unemployment insurance (UI) wage records from New York State and earnings data from the National Directory of New Hires (NDNH) database. Incarceration calculations use data from the New York State Division of Criminal Justice Services (DCJS) and the New York City Department of Correction (DOC).

NOTES: Results in this table are weighted by week of random assignment and adjusted for pre-random assignment characteristics.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

[a]Standard errors are presented in this report for all impacts with a p-value of 0.000. Following are the standard errors for all impacts with a p-value of 0.000 (presented in the order in which they appear in the table and beginning with the "Less Than 29 Years Old" subgroup): Employment: 4.013, 3.425, and 2.970.

[b]When comparing impacts between two subgroups, an H-statistic is generated. The H-statistic is used to assess whether the difference in impacts between the subgroups is statistically significant. It is interpretable in much the same way as the t-statistic and the F-statistic from analysis of variance (ANOVA) tests are interpreted. Statistically significant differences across subgroups are indicated as: ††† = 1 percent; †† = 5 percent; † = 10 percent.

[c]These outcomes reflect only UI-covered employment and earnings.

[d]CEO transitional employment is estimated from NDNH and CEO's management information system (MIS).

[e]Due to missing earnings data for Year 1, this report includes impacts for only Years 2 and 3.

[f]Each arrest date is counted only as a single event. If there are multiple crimes or charges on the same date, only one the most serious charge is recorded in the analysis.

[g]A total of 23 convictions were found to be associated with an arrest that occurred prior to random assignment. These convictions are counted in the analysis as occurring after random assignment.

[h]Violent crimes are based on conviction charges defined by Langan and Levin (2002).

[i]Includes all reasons for incarceration, such as sentences for new crimes, technical violations of parole, detainee (jail), and other admission reasons. A sample member may have multiple admissions; therefore, incarcerations for new crimes and parole violations do not sum to the percentage incarcerated.

The Enhanced Services for the Hard-to-Employ Demonstration

Appendix Table E.3

Three-Year Impacts on Employment and Recidivism, by Recidivism Risk Index

Center for Employment Opportunities

	Recidivism Risk Index												Difference
	Low				Medium				High				Between
Outcome	Program Group	Control Group	Difference (Impact)	P-Value[a]	Program Group	Control Group	Difference (Impact)	P-Value[a]	Program Group	Control Group	Difference (Impact)	P-Value[a]	Subgroup Impacts[b]
Employment outcomes (%)													
Ever any employment[c]	89.2	72.7	16.5 ***	0.009	82.8	71.5	11.3 **	0.017	82.6	58.7	23.9 ***	0.001	
Ever worked in a CEO transitional job[d]	72.5	3.2	69.3 ***	0.000	65.0	6.7	58.3 ***	0.000	71.7	2.6	69.1 ***	0.000	
Ever worked in an unsubsidized job	72.2	69.1	3.1	0.689	61.9	69.7	-7.8	0.159	60.0	57.4	2.6	0.743	
Postprogram employment and earnings outcomes (Years 2-3)[e]													
Average quarterly unsubsidized employment (%)	35.2	33.8	1.4	0.817	25.5	27.0	-1.5	0.704	27.0	16.0	11.0 **	0.036	
Total UI-covered earnings[c] ($)	13,503	14,320	-817	0.822	10,924	8,778	2,146	0.455	6,564	7,565	-1,001	0.664	
Recidivism outcomes													
Arrested[f] (%)	36.0	29.1	6.9	0.384	48.4	54.5	-6.2	0.304	58.3	64.7	-6.4	0.407	
Convicted of a crime[g] (%)	32.1	29.4	2.7	0.726	43.3	50.4	-7.1	0.237	53.6	61.7	-8.0	0.302	
Convicted of a felony	3.4	3.2	0.2	0.946	14.2	11.5	2.6	0.522	8.1	19.4	-11.3 **	0.027	†
Convicted of a misdemeanor	28.7	26.9	1.7	0.819	30.1	38.3	-8.2	0.141	46.3	52.9	-6.6	0.396	
Convicted of a violent crime[h] (%)	4.6	2.4	2.2	0.525	7.6	13.6	-6.0 *	0.087	8.2	13.8	-5.6	0.258	
Incarcerated[i] (%)	41.8	40.7	1.1	0.895	62.5	68.8	-6.3	0.255	66.1	78.6	-12.5 *	0.080	
Total days incarcerated	104	80	24	0.449	194	168	25	0.409	205	293	-88 **	0.036	†
Sample size (total = 773)	144	49			275	94			149	62			

(continued)

Appendix Table E.3 (continued)

SOURCES: MDRC employment calculations using data from unemployment insurance (UI) wage records from New York State and earnings data from the National Directory of New Hires (NDNH) database. Incarceration calculations use data from the New York State Division of Criminal Justice Services (DCJS) and the New York City Department of Correction (DOC).

NOTES: A working paper from this evaluation describes the method used to assess a sample member's level of risk. That analysis showed that CEO's impacts on recidivism were larger for those at "high" risk of recidivism when they entered the study. See Zweig, Yahner, and Redcross (2010).

Results in this table are weighted by week of random assignment and adjusted for pre-random assignment characteristics.

Statistical significance levels are indicated as: *** = 1 percent; ** = 5 percent; * = 10 percent.

[a]Standard errors are presented in this report for all impacts with a p-value of 0.000. Following are the standard errors for all impacts with a p-value of 0.000 (presented in the order in which they appear in the table and beginning with the "Low-Risk" subgroup): Employment: 6.645, 5.176, and 6.494.

[b]When comparing impacts between two subgroups, an H-statistic is generated. The H-statistic is used to assess whether the difference in impacts between the subgroups is statistically significant. It is interpretable in much the same way as the t-statistic and the F-statistic from analysis of variance (ANOVA) tests are interpreted. Statistically significant differences across subgroups are indicated as: ††† = 1 percent; †† = 5 percent; † = 10 percent.

[c]These outcomes reflect only UI-covered employment and earnings.

[d]CEO transitional employment is estimated from NDNH and CEO's management information system (MIS).

[e]Due to missing earnings data for Year 1, this report includes impacts for only Years 2 and 3.

[f]Each arrest date is counted only as a single event. If there are multiple crimes or charges on the same date, only one the most serious charge is recorded in the analysis.

[g]A total of 23 convictions were found to be associated with an arrest that occurred prior to random assignment. These convictions are counted in the analysis as occurring after random assignment.

[h]Violent crimes are based on conviction charges defined by Langan and Levin (2002).

[i]Includes all reasons for incarceration, such as sentences for new crimes, technical violations of parole, detainee (jail), and other admission reasons. A sample member may have multiple admissions; therefore, incarcerations for new crimes and parole violations do not sum to the percentage incarcerated.

Appendix F

Sources and Additional Tables for the Benefit-Cost Analysis

Data Sources

The cost of the Center for Employment Opportunities (CEO) program was estimated using CEO's detailed financial expenditure reports and in-depth interviews with the program's fiscal staff.[1] Study participants were randomly assigned to the two research groups and were enrolled in CEO's program between January 2004 and October 2005. Therefore, Fiscal Year 2005 represents a steady-state period of CEO program operations as experienced by program group members. To estimate the cost of each program component per participant engaged in that activity, expenditure data were combined with data from CEO's management information system concerning the number of individuals served by CEO in various activities during the same period. Information on the use of services outside CEO was obtained from the client survey, which included questions about the types of education and training received and the duration of those services. It was assumed that the costs of non-CEO transitional jobs (for example, transitional jobs that research sample members may have obtained from other organizations in the community) were the same as the costs of transitional jobs provided by CEO; the costs of college classes were estimated using published tuition rates.

Marginal costs of criminal justice agencies were estimated by contacting the New York City and the New York State criminal justice agencies, most of which measure marginal costs in order to forecast and allocate funding. Specifically, the research team communicated with the New York City Department of Correction (DOC), to obtain the marginal costs of operating the city's jail system; with the New York State Division of Correction Services (DOCS), to acquire the marginal costs of the state's prisons; and with the New York City Police Department (NYPD), to learn the marginal costs of making arrests and conducting investigations. Marginal costs of plea bargains were obtained from a peer-reviewed journal article, which contains the best available estimate of plea bargaining costs.[2] The research team also contacted staff at the New York City Office of Management and Budget and the New York State Division of the Budget to ensure the accuracy of the marginal cost estimates.

The average lengths of stay in jail and prisons for various offense categories were calculated using data provided by DOC and DOCS. The research team used case-level DOC data to calculate the average length of stay for detainees, parole violators, and those convicted of new crimes. For the last group, the weighted average length of stay was calculated using the number of reconvictions in the CEO sample and the estimated length of stay for 10 offense categories: robbery, burglary/larceny, stolen property/motor vehicle theft, simple assault, aggravated assault, public order, fraud/forgery, drug possession, drug sale, and other misdemeanors. The average length of stay in prisons was calculated using a report published by

[1]Fiscal Year 2005 covers the period from July 1, 2004, through June 30, 2005.
[2]Taifa (1995).

DOCS.[3] As with jails, the weighted average length of stay was calculated using the number of reconvictions and the average length of stay for conviction offenses that resulted in prison incarceration, including robbery, burglary/larceny, aggravated assault, drug possession, and drug sale.

To estimate the proportion of crime reports that result in a conviction in New York State, the research team relied on data provided by the New York State Division of Criminal Justice Services. This agency publishes a report containing the number of crime reports for seven major crime categories.[4] Agency staff also provided researchers with the number of convictions made for these offenses.[5]

The Enhanced Services for the Hard-to-Employ Demonstration

Appendix Table F.1

Data Sources for Estimates in the Benefit-Cost Analysis

Center for Employment Opportunities

Data Type	Source
Value of output from CEO transitional jobs	New York City Department of Citywide Administrative Services
Arrests	New York City Police Department
Jail	New York City Department of Correction
Prison	New York State Department of Correction
Parole	New York State Department of Corrections and Community Supervision; State of New York Executive Budget Agencies.
Victim	McCollister, French, and Fang (2010)
Court (plea bargain)	Taifa (1995)

[3]New York State DOCS "Statistical Overview, Year 2008 Discharges" (2009).
[4]DCJS "Crime in New York State, 2009 Preliminary Data" (2010).
[5]Communication with DCJS staff (June 24, 2010).

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Appendix Table F.2

Three-Year Progam Impacts and Criminal Justice System Cost Savings (in 2009 Dollars), Recently Released Subgroup

Center for Employment Opportunities

Part of the System	Program Group	Control Group	Marginal Cost ($)	Average Length of Stay (Days)[a] Program Group	Average Length of Stay (Days)[a] Control Group	Net Present Value of Impact ($)[b]
Police (number of arrests)	1.135	1.373	359	NA	NA	83
Court (number of plea bargains)	0.812	1.013	884	NA	NA	172
Jail (number of admissions for a new crime)	0.287	0.375	72/day	44	43	252
Jail (number of admissions for a parole violation)	0.447	0.460	72/day	29	29	38
Jail (number of admissions as a detainee)	0.710	0.922	72/day	50	50	731
Prisons (number of admissions for new crime)	0.112	0.125	129/day	946	1,095	4,026
Prisons (number of admissions for a parole violation)	0.296	0.344	129/day	222	222	1,351
Prisons (number of admissions for other reasons)	0.075	0.094	129/day	222	222	577
Total criminal justice cost savings						7,230

SOURCES: Marginal costs were estimated using information provided by New York City and state criminal justice agencies, New York State executive budget agencies, and Taifa (1995). See Appendix Table F.1 for more detail. For additional criminal justice data sources, see Table 3.2.

NOTES:

[a]The average length of stay in jail for a new crime is estimated by looking at the types of crimes sample members were charged with, then tying each crime type with an observed average length of stay derived from an analysis conducted by the Vera Institute's Substance Abuse and Mental Health Program using data from the New York City Department of Correction (DOC).

The average length of stay in prison for a new crime is estimated by looking at the types of crimes sample members were charged with, then tying each crime type with the average length of stay presented in "Statistical Overview: Year 2008 Discharges" published by the New York State Department of Correctional Services (DCJS).

[b]The net present value of the impacts is calculated using yearly impacts that are adjusted with a 3 percent annual discount rate. Due to rounding and discounting, it is not possible to derive the exact net present value of each impact with only this table.

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Appendix Table F.3

Number of Convictions, by Type of Offense

Center for Employment Opportunities

Type of Offense	Number of Convictions (Years 1-3)
Drug possession (misdemeanor)	250
Other crime	122
Burglary/larceny (misdemeanor)	121
Fraud and forgery (misdemeanor)	77
Simple assault (misdemeanor)	54
Drug sale (felony)	30
Burglary/larceny (felony)	30
Public-order crime	24
Drug possession (felony)	20
Stolen property/motor vehicle theft	16
Drug sale (misdemeanor)	12
Robbery (felony)	9
Aggravated assault (felony)	7
Aggravated assault (misdemeanor)	1
Fraud and forgery (felony)	1
Robbery (misdemeanor)	0
Simple assault (felony)	0
Total	783

SOURCE: MDRC calculations using data from the New York State Division of Criminal Justice Services (DCJS).

NOTES: Rounding may cause slight discrepancies in sums and differences.

This table includes convictions for both the program group and the control group.

The sample size is 973.

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Appendix Table F.4

Net Benefits and Costs (in 2009 Dollars), Not Recently Released Subgroup

Center for Employment Opportunities

	Taxpayer ($)	Victim ($)	Participant ($)	Total ($)
Benefits				
Criminal justice	542	306	0	848
Employment				
Earnings	0	0	1,562	1,562
Tax payments and credits[a]	590	0	-590	0
Value of output from CEO transitional jobs[b]	4,739	0	0	4,739
Costs				
CEO program costs[c]	-3,603	0	0	-3,603
Net benefits (per person)	2,269	306	972	3,546
Benefit-cost ratio	1.63	NA	NA	1.98

SOURCES: Marginal costs of criminal justice agencies were estimated using information provided by New York City and state criminal justice agencies, New York State executive budget agencies, and Taifa (1995). See Appendix Table F.1 for more detail. For additional employment data sources, see Table 5; for additional criminal justice data sources, see Table 6.

NOTES:

[a]Income taxes and the federal and state Earned Income Tax Credit were based on tax rules for filing year 2009.

[b]The value of output from CEO transitional jobs was calculated using information from the Department of Citywide Administrative Services.

[c]CEO program costs were calculated using CEO's financial expenditure reports for Fiscal Year 2005 and CEO's management information system (MIS), adjusted for inflation.

The Enhanced Services for the Hard-to-Employ Demonstration

Appendix Table F.5

Net Benefits and Costs (in 2009 Dollars): Sensitivity Analysis Using the Most Conservative Low-End Assumptions, Full Sample

Center for Employment Opportunities

	Taxpayer ($)	Victim ($)	Participant ($)	Total ($)
Benefits				
Criminal justice	710	432	0	1,141
Employment				
Earnings	0	0	590	590
Tax payments and credits[a]	190	0	-190	0
Value of output from CEO transitional jobs[b]	2,798	0	0	2,798
Costs				
CEO program costs[c]	-3,603	0	0	-3,603
Net benefits (per person)	95	432	400	927
Benefit-cost ratio	1.03	NA	NA	1.26

SOURCES: Marginal costs of criminal justice agencies were estimated using information provided by New York City and state criminal justice agencies, New York State executive budget agencies, and Taifa (1995). See Appendix Table F.1 for more detail. For additional employment data sources, see Table 3.1; for additional criminal justice data sources, see Table 3.2.

NOTES: The "extreme low" scenario assumes a low-end prison marginal cost ($10,047 per bed per year), a low-end jail marginal cost ($5,000 per bed per year), and a low-end value of work for transitional jobs ($2,798, which equals the wages actually paid to CEO participants and supervisors).

[a]Income taxes and the federal and state Earned Income Tax Credit were based on tax rules for filing year 2009.

[b]The value of output from CEO transitional jobs was calculated using information from the Department of Citywide Administrative Services.

[c]CEO program costs were calculated using CEO's financial expenditure reports for Fiscal Year 2005 and CEO's management information system (MIS), adjusted for inflation.

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Appendix Table F.6

Net Benefits and Costs (in 2009 Dollars): Sensitivity Analysis Using the Most Conservative Low-End Assumptions, Recently Released Subgroup

Center for Employment Opportunities

	Taxpayer ($)	Victim ($)	Participant ($)	Total ($)
Benefits				
Criminal justice	1,718	882	0	2,599
Employment				
Earnings	0	0	1,200	1,200
Tax payments and credits[a]	157	0	-157	0
Value of output from CEO transitional jobs[b]	2,798	0	0	2,798
Costs				
CEO program costs[c]	-3,603	0	0	-3,603
Net benefits (per person)	1,070	882	1,043	2,995
Benefit-cost ratio	1.30	NA	NA	1.83

SOURCES: Marginal costs of criminal justice agencies were estimated using information provided by New York City and state criminal justice agencies, New York State executive budget agencies, and Taifa (1995). See Appendix Table F.1 for more detail. For additional employment data sources, see Table 3.1; for additional criminal justice data sources, see Table 3.2.

NOTES: The "extreme low" scenario assumes a low-end prison marginal cost ($10,047 per bed per year), a low-end jail marginal cost ($5,000 per bed per year), and a low-end value of work for transitional jobs ($2,798, which equals the wages actually paid to CEO participants and supervisors).

[a]Income taxes and the federal and state Earned Income Tax Credit were based on tax rules for filing year 2009.

[b]The value of output from CEO transitional jobs was calculated using information from the Department of Citywide Administrative Services.

[c]CEO program costs were calculated using CEO's financial expenditure reports for Fiscal Year 2005 and CEO's management information system (MIS), adjusted for inflation.

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