

**Transitional Jobs for Ex-Prisoners**  
**Implementation, Two-Year Impacts, and Costs of**  
**the Center for Employment Opportunities (CEO)**  
**Prisoner Reentry Program**

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## Overview

Almost 700,000 people are released from state prisons each year. Ex-prisoners face daunting obstacles to successful reentry into society, and rates of recidivism are high. Most experts believe that stable employment is critical to a successful transition, but ex-prisoners have great difficulty finding steady work.

This report presents interim results from a rigorous evaluation of the New York City-based Center for Employment Opportunities (CEO), a highly regarded employment program for ex-prisoners. CEO participants are placed in paid transitional jobs shortly after enrollment; they are supervised by CEO staff and receive a range of supports. Once they show good performance in the transitional job, participants get help finding a permanent job and additional support after placement.

CEO is one of four sites in the Enhanced Services for the Hard-to-Employ Demonstration and Evaluation Project, which is sponsored by the Administration for Children and Families and 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 project is being conducted under contract to HHS by MDRC, a nonprofit research organization, along with the Urban Institute and other partners.

The impacts of CEO's program are being assessed using a rigorous research design. In 2004-2005, a total of 977 ex-prisoners who reported to CEO were assigned, at random, to a program group that was eligible for all of CEO's services or to a control group that received basic job search assistance. So far, the two groups have been followed for two years after study entry.

## Key Findings

- **CEO's program operated smoothly during the study period, and most program group members received the core services.** More than 70 percent of the program group worked in a transitional job; the average length of that employment was about eight weeks.
- **CEO generated a large but short-lived increase in employment; the increase was driven by CEO's transitional jobs.** By the end of the first year of the study period, the program and control groups were equally likely to be employed, and their earnings were similar.
- **CEO reduced recidivism during both the first and the second year of the study period.** The program group was significantly less likely than the control group to be convicted of a crime, to be admitted to prison for a new conviction, or to be incarcerated for any reason in prison or jail during the first two years of the study period. In Year 1, CEO reduced recidivism only for those who came to the program within three months after their release from prison; in Year 2, however, the program reduced recidivism both for recently released study participants and for those who were not recently released at study entry.

The study will follow the two groups for a third year, but the results so far show that CEO's program reduced recidivism, even after the employment gains faded. Decreases in recidivism have rarely been found in rigorous evaluations. Further research is needed to identify approaches that can produce more sustained increases in employment and earnings for ex-prisoners.



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The Authors

## **Executive Summary**

This report presents interim results from a rigorous evaluation of the Center for Employment Opportunities (CEO), one of 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 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. It is being conducted under contract to HHS by MDRC, a nonprofit, nonpartisan research organization, along with the Urban Institute and other partners.

### **The Policy Context: Prisoner Reentry and Transitional Work**

The number of people incarcerated in the United States has increased more than four-fold since the 1970s. Today, more than 2 million people are incarcerated in federal and state prisons and local jails, and almost 700,000 people are released from state prisons each year. Corrections costs exceed \$65 billion per year, with most of this total being borne by state and local governments.

Ex-prisoners face daunting obstacles to successful reentry — for example, difficulties finding jobs, housing, and services for substance abuse or mental health problems — and rates of recidivism are very high. The most recent national statistics show that two-thirds of ex-prisoners are rearrested and that half are reincarcerated within three years of release.

Most experts believe that stable employment is critical to a successful transition 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. 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 with criminal records.

Very little is known about effective employment strategies for ex-prisoners, but many people see transitional jobs as a promising model. Transitional job programs rapidly place participants into temporary, paid jobs, usually in nonprofit or government agencies; provide various kinds of support; and then help participants find permanent jobs. When targeted to ex-prisoners, transitional jobs give participants a source of legitimate income during the critical

period after release from prison, and they also provide staff with an opportunity to identify and address workplace problems before participants move to the regular labor market.

## **The Center for Employment Opportunities**

The Center for Employment Opportunities was created by the Vera Institute of Justice in the 1970s to provide immediate, paid work to individuals returning to New York City after being released from prison. CEO became an independent nonprofit organization in 1996 and currently serves about 2,000 parolees each year.

Paid, temporary employment still forms the core of CEO's model. Individuals who are referred to CEO by their parole officer are placed in paid jobs immediately after completing a four-day preemployment class that teaches job readiness skills. Participants work in crews, performing maintenance and repair work for city and state agencies at several dozen sites around New York City. They report to CEO work site supervisors.

Participants generally work four days a week and are paid the minimum wage (\$7.25 per hour in New York State as of July 24, 2009, but it ranged from \$5.15 to \$6.00 per hour during the study period); they receive paychecks each afternoon, at their work sites. The fifth day of each week is spent in CEO's office in lower Manhattan, where participants meet with CEO staff and take part in supplementary activities, such as an extensive fatherhood program that helps them resolve child support issues and improve their family relationships.

Participants are continuously evaluated by their work site supervisors, and both supervisors and office-based job coaches (employment-focused case managers) seek to identify and address workplace problems. Once participants have demonstrated good performance on the job, they are deemed "job ready" and begin working with a job developer to find permanent employment. CEO continues to provide support for up to one year after placement.

## **The CEO Evaluation**

The impacts of CEO's program are being assessed using random assignment, which is generally considered the most reliable research design for evaluations of this type. Between January 2004 and October 2005, a total of 977 ex-prisoners who were referred to CEO by their parole officer, who reported to the program, and who met the study's eligibility criteria were randomly assigned to one of two groups:<sup>1</sup>

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<sup>1</sup>The MDRC team worked with CEO and the New York State Division of Parole to design a random assignment process that ensured that the study did not decrease the number of people who received transitional  
(continued)

- **Program group (total = 568).** Individuals who were randomly assigned to this group were eligible for all of CEO’s program services, including the four-day preemployment class, placement in a transitional job, job coaching, additional services (such as the fatherhood program), job development services, and postplacement services.
- **Control group (total = 409).** Individuals who were randomly assigned to this group started with a condensed version of the preemployment class that lasted one and a half days and then 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. When clients came into the resource room, a staff person was available to assist them with many aspects of job search.

The research team is tracking both groups for three years using administrative records and a survey of participants. Any significant differences between the groups that emerge over time are described as “impacts,” or “effects,” of CEO because, owing to the random assignment design, one can be confident that the groups were comparable when they entered the study.

Three points about the design are critical. First, in designing the study, the MDRC team, 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 with similar services that control group members might find if they were sent away. Nevertheless, as discussed further below, control group (and program group) members were free to seek out other services in the community, and many did so.

Second, the individuals who were targeted for the study — parolees who were referred to CEO by their parole officer — represent a subset of the parolees that CEO serves. For example, everyone who graduates from New York State’s Shock Incarceration (boot camp) program and returns to New York City is required to participate in CEO’s program. For contractual reasons, boot camp graduates and participants in some other special programs were not included in the study. CEO provides very similar services to all these groups of parolees.

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job slots and that ensured that CEO had enough participants to fill its contractual obligations to the sponsors of transitional job work sites. Random assignment was conducted only during weeks when the number of new enrollees exceeded the number of available slots at transitional job work sites.

Third, during the study period, CEO engaged in a large-scale program review and business planning process sponsored by a private foundation. This process resulted in several program enhancements, particularly in the assistance provided to participants after they had been placed in jobs. Because these changes were phased in during the study period, they did not affect many of the study participants. Thus, the results might be different if the study were conducted today and all participants received the enhanced services.

### **Characteristics of the Study Participants**

The vast majority of sample members are male, and almost all are black or Hispanic. Fifty-seven percent were over 30 years old when they entered the study. About half of the study sample had at least one child under age 18, although only 16 percent lived with any of their children. Just over half the sample had a high school diploma or a General Educational Development (GED) certificate, and 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.

Study participants had been arrested an average of eight times and had been convicted an average of seven times. More than half had been convicted of a violent offense, and nearly three-quarters of the sample had prior drug offense convictions. On average, sample members had spent five years in state prison over their lifetime.

Interestingly, only 41 percent of the study sample came to CEO within three months after release from prison. 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. For example, those excluded from the study — boot camp graduates and participants in some other special programs — always come to CEO just after release. Those in the study sample came to CEO after referral by a parole officer who is not involved with special programs. These officers base their referral decisions on many factors. For instance, some parole officers may have referred parolees to CEO after they had worked for a time and then lost a job.

### **Program Implementation and Service Receipt**

The study assessed CEO's implementation using site visits to the program and its work sites, interviews with staff and participants, and data from CEO's management information system.

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



CEO is a well-managed program that largely operates as designed. Program tracking and payroll data show that almost 80 percent of the program group completed the initial four-day preemployment class and that more than 70 percent worked in a transitional job. The average length of transitional employment was about eight weeks. The large majority of program group members who worked in a transitional job also met with CEO job coaches and job developers. About 40 percent of those who worked in a transitional job (30 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 several kinds of employment services, but many control group members got help at CEO or elsewhere; some control group members worked in transitional jobs at other organizations.**

As noted earlier, two critical elements of the CEO model were generally available to the program group but not to the control group: (1) transitional jobs and associated job coaching and (2) assistance from CEO job developers. However, control group members could receive other kinds of employment help from CEO. Moreover, it was expected that sample members from both 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 certain kinds of employment services.

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. 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 receiving advice about job interviews or help with résumés, the differences between groups were 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.

### **Impacts on Employment and Earnings**

CEO's impacts on employment and earnings were assessed using data from the New York State Department of Labor and the National Directory of New Hires — both of which track quarterly earnings in jobs covered by unemployment insurance (UI) — as well as data from the client survey.

- **CEO generated a large but short-lived increase in UI-covered employment; the increase was driven by access to CEO transitional jobs.**

Figure ES.1 shows the percentage of program and control group members who worked in UI-covered jobs in each quarter during the two-year follow-up period. (CEO's transitional jobs are covered by unemployment insurance and thus are counted as employment.) During the first quarter after study entry, the difference between groups was very large: 66 percent of the program group worked, compared with 26 percent of the control group. By the fourth quarter, however, after most program group members had left the CEO transitional jobs, the difference between groups was no longer statistically significant. There were few statistically significant differences in earnings, or in job characteristics as measured by the client survey.

### **Impacts on Recidivism**

CEO's impacts on recidivism were measured using data from the New York State Division of Criminal Justice Services and the New York City Department of Correction. These data show all arrests and convictions in New York State plus admissions to both New York State prisons and New York City jails.

- **CEO reduced recidivism during both the first and the second year of the study period; different measures of recidivism were affected in each year.**

As shown in Table ES.1, over the full two-year follow-up period, the program group was significantly less likely than the control group to be convicted of a crime, to be admitted to state prison for a new crime conviction, and to be incarcerated for any reason in prison or jail. These findings are noteworthy, as impacts on recidivism have rarely been found in rigorous evaluations of programs for ex-prisoners.

Although CEO's employment impacts faded in Year 1, there were reductions in recidivism in both Year 1 and Year 2. The pattern of results is complex, however, and different measures of recidivism were affected in each year. For example, there was a reduction in arrests in Year 2 but not in Year 1, and there was a reduction in felony convictions in Year 1 but not in Year 2. (These results are not shown in the table.)

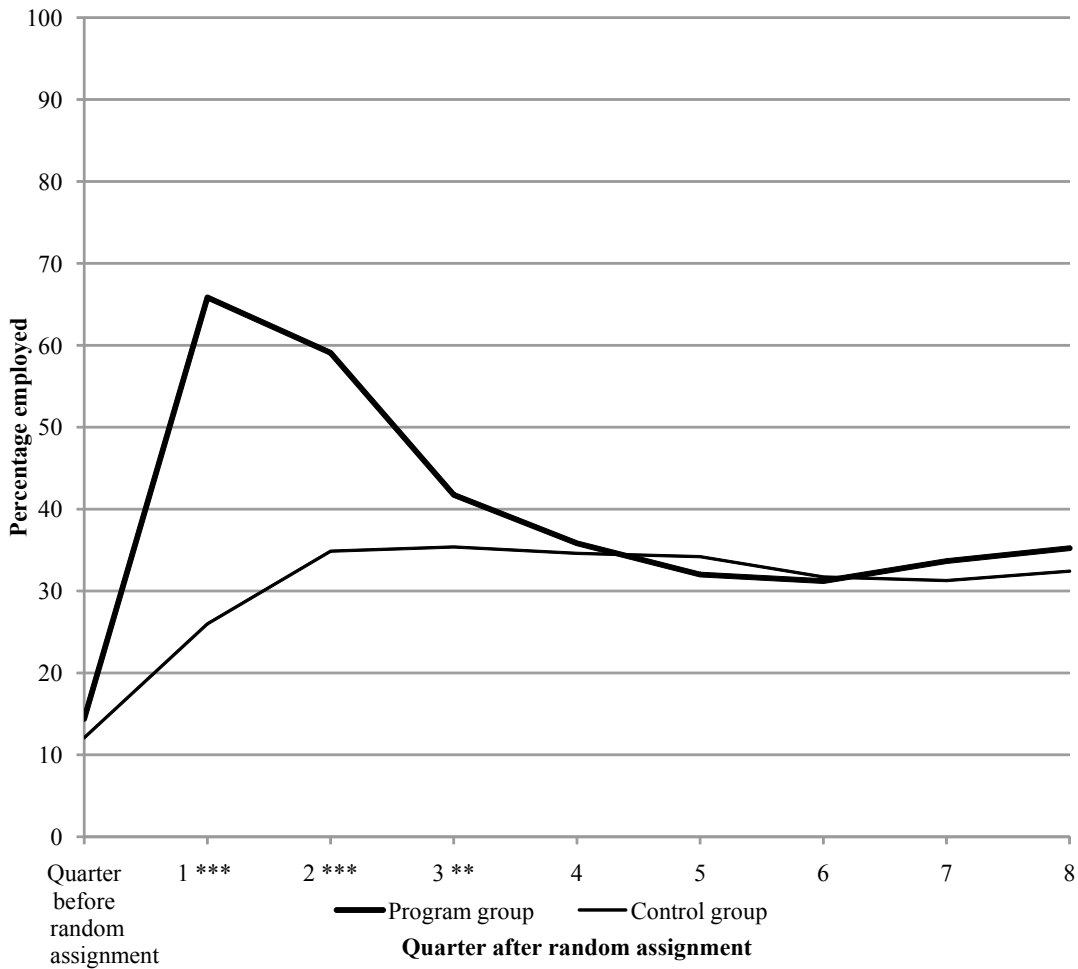
- **CEO reduced recidivism both for sample members who enrolled in the study shortly after leaving prison and for those who were not recently released at study entry.**

An earlier report from this study shows that, in Year 1 of the study period, CEO produced large decreases in key recidivism measures for individuals who entered the study within three months after release from prison but that it had no impacts on recidivism for those who

The Enhanced Services for the Hard-to-Employ Demonstration

Figure ES.1

Quarterly Impacts on Employment  
Center for Employment Opportunities



SOURCES: MDRC calculations from the National Directory of New Hires (NDNH) database and 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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

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

**The Enhanced Services for the Hard-to-Employ Demonstration**

**Table ES.1**

**Two-Year Impacts on Recidivism**

**Center for Employment Opportunities**

Outcome	Program Group	Control Group	Difference (Impact)	P-Value
Arrested <sup>a</sup> (%)	37.7	41.8	-4.1	0.192
Convicted of a crime <sup>b</sup> (%)	30.5	38.3	-7.7 **	0.011
Convicted of a felony	6.6	7.7	-1.1	0.536
Convicted of a misdemeanor	22.8	29.4	-6.6 **	0.019
Convicted of a violent crime <sup>c</sup> (%)	4.4	5.8	-1.5	0.309
Incarcerated <sup>d</sup> (%)	49.5	55.4	-5.9 *	0.064
Prison	25.3	27.7	-2.4	0.399
Jail	49.1	53.5	-4.4	0.168
Incarcerated for a new crime (%)	13.9	16.9	-2.9	0.212
Prison	4.2	6.8	-2.6 *	0.077
Jail	10.1	10.8	-0.7	0.718
Incarcerated for a technical parole violation (%)	31.8	28.9	2.9	0.340
Prison	17.4	15.5	1.9	0.428
Jail	29.1	25.6	3.5	0.234
Total days incarcerated	100	107	-7	0.500
Prison	46	54	-8	0.244
Jail	54	53	1	0.828
Arrested, convicted, or incarcerated (%)	55.7	62.5	-6.8 **	0.032
<b><u>Status in the last quarter of Year 2<sup>e</sup> (%)</u></b>				
Incarcerated and employed	3.5	1.9	1.6	0.135
Incarcerated and not employed	21.4	25.0	-3.7	0.177
Not incarcerated and employed	29.3	26.9	2.4	0.404
Not incarcerated and not employed	45.9	46.3	-0.4	0.911
Sample size (total = 977)	568	409		

(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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>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.

<sup>b</sup>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. The total includes convictions for "other" reasons, felonies, and misdemeanor crimes.

<sup>c</sup>Violent crimes are based on conviction charges defined by Langan and Levin, *Recidivism of Prisoners Released in 1994*, Special Report NCJ 193427 (Washington, DC: U.S. Department of Justice, Office of Justice Programs, Bureau of Justice Statistics, 2002). See Appendix C for charges defined as violent.

<sup>d</sup>Includes all reasons for incarceration, such as sentences for new crimes, technical violations of parole, detainee (jail), and other reasons. Therefore, incarcerations for new crimes and parole violations do not sum to the percentage incarcerated. See Appendix Table C.1 for more details about reasons for incarceration.

<sup>e</sup>Incarceration status based on Quarter 8 after random assignment. Incarceration includes both prison and jail.

entered the study further from the point of release.<sup>1</sup> As shown in Table ES.2, this pattern was much less clear in Year 2. For example, among those who entered the study more than three months after release, CEO reduced felony convictions and incarceration for new crimes in Year 2. At the same time, decreases in recidivism were not as consistent in Year 2 for the recently released sample members; for example, although there was a statistically significant decrease in violent crime convictions, there was a significant increase in felony convictions.

### **Program Costs**

Using fiscal and program tracking data from CEO and survey data on service receipt, the study estimated the net cost per program group member — that is, the cost of CEO services over and above the cost of services received by the control group.

- **The cost of all employment and training services was \$3,200 more per program group member than the cost of services for the average control group member.**

The gross cost of the CEO program was estimated to be about \$4,263 per program group member, which includes nearly \$1,000 in direct payments to participants. More than half

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<sup>1</sup>Dan Bloom, Cindy Redcross, Janine Zweig, and Gilda Azurdia, “Transitional Jobs for Ex-Prisoners: Early Impacts from a Random Assignment Evaluation of the Center for Employment Opportunities (CEO) Prisoner Reentry Program” (New York: MDRC, 2007).

The Enhanced Services for the Hard-to-Employ Demonstration

Table ES.2

Impacts on Selected Measures of Recidivism in Year 1 and Year 2,  
by Time Between Prison Release and Random Assignment

Center for Employment Opportunities

Outcome	Length of Time Between Prison Release and Random Assignment								H-Stars <sup>a</sup>
	Less Than 3 Months				More Than 3 Months				
	Program Group	Control Group	Difference (Impact)	P-Value	Program Group	Control Group	Difference (Impact)	P-Value	
Arrested <sup>b</sup> (%)									
Year 1	20.8	24.2	-3.4	0.429	24.0	22.1	1.9	0.612	
Year 2	25.8	33.7	-7.8	0.109	20.0	24.5	-4.5	0.222	
Convicted of a felony (%)									
Year 1	1.4	4.8	-3.4 *	0.051	1.3	2.3	-1.1	0.366	
Year 2	11.3	4.2	7.1 **	0.020	1.6	4.5	-2.9 **	0.048	***
Convicted of a misdemeanor (%)									
Year 1	9.5	11.9	-2.4	0.461	14.2	12.5	1.7	0.578	
Year 2	13.2	25.5	-12.3 ***	0.003	16.3	19.9	-3.7	0.280	
Convicted of a violent crime <sup>c</sup> (%)									
Year 1	2.5	1.3	1.2	0.432	1.6	1.5	0.1	0.937	
Year 2	1.7	7.0	-5.3 **	0.015	3.0	2.9	0.1	0.951	**
Admitted to state prison (%)									
Year 1	9.4	19.5	-10.1 ***	0.005	12.8	11.6	1.3	0.658	**
Year 2	21.1	19.0	2.2	0.616	15.8	16.4	-0.6	0.846	
Incarcerated in prison for a new crime (%)									
Year 1	0.5	4.9	-4.4 ***	0.005	1.1	2.1	-1.0	0.365	*
Year 2	6.6	3.2	3.4	0.164	1.4	4.2	-2.8 *	0.051	**
Incarcerated in prison for a technical parole violation (%)									
Year 1	7.2	12.1	-4.9	0.119	9.1	8.6	0.5	0.840	
Year 2	12.7	11.3	1.5	0.681	11.7	7.8	3.9	0.124	

(continued)

ES-10

**Table ES.2 (continued)**

Outcome	Length of Time Between Prison Release and Random Assignment								H-Stars <sup>d</sup>
	Less Than 3 Months				More Than 3 Months				
	Program Group	Control Group	Difference (Impact)	P-Value	Program Group	Control Group	Difference (Impact)	P-Value	
Total days incarcerated in prison									
Year 1	10	20	-10 **	0.036	14	9	4	0.184	**
Year 2	41	56	-15	0.132	33	31	2	0.793	
Sample size (total = 929) <sup>d</sup>	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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>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.

<sup>b</sup>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.

<sup>c</sup>Violent crimes are based on conviction charges defined by Langan and Levin, *Recidivism of Prisoners Released in 1994*, Special Report NCJ 193427 (Washington, DC: U.S. Department of Justice, Office of Justice Programs, Bureau of Justice Statistics, 2002). See Appendix C for charges defined as violent.

<sup>d</sup>A total of 48 sample members are missing the last prison release date and are therefore missing from all outcomes in this table.

of this total stemmed from costs associated with the transitional jobs and wages paid to participants. When the total cost of all services received by the control group — both in and out of CEO — is subtracted from the total cost of all services received by the program group, the net cost is \$3,195 per person. Although the study does not include a formal benefit-cost analysis, CEO is already producing offsetting budgetary savings by reducing recidivism. For example, CEO reduced the number of days in prison, and given the high expenditures by government agencies on incarceration, any reduction in prison days could translate into substantial cost savings.

## **Conclusions and Implications**

Although the prisoner reentry issue has attracted considerable attention in recent years, there is very little evidence about which programmatic strategies are effective in reducing recidivism and promoting successful transitions from prison to the community. CEO is one of the only major programs for ex-prisoners that has been subject to a rigorous evaluation using a random assignment design, so this evaluation is providing much-needed evidence to inform the field.

So far, the results are mixed. On the one hand, CEO reduced several key measures of recidivism. This is a noteworthy result, since few programs have been found in rigorous studies to decrease recidivism. On the other hand, while CEO's transitional jobs generated a very large increase in formal employment in the early months of the study period — suggesting that subsidized jobs may be necessary to substantially increase employment for ex-prisoners — there is no evidence that the program increased employment or earnings in the long term. Interestingly, the recidivism impacts continued even after the employment impacts faded.

The study will continue to track employment and recidivism outcomes for another year, but some preliminary conclusions are possible at this point. First, the results suggest that while there is a link between employment and recidivism, this connection is not straightforward. In Year 1, CEO increased employment for both recently released and not recently released sample members, but it reduced recidivism only for those who were recently released. In Year 2, CEO generated reductions in recidivism for both groups, even though the employment impacts had long since faded. Future analyses will seek to understand more about how CEO produced impacts on recidivism — for example, by investigating the links among outcomes, individual characteristics, and CEO participation patterns for program group members.

Second, while the results suggest that subsidized jobs may be needed to substantially boost employment for ex-prisoners, they also suggest that short-term transitional jobs do not necessarily lead to long-term increases in unsubsidized employment. It is notable that less than half of the program group members who worked in a CEO transitional job were placed in



permanent employment. Given the sharp drop in the program group's employment rate over time, it may be worth considering models that offer subsidized jobs over a longer period, use earnings supplements to encourage sustained employment in low-paying unsubsidized jobs, provide training to try to raise ex-prisoners' skill levels so that they can qualify for high-paying jobs, or take other approaches.

## Chapter 1

# Introduction

This report presents interim results from a rigorous evaluation of the New York City-based Center for Employment Opportunities (CEO), one of 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 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. 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 four-fold since the 1970s.<sup>1</sup> Today, more than 2 million people are incarcerated in federal and state prisons and local jails, and almost 700,000 people are released from state prisons each year. Corrections costs exceed \$65 billion per year, with most of this total being borne by state and local governments.<sup>2</sup>

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 are concentrated in 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. The most recent national statistics show that two-thirds of ex-prisoners are rearrested and that half are reincarcerated within three years of release.<sup>3</sup> The rate of recidivism is slightly lower in New York State but is still of great concern. Of the nearly 25,000 people released from New York State prisons in 2004, about 40 percent returned to prison within three years — 11 percent

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<sup>1</sup>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).

<sup>2</sup>Bureau of Justice Statistics (2007).

<sup>3</sup>Langan and Levin (2002).

for a new crime and 29 percent for a “rule violation,” such as a failure to comply with parole conditions.<sup>4</sup>

Most experts believe that stable employment is critical to a successful transition 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. 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 quite reluctant to hire people with criminal records.<sup>5</sup> At the end of 2007, only 12 percent of New York State parolees who were available for work were employed full time and earning more than the minimum wage; 49 percent were not employed at all.<sup>6</sup>

Very little is known about effective employment strategies for ex-prisoners, but many people see transitional jobs as a promising model.<sup>7</sup> This is the latest in a long line of subsidized employment models that have been targeted to hard-to-employ individuals. Transitional job programs rapidly place participants into temporary, paid jobs, usually in nonprofit or government agencies; provide various kinds of support (for example, 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 period after release from prison, and they also provide staff with an opportunity to identify and address workplace problems before participants move to the regular labor market.<sup>8</sup>

## **The Center for Employment Opportunities**

The Center for Employment Opportunities was developed by the Vera Institute of Justice in the 1970s to provide immediate, paid work to individuals returning to New York City after being released from prison. CEO became an independent nonprofit organization in 1996 and currently serves about 2,000 parolees each year.

Paid, temporary employment still forms the core of CEO’s model. Individuals who are referred to CEO by their parole officer are placed in paid jobs immediately after completing a

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<sup>4</sup>New York State Division of Criminal Justice Services (2008). The three-year recidivism rates are similar for those released in 1997, 2000, and 2004. However, the percentage who returned to prison for a new crime fell over time, while the percentage who returned for a parole violation increased.

<sup>5</sup>Holzer, Raphael, and Stoll (2003); Pager (2003).

<sup>6</sup>New York State Division of Criminal Justice Services (2008). These figures are not directly comparable to the outcome data for study participants presented later in this report.

<sup>7</sup>For a review of research on employment-focused programs for ex-prisoners, see Bloom (2006).

<sup>8</sup>For more information on the transitional job model, see the National Transitional Jobs Network Web site: [www.transitionaljobs.net](http://www.transitionaljobs.net).

four-day preemployment class. Participants work in crews, performing maintenance and repair work for city and state agencies at several dozen sites around New York City. They report to CEO work site supervisors.

Participants generally work four days a week and are paid the minimum wage (\$7.15 per hour in New York State as of July 24, 2009, but it ranged from \$5.15 to \$6.00 per hour during the study period); they receive paychecks each afternoon, at their work sites. The fifth day each week is spent in CEO's office in lower Manhattan, where participants meet with staff and take part in supplementary activities, such as a fatherhood program.

Participants are continuously evaluated by their work site supervisors. Both supervisors and office-based job coaches (employment-focused case managers) seek to identify and address workplace problems. Once participants have demonstrated good performance on the job, they are deemed "job ready" and begin working with a job developer to find permanent employment. CEO continues to provide support after placement, though, as discussed further in the next section, the nature and intensity of those postemployment services changed during the period of the study.

The CEO model is designed on the assumption that helping ex-prisoners find jobs — particularly during the period immediately after release — will ultimately lead to lower recidivism. In the short term, the program raises 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. Although CEO offers some supplementary services, such as the fatherhood program, it maintains a sharp focus on employment. Job coaches typically refer participants to other organizations for help with housing, substance abuse, or other issues.

## **The CEO Evaluation**

The impacts of CEO's program 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, who reported to the program, and who met the study's eligibility criteria,<sup>9</sup> were randomly assigned to one of two groups:<sup>10</sup>

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<sup>9</sup>For both ethical and methodological reasons, individuals who had worked in a CEO transitional job in the past year ("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.

<sup>10</sup>The MDRC team worked with CEO and the New York State Division of Parole to design a random assignment process that ensured that the study did not decrease the number of people who received transitional  
(continued)

- **Program group (total = 568).** Individuals who were randomly assigned to this group were eligible for all of CEO’s program services, including the four-day preemployment class, placement in a transitional job, job coaching, additional services (such as the fatherhood program), job development services, and postplacement 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 then 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 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 the participants.<sup>11</sup>

The research team is tracking both groups for three years using a number of data sources (described below). Any significant differences between the groups that emerge over time are described as “impacts,” or “effects,” of CEO because, owing to the random assignment design, one can be confident that the groups were comparable when they entered the study.<sup>12</sup>

Three points about the design are critical. First, 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 with similar services that

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job slots and that ensured that CEO had enough participants to fill its contractual obligations to the sponsors of transitional job work sites. Random assignment was conducted only during weeks when the number of new enrollees exceeded the number of available slots at transitional job work sites.

<sup>11</sup>If control group members worked diligently in the resource room for three months but were unable to find employment on their own, they were offered CEO’s job development services (but not a transitional job), but this happened very rarely. Of course, control group members could — and did — seek assistance from other employment programs in the community. (See Chapter 3.)

<sup>12</sup>Since the proportion of people randomly assigned to the program group varied from week to week, weights were used so that the impact results were not dominated by a disproportionate assignment to one group or the other in any given week. In particular, program group members received a smaller weight in the impact calculation if they entered the study in a week when a higher proportion of individuals were assigned to the program group, and vice versa. The same was true for control group members. In addition, following standard practice in studies such as this, the estimates were regression-adjusted using ordinary least squares, controlling for the characteristics of sample members before random assignment.

control group members might find if they were simply sent away. Nevertheless, as discussed further below, sample members 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.

Second, it is important to note that the individuals who were targeted for the study — parolees who were referred to CEO by their parole officer — represent a subset of the parolees that CEO serves. For example, everyone who graduates from New York State’s Shock Incarceration (boot camp) program and returns to New York City is required to participate in CEO’s program. Because all boot camp graduates are mandated to participate in 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. CEO provides very similar services to all these groups of parolees.

Third, during the study period, CEO engaged in a large-scale program review and business planning process sponsored by a private foundation. As discussed further in Chapter 2, this process resulted in several program enhancements, particularly in the program’s job placement services and assistance for participants after they had been placed in jobs. The plan was officially implemented in July 2005 — just a few months before random assignment ended — though some of its components began earlier. Because these program enhancements were phased in during the study period, they affected only a subset of the study participants. If the new services resulted in better outcomes, then the results of the study might have been different if it had been conducted later.

### **Data Sources Used in This Analysis**

The analysis in this report uses data 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.
- **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 in-

carceration in state prison — for each member of the study sample, both before and after study entry.<sup>13</sup>

- **Local criminal justice data.** These data, provided by the New York City Department of Correction, show admissions and releases from New York City jails.
- **Child support data.** The New York State Office of Temporary and Disability Assistance, which administers the state’s child support enforcement program, provided data on formal child support orders and payments.
- **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. These data cover the period from three years prior to study entry to two years after study entry for all sample members.<sup>14</sup>
- **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 the person was eligible, including transitional jobs and appointments with job coach and job developer.
- **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, hous-

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<sup>13</sup>DCJS provided MDRC with three SPSS (“Statistical Package for the Social Sciences”) portable files matched to a sample request file using New York State Identification (NYSID) Numbers: an arrest, a parole, and a corrections file. Each file includes information about unsealed events only. The arrest file includes data from the DCJS rap sheet system, including arrests, convictions, and sentencing. The corrections file contains information furnished to DCJS from the New York State Department of Corrections, including state prison admissions and releases (with a small number of records on jail stays). The parole file contains information furnished to DCJS from the New York State Division of Parole, including parole term start and end dates, abscond dates, and parole revocation information. The arrest and corrections files contain data reported to DCJS from January 1970 to present, and the parole file contains data reported to DCJS from January 1960 to present. In addition, DCJS provided a separate file containing data on sealed events; that file was provided in a form that did not allow individuals to be identified. DCJS takes no responsibility for the methods of statistical analysis or for the conclusions derived from the analysis.

<sup>14</sup>The analysis uses data from both the NDNH and the New York State Department of Labor. The Department of Labor provided individual-level data on employment for all sample members from 2001 through 2007. It did not, however, provide individual-level data on quarterly earnings. The NDNH includes individual-level data on both employment and earnings but covers only the period from Quarter 4, 2004, through Quarter 3, 2007.

ing, drug treatment, family relationships, and other issues. The response rate for the survey was about 68 percent.<sup>15</sup>

- **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.

At this point, data from all the administrative records are available for at least two years after study entry for all sample members. The study will eventually follow sample members for three years.

### **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.<sup>16</sup>

The vast majority of sample members are male (93 percent).<sup>17</sup> Almost all are black or Hispanic. (Nationally, about one-third of those released to parole are white; in New York City, fewer than 10 percent of parolees are white.) On study entry, 57 percent of the sample members were over 30 years old, and the average age was 34. (Nationally, the average age of prisoners being released in recent years is 34; about two-thirds of New York City parolees are over age 30.)

Before random assignment, 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 lived with any of their children. Only 19 percent reported having a formal child support order in place.

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<sup>15</sup>The survey targeted 777 of the 977 sample members. See Appendix A for further information.

<sup>16</sup>National data from the Bureau of Justice Statistics (2002); state data from the New York State Division of Parole (2008).

<sup>17</sup>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 4 through 6 are adjusted to account for this difference. Also, all results were examined for males separately, and the results are very similar to those presented.



**The Enhanced Services for the Hard-to-Employ Demonstration**

**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 employed (%)	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 <sup>a</sup> (%)	14.9	11.7	13.6
UI-covered employment in the year prior to random assignment <sup>a</sup> (%)	24.1	24.0	24.0
Sample size	568	409	977

SOURCES: MDRC calculations using data from the 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. The significance level indicates the probability that one would be making an error in concluding that there is a difference between research groups for the variable in question.

<sup>a</sup>This measure was created using data from unemployment insurance (UI) wage records from New York State.

The process of obtaining affordable housing on release from prison is complicated because most returning prisoners do not have income from employment and are not eligible for many other forms of public assistance. In addition, current federally subsidized housing programs give public housing authorities the power to deny housing or to terminate the leases of individuals with a history of drug use or criminal behavior. Thus, 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 a high school diploma or a General Educational Development (GED) certificate at baseline, and a very small proportion (4 percent) had a postsecondary degree. 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 and the control group before random assignment and including the offense for which they were recently released from prison.<sup>18</sup> Again, there are few differences between the groups. As expected, all the study participants have a history of arrest, conviction, and incarceration. The average lifetime total time spent in state prison for study participants is about 60 months, or five years.

Before random assignment, study participants had been arrested an average of eight times — similar to the 1994 national release cohort<sup>19</sup> — with about five felony arrests and three misdemeanor arrests. Before random assignment, 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 violent offenses. Nearly three-quarters of the sample had prior drug offense convictions.

Table 1.2 also shows the time between the last prison release and the date of random assignment. These data confirm that only 41 percent of the study sample came to CEO within three months after 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.

As noted above, boot camp graduates 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 is not involved with special programs. These parole officers base their referral decisions on a wide variety of concerns and circumstances. For example, some parole officers may have referred parolees to CEO after they had worked for a time and then lost a job. Because the CEO model was designed to serve ex-prisoners just after release, and because most of CEO's broader population fits this profile, the results presented in this report are examined separately for people who came to CEO soon after release and for those who came later.

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<sup>18</sup>These data include incarceration in state prison but not local jails.

<sup>19</sup>Langan and Levin (2002).

**The Enhanced Services for the Hard-to-Employ Demonstration**

**Table 1.2**

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

**Center for Employment Opportunities**

Characteristic	Program Group	Control Group	Full Sample
<b><u>Arrest history</u></b>			
Any prior arrests (%)	100	100	100
Average number of arrests <sup>a</sup>	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 <sup>b</sup> (%)	67.5	67.5	67.5
Number of prior arrests for a violent crime	1.4	1.4	1.4
<b><u>Conviction history</u></b>			
Any prior conviction <sup>c</sup> (%)	100	100	100
Average number of prior convictions <sup>d</sup>	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
<b><u>State prison history</u></b>			
Lifetime number of months in state prison <sup>e</sup>	60.6	59.1	60.0
Months between latest state prison release and random assignment <sup>f</sup> (%)			
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
<b><u>Parole</u></b>			
Months remaining on parole	34.2	32.9	33.6 **
Sample size	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. The significance level indicates the probability that one would be making an error in concluding that there is a difference between research groups for the variable in question.

Due to missing data, seven sample members are missing prior criminal histories.

Sample sizes vary from 924 to 970 due to missing data.

Prior criminal history includes the arrest, conviction, and incarceration related to the offense for which one was on parole for at the time of random assignment (the current offense).

<sup>a</sup>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.

<sup>b</sup>The violent crime indicator is defined by Penal Law 70.02 and also includes underlying offenses.

<sup>c</sup>This outcome excludes convictions where a final disposition was not found.

<sup>d</sup>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.

<sup>e</sup>"Lifetime" includes historical data as early as 1970.

<sup>f</sup>A total of 48 sample members are missing the latest prison release date and are excluded from this measure.

## Roadmap of the Report

The remainder of the report is organized as follows:

Chapter 2 describes the implementation of CEO's program during the study period and presents data on what proportion of the program group received the core program components.

Chapter 3 describes the differences in service receipt between the two research groups, including both services provided by CEO and those provided by other organizations; the analysis uses data from the client survey.

Chapters 4 through 6 discuss the CEO program's impacts. Chapter 4 describes the impacts on employment; Chapter 5 presents impacts on recidivism; and Chapter 6 discusses impacts on a variety of other outcomes.

Chapter 7 discusses the net costs of the CEO program.

## Chapter 2

# The Implementation of the CEO Program

The New York City-based Center for Employment Opportunities (CEO) is one the largest and most highly regarded employment programs for former prisoners who are returning to the community. To understand the impact that CEO has on participants' outcomes, this chapter examines the program's context and operational strategies. Data from site visits to CEO and its work sites and data from its management system are used to describe program activities and their implementation. First the chapter discusses CEO's institutional structure and staffing strategy, program flow, and program activities, highlighting differences in the services offered to this study's two research groups. Then data describe the extent to which participants in the program group and those in the control group actually received those services.

The purpose of the CEO program model is to provide individuals who have recently been released from incarceration opportunities for immediate work and pay, necessary work experience for more permanent employment, and a chance to build work-related soft skills. It uses a day-labor model whereby clients are paid daily for their work. The work itself is not intended to train employees with new hard skills for particular types of jobs but, instead, provides participants the chance to build a recent and relevant work history and practice work behavior to take to permanent positions.

During the study enrollment period of January 2004 through October 2005, CEO was also participating in a major program analysis and business plan to improve its efficiency and processes. Thus, the program's service package was enhanced and refined over the course of the study enrollment period. Most of the changes did not affect what happened at the work sites but, rather, the services that clients were offered to support their work in transitional jobs, to help them find permanent jobs, and to promote retention in permanent jobs once they were placed in them. Changes made during the study period are described below, along with the timing of when they were implemented. Regardless of the timing of the enhancements, however, the core of CEO's services — transitional job work crews, job coaching, and job development — remained in place during the entire study period.

## CEO's Institutional Structure and Staffing

CEO provides services to former prisoners living all over New York City through a program office located in lower Manhattan. The organization has two operational focuses: (1)

implementing the transitional work component of the CEO model<sup>1</sup> and (2) supporting individuals in transitional jobs and helping them find permanent positions. Staff focusing on transitional jobs include those involved in identifying and developing work sites, assigning participants to work crews, supervising participants while employed at work sites, and paying participants daily for the work they do. *Work site supervisors*, who oversee participants in transitional jobs, represent the largest category of staff within the CEO structure and are in the only staff category at CEO that is unionized.

Several types of staff focus on the other CEO service offerings. *Recruitment and intake staff* recruit participants through local parole offices and register them at CEO intake sessions every Friday morning. *Job coaches* primarily serve as individual case managers, supporting participants while in transitional jobs and after they are placed in permanent jobs. *Job coaches* also share the responsibility of teaching the preemployment job readiness classes — called “life skills” classes — that are required before participants begin work in transitional jobs. *Job developers* focus on moving participants into permanent employment by working with both outside employers and participants. Other staff implement specialized programs, such as the Responsible Fatherhood Program focused on child support services (described below). Partway through the study enrollment period (in the first quarter of 2005), CEO added another type of staff — *retention workers* — who took over some responsibilities of job coaches; these staff focus on supporting participants after they have been placed in permanent employment.

## CEO Program Flow

Typically, all CEO clients follow a single program path starting at intake sessions and leading to transitional employment and other services. However, during the course of study enrollment, participants were randomly assigned during intake sessions either to the program group — representing CEO’s usual service offerings — or to the control group. Intake sessions occurred every Friday during the study period. Participants came to the program with a referral from a parole officer, although the parolees in this study were not mandated through parole orders to participate in the CEO program. (They were, however, expected to seek, obtain, and maintain employment.)<sup>2</sup> In most cases, CEO staff knew in advance who was expected each Friday, but random assignment occurred only among those who actually came to the program that day.

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<sup>1</sup>During the study enrollment period, the transitional job portion of the program was called the “Neighborhood Work Project.”

<sup>2</sup>Some criminal justice populations that CEO serves are mandated to participate in the program (for example, the New York State Shock Incarceration, or “boot camp,” participants). Presumably, mandated populations may more fully participate in programs, given the ability of criminal justice personnel to hold people accountable for their participation by levying sanctions against them if they do not.

Once participants were signed in, CEO staff accessed an MDRC database specifically designed to conduct random assignment for this study. Staff noted the number of transitional job slots that needed to be filled in order for CEO to fully staff its work sites. If at least five more people came to the Friday session than the number of available slots, random assignment was conducted, and the “excess” participants were assigned to the control group.

Once participants were randomly assigned, the control group was escorted to a different floor of the building, and the program group remained in the original intake area. Both the program and the control group immediately started the first day of the preemployment life skills classes specifically tailored for them. On occasion, CEO staff encountered parolees who were resistant to being placed in the control group because they had come to the program expecting a job. Because of CEO’s longstanding presence in the community as a group that gives ex-prisoners jobs right away, some parolees arrived assuming that they would be placed into a transitional job. When this did not happen, some clients were disappointed and, occasionally, irritated or angry. Staff found these incidents to be challenging and were placed in the position of explaining the random assignment process multiple times. In some cases, they needed to speak with a client about his or her attitude because of concerns that it might affect other clients’ attitudes toward the control group services. Although these incidents occurred throughout the study’s enrollment period, fewer than five participants asked to be removed from the study as a result of being placed in the control group.<sup>3</sup>

Program group members were offered the full array of CEO services. In addition to starting the life skills class on the first day, the program group members were given CEO intake assessments. These one-on-one sessions conducted by job coaches recorded information about participants’ demographic characteristics, criminal history, current housing status, education and employment histories, marital status, parental status, and more. The preemployment life skills class was a four-day class, which 79 percent of the program group completed. Participants were placed in transitional job work sites on the fifth day. Once in transitional jobs, participants worked four days a week and were in the CEO program office during the fifth day. During the office day, participants met with job coaches and job developers and took part in specialized programming, such as the Responsible Fatherhood Program. Participants also had access to a resource room. Once a participant was considered job ready, he or she began working with a job developer to find a permanent job. After the participant was placed into a permanent position, CEO followed the person for six months to one year, and the participant was eligible for the Rapids Rewards incentive program (described below).

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<sup>3</sup>CEO staff had experience dealing with situations in which the number of available work slots was not large enough to accommodate everyone seeking services. In the past, however, there had been no random assignment, and staff had the option of telling some applicants to come back at a later date.



The control group received a limited service package. This group also participated in a preemployment life skills class, but, unlike the program group's class, the class for the control group lasted for only one and a half days. It covered 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.<sup>4</sup> After completing the class, participants were given access to a resource room, in which they could use equipment for job searching activities, such as computers, fax machines, and phones with voice mail accounts. Control group participants had access to the room at different times during the day than program group members. During the first 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 "librarian" who was available to answer questions about available equipment and computers. Very few control group members visited the resource room more than two or three times. If members of the control group completed the life skills class and consistently attended the resource room for three months but were unable to find a job, they were then offered job development services. This happened for only seven control group members.

## **Activities for the Program Group**

The sections below more fully describe CEO program components and describe changes that occurred during the study period as a result of CEO's implementing its new business plan. The services described are those that were offered to the program group, though not everyone followed through and used this help and support.

### **The Preemployment Life Skills Class**

As noted above, typical participation in CEO begins with the preemployment life skills class. The purpose of the class is to brief participants on the transitional job program, to introduce them to the process of building soft skills that takes place during the transitional job experience, and to begin preparing them for job searches and employment after the transitional job. Job coaches rotate the responsibility of teaching the four-day class, beginning Friday mornings (directly after random assignment during the study enrollment period).

The class covers several important issues — particularly, what to expect from the CEO program and services. It addresses 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

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<sup>4</sup>Some clients were disappointed about not getting a transitional job and about being placed in the control group. It is not clear whether this disappointment affected the dropout rate from the life skills class.

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 — or the “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 by which staff teach participants soft skills related to employment. The framework is introduced during class and built upon while at transitional job work sites. Participants are expected to demonstrate the skills on the job, and the hope is that the skills will be translated into permanent positions, making clients more employable and helping them retain their new jobs.

The life skills class also addresses other issues related to employment. For instance, former prisoners often do not have the proper identification needed to secure employment, such as Social Security cards. If clients do not have such identification, CEO staff help them obtain it. It is a critical first step in the employment readiness process, and CEO tries to ensure that this issue does not remain a barrier to work. 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. Finally, instructors attempt to motivate clients to strive for success both in transitional jobs and in their pursuits for permanent lawful employment.

Once the class is complete, participants are assigned a job coach. Clients begin formally meeting with job coaches at this point in the programming. No particular system was used to make such assignments for most of the study enrollment period; however, a system change occurred midway through 2005 whereby participants were assigned in clusters to pairs of job coaches and job developers. The idea was that by pairing job coach and job developer caseloads, the staff would work in closer collaboration to move clients through transitional work and into permanent employment. It is not clear whether this change was implemented fully; if it was, however, then this new staffing approach affected approximately one-third of study participants.

### **CEO's Transitional Jobs with Job Coaching**

Participants begin their transitional jobs after they finish the life skills class. They are assigned to daily work crews, each having a CEO supervisor. Along with daily work, the CEO program provides job coaching to participants to help build soft skills that facilitate long-term employability. The program does this in two ways: through hands-on job coaching provided on-site by work site supervisors and through support and guidance provided by office-based job coaches. Participants formally meet with their job coach once a week on the day that they are in

the CEO offices, although they may be in contact with a job coach in other ways during the workweek.

### *Working in Transitional Jobs*

CEO staff identify and develop appropriate work sites for their clients. Then the transitional job work sites, which are both city and state agencies, contract with the New York State Division of Parole as part of an Internal Service Fund. CEO acts as an agent of Parole to meet the contractual requirements that CEO developed with the site. On a typical day, more than 200 CEO participants work at 30 or more work sites across New York City, conducting mostly maintenance, repair, and janitorial work; 9 work sites were observed for this study, to understand how the work activity at the transitional jobs was conducted.<sup>5</sup> Table 2.1 includes examples of the work site locations, the number of participants assigned to each site, and the type of work being conducted at the site.

Participants contact CEO daily in the afternoon to find out where they will be working the next day — a process called “plugging in.” Because CEO emphasizes that the jobs are transitional in nature; staff do not attempt to match participants with work in their field of interest but, instead, are focused on fulfilling the contractual obligations to work site clients in terms of crew sizes needed at each site. Participants may be at the same site for some time or may switch work sites frequently, depending on the needs of each site. At the start of each shift, supervisors check in with CEO’s central office if too few or too many workers show up at particular work sites, and people are shifted as necessary to deploy the appropriate number of workers at each site.

Some sites require one crew while others require more than one. In general, the work is steady and repetitive in nature. Examples of tasks include sweeping, mopping, dusting, cleaning bathrooms, breaking down boxes for recycling, and groundskeeping. The researchers found that participants spent the majority of their time at most sites engaged in work activity and completing tasks. Only in two of the nine studied work sites did participants have low levels of work engagement during the observation period.

Interaction between program participants and CEO supervisors varied across the nine sites. In five sites, participants had intermittent interaction with supervisors for task assignment, quality control checks, and administrative tasks. In three sites, participants had consistent and significant positive interaction with supervisors providing instruction and structure to the work crew activities in addition to quality control checks and administrative tasks. In only one site

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<sup>5</sup>Based on field research that was conducted for the Hard-to-Employ project by Demetra Nightingale of the Johns Hopkins Institute for Policy Studies and John Trutko of Capital Research Corporation.

**The Enhanced Services for the Hard-to-Employ Demonstration**

**Table 2.1**

**Examples of CEO Work Sites**

**Center for Employment Opportunities**

Location	Number in Work Crew <sup>a</sup>	General Nature of Work	Main Activities
Brooklyn Criminal Court	8	Custodial/janitorial	Participants clean courthouse building floors, steps, bathrooms, and courtrooms
Brooklyn Criminal Court	8	Custodial/janitorial	Participants sweep courtyard, clean bathrooms, and dust offices
City Administration Building	7	Custodial/janitorial	Participants mop/wash floors, clean and sanitize bathrooms, remove trash, and break down boxes
City of New York Office Building	6	Custodial/janitorial	Participants clean floors, hallways, and bathrooms and remove trash
City of New York Office Building	8	Custodial/janitorial	Participants mop, vacuum, and clean building floors; sanitize bathrooms; and take out trash
City of New York Office Building	8	Custodial/janitorial	Participants mop and sweep hallways
Criminal Courts Building	12	Custodial/janitorial	Participants sweep, wash, and buff floors and take out trash
Criminal Courts Building	2	Custodial/janitorial	Participants sweep, mop, and wax floors and break down boxes
Bronx Community College (City University of New York)	13	Custodial/janitorial	Participants pick up litter, rake leaves, shovel snow, and generally keep grounds around community college clean

SOURCE: These data come from an observational substudy across the sites in the Hard-to-Employ project that was conducted by Demetra Nightingale of the Johns Hopkins Institute for Policy Studies and John Trutko of Capital Research Corporation.

NOTE: <sup>a</sup>Number of individuals who were observed at the work site for this study.

was there considerable negative interaction between the supervisor and participants about behavioral issues, such as the use of cell phones on-site.

Usually, CEO participants do not interact with the general public or the personnel in the buildings where they work. In some sites, the work is completed after business hours. In other sites, however, where jobs are completed during business hours, CEO participants sometimes have such interaction throughout the day. Also, in some sites, participants interact with other custodial staff who are not affiliated with CEO. Anticipating such occasions, on-site behavior is discussed during the preemployment life skills class.

Two other activities occur every day at work sites. First, 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 a way to help those who were incarcerated immediately start working toward self-sufficiency, by having cash in hand. It is a way to ease the day-to-day life of people in this situation so that they do not need to wait several weeks for a paycheck after they start working, and this offsets the appeal of getting “quick money” through illegal activities.

Second, CEO work site supervisors play a role in identifying problems on work sites and intervening around these issues. They also identify individuals who may require disciplinary action. They complete work site evaluations every day for each participant. As noted earlier, CEO does not emphasize teaching particular work skills at sites; rather, the focus is on learning and practicing the soft skills of the CEO CPs. Thus, participants are rated daily on each aspect of this philosophy:

1. Cooperation with supervisors (such as having a positive relationship, being respectful, responding to instruction, being able to teach others)
2. Effort at work (such as being productive, responsible, and satisfied with the results of one’s efforts, and also having initiative at work)
3. On time (such as being ready to work at start time and after breaks and working until dismissal time, giving advance notice of the need to take time off, and avoiding absences)
4. Cooperation with co-workers (such as not having conflicts with others, being able to work in a team, practicing positive workplace behaviors, and being a role model)
5. Personal presentation (such as being respectful in speech, being clean, and having high energy)

Participants' work site evaluations are communicated to their job coaches so that the coaches can address with them any issues that occur at work sites and can provide guidance and support in how best to improve their performance. During the first portion of the study enrollment period, work site supervisors filled out evaluations on forms that were left in the office at a particular location. It was expected that job coaches would then search through the pile of forms to find their participants' evaluations in order to review behavior and progress at work sites. This process proved to be cumbersome and time-consuming for job coaches, and so the information was not being used to its fullest potential.

In response, CEO implemented the "Passport to Success" in May 2005.<sup>6</sup> This new evaluation system requires participants to carry their Passport booklet daily (the booklet is kept in a plastic pocket that hangs from a necklace). On days that participants do transitional work, the 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 to address any issues or concerns, and they give participants positive feedback about their performance, as appropriate. It appears that this system puts the work site supervisors' evaluation information to good use. According to staff respondents during site visits, the Passport system is a straightforward way to keep track of clients' progress at work sites.

Participants work in transitional jobs until they find permanent employment, either with or without assistance from CEO, or until they are discharged from the program. Inappropriate work behavior at a site might lead to discharge, as it might on any job. In addition, unexcused absences from work sites can lead to disciplinary action and suspensions from the transitional work. Participants can be reinstated to transitional jobs, but they will eventually be dismissed if they continue to miss work. It is CEO's policy to notify parole officers when participants are discharged from the program.

### *Providing Support Through Office-Based Job Coaching*

Job coaches implement the case management component of the CEO program. They provide one-on-one guidance and support to help clients succeed both while in the program and while transitioning into permanent employment. CEO employs about 10 job coaches on staff who each work with about 80 to 100 cases at a time, with 30 to 50 of those cases being actively engaged in transitional jobs.

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<sup>6</sup>This change affected approximately one-third of the study participants who were enrolled between May and October 2005 as well as any others who were enrolled earlier but were still in programming when the initiative began.

Job coaches meet with participants at least once a week, usually in the CEO office. During these one-on-one meetings, job coaches review individual participants' job performance using the Passport to Success booklet as a guide, and they address any barriers to work that require additional support and referrals to other programs and services.<sup>7</sup> Job coaches also work with participants to develop a résumé, to build skills in how to search for jobs, and to prepare for job interviews (such as how to answer potential employers' questions related to criminal history — the so called “conviction question”). Participants are also encouraged to raise their own issues and concerns during these meetings so that staff can help them address problems on the work site or in their personal lives.

Job coaches sometimes visit work sites to meet with participants. Although such visits were always an element of the program, partway through the study enrollment period, job coaches were encouraged to visit work sites more often and were required to conduct at least some such visits each week. Typically, work site visits are conducted to observe a new participant or to observe a participant who is encountering problems on-site. The extent to which job coaches actually went to work sites during the study period is unclear.

Job coaches determine when participants are considered “job ready” and are able to actively move toward placement in permanent positions. Coaches typically begin reviewing cases for job readiness after two weeks in a transitional job, and the goal is to move people into permanent employment as soon as possible. Staff use a standard form that they complete to decide whether a participant is ready to move on. Job readiness is based primarily on work site performance; however, participants who have a significant employment history may be considered job ready right away. At the other end of the spectrum, staff reported that some participants become “comfortable” in their transitional job and have to be urged to work harder to find a permanent position. CEO creates an environment where clients feel valued and feel safe when it comes to dealing with their conviction history. Some individuals may be reluctant to leave employment that is accepting and to move on to unknown and unfamiliar situations.

### **Finding Permanent Positions**

CEO employs job developers to work with both CEO participants and outside employers to move people from transitional jobs into permanent jobs. About 10 to 14 job developers work with about 40 participants each. The cases do not transition from job coaches to job developers; instead, participants work with both types of staff simultaneously. Although job

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<sup>7</sup>CEO does not have a formal process for referring clients to other types of services that they might need. Most referrals are to housing programs on an as-needed basis, if that issue arises. The New York State Division of Parole is responsible for referring parolees to services that are related to special conditions for release, such as substance abuse treatment or anger management.

coaches and job developers do not necessarily collaborate on addressing the needs of individual participants, they do work together for individual clients as needed — for example, preparing a client when a job opportunity arises. The job developer might identify the job and help arrange the interview, and the job coach might do mock interviews with the client in preparation for this specific interview.

After participants have worked in transitional jobs for two weeks, job developers generally meet 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. If the participant has been deemed job ready by the job coach, then the participant and job developer begins to have weekly meetings. If the participant is not yet job ready, the weekly meetings wait until that occurs.

Although job developers work with participants directly, the bulk of their time is spent developing employer accounts and identifying job opportunities for participants. Job developers identify accounts through many means, but they rely heavily on “cold-calling” employers; that is, they contact businesses that might have positions appropriate for CEO participants and “sell” the idea of hiring them. As selling points to potential employers, job developers use the tax credits associated with hiring former prisoners along with bonding insurance, which protects employers from any risk involved with such hiring. Under the federal bonding program, CEO pays for up to \$10,000 of bonding insurance for the first six months, 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.<sup>8</sup> Each job developer is responsible for specific employer accounts, but all the developers consistently need to add new employers to their set of accounts. Some employers come back to CEO to fill positions again and again, but others do not.

Once the job developers identify employer accounts and job openings, they try to match them to appropriate clients in their caseloads. If they cannot find a match from their own caseload, they go to other developers' caseloads to identify someone. The job developers schedule interviews for those clients who seem appropriate for the position. When possible, they send more than one person to the employer to interview, to be sure to meet that employer's needs by offering options. Job developers prepare clients for interviews by helping them anticipate the questions that will be asked, by developing responses, and by explaining how clients should behave during the interview. Afterward, job developers contact the employer to ask how the interviews went and whether the employer is planning to hire a CEO participant; if not, they may offer to send other clients to interview. Job developers work hard to meet em-

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<sup>8</sup>The majority of employers in this study did not opt for CEO to pay for bonding insurance.



ployers' needs, with the hope that employers who have vacant positions will come back to CEO over time.

Each job developer is required to meet a job placement quota each month; the number to be met depends on seniority in the position. There are three levels of job developers. During the study period, first-level job developers were required to make five job placements each month; second-level developers were expected to make seven placements per month; and third-level job developers had to make eight placements each month. If job developers meet their quotas for six consecutive months, they are eligible for pay raises and promotions. They also are eligible for financial bonuses if they exceed their monthly placement quotas.

Box 2.1 gives a sense of the challenges faced by CEO staff by presenting examples of how six sample members in the study moved through transitional jobs to permanent positions.

### **Services After Job Placement**

CEO attempts to track whether clients stay employed in the jobs that they are placed in. For most of the study enrollment period, job coaches were responsible for connecting with clients to confirm their employment. Job coaches asked clients to come to the office with their paycheck stubs so that CEO could track employment. Job coaches were paid a bonus for collecting clients' pay confirmation. In early 2005, a new unit of retention workers was added to take over this function from job coaches.

In August 2004, CEO also started an incentive program — called “Rapid Rewards — for clients to confirm their continued employment.”<sup>9</sup> CEO created this program as a way to keep better track of employment retention, in order to meet contractual benchmarks for various funding streams (though it is possible that the rewards also encourage clients to remain employed). Through Rapid Rewards, both clients who are placed in positions by CEO and those who find jobs on their own are eligible for noncash incentives. To sign up for the program, a participant has to keep a job for 30 days, have a paycheck stub, and work at least 20 hours per week. Then, at different milestones, people receive rewards — such as gift certificates to various stores and paid Metrocards (for mass transit) — for continuing employment and bringing their paycheck stub to the CEO office. Rewards are given every 30 days through one year, at which point clients can receive an additional \$250 gift certificate.

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<sup>9</sup>Although 56 percent of the sample were enrolled in the study after this change occurred, it affected only the 22 percent of participants who were enrolled between August 2004 and October 2005 and who were placed in permanent positions.

## Box 2.1

### Case Examples: Center for Employment Opportunities

#### Client 1

Client 1 received support from CEO to find a permanent job. This client completed the life skills class and was sent to a transitional job, where he worked and had good attendance. He reportedly was doing well on-site and had no problems. The client met weekly with both his job coach and his job developer, and he also met with a social worker from the Responsible Fatherhood Program. The job developer had the client in mind for a particular position when they first started working together: a bike-riding delivery position. Once the position became available, the job coach worked with the client to tailor his résumé to the position. The job coach also prepared the client for the interview and conducted a mock interview with him. The job developer went to the interview with the client. The client was offered the job and was placed within six weeks of finishing the life skills class. Unfortunately, the position lasted only until the client's bike was stolen. The client then chose not to return to CEO for further help finding another job, although that option was offered to him.

#### Client 2

Client 2 had a slower start than Client 1, but he was still placed in a permanent position by CEO. This client started the life skills class but did not complete it the first time he took it. The next week, he started the life skills class again and attended every day. Once he was in the transitional job, his attendance was consistent, and he even worked double shifts when needed.

The job developer who placed the client met him about six weeks into his time with CEO and sent him on an interview the day they met. This client was originally on a different job developer's caseload, but he was transferred to a second job developer because his work history and personality matched the openings that that job developer had in his employer accounts. The client was hired by a marketing research firm. At the time of the site visit, the client had worked in the position for 20 days.

#### Client 3

Client 3 also moved to a second job developer — in this case, because that job developer needed a client who had a driver's license in order to fill an available position. The employer was interested in the tax credit for hiring former prisoners, and the job developer completed the paperwork for the employer to claim this credit.

Before the interview, the job developer talked with the client about how to dress for it, because she knew from experience that this company did not like to hire people who dressed too formally or who were unwilling to get dirty, which the job required. The client was hired and was making \$8.00 per hour but was fired two months later for refusing to do certain types of work. He returned to CEO for further assistance, but CEO declined to provide it because the client was fired from the position it had placed him in. The job developer was glad that she had placed people in this company before, because she believes that if this were the first time working with the company, she would have lost the employer account.

(continued)

### Box 2.1 (continued)

#### **Client 4**

Client 4 was doing well in the CEO transitional job and was considered to be job ready. A job developer met with her and sent her to an interview. She did not get that job, and the job developer planned to continue working with her. However, the client began to miss days of work on the transitional job, and then she started to miss even her meetings with staff. Staff later learned that this client had relapsed into drug abuse.

#### **Client 5**

Client 5 was considered an ideal CEO client whose case went smoothly throughout the process of working in a transitional job and meeting with job coaches and job developers. At the time of the site visit, the client worked in a permanent job and had stayed in contact with CEO to earn retention bonuses for keeping this position.

#### **Client 6**

At the time of the site visit, Client 6 had been successfully working in a CEO transitional job for three months and had a work history as a dishwasher, laborer, demolition worker, and machine glass-cutter. The job developer had sent him on three interviews for permanent positions, but he had not yet been hired. The job developer believed that the client had not been hired because he was an older man; the job developer reported that when it came to hiring laborer positions, employers tend to want younger men. The job developer planned to continue to work with this client to find a permanent placement. He was comfortable sending this client to several employers for interviews because the client was eager to work.

SOURCE: Case reviews conducted during an in-person site visit in June 2005.

In addition, twice a year — in April and November — CEO holds retention events. These are times when the staff come together with former clients to celebrate employment. Clients can get rewards and certificates for maintaining their jobs.<sup>10</sup>

Finally, if former clients find themselves losing permanent positions that they were placed in or if other issues affecting employment occur, such as relapsing in substance abuse, CEO will offer them services once more under the following conditions. If a parole officer refers a former client back to CEO, that person may return to the program for assistance as long as he was not terminated from the permanent position for theft or violence. If the period of absence from the program is shorter than six months, then the person simply resumes full

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<sup>10</sup>At the time of the study, CEO job coaches did not continue to work with clients after they were placed in permanent placements. Notably, after the study enrollment period was completed, CEO staff continued to alter and refine their approaches to retention services.

services: a transitional job, job coaching, and job development. If a former client is no longer on parole but still would like CEO's help, that person can receive job development and postplacement services.

### **The Responsible Fatherhood Program**

The final major service that CEO provides is its Responsible Fatherhood Program, which attempts to help clients better manage their parenting responsibilities and their child support orders. The program is described to clients during the life skills class, and participation in it is voluntary.

The Responsible Fatherhood Program offers assistance managing child support orders. This is seen as supporting CEO's employment mission, because clients who have large support orders may have little incentive to work in the formal labor market, since their employer will be required to withhold support payments from their paycheck. The first step is for CEO to receive a report of a client's account history so that it is aware of any outstanding orders. CEO staff assist clients in understanding their support orders, help prepare them for dealing with the Child Support Enforcement agency, and brief them on what to expect during court hearings related to orders. CEO focuses on current payments but not on arrears, which may be very large. (In many cases, noncustodial parents accrue arrears while they are incarcerated.) Clients may owe some or all arrears to the state, in which case, they are still obligated to pay. If the arrears are due the mother of the child and not the state, CEO helps fathers fill out the related paperwork.

CEO staff have an ongoing relationship with one customer service outreach person at the Child Support Enforcement agency. Staff accompany clients to meet with this staff person about modifying clients' child support orders, and because the CEO staff have this existing relationship, clients do not have to wait to be seen. CEO staff help provide information to customer service about the type of work that the clients are participating in. Typically, support orders are modified to \$25 per month, and these funds are collected through wage withholding. The order changes are temporary, however, and clients need to go back to court to have them readjusted based on earnings in about a three-month period.

CEO also offers a parenting class to those in the Responsible Fatherhood Program. The class includes six sessions over the course of six weeks. Clients attend the class during their day in the office, when they meet with other CEO staff, such as job coaches and job developers.

### **Participation in CEO Program Activities**

Although CEO staff intend for participants to receive all the services outlined above, not everyone remains in programs to accept all the help offered. To understand program impacts

on participants' outcomes, it is important to know how much of the CEO service package participants received.<sup>11,12</sup>

As shown in Table 2.2, almost 80 percent of the program group completed the four-day preemployment life skills class.<sup>13</sup> As expected, almost everyone who completed the class — about 72 percent of the total program group — worked at least one day in a work crew in the transitional job program. The bottom panel of the table shows that, of the participants who worked in a CEO transitional job work crew, 29 percent worked 1 to 4 weeks; 40 percent worked 5 to 12 weeks; and 24 percent worked 13 weeks or more. The average length of time in a work crew was 8 weeks.

Although 28 percent of the sample did not participate in a CEO job, they were still followed throughout the study. They were not asked why they did not work a CEO job, but these clients likely left the program before completing the life skills class, for any number of reasons. They may have left for lack of interest, to take another job, or because of personal issues.

Among the total program group (top panel), 60 percent met with a job coach at least once, and 22 percent met with a job coach more than four times (not shown in table). However, one would not expect those who never worked in a transitional job to meet with a job coach. In fact, whether or not clients worked a transitional job and how long they worked that job mattered for receiving other services. For example, as shown in Table 2.3, among those who worked a transitional job for 13 weeks or more, 94 percent met with a job coach at least once, and 72 percent met with a job coach more than four times. Contact with job developers follows the same pattern. For the total program group (Table 2.2), 57 percent met with a job developer at least once, and about 12 percent met with a job developer more than four times (not shown in table). As expected, however, more of the group received job developer services the longer they worked in transitional jobs. Among those who worked a transitional job for 13 weeks or more (Table 2.3), 99 percent met with a job developer at least once, and 71 percent met with a job developer more than four times.

Only a portion of those in the program group found permanent positions. Independent of the number of times that participants met with job developers, 30 percent of the program group were placed in permanent jobs or found permanent jobs through their own efforts (Table 2.2). However, when one considers the amount of time that participants worked in a transitional

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<sup>11</sup>Participants in both the program and the control group reported also receiving services from organizations other than CEO. The details of this participation are documented in Chapter 3.

<sup>12</sup>The data presented in this section come from CEO's management information and payroll systems. Because of variations in data entry by staff, these data may not fully capture all CEO contacts with participants.

<sup>13</sup>Thus, 21 percent of participants did not participate in CEO services because they did not complete the life skills class. No data are available to indicate how much of the class they received.

**The Enhanced Services for the Hard-to-Employ Demonstration**

**Table 2.2**

**Program Group's Participation in CEO Activities**

**Center for Employment Opportunities**

Outcome	Control group
Completed life skills/preemployment class (%)	78.9
Worked in CEO transitional job (%)	71.7
Weeks worked in CEO transitional job <sup>a</sup> (%)	
Never worked in CEO transitional job	28.4
Less than 1 week	5.5
1-4 weeks	20.8
5-12 weeks	28.5
13-24 weeks	14.1
More than 24 weeks	2.8
Days between random assignment date and start of CEO transitional job (%)	
Never worked in CEO transitional job	28.4
0-7 days	48.4
8-14 days	14.8
More than 14 days	8.5
Met with a job coach (%)	59.5
Met with a job developer (%)	57.4
Placed in an unsubsidized job <sup>b</sup> (%)	30.2
<b><u>Among those who worked in a CEO transitional job (total = 407)</u></b>	
Weeks worked in CEO transitional job <sup>a</sup> (%)	
Less than 1 week	7.6
1-4 weeks	29.0
5-12 weeks	39.8
13-24 weeks	19.7
More than 24 weeks	3.9
Average weeks worked in CEO transitional job	8.3
Number of meetings with job coach (%)	
None	18.4
1-2	31.0
3-4	20.1
More than 4	30.5
Number of meetings with job developer (%)	
None	21.4
1-2	34.6
3-4	15.5
More than 4	28.5

(continued)

**Table 2.2 (continued)**

Outcome	Percentage
Placed in an unsubsidized job (%)	40.8
Self-placement	7.9
Direct placement	34.4
Participated in fatherhood activity (among fathers) (%)	39.2
Sample size	568

SOURCE: MDRC calculations from CEO's Network for Information Gathering Evaluation and Learning (NIGEL) database.

NOTES: This table reflects program participation and CEO employment between January 2004 and October 2006. 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.

<sup>a</sup>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.

<sup>b</sup>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.

job, the placement rates increase. For example, 56 percent of those who worked in a transitional job for 13 weeks or more found permanent positions (Table 2.3). Of all those placed in permanent positions since the Rapid Rewards program began in August 2004, 52 percent received at least one retention bonus. (See Box 2.2 for more information about program participation.)

## Conclusion

To understand the impact of a program on participants' lives, one must understand the services that people were offered and those that they actually received. The types of help and support received and how participants experience programs are valuable contextual backdrops for evaluation findings. Following are some important themes to reflect on when considering these impact findings:

- The CEO model includes preemployment job readiness activities, transitional work, placement services into permanent positions, and postplacement retention support and incentives.

**The Enhanced Services for the Hard-to-Employ Demonstration**

**Table 2.3**

**Program Group's Participation in CEO Services, by Length of Time Working a CEO Transitional Job**

**Center for Employment Opportunities**

Outcome (%)	Among Those Who Worked a CEO Transitional Job <sup>a</sup>			
	At least 1 day	At least 2 weeks	5 weeks or more	13 weeks or more
Met with a job coach				
At least once	81.6	88.3	91.5	94.4
More than four times	30.5	35.5	51.3	71.9
Met with a job developer				
At least once	78.6	87.1	94.9	98.9
More than four times	28.5	34.0	47.0	70.8
Placed in or found permanent jobs <sup>b</sup>	40.8	45.0	49.1	56.2
<b>Sample size (total = 407)</b>				

SOURCE: MDRC calculations from CEO's Network for Information Gathering Evaluation and Learning (NIGEL) database.

NOTES: This table reflects program participation and CEO employment between January 2004 and October 2006. The sample in this table includes program group clients randomly assigned between January 2004 and October 2005. 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.

<sup>a</sup>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.

<sup>b</sup>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.



## Box 2.2

### Index of Program Participation

Although the information in this chapter sheds light on program participation rates, it is still not clear just how much of the program people received. Thus, an index of program participation (including time worked in a transitional job, number of meetings with job coaches, and number of meetings with job developers) was created to examine the proportions of the program group that received small, medium, or high amounts of CEO services. About 27 percent of the program group never worked in a transitional job and also never had any meetings with job coaches or job developers. These people remained in the study and were tracked over time, although they never completed the first step of CEO's services — the life skills class — or they completed the class but never returned to CEO for the transitional job.

However, among those who got any transitional job or job coaching or job development services, 37 percent received a low amount of services; 34 percent received a medium amount of services; and 29 percent received a high amount of services. Notably, this index is meant only to shed light on how much service was provided to clients. Higher service receipt does not necessarily mean greater program success. Some clients who had low service receipt may be in this category because they did not require many services. They may have moved quickly to a permanent position due to their job readiness or arising opportunities.

- **Low service receipt** is defined in two ways: (1) anyone who worked in a transitional job for four weeks or less and received up to two appointments with job coaches or job developers or (2) anyone who worked in a transitional job for less than one week but who somehow received three or more meetings with job coaches or job developers.
- **Medium service receipt** is defined in two ways: (1) anyone who worked a transitional job for five to twelve weeks and received up to four appointments with job coaches or job developers or (2) anyone who worked in a transitional job for one to four weeks and also received three or more meetings with job coaches or job developers.
- **High service receipt** is defined in two ways: (1) anyone who worked in a transitional job for thirteen or more weeks or (2) anyone who worked in a transitional job for five to twelve weeks and received four or more meetings with job coaches or job developers.

- CEO clients have many opportunities to receive services and assistance through direct contact with staff, including work site supervisors, job coaches, job developers, and staff from the Responsible Fatherhood Program (for those enrolled in it).
- CEO provides two ways for clients to build soft skills — hands-on experience in a transitional job and office-based job coaching.
- The CEO model requires efficient and capable staff to support over 30 work sites and to pay clients on a daily basis.
- CEO emphasizes the transitional nature of the work and tries to move clients to permanent positions as soon as they are able.
- During the study enrollment period, CEO made several service enhancements (for example, the Passport to Success evaluation system, Rapid Rewards, and the addition of retention workers); however, the core of the program's services — transitional job work crews, job coaching, and job development — remained the same during that time. The implemented enhancements did not affect the full program group.

Finally, Box 2.3 concludes this chapter by sharing the thoughts of 19 members of the program group who gave information in ethnographic interviews about their perceptions of CEO services and their feelings about participation in the program.

### Box 2.3

#### Findings from Ethnographic Interviews

With a few exceptions, participants were positive about their experiences in the CEO program. A number of participants expressed the sentiment that, in a time of need, “they help you out.” They reported that, by and large, supervisors were fair and did not harass them. A few negative incidents and personality clashes were noted; however, those who had such experiences usually were able to transfer to another site. Participants who were interviewed reported that their relations with other CEO participants were fine. Most of them seemed to see themselves as able to get along with others well. Although no participants reported significant problems with other participants, no one reported befriending other participants either.

Participants discussed a number of aspects of the program that they felt were helpful. First was the work itself, which met a number of important and immediate needs. The job gave them somewhere to go, filled their days, and offered a regular routine that kept them off the streets and away from former undesirable behavior or habits. This was an important aspect of the program for some participants. Establishing a regular routine was an important step in their pursuit of regular legal employment. Additionally, for most participants, the work that they were asked to do (maintenance) was familiar and not difficult. No one spoke of feeling overworked in the program.

Another benefit of going to work was, of course, the pay. All the participants found the money useful to cover basic expenses, like transportation, meals, some utilities, items for their children, and cigarettes. The money was not enough to make substantial contributions to housing expenses. In addition to the pay, a few participants mentioned appreciating the incentives that went with program participation.

Participants were most grateful for the help that CEO staff gave in having to answer the most difficult question regarding convictions or felonies. “They just helped me with the answer, you know, if an employer asks you a question like “Have you been convicted of a crime?” That was a very difficult question for me to answer.” Participants were relieved by the groundwork that CEO staff did in arranging interviews with employers, who were aware of the felony status of their applicants. “And the only good thing that I was looking forward to them sending me to these jobs was that the employers already knew . . . they basically knew your background.”

Among the participants interviewed, only about one-quarter were currently employed. Most of those got their current jobs through CEO contacts. Of the others, some were still with CEO; others had been discharged from the program for various reasons; and others had simply left to job-search on their own. Some in the unplaced category reported never being sent out on any interviews by CEO, while others had been sent on interviews and even began work with some employers in positions that did not work out. A few were very positive about CEO (those who had been placed into jobs) and reported maintaining contact with the program (for example, by giving speeches at events and sharing employment opportunities at their work site).

(continued)

**Box 2.3 (continued)**

I talk positive about CEO because they helped me. I honestly feel like they did care and they did help, so it was, like, I can't even talk bad about them, because they did get me employment, and they did help me out while I was looking for employment. They put a little money in my pocket and kept me out of trouble, I had a little cigarette money.

Some participants were unsure about approaching the program for assistance after having left, but a few who were unsuccessful in finding employment noted that they are considering returning to the program for work. One participant expressed the intention of returning to CEO for employment as a site supervisor.

SOURCE: Ethnographic study of 19 program group members by Alissa Gardenhire Crooks.



## Chapter 3

# Impacts of CEO on Participation and Service Receipt

Chapter 2 describes the Center for Employment Opportunities (CEO) program for former prisoners who are returning to the community and provides information on the level of their participation in CEO activities, using data from the program's tracking and payroll systems. This chapter uses data from the client survey to present information on the receipt of such services as job search assistance, education or training, and parenting classes — whether sample members received these services from CEO or from another organization. It focuses primarily on differences in the services received by survey respondents in the study's two research groups. The client survey achieved a 70 percent response rate for the program group and a 66 percent response rate for the control group. Most individuals were surveyed 16 to 23 months after being randomly assigned to the two groups; random assignment occurred between January 2004 and October 2005.<sup>1</sup>

The findings in this chapter help to inform the analysis of CEO's effects on employment and recidivism, which are presented in Chapters 4 and 5. As discussed in Chapter 2, both research groups received certain preemployment services from CEO. Thus, it is expected that a relatively similar proportion of program and control group members would receive employment services. However, the main treatment difference between the two groups was the receipt of transitional jobs by program group members. Thus, the program group's receipt of transitional jobs is expected to greatly exceed the control group's.

Although the survey collected information on the types of activities that individuals participated in and the types of services and supports that they received, the survey does not address whether program or control group respondents received higher-quality services. Also, the survey did not collect information on the intensity of the services that sample members received.

Box 3.1 explains how to interpret the impact tables presented in the remainder of the report.

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<sup>1</sup>The survey interview dates ranged from 15 to 28 months after random assignment. Note that, according to administrative data, the CEO survey respondents experienced stronger employment and recidivism effects than the rest of the research sample. Thus, the survey might show bigger impacts on outcomes linked to recidivism than might exist for the full sample. Refer to Appendix A for additional information regarding the client survey.

**Box 3.1**

**How to Read the Estimated Impact Tables in This Report**

Most tables in this report use a similar format, illustrated below. Several participation outcomes are shown for the program group and the control group. For example, the table shows that about 68 (67.9) percent of the program group and about 64 (64.3) percent of the control group ever participated in any employment or education-related activity.

The “Difference” column in the table shows the differences between the two research groups’ participation rates — that is, the CEO program’s estimated impact on participation. For example, the estimated impact on participating in job search activities can be calculated by subtracting 58.9 percent from 60.6 percent, yielding a 1.6 percentage point difference.

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). For example, as shown in the last row of data, the program group model had a statistically significant impact of 9.9 percentage points at the 5 percent level on participating in individual job searches. (One asterisk corresponds to the 10 percent level; two asterisks, the 5 percent level; and three asterisks, the 1 percent level.) The p-value shows the exact levels of significance.

Impact estimates are regression-adjusted using background characteristics of the sample, including gender, age, race/ethnicity, prior employment, prior recidivism, length of time between release from prison and random assignment, and other relevant characteristics. In addition, the impact estimates are weighted by the week of random assignment to reflect the representation of each research group in the research sample.

See Appendix F for additional information about the impact tables.

**Impacts on Participation in Employment and Education Activities**

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
Sample size (total = 531)	316	215		

SOURCE: MDRC calculations based on responses to the client survey.

## Impacts on Participation in Employment and Educational Activities

Table 3.1 shows the impacts of CEO on participation in employment and education activities. As noted in Chapter 2, both the program and the control group members were offered preemployment services, but only the program group members were placed in transitional jobs. According to the survey data, program group members were twice as likely to participate in a transitional job: 32 percent of the program group and 16 percent of the control group reported doing so. Although the difference of 16 percentage points is large and statistically significant, participation was definitely underreported because CEO's payroll data show that 72 percent of the program group received a transitional job paycheck from CEO. (See Chapter 2.) The survey was administered over a year after random assignment, and if sample members had a transitional job early in that period, or if they worked the transitional job for only a short period, they might not have remembered the job when asked.<sup>2</sup> It is impossible, however, to know how much each research group underreported transitional job participation, since both groups may have worked in such jobs for organizations other than CEO.<sup>3</sup>

As expected, the two groups were equally likely to report participating in job search activities. The services that CEO provided to the control group were much more limited than those provided to the program group, but, as noted above, the survey did not measure the intensity or quality of such services. Program group members were more likely than control group members to report participating in individual job search (that is, a job search supervised by an agency staff person). The results also show that about one-quarter of each research group reported participating in education or training — mostly in General Educational Development (GED) classes or vocational training.

Table 3.2 shows the percentage of each respondent group that received various types of employment-related assistance. The bottom rows of the table show that a large proportion of both research groups reported receiving at least one employment service since random assignment: 83 percent of the program group and 79 percent of the control group. The results also show that control group members were more likely to receive help from organizations other

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<sup>2</sup>Transitional job participation was measured in two ways. First, among survey respondents who were ever employed since random assignment, the survey asked whether or not they were employed in a transitional or a subsidized job for which they got a paycheck from an employment program — for example, CEO, Wildcat, and the Doe Fund — rather than from their company or employer. Second, respondents were asked whether or not they were ever employed in a day-labor job.

<sup>3</sup>One limitation of collecting data through surveys is that individuals may misremember or misreport some of the outcomes. In this case, however, there is no indication that the underestimates are more severe for one group than the other. Additional analysis, which compared outside data sources with the survey responses, found similar underreporting for both research groups. (See Appendix B.)



**The Enhanced Services for the Hard-to-Employ Demonstration**

**Table 3.1**

**Impacts on Participation in Employment and Education Activities**

**Center for Employment Opportunities**

Outcome	Program Group	Control Group	Difference (Impact)	P-Value <sup>a</sup>
Worked in a transitional job <sup>b</sup> (%)	32.3	16.2	16.1 ***	0.000
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
Average number of weeks participating in:				
Job search activities	3.3	2.7	0.6	0.236
Education/training activities	1.2	1.1	0.1	0.785
Sample size (total = 531)	316	215		

SOURCE: MDRC calculations based on responses to the client survey.

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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>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 on the table): 3.8.

<sup>b</sup>This measure includes respondents who said “yes” to having worked in a transitional or subsidized job where they got a paycheck from an employment program (such as CEO, Wildcat, or the Doe fund) or having worked in a day labor job.

**The Enhanced Services for the Hard-to-Employ Demonstration**

**Table 3.2**

**Impacts on Receipt of Employment Services  
Center for Employment Opportunities**

Outcome (%)	Program Group	Control Group	Difference (Impact)	P-Value <sup>a</sup>
Received referral to specific job opening	32.3	18.8	13.5 ***	0.001
At CEO	27.9	9.6	18.3 ***	0.000
At another organization	5.6	11.6	-6.0 **	0.014
Received advice about job interviews	64.6	56.3	8.4 *	0.054
At CEO	55.0	39.0	16.0 ***	0.000
At another organization	14.2	25.2	-10.9 ***	0.002
Received advice about discussing criminal history with potential employers	69.1	60.7	8.4 **	0.044
At CEO	58.7	44.9	13.8 ***	0.002
At another organization	12.9	21.6	-8.7 ***	0.009
Received advice about how to behave on a job	66.4	55.9	10.6 **	0.013
At CEO	57.3	42.7	14.6 ***	0.001
At another organization	10.4	20.2	-9.8 ***	0.002
Received names of people to contact about jobs	29.7	22.6	7.1 *	0.076
At CEO	27.8	15.3	12.5 ***	0.001
At another organization	5.9	10.9	-5.1 **	0.038
Received help putting a résumé together	57.7	60.0	-2.3	0.611
At CEO	52.5	43.7	8.9 **	0.049
At another organization	11.7	26.3	-14.6 ***	0.000
Received advice about filling out job applications	58.5	53.8	4.7	0.285
At CEO	49.7	41.4	8.3 *	0.060
At another organization	9.8	18.6	-8.8 ***	0.004
Received any of the above advice/assistance	82.5	78.9	3.6	0.295
At CEO	74.0	62.8	11.2 ***	0.005
At another organization	27.0	38.5	-11.5 ***	0.006
Sample size (total = 531)	316	215		

SOURCE: MDRC calculations based on responses to the client survey.

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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>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 on the table): 3.4, 4.3, and 3.4.

than CEO — almost 39 percent of the control group received help from another organization, compared with only 27 percent of the program group.

Program group members were more likely to report receiving help in most of the specific areas. Notably, they were much more likely than control group members to receive a referral to a specific job opening: 32 percent of program group members received job referrals, compared with only 19 percent of control group members. The difference of 13.5 percentage points is statistically significant. This result is not surprising, since control group members generally could not receive help from CEO job developers and only 12 percent received such assistance from another organization. As expected, the research groups were equally likely to report receiving assistance in putting together a résumé and getting advice about filling out job applications; many control group members reported receiving such assistance from CEO.

## **Impacts on Support and Guidance**

In addition to providing employment services and transitional jobs to program group members, CEO staff members provided an array of support and guidance to address issues that could prevent clients from successfully reentering the community and the workforce. To estimate the effects of staff interactions with participants, the survey asked participants questions about the support and guidance that they received from CEO and from other organizations. Table 3.3 shows that program group respondents were more likely than control group respondents to report that they could turn to a program staff person for advice and support with personal or family issues: 41 percent of the program group, compared with 31 percent of the control group. The difference is statistically significant. There is also a statistically significant difference between the research groups in an individual's likelihood of viewing a staff member as a mentor or guide. Note that about 15 percent of control group members reported receiving any of these types of support and guidance from CEO staff. In fact, control group members were more likely to report viewing a CEO staff member as a mentor or guide than staff from other organizations.

## **Impacts on Receipt of Child Support and Parenting Services**

Table 3.4 shows that survey respondents in the program group were more likely than those in the control group to participate in child support and parenting activities. Almost 19 percent of program group members reported receiving help with child support issues and

**The Enhanced Services for the Hard-to-Employ Demonstration**

**Table 3.3**

**Impacts on Support and Guidance  
Center for Employment Opportunities**

Outcome (%)	Program Group	Control Group	Difference Impact	P-Value <sup>a</sup>
Can turn to someone on staff for advice and support with personal or family issues	40.5	30.8	9.7 **	0.024
At CEO	30.3	14.6	15.6 ***	0.000
At another organization	17.0	22.4	-5.4	0.119
Views someone on staff as a mentor or guide	28.4	21.4	7.0 *	0.073
At CEO	22.6	12.3	10.3 ***	0.003
At another organization	3.7	3.4	0.3	0.835
Has someone on staff who goes out of the way to help when needed	38.5	34.0	4.5	0.287
At CEO	30.7	18.5	12.2 ***	0.002
At another organization	11.9	21.3	-9.4 ***	0.003
Sample size (total = 531)	316	215		

SOURCE: MDRC calculations based on responses from the client survey.

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. The significance level indicates the probability that one would incorrectly conclude that a difference in the corresponding variables exists between research groups.

<sup>a</sup>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 on the table): 3.8.

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**Table 3.4**

**Impacts on Participation in Parenting Activities**

**Center for Employment Opportunities**

Outcome (%)	Program Group	Control Group	Difference (Impact)	P-Value <sup>a</sup>
Received help with child support issues	18.4	6.0	12.4 ***	0.000
At CEO	15.6	3.9	11.7 ***	0.000
At another organization	2.1	1.4	0.7	0.554
Participated in parenting program	16.9	6.6	10.4 ***	0.000
At CEO	13.6	3.0	10.6 ***	0.000
At another organization	2.8	3.2	-0.4	0.811
Participated in fathers' discussion group <sup>b</sup>	17.9	7.2	10.7 ***	0.000
At CEO	15.4	3.8	11.6 ***	0.000
At another organization	3.3	3.2	0.2	0.928
Sample size (total = 531)	316	215		

SOURCE: MDRC calculations based on responses to the client survey.

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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

The sample in this table includes all respondents, including those who are not parents.

<sup>a</sup>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 on the table): 2.9, 2.7, 3.0, 2.7, 3.0, and 2.7.

<sup>b</sup>Female respondents are excluded from this measure (total = 37).

participated in parenting classes, compared with about 6 percent of the control group.<sup>4</sup> These results are not surprising, given that CEO participants could spend one day a week participating in voluntary activities, which included an extensive fatherhood program that assisted participants with child support issues and parenting classes.

## **Conclusion**

The survey results show that the program group was significantly more likely than the control group to report participating in transitional jobs. As discussed above, since the survey likely underestimated participation in these jobs, it is impossible to estimate CEO's effect on transitional job participation. As expected, program and control group respondents reported being just as likely to participate in employment activities and to receive employment assistance, such as help preparing a résumé and filling out a job application. However, the program and control groups received different types of services. For instance, program group members were more likely to receive referrals to specific job openings, advice on how to behave on a job, help with child support issues, and support addressing personal issues. Finally, the survey results show that the program group members were more likely than control group members to report viewing someone on the CEO staff as a mentor or guide.

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<sup>4</sup>These percentages refer to the entire program and control groups, even though about half the people in each group were not parents.



## Chapter 4

# Impacts of CEO on Employment and Earnings

Many former prisoners have low levels of education and little work experience, along with other characteristics that make it difficult for them to find employment. The added burden of a criminal record can make finding employment even more challenging. The New York City-based Center for Employment Opportunities (CEO) seeks to place program participants in immediate, wage-paying transitional jobs that provide legitimate income during the uncertain period after release from prison. The transitional jobs help participants obtain experience while allowing staff to identify and address workplace problems through observation, job coaching, and other supportive services. Ultimately, the program seeks to increase clients' placement in unsubsidized employment and improve their job retention.

This chapter presents the CEO program's two-year impacts on employment and earnings, using unemployment insurance (UI) data from New York State and the National Directory of New Hires (NDNH) and payroll data from CEO. 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 in the two years following random assignment.<sup>1</sup> CEO's transitional jobs are included in UI wage records. UI-covered earnings data from the National Directory of New Hires are available only for the second year of follow-up.<sup>2</sup> The chapter also presents data on employment and job characteristics as captured by the client survey (discussed in Appendix A).<sup>3</sup>

## Impacts for the Full Sample

- **CEO substantially increased employment early in the follow-up period, but the impact faded over time.**

Table 4.1 presents CEO's impacts on UI-covered employment and earnings. Results are shown separately, by year, for CEO transitional jobs, unsubsidized employment, and total UI-

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<sup>1</sup>Random assignment occurred between January 2004 and October 2005.

<sup>2</sup>Due to data archiving, NDNH earnings data are not available for the first three quarters following random assignment. Therefore, this report presents annual average earnings impacts only for Year 2.

<sup>3</sup>Some transitional employment programs report to the UI system, and some do not; therefore, there is no way of knowing whether any of the non-CEO jobs in the UI records are transitional jobs. In this report, non-CEO employment found in the UI data is considered unsubsidized employment.



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**Table 4.1**

**Impacts on Employment and Earnings**

**Center for Employment Opportunities**

Outcome	Program Group	Control Group	Difference (Impact)	P-Value <sup>a</sup>
<b><u>Employment (%)</u></b>				
CEO transitional employment				
Year 1 (Q1-Q4)	69.5	2.7	66.8 ***	0.000
Year 2 (Q5-Q8)	6.3	1.4	4.9 ***	0.000
Years 1-2 (Q1-Q8)	69.8	3.8	66.0 ***	0.000
Unsubsidized employment <sup>b</sup>				
Year 1 (Q1-Q4)	45.9	48.6	-2.7	0.388
Year 2 (Q5-Q8)	48.7	49.3	-0.7	0.833
Years 1-2 (Q1-Q8)	59.6	62.8	-3.2	0.307
Total UI-covered employment				
Year 1 (Q1-Q4)	81.5	57.5	23.9 ***	0.000
Year 2 (Q5-Q8)	51.4	50.0	1.4	0.664
Years 1-2 (Q1-Q8)	85.2	69.3	16.0 ***	0.000
<b><u>Earnings (\$)</u></b>				
CEO transitional job earnings <sup>c</sup>				
Year 1 (Q1-Q4)	887	31	856 ***	0.000
Year 2 (Q5-Q8)	61	9	52 ***	0.009
Years 1-2 (Q1-Q8) <sup>d</sup>	947	40	907 NA	
Unsubsidized earnings <sup>c</sup>				
Year 1 (Q1-Q4)	---	---	---	---
Year 2 (Q5-Q8)	5,106	4,661	445	0.531
Years 1-2 (Q1-Q8)	---	---	---	---
Total UI-covered earnings <sup>e</sup>				
Year 1 (Q1-Q4)	---	---	---	---
Year 2 (Q5-Q8)	5,167	4,670	497	0.484
Years 1-2 (Q1-Q8)	---	---	---	---
Sample size (total = 973) <sup>f</sup>	564	409		

(continued)

SOURCES: MDRC calculations from the National Directory of New Hires (NDNH) database and unemployment insurance (UI) wage records from New York State and payroll data from CEO's Network for Information Gathering Evaluation and Learning (NIGEL) database.

**Table 4.1 (continued)**

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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>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 on the table): Employment: 2.3604, 1.3036, 2.4045, 2.8239, and 2.6085. Earnings: 52.487.

<sup>b</sup>Unsubsidized employment is estimated from NDNH data, which are incomplete for Quarters 1 to 3. A comparison of aggregate estimates provided for the full sample from the NYS Department of Labor indicates that the results from NDNH are representative of the full sample.

<sup>c</sup>CEO earnings in Year 1 are based on payroll data from CEO's NIGEL.

<sup>d</sup>This difference is not tested for statistical significance because the UI data are estimated from different sources in Year 1 and Year 2 and the years could not be combined on an individual level to provide a total.

<sup>e</sup>Earnings data are incomplete in Year 1 and therefore cannot be estimated accurately in Year 1 or in the total for both years.

<sup>f</sup>Four sample members are missing Social Security numbers and therefore could not be matched to UI data.

covered employment (which includes CEO transitional jobs and unsubsidized jobs).<sup>4</sup> CEO's ultimate goal was to increase employment in unsubsidized jobs. Therefore, it is helpful to examine employment results separately in order to account for the program's effects on unsubsidized jobs separate from CEO transitional jobs. The upper half of the table shows employment in CEO's transitional jobs, unsubsidized employment, and total UI-covered employment. The first four rows show, as expected, that most program group members (70 percent) and only a

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<sup>4</sup>NDNH employment data are available for only a partial sample in Quarters 1 to 3. New York State employment data are available for the full follow-up period but do not provide information to allow a distinction between CEO transitional jobs and unsubsidized employment. Additional state aggregate data for the full sample allowed a comparison of NDNH data for the partial sample in Quarters 1 to 3 and the full sample in those quarters. This comparison showed that the partial sample from NDNH represents findings for the full sample. NDNH data are, therefore, used in this chapter's analysis. Appendix Table B.1 compares NDNH partial sample results and New York State aggregate results for the full sample.

few control group members (3 percent) worked in a CEO transitional job in Year 1.<sup>5</sup> Few sample members worked in a CEO transitional job in Year 2.

The next four rows of the table show impacts on unsubsidized employment. In both years, less than half of both research groups worked in an unsubsidized job. In Year 1, there was no statistically significant difference in working in an unsubsidized job.<sup>6</sup> In Year 2, there was no statistically significant impact on unsubsidized employment or total UI-covered employment.

The final rows of the top panel of the table show that, overall, 85 percent of the program group worked in a UI-covered job for at least one day, compared with 69 percent of the control group, for a 16.0 percentage point increase over the two years.

- **Employment increases were driven by access to CEO’s transitional jobs. Without the CEO transitional jobs, rates of UI-covered employment were low throughout the follow-up period.**

When comparing the results in the upper panel of Table 4.1, it becomes evident that CEO’s large increases in employment were driven by participation in the program’s transitional jobs. As the table shows, there was no statistically significant impact on unsubsidized employment during the follow-up period.<sup>7</sup>

The bottom panel of Table 4.1 presents CEO’s impacts on earnings in each type of employment. Due to data limitations, earnings are not available for Year 1.<sup>8</sup> Earnings data are available for Year 2, and they show that CEO did not significantly increase earnings.

Figure 4.1 presents quarterly employment rates over the follow-up period. The figure shows UI-covered employment patterns for both research groups and examines how the

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<sup>5</sup>The results presented in Table 4.1 are from UI records and, therefore, do not exactly match the results from the program’s management information system data, presented in Chapter 2, which show that about 72 percent of the program group worked in a transitional job in Year 1 (Table 2.2).

<sup>6</sup>Analysis presented below shows that there was a statistically significant reduction in unsubsidized employment in Quarter 1 of the follow-up period.

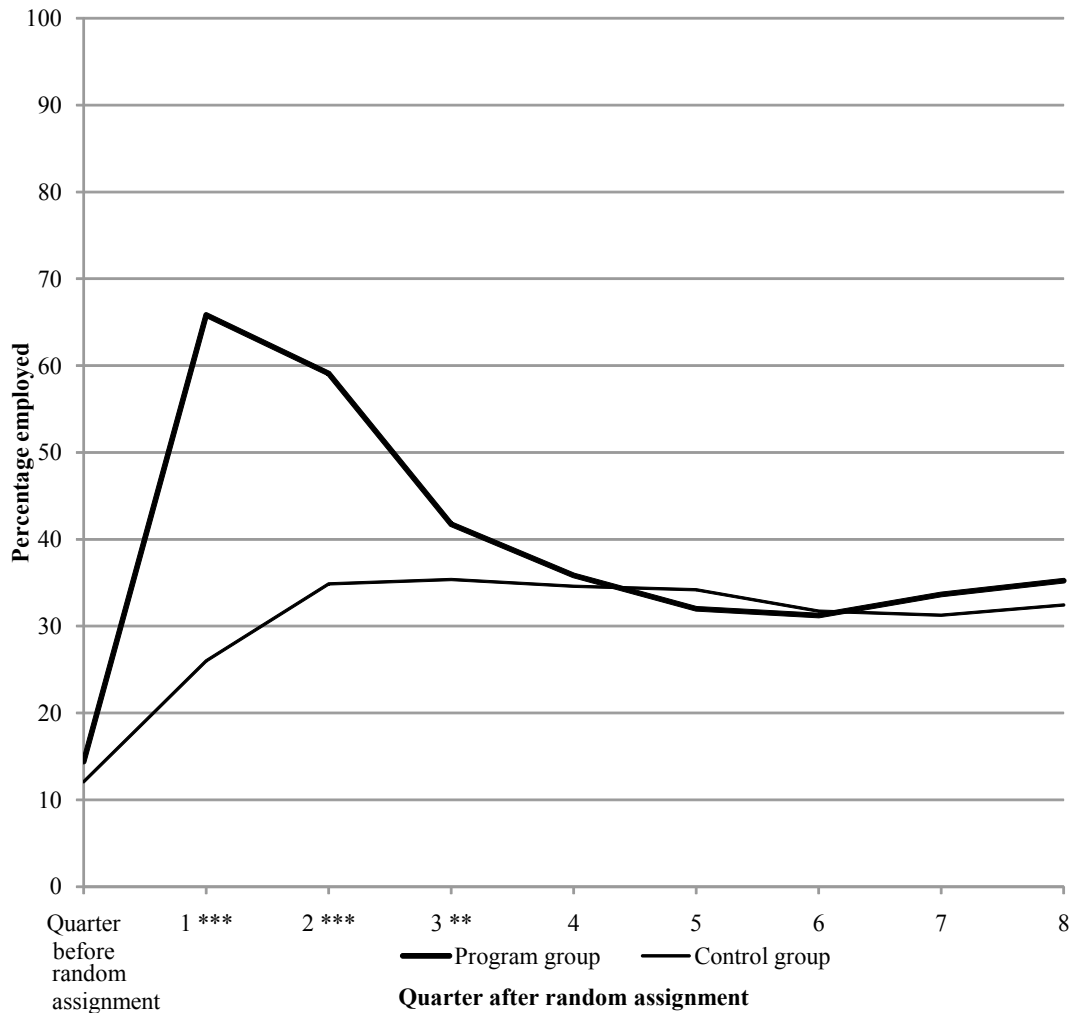
<sup>7</sup>It is important to note that adding CEO transitional employment to unsubsidized employment in a particular year does not produce the total UI-covered employment for that year. This is because total UI-covered employment is defined as working in *either* a transitional job *or* an unsubsidized job and because many sample members worked in both types of jobs in any given period.

<sup>8</sup>Given the large increase in average employment in Year 1, it might seem likely that there would be a corresponding increase in earnings. However, some of CEO’s early increases in transitional employment were offset by a reduction in unsubsidized employment (Appendix Table B.2). In addition, CEO’s jobs were part time and paid only minimum wage, whereas employment obtained by members of the control group may have consisted of higher wages and/or more hours. Taken together, these circumstances make it difficult to predict whether CEO increased earnings in Year 1.

**The Enhanced Services for the Hard-to-Employ Demonstration**

**Figure 4.1**

**Quarterly Impacts on Employment  
Center for Employment Opportunities**



SOURCES: MDRC calculations from the National Directory of New Hires (NDNH) database and 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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

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

patterns changed over time. Differences that are statistically significant are indicated by stars next to the quarter number at the bottom of the graph.

As discussed above, CEO produced large impacts on UI-covered employment early in the follow-up period. The figure shows that 66 percent of the program group worked for at least one day in Quarter 1, compared with only 26 percent of the control group — for an impact of 40 percentage points. (Appendix Table B.2 shows the quarterly employment numbers that were used to produce Figure 4.1.) Results for the control group provide evidence of what the employment patterns would be without CEO’s transitional jobs and related services. The figure shows that, in the absence of CEO, rates of employment in UI-covered jobs were low. Less than one-third of the control group worked in each quarter, and this proportion did not increase substantially over time. For the program group, employment peaked in Quarter 1 and declined until Quarter 4, after which it remained stable and similar to the control group’s employment rate. This pattern shows that the diminishing impacts are a result of a decline in employment for the program group and are not a result of the control group’s “catching up” to the program group over time, as is common in evaluations of employment programs serving low-income populations.

## Impacts on Job Characteristics

Table 4.2 presents CEO’s impacts on employment and on the characteristics of the current or most recent job, as reported in the client survey, which was administered 16 to 23 months after random assignment in most cases. The table shows that roughly equal numbers (45 percent) of program and control group respondents reported that they were working at the time of the interview. This is broadly consistent with the results from UI wage data in Figure 4.1, which shows no impact on employment in Year 2.<sup>9</sup>

Surprisingly, Table 4.2 also shows that the control group was as likely as the program group to report being employed at any time since random assignment. As discussed above, Table 4.1 shows that CEO increased UI-covered employment by 16 percentage points during the same time period. Two factors may account for the difference in results between the two data sources. First, the control group may have been more likely to work in jobs not covered by

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<sup>9</sup>Further analysis was conducted that lined up reported employment at the time of the survey with employment from the UI data in the same quarter for each respondent. This analysis revealed that, most of the time, the two data sources agreed. Roughly 20 percent of both research groups reported employment in the survey that did not show up in UI data. Appendix Table B.3 presents an analysis comparing UI and survey reports of employment in the quarter of the client interview. Some respondents were interviewed while they were incarcerated. The table shows that a significant proportion of the discrepancy in employment between the survey and the administrative records data is explained by employment in prison or jail, which is not recorded in UI data sources.

The Enhanced Services for the Hard-to-Employ Demonstration

Table 4.2

Impacts on Employment and Job Characteristics

Center for Employment Opportunities

Outcome	Program Group	Control Group	Difference (Impact)	P-Value <sup>a</sup>
<b><u>Employment (%)</u></b>				
Ever worked for pay since random assignment	83.4	86.2	-2.8	0.378
Ever worked in temporary, odd job, or day labor since random assignment <sup>b</sup>	37.1	35.2	1.9	0.661
Temporary or odd job	8.5	16.2	-7.7 ***	0.008
Day labor through a program	20.6	7.1	13.5 ***	0.000
Day labor not through a program	13.4	17.8	-4.4	0.174
Currently working for pay	44.6	45.2	-0.6	0.896
In prison	6.2	8.3	-2.1	0.359
In the community	38.4	36.8	1.6	0.715
<b><u>Characteristics of current/most recent job</u></b>				
Current or most recent job is day labor, temporary, or odd job (%)	14.4	10.8	3.6	0.227
Hours per week (%)				
Fewer than 30	14.2	14.0	0.2	0.955
30-34	7.4	5.4	2.0	0.364
35-44	44.9	49.3	-4.4	0.328
45 or more	16.7	17.0	-0.3	0.943
Average hours per week	31.1	33.3	-2.2	0.160
Hours per week vary (%)				
A lot or a fair amount	22.7	28.5	-5.8	0.139
A little or hardly at all	59.9	56.3	3.6	0.423
Usual work schedule (%)				
Regular daytime shift	53.4	61.6	-8.2 *	0.063
Regular evening shift	12.2	7.1	5.1 *	0.064
Regular night shift	4.9	5.7	-0.8	0.684
Other	11.8	10.6	1.3	0.651
Usual schedule includes weekend work (%)	39.8	41.6	-1.8	0.684
Hourly wage (%)				
Less than \$6.00	10.3	11.0	-0.7	0.793
\$6.00-\$7.99	25.3	25.1	0.2	0.960
\$8.00-\$9.99	20.0	18.0	2.0	0.594
\$10.00-\$11.99	12.3	14.8	-2.5	0.421
\$12.00 or more	14.8	16.2	-1.4	0.673
<i>Average hourly wage, among those employed (\$) <sup>c</sup></i>	9.55	9.41		

(continued)

**Table 4.2 (continued)**

Outcome	Program Group	Control Group	Difference (Impact)	P-Value
Employer-provided benefits (%)				
Sick days with full pay	16.3	14.1	2.2	0.500
Paid vacation	21.6	15.2	6.4 *	0.067
Paid holidays other than Christmas and New Year	20.0	19.7	0.3	0.937
Retirement plan	9.2	9.3	0.0	0.991
Dental benefits	20.3	21.5	-1.2	0.738
Health benefits	8.9	11.7	-2.8	0.300
Received a promotion at this job (%)	11.8	7.1	4.7 *	0.085
Sample size (total = 531)	316	215		

SOURCE: MDRC calculations based on responses to the client survey.

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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>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 on the table): 3.18.

<sup>b</sup>Subcategories are not mutually exclusive and may not sum to the total.

<sup>c</sup>This measure is calculated among those who worked since random assignment and is therefore considered nonexperimental and is not tested for statistical significance.

unemployment insurance. This is important because jobs not covered by the UI system may be more informal and of lower quality than jobs that are covered. Second, Chapter 3 shows that many survey respondents in the program group did not remember working in a CEO transitional job. Indeed, further analysis (not shown) found that almost all the program group members who reported on the survey that they had not worked since random assignment received at least one transitional job paycheck from CEO. This analysis suggests that had program group respondents accurately recalled their employment in CEO's transitional jobs, the "ever employed" impact estimated from the survey would have closely mirrored the results from administrative records. As discussed in Chapter 3, it is not possible to determine how many control group members accessed transitional jobs through organizations other than CEO.

The second panel of Table 4.2 presents CEO's impacts on the types of jobs obtained by participants since random assignment. These measures show that about a third of both research groups worked in day-labor, temporary, or odd jobs since random assignment. For the program group, a large proportion of these jobs were day labor through CEO. These results also show that the control group was more likely to report working in jobs described as temporary or odd

jobs, indicating that CEO reduced employment in temporary or odd jobs by 7.7 percentage points. This finding suggests that some of CEO's impact on employment may have been a result of moving people from an informal job to a UI-covered job.

The remainder of Table 4.2 shows that there were no significant differences between the research groups in the characteristics of their current or most recent job at the time of the survey interview. Most reported working full time — just over 31 hours per week for both research groups. About one-fourth reported that their hours varied quite a bit. Most respondents reported fairly low hourly wages. Fewer than one in five reported earning \$12.00 or more per hour, with about a third earning less than \$8.00 per hour. Among those who had worked since random assignment, the average hourly wage was \$9.55 for program group members and \$9.41 for their control group counterparts. A small proportion reported receiving any type of employer-provided benefits, such as paid days off or health insurance. Notably, program group members were more likely than their control group counterparts to report having paid vacation and having received a promotion at their current or most recent job (12 percent and 7 percent, respectively). Among those who worked, the average length of time at the most recent job was about eight months for both research groups (not shown).

## **Employment Impacts for Subgroups**

### **Subgroups Defined by Time Since Release from Prison**

Research on reentry issues stresses the importance of support immediately on release from prison. The first days and weeks after release are the most uncertain and are thought to be the crucial point for intervention. CEO's program was designed to assist people coming directly out of prison. As discussed in Chapter 1, however, a large proportion of the study sample members had been out of prison for a long time before coming to CEO. On average, sample members had been out of prison for a year before enrolling in the study; 41 percent of program group members and 39 percent of control group members were released from prison within three months before random assignment (Table 1.2). Results were examined separately for the 375 sample members who came to CEO within three months after release from state prison.<sup>10</sup> A large majority of CEO's overall client population comes to the program either immediately on release or shortly thereafter (for example, Shock Incarceration, or "boot camp," graduates). More broadly, the "recently released" group better fits the usual definition of a "reentry" population, and the results for that group can test the widely held assumption that reentry programs are more effective if they begin working with ex-prisoners immediately after release.

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<sup>10</sup>Some sample members were released from New York City jails during the three months prior to reporting to CEO. These sample members are *not* included in the "recently released" subgroup.



One-year findings from this evaluation showed that CEO's impacts on employment for the recently released subgroup were similar to impacts for the group that reported to CEO further from release, though they were somewhat larger and declined more slowly.<sup>11</sup>

- **Employment impacts for the recently released subgroup are similar to results for the full sample.**

Table 4.3 presents CEO's impacts on employment for subgroups defined by length of time between prison release and random assignment. As was shown for the full sample, impacts on UI-covered employment were very large for both subgroups in Year 1 and diminished by Year 2. The table shows that CEO increased employment over the two-year follow-up period by 22.7 percentage points for the recently released subgroup and by 14.3 percentage points for those who reported to CEO further from release. As was shown in the one-year report, the impacts on employment were larger for the recently released group in Year 1, but overall effects for the two-year follow-up are not statistically different for the subgroups. (The rightmost column of Table 4.3 shows H-stars to indicate whether the impacts for the two subgroups are statistically different from each other.)<sup>12</sup> Appendix Tables E.1 and E.2 present detailed quarterly employment and earnings impacts for the subgroups, by length of time between prison release and random assignment.

### **Other Subgroups**

The research team examined results for other subgroups based on literature suggesting that CEO may have affected certain subgroups differently. For example, prior research has shown that employment programs can be more successful for older men and women coming out of prison.<sup>13</sup> Results were also examined for those who had a high school diploma or a General Educational Development (GED) certificate at baseline and for those who did not, using the hypothesis that the program may have better results for a more disadvantaged subgroup that is less likely to find employment without assistance. Results for these other subgroups are generally similar to results for the full sample. Findings for other subgroups are presented in Appendix E.

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<sup>11</sup>Bloom, Redcross, Zweig, and Azurdia (2007). Recidivism impacts in Year 1 were much larger for the recently released subgroup.

<sup>12</sup>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 F-statistic from analysis of variance (ANOVA) tests is interpreted.

<sup>13</sup>Uggen (2000).

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Table 4.3

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

Center for Employment Opportunities

Outcome (%)	Length of Time Between Prison Release and Random Assignment								H-Stars <sup>b</sup>
	Less Than 3 Months				More Than 3 Months				
	Program Group	Control Group	Difference (Impact)	P-Value	Program Group	Control Group	Difference (Impact)	P-Value <sup>a</sup>	
CEO transitional employment									
Year 1 (Q1-Q4)	73.3	-0.6	74.0 ***	0.000	67.9	3.5	64.4 ***	0.000	*
Year 2 (Q5-Q8)	2.9	0.2	2.6 *	0.062	8.4	2.2	6.2 ***	0.003	
Years 1-2 (Q1-Q8)	73.3	-0.6	74.0 ***	0.000	68.1	5.4	62.7 ***	0.000	**
Total UI-covered employment									
Year 1 (Q1-Q4)	86.6	54.0	32.7 ***	0.000	79.8	58.2	21.6 ***	0.000	*
Year 2 (Q5-Q8)	53.2	48.0	5.2	0.325	49.5	50.7	-1.2	0.774	
Years 1-2 (Q1-Q8)	89.4	66.7	22.7 ***	0.000	83.8	69.4	14.3 ***	0.000	
Earnings in Year 2 (\$)	6,322	5,703	620	0.593	4,800	3,729	1,071	0.249	
Sample size (total = 925) <sup>c</sup>	220	155			313	237			

SOURCES: MDRC calculations from the National Directory of New Hires (NDNH) database and unemployment insurance (UI) wage records from New York State and payroll data from CEO's Network for Information Gathering Evaluation and Learning (NIGEL) database.

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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>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 on the table and beginning with the "Less Than 3 Months" subgroup): 3.7945, 3.7945, 4.5319, 4.1909, 3.1721, 3.282, 3.8253, and 3.5286.

<sup>b</sup>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.

<sup>c</sup>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. In addition, four sample members are missing Social Security numbers and therefore could not be matched to UI data.

## The Role of CEO's Transitional Jobs

The results presented above show that CEO's transitional jobs contributed a great deal to the program's impacts on UI-covered employment. However, the increase in employment in transitional jobs did not lead to higher employment in unsubsidized jobs. Survey data suggest that the quality of jobs obtained by both the program group members and the control group members was similar.

Figure 4.2 examines CEO's quarterly impacts on UI-covered employment over the follow-up period, excluding CEO's transitional jobs. The figure shows that, in the absence of CEO's jobs, employment rates for the program group are similar, on average, to the control group's rates. In the early quarters, however, CEO led to reductions in unsubsidized employment. In Quarter 1, 19 percent of program group members worked in an unsubsidized job, compared with 28 percent of control group members. The difference of minus 9 percentage points is statistically significant. (Appendix Table B.2 shows the numbers used to generate this figure.) The reduction quickly faded after the first quarter, however. This result is not unexpected and suggests that a small number of participants would have been able to obtain employment quickly early on, without CEO, and ended up substituting CEO's transitional employment for these other jobs.<sup>14</sup>

To shed light on how the CEO transitional job experience may have influenced unsubsidized employment after participants left the program, Box 4.1 presents a diagram of the employment patterns for a typical sample of 100 program group members and examines the available data more closely. Data from CEO's management information system are used to identify the point at which participants left the program, and then this information is matched to UI data from NDNH.<sup>15</sup> This analysis uses administrative records on UI-covered jobs to estimate employment. As discussed above, some participants worked in jobs that are not covered by unemployment insurance.

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<sup>14</sup>Data limitations in both the survey and the UI records prevent the researchers from knowing exactly how many control group members accessed transitional jobs through organizations other than CEO. It is also possible that some control group participants enrolled in transitional employment at another agency. These transitional jobs may also be in the UI data, but it is not possible to identify whether UI-covered employment is transitional using the available data.

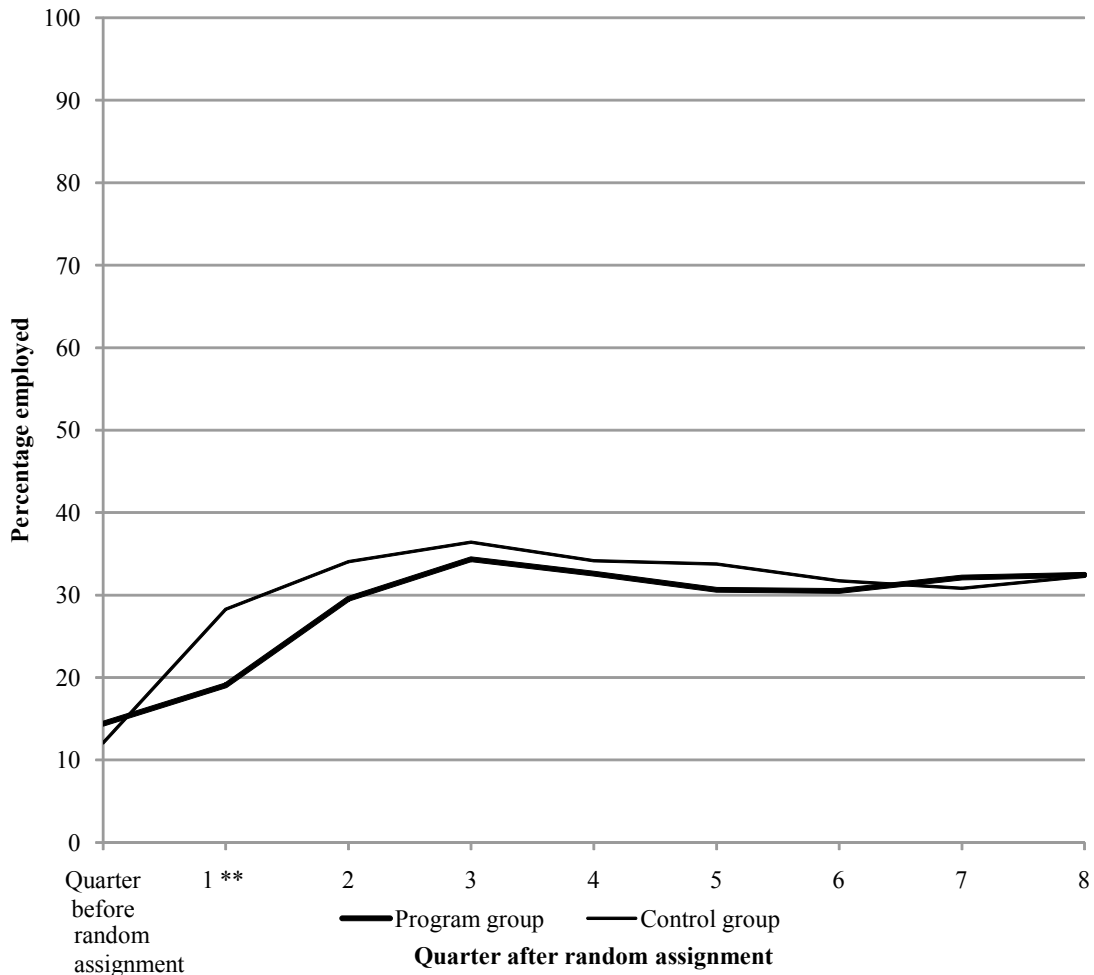
<sup>15</sup>Note that, in this analysis, CEO's transitional jobs are identified using administrative data, which, therefore, may not exactly match the data in CEO's management information system, presented in Chapter 2.

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**Figure 4.2**

**Quarterly Impacts on Employment Without CEO Transitional Jobs**

**Center for Employment Opportunities**



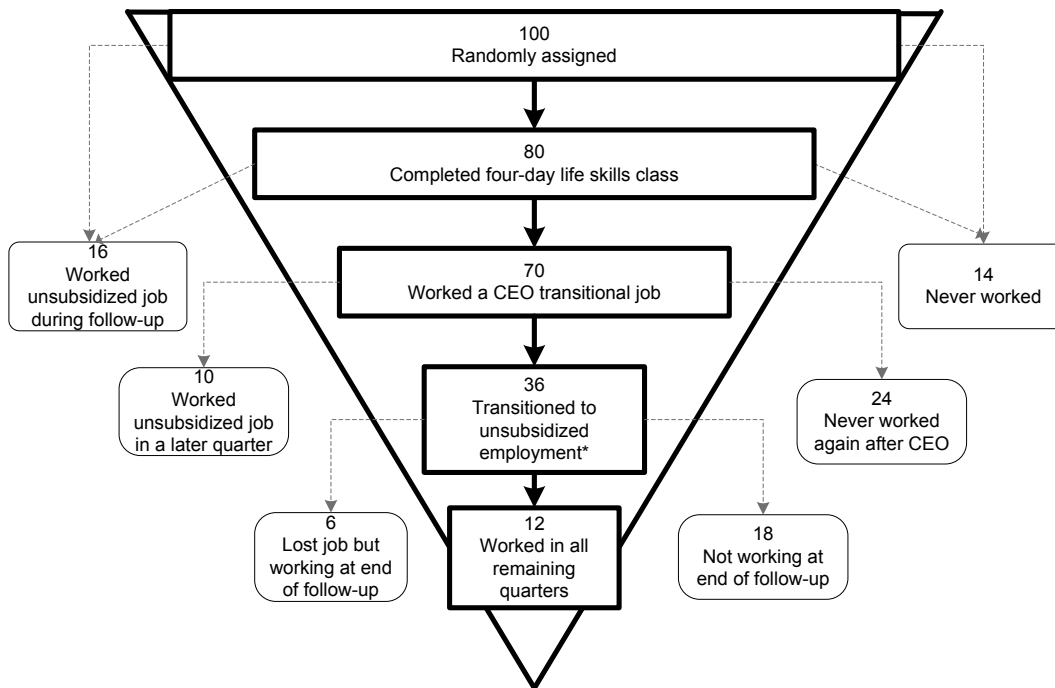
SOURCE: MDRC calculations from the National Directory of New Hires (NDNH) database and unemployment insurance (UI) wage records from New York State.

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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

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

**Box 4.1**  
**Employment Patterns for a Typical Sample of 100 Program Group Members**



The diagram above shows the UI-covered employment outcomes for a typical sample of 100 program group participants. As the figure shows, many participants did not participate in all aspects of the program and, therefore, did not get CEO’s full intended treatment. The diagram shows that 20 participants dropped out before completing the initial four-day life skills class and that an additional 10 participants completed the class but did not work in a CEO transitional job. Ultimately, 36 worked in a transitional job and were placed in unsubsidized employment.\* For those who did graduate from the program, only a small proportion (12) were able to remain steadily employed after leaving CEO. Although some participants who lost jobs after they left the program were able to regain employment by the end of the follow-up period, many experienced long spells of unemployment between jobs.

SOURCES: MDRC calculations based on unemployment insurance (UI) wage records from New York State provided by the National Directory of New Hires (NDNH) database and CEO’s Network for Information Gathering Evaluation and Learning (NIGEL) database.

NOTES: The numbers in this diagram are an approximation and may not exactly match similar outcomes presented in other sections of this report due to differences in data sources and rounding.

\*The analysis assumes that a person “transitioned” to regular employment if he worked in an unsubsidized job in the same quarter as the CEO transitional job or in the quarter immediately following. This assumption may overestimate the number of people who transitioned. For example, it is possible that a person could have worked for one day in a CEO transitional job, then left the program, and then not worked in an unsubsidized job for another three months. Notably, however, the proportion that was assumed to transition in this analysis — 36 percent — closely matches data in CEO’s management information system, which shows that 30 percent of the program group were placed in an unsubsidized job.

## **Conclusion**

The findings above show that, in the absence of CEO, employment levels for these returning prisoners were very low throughout the two-year follow-up period. Few were able to find and keep steady jobs in the absence of intervention and provision of a transitional job.

CEO employed people who otherwise would not have worked in a UI-covered job — as shown by the 16 percentage point increase in the proportion who worked at all during the follow-up. Although CEO increased UI-covered employment early in the follow-up as a result of the transitional jobs, this increase was short-lived and did not lead to an increase in unsubsidized employment through two years after program entry. About one-third of those who started the program eventually graduated and were placed in an unsubsidized job; however, among those, few were able to remain employed steadily after leaving CEO.

Informal and odd jobs were a source of employment for about a fourth of the sample. Some of CEO's impact on employment was the apparent result of moving people from informal jobs to UI-covered employment, in the form of a CEO-provided transitional job.

Taken together, the employment results suggest that transitional jobs are an effective strategy to increase employment among former prisoners but that additional supports or supplements may be needed to help them gain and retain regular employment and increase earnings. Evidence from the evaluation suggests that short-term subsidized employment does not necessarily lead to increases in unsubsidized employment. As the program group moved out of CEO's transitional jobs, the employment rate declined. Earnings supplements in combination with additional skills training may be needed to sustain unsubsidized employment for this population.



## Chapter 5

# Impacts of CEO on Recidivism

It is widely believed that interventions designed to increase employment for former prisoners may also lead to reduced recidivism. Legitimate employment could reduce the incentive to commit crimes, and it may also connect former prisoners to more positive social networks and daily routines, helping to ease the transition into the community after leaving prison.<sup>1</sup> The premise of the program at the Center for Employment Opportunities (CEO) in New York City is that the provision of transitional jobs and other employment services will lead to higher levels of employment and, ultimately, reductions in recidivism.

Table 4.1 in Chapter 4 shows that CEO led to substantial increases in employment early in this study's follow-up period and that these increases were primarily the result of CEO's transitional jobs. For the program group, employment peaked in Quarter 1 after random assignment, and it declined until Quarter 4, after which it remained stable and similar to the control group's employment rate.<sup>2</sup> The program did not increase employment or earnings in the second year of follow-up.

This chapter presents CEO's impacts on key measures of recidivism. Administrative data from the New York State Division of Criminal Justice Services (DCJS) are used to measure arrests, convictions, prison incarceration, and parole outcomes. Administrative data from the New York City Department of Correction are used to measure jail incarceration. Appendix C describes how key recidivism measures are defined in this analysis.

## Impacts for the Full Sample

- **For the full sample, CEO led to reductions in convictions and incarceration in prison over the first two years of the follow-up period.**

Table 5.1 presents CEO's impacts on measures of recidivism for the two-year follow-up period. The data provide a complete picture of convictions and incarceration in both New

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<sup>1</sup>Bloom (2006).

<sup>2</sup>Random assignment occurred between January 2004 and October 2005.



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**Table 5.1**

**Two-Year Impacts on Recidivism  
Center for Employment Opportunities**

Outcome	Program Group	Control Group	Difference (Impact)	P-Value
Arrested <sup>a</sup> (%)	37.7	41.8	-4.1	0.192
Convicted of a crime <sup>b</sup> (%)	30.5	38.3	-7.7 **	0.011
Convicted of a felony	6.6	7.7	-1.1	0.536
Convicted of a misdemeanor	22.8	29.4	-6.6 **	0.019
Convicted of a violent crime <sup>c</sup> (%)	4.4	5.8	-1.5	0.309
Incarcerated <sup>d</sup> (%)	49.5	55.4	-5.9 *	0.064
Prison	25.3	27.7	-2.4	0.399
Jail	49.1	53.5	-4.4	0.168
Incarcerated for a new crime (%)	13.9	16.9	-2.9	0.212
Prison	4.2	6.8	-2.6 *	0.077
Jail	10.1	10.8	-0.7	0.718
Incarcerated for a technical parole violation (%)	31.8	28.9	2.9	0.340
Prison	17.4	15.5	1.9	0.428
Jail	29.1	25.6	3.5	0.234
Total days incarcerated	100	107	-7	0.500
Prison	46	54	-8	0.244
Jail	54	53	1	0.828
Arrested, convicted, or incarcerated (%)	55.7	62.5	-6.8 **	0.032
<b><u>Status in the last quarter of Year 2<sup>e</sup> (%)</u></b>				
Incarcerated and employed	3.5	1.9	1.6	0.135
Incarcerated and not employed	21.4	25.0	-3.7	0.177
Not incarcerated and employed	29.3	26.9	2.4	0.404
Not incarcerated and not employed	45.9	46.3	-0.4	0.911
Sample size (total = 977)	568	409		

(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).

**Table 5.1 (continued)**

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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>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.

<sup>b</sup>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. The total includes convictions for "other" reasons, felonies, and misdemeanor crimes.

<sup>c</sup>Violent crimes are based on conviction charges defined by Langan and Levin (2002). See Appendix C for charges defined as violent.

<sup>d</sup>Includes all reasons for incarceration, such as sentences for new crimes, technical violations of parole, detainee (jail), and other reasons. Therefore, incarcerations for new crimes and parole violations do not sum to the percentage incarcerated. See Appendix Table C.1 for more details about reasons for incarceration.

<sup>e</sup>Incarceration status based on Quarter 8 after random assignment. Incarceration includes both prison and jail.

York State prisons and New York City jails. Arrests include only unsealed arrests. Impacts on all arrests, including unsealed and sealed events, are shown in Appendix Table C.1.<sup>3</sup>

The first row in Table 5.1 shows that there is no significant difference between the program and control groups in the percentage arrested within two years of release from prison. About two-fifths of both groups were arrested within two years. The program group was significantly less likely to be convicted of a crime (31 percent, compared with 38 percent). The reduction in convictions was driven primarily by CEO's impact on misdemeanor-level charges (minus 6.6 percentage points). CEO reduced the proportion incarcerated in either prison or jail by 5.9 percentage points. The program also generated reductions in incarceration in state prison due to a sentence for a new crime (minus 2.6 percentage points).<sup>4</sup> It is interesting to note that incarceration resulting from a new sentence was lower than the proportion convicted of a new crime; 38 percent of the control group were convicted of a crime, but only 17 percent were admitted to prison or jail because of a new sentence. This could be related to the fact that the majority of convictions were for misdemeanor-level crimes that may not carry additional prison or jail time. It is also possible that some of these misdemeanor convictions were classified as

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<sup>3</sup>Data containing sealed arrests were provided to MDRC in a form that did not permit the identification of any individual sample member. The researchers could not tell exactly which members of the sample had a sealed event; it was possible to tell only that an individual was a program group or control group member.

<sup>4</sup>Between 4 percent and 6 percent were incarcerated for other or "unknown" reasons (Appendix Table C.1).

**Box 5.1**  
**Reasons for Convictions**

Reason (%)	Program Group	Control Group	Difference (Impact)	P-Value
Violent crime	4.4	5.8	-1.5	0.309
Drug crime	15.6	19.9	-4.3 *	0.079
Property crime	14.9	18.3	-3.4	0.159
Public order crime	3.4	2.9	0.5	0.674
Other	0.6	0.5	0.1	0.871

technical parole violations as the reason for admission.<sup>5</sup> A small proportion of convictions were for felony-level charges, predominantly stemming from drug- or property-related crimes. Box 5.1 presents detailed data about the reasons for conviction for both research groups. As shown in Box 5.1 and Table 5.1, very few convictions were for violent crimes.<sup>6</sup> In this analysis, violent crime consists mainly of misdemeanor-level assault charges.

Row 6 of Table 5.1 shows that 55 percent of the control group were reincarcerated in jail or prison in the two-year follow-up period, compared with 50 percent of the program group. This reduction of 5.9 percentage points in incarceration is statistically significant. Incarceration includes admissions to prison or jail for new sentences (crimes), technical parole violations, detainees (jail only), and other or unknown reasons (prison only).<sup>7</sup> The table shows that more than one-fourth of both research groups were reincarcerated for a parole violation; about half were returned to prison for these violations.

Near the bottom of Table 5.1, a summary measure that combines arrests, convictions, and incarceration shows that CEO reduced recidivism by 6.8 percentage points. More than half of the program group and nearly two-thirds of the control group experienced some form of recidivism in the two years following random assignment.<sup>8</sup>

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<sup>5</sup>In some cases, there is a lag in time between sentencing and admission to prison; therefore, for a small number of convictions, the admission to prison may not have occurred during the two-year follow-up period.

<sup>6</sup>The classification of charges is based on definitions from Langan and Levin (2002).

<sup>7</sup>Appendix Table C.1 presents a detailed breakdown of reasons for incarceration in both prison and jail.

<sup>8</sup>Note that the levels of the summary recidivism measure are higher than the proportion of the sample who were arrested. This measure accounts for all incarcerations, convictions, and unsealed arrests. Incarceration data  
(continued)

Although recidivism rates seem high, the research sample's rate is actually lower than the state average for prisoners returning to New York State.<sup>9</sup> About 33 percent of people released from prison in New York State in 2004 and 2005 returned to prison within two years,<sup>10</sup> compared with only about 28 percent of the study's control group. Returns to prison for parole violations were much lower than the state average.<sup>11</sup> Roughly 60 percent of the research sample had already been out of prison for more than three months when they entered the study; on average, the research sample had been out of prison for a year before entering the study. Thus, it is also useful to compare the two-year recidivism rates from the study with the three-year rates for returning prisoners in New York State. The research sample has even lower recidivism rates: 40 percent of state releases returned to prison within three years, compared with 28 percent of the study's control group. The fact that recidivism rates for the research sample are lower than the state average suggests that this group may have been relatively more motivated than the average ex-prisoner. Indeed, the fact that individuals in the study reported to CEO for employment assistance may also indicate a higher level of motivation among this group.

The bottom panel of Table 5.1 provides information about the sample members' employment and incarceration status in the last quarter of follow-up.<sup>12</sup> By the end of Year 2, both research groups show similar combined outcomes. Over 45 percent were neither working nor employed in a UI-covered job during the last quarter of follow-up. Chapter 6 presents impacts on outcomes as reported by the client survey. The survey findings may provide insights into the ways that individuals were supporting themselves and into their housing and family circumstances.

Impacts on additional measures of recidivism for the two research groups are presented in Appendix Table C.1.

- **CEO reduced recidivism in each of the first two years of the study period.**

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include all admissions to and releases from prison and jail, regardless of arrest and conviction status, whereas many arrests are "sealed" if they do not end in conviction. In addition, some returns to jail or prison as a result of a parole violation would not show up as an "arrest" on the administrative data because parole officers sometimes transport a parolee directly to jail.

<sup>9</sup>See the 2007 Crimestat report (New York State Division of Criminal Justice Services, 2008).

<sup>10</sup>New York State recidivism rates are based on prison releases in 2004 and 2005. For the vast majority of study participants, the calendar period for prison release ranged from 2003 to 2005.

<sup>11</sup>Some of this difference may be a result of categorization of other or unknown admission reasons. DCJS does not show how these admissions are categorized in the 2007 Crimestat report. For the study, these admissions are categorized separately from parole violations and new crimes, and they account for about 5 percent of prison incarcerations.

<sup>12</sup>This measure is based on an individual's status in Quarter 8 and can be measured only quarterly because that is when UI employment data are reported.

Table 5.2 presents CEO's impacts on several key measures of recidivism separately for Year 1 and Year 2. This table focuses only on measures presented in the first-year report and shows impacts on arrests, convictions, and incarceration in prison.<sup>13</sup> This analysis is a useful follow-up to the earlier findings and can aid in understanding how CEO's recidivism impacts played out over time. This analysis may also help to shed light on whether CEO's impacts on employment — which were concentrated in the first year — match its impacts on recidivism.

The table shows that the program's positive impacts on recidivism can be found in both years, though the exact outcomes that were affected in each year differ. Notably, CEO's impacts on arrests and convictions were larger in the second year of follow-up. The first rows of Table 5.2 show that CEO had no impact on arrests in Year 1 but that the program reduced arrests by nearly 5 percentage points in Year 2. The next rows show that CEO reduced felony convictions in Year 1 but that the program's impact on convictions was driven by a 7.1 percentage point reduction in misdemeanors in Year 2. Reductions on prison incarceration for a new crime were found in Year 1 but not in Year 2. Impacts on additional measures of recidivism, by year, are presented in Appendix Tables C.2 and C.3.

These findings show that CEO reduced recidivism in both years of follow-up and that, for some outcomes, the impacts were larger in Year 2. The relationship between recidivism and employment, however, is not straightforward. As reported in Chapter 4 (Table 4.1), the impacts on employment were largest earlier in the follow-up, and there were no impacts on employment in the second year. The findings also raise questions about the pathway through which CEO reduced recidivism. As an employment program, CEO was designed to reduce recidivism indirectly through employment. Because CEO had no impact on employment in Year 2, however, these findings raise questions about *how* CEO reduced recidivism during that time. Future analyses will seek to understand more about the ways in which CEO produced impacts on recidivism by investigating the links among outcomes, individual characteristics, and CEO participation patterns for program group members.

## **Impacts on Recidivism for Subgroups**

### **Subgroups Defined by Time Since Release from Prison**

The first days and weeks out of prison are thought to be a crucial point for program intervention, and programs that intervene early may be more successful. Indeed, the one-year results published from this evaluation show that CEO's positive impacts on recidivism in Year

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<sup>13</sup>The earlier report (Bloom, Redcross, Zweig, and Azurdia, 2007) does not include measures on incarceration in jail as these data were not available at the time.

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**Table 5.2**

**Impacts on Selected Measures of Recidivism in Year 1 and Year 2**

**Center for Employment Opportunities**

Outcome	Program Group	Control Group	Difference (Impact)	P-Value
Arrested <sup>a</sup> (%)				
Year 1	21.7	22.9	-1.3	0.638
Year 2	22.8	27.5	-4.6 *	0.098
Convicted of a felony (%)				
Year 1	1.4	3.1	-1.7 *	0.071
Year 2	5.2	4.5	0.7	0.630
Convicted of a misdemeanor (%)				
Year 1	11.8	12.1	-0.3	0.897
Year 2	14.5	21.5	-7.1 ***	0.004
Convicted of a violent crime (%)				
Year 1	1.9	1.4	0.5	0.542
Year 2	2.5	4.5	-1.9	0.106
Admitted to state prison <sup>b</sup> (%)				
Year 1	11.0	14.0	-3.0	0.166
Year 2	16.7	17.5	-0.8	0.733
Incarcerated in prison for a new crime (%)				
Year 1	0.8	3.0	-2.2 **	0.012
Year 2	3.4	3.8	-0.4	0.742
Incarcerated in prison for a technical parole violation (%)				
Year 1	8.0	9.5	-1.5	0.412
Year 2	11.2	9.5	1.7	0.395
Total days incarcerated in prison				
Year 1	12	13	-1	0.583
Year 2	34	40	-7	0.228
Sample size (total = 977)	568	409		

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

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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>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.

<sup>b</sup>Includes all reasons for incarceration, such as sentences for new crimes, technical violations of parole, detainee (jail), and other reasons. Therefore, incarcerations for new crimes and parole violations do not sum to the percentage incarcerated. See Appendix Table C.1 for more details about reasons for incarceration.

1 are concentrated in the subgroup that had been released recently from prison (41 percent of the program group and 39 percent of the control group reported to CEO within three months of release).<sup>14</sup> For this subgroup, CEO substantially reduced prison incarceration and felony convictions in the first year of follow-up. For the subgroup that came to CEO later after their release, early results show that the program had no effect on recidivism.

Table 5.3 presents CEO's impacts on recidivism for subgroups defined by time since release from prison for the full follow-up period. The columns on the left side of the table show impacts for the group that reported to CEO within three months of release from prison, and the columns on the right side show impacts for the group that came to CEO more than three months after release. The column labeled "H-stars" on the rightmost side of the table indicates whether the difference in impacts between the two subgroups is statistically significant. This statistic shows that, over the two-year follow-up period, impacts on only a few measures of recidivism are statistically different across the subgroups.

- **CEO reduced recidivism both for people who came to CEO soon after release and for those who came later; earlier differences in results for these two groups are less pronounced in Year 2.**

As mentioned above, in Year 1, recidivism reductions were found only among the recently released group. The results in Table 5.3 show that, over the course of the two-year follow-up period, reductions in recidivism can be found among both subgroups. For example, the table shows that among those who came to CEO within three months of prison release, CEO led to a 12.2 percentage point reduction in misdemeanor convictions. For those who reported further from release, the program also significantly reduced convictions, but these reductions were driven by a decrease in felony-level crime. Program group members in the recently released subgroup spent 25 fewer days in prison over the two-year period. This reduction is not seen in the subgroup that reported to CEO further from release.

Table 5.4 focuses on CEO's impacts on selected measures of recidivism and shows details about exactly which recidivism outcomes were affected for both subgroups in the first and second years of follow-up. This analysis provides some evidence about the reason that impact differences between the two subgroups became less clear over time; it shows that recidivism impacts continued into Year 2 for the recently released subgroup while new impacts emerged later in the follow-up for the group that reported to CEO later. Although there were no impacts on recidivism in Year 1 for the group that reported to CEO further from release, the program led

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<sup>14</sup>Bloom, Redcross, Zweig, and Azurdia (2007).

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Table 5.3

Two-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								H-Stars <sup>g</sup>
	Less Than 3 Months				More Than 3 Months				
	Program Group	Control Group	Difference (Impact)	P-Value	Program Group	Control Group	Difference (Impact)	P-Value	
Arrested <sup>a</sup> (%)	40.2	46.3	-6.1	0.235	36.2	39.8	-3.7	0.392	
Convicted of a crime <sup>b</sup> (%)	32.2	42.9	-10.7 **	0.030	29.9	36.4	-6.5	0.118	
Convicted of a felony	12.7	9.0	3.7	0.280	2.8	6.8	-4.0 **	0.031	**
Convicted of a misdemeanor	20.8	33.0	-12.2 ***	0.007	25.5	27.9	-2.4	0.536	*
Convicted of a violent crime <sup>c</sup> (%)	4.3	8.4	-4.1	0.123	4.4	4.4	0.0	0.990	
Incarcerated <sup>d</sup> (%)	48.5	60.8	-12.3 **	0.016	50.4	53.6	-3.2	0.457	
Prison	28.1	32.7	-4.6	0.338	25.9	25.3	0.6	0.877	
Jail	47.5	60.4	-13.0 **	0.011	50.4	50.7	-0.3	0.936	*
Incarcerated for a new crime (%)	16.7	19.9	-3.2	0.436	13.5	15.1	-1.5	0.620	
Prison	7.1	8.1	-1.0	0.717	2.5	6.2	-3.8 **	0.033	
Jail	10.2	12.3	-2.1	0.540	11.3	9.7	1.6	0.551	
Incarcerated for a technical parole violation (%)	29.2	33.0	-3.8	0.436	33.7	27.9	5.8	0.149	
Prison	18.4	18.6	-0.3	0.950	18.7	13.7	5.0	0.119	
Jail	24.3	29.1	-4.8	0.311	32.4	25.0	7.4 *	0.062	**
Total days incarcerated	119	146	-27	0.144	93	84	9	0.439	*
Prison	51	76	-25 **	0.049	47	41	6	0.467	**
Jail	68	70	-2	0.844	46	43	3	0.612	
Arrested, convicted, or incarcerated (%)	54.2	66.4	-12.2 **	0.016	56.8	62.2	-5.3	0.212	

(continued)



**Table 5.3 (continued)**

Outcome	Length of Time Between Prison Release and Random Assignment								H-Stars <sup>g</sup>
	Less Than 3 Months				More Than 3 Months				
	Program Group	Control Group	Difference (Impact)	P-Value	Program Group	Control Group	Difference (Impact)	P-Value	
<b>Status in the last quarter of Year 2<sup>f</sup> (%)</b>									
Incarcerated and employed	3.2	2.5	0.7	0.684	3.9	1.4	2.5 *	0.094	
Incarcerated and not employed	28.1	31.0	-2.9	0.546	17.4	21.1	-3.8	0.267	
Not incarcerated and employed	33.3	25.5	7.7 *	0.095	27.4	27.4	0.0	0.994	
Not incarcerated and not employed	35.4	40.9	-5.5	0.289	51.4	50.1	1.3	0.757	
Sample size (total = 929) <sup>e</sup>	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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>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.

<sup>b</sup>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.

<sup>c</sup>Violent crimes are based on conviction charges defined by Langan and Levin (2002). See Appendix C for charges defined as violent.

<sup>d</sup>Includes all reasons for incarceration, such as sentences for new crimes, technical violations of parole, detainee (jail), and other reasons. Therefore, incarcerations for new crimes and parole violations do not sum to percentage incarcerated. See Appendix Table C.1 for more details about reasons for incarceration.

<sup>e</sup>A total of 48 sample members are missing the last prison release date and are therefore missing from all outcomes in this table.

<sup>f</sup>Incarceration status based on Quarter 8 after random assignment. Incarceration includes both prison and jail.

<sup>g</sup>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.

The Enhanced Services for the Hard-to-Employ Demonstration

Table 5.4

Impacts on Selected Measures of Recidivism in Year 1 and Year 2, by Time Since Release from Prison  
Center for Employment Opportunities

Outcome	Length of Time Between Prison Release and Random Assignment								H-Stars <sup>d</sup>
	Less Than 3 Months				More Than 3 Months				
	Program Group	Control Group	Difference (Impact)	P-Value	Program Group	Control Group	Difference (Impact)	P-Value	
Arrested <sup>a</sup> (%)									
Year 1	20.8	24.2	-3.4	0.429	24.0	22.1	1.9	0.612	
Year 2	25.8	33.7	-7.8	0.109	20.0	24.5	-4.5	0.222	
Convicted of a felony (%)									
Year 1	1.4	4.8	-3.4 *	0.051	1.3	2.3	-1.1	0.366	
Year 2	11.3	4.2	7.1 **	0.020	1.6	4.5	-2.9 **	0.048	***
Convicted of a misdemeanor (%)									
Year 1	9.5	11.9	-2.4	0.461	14.2	12.5	1.7	0.578	
Year 2	13.2	25.5	-12.3 ***	0.003	16.3	19.9	-3.7	0.280	
Convicted of a violent crime <sup>b</sup> (%)									
Year 1	2.5	1.3	1.2	0.432	1.6	1.5	0.1	0.937	
Year 2	1.7	7.0	-5.3 **	0.015	3.0	2.9	0.1	0.951	**
Admitted to state prison (%)									
Year 1	9.4	19.5	-10.1 ***	0.005	12.8	11.6	1.3	0.658	**
Year 2	21.1	19.0	2.2	0.616	15.8	16.4	-0.6	0.846	
Incarcerated in prison for a new crime (%)									
Year 1	0.5	4.9	-4.4 ***	0.005	1.1	2.1	-1.0	0.365	*
Year 2	6.6	3.2	3.4	0.164	1.4	4.2	-2.8 *	0.051	**
Incarcerated in prison for a technical parole violation (%)									
Year 1	7.2	12.1	-4.9	0.119	9.1	8.6	0.5	0.840	
Year 2	12.7	11.3	1.5	0.681	11.7	7.8	3.9	0.124	

(continued)

**Table 5.4 (continued)**

Outcome	Length of Time Between Prison Release and Random Assignment								H-Stars <sup>d</sup>
	Less Than 3 Months				More Than 3 Months				
	Program Group	Control Group	Difference (Impact)	P-Value	Program Group	Control Group	Difference (Impact)	P-Value	
Total days incarcerated in prison									
Year 1	10	20	-10 **	0.036	14	9	4	0.184	**
Year 2	41	56	-15	0.132	33	31	2	0.793	
Sample size (total = 929) <sup>c</sup>	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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>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.

<sup>b</sup>Violent crimes are based on conviction charges defined by Langan and Levin (2002). See Appendix C for charges defined as violent.

<sup>c</sup>A total of 48 sample members are missing the last prison release date and are therefore missing from all outcomes in this table.

<sup>d</sup>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.

to reductions in felony convictions and prison incarceration for new crimes for that group in Year 2. For the recently released subgroup, the table shows that there was a significant increase in felony convictions in Year 2.<sup>15</sup> Importantly, however, CEO reduced convictions for violent crimes by 5.3 percentage points during the same time period. Further analysis reveals that the violent crime convictions consisted primarily of misdemeanor-level simple assault charges, whereas the felony convictions were due to drug and property crimes for the most part (not shown).

With the additional follow-up, the overall trend in results for both subgroups appears similar to the results for the full sample presented above. In general, these results highlight the complexity of CEO's impacts on recidivism.

Appendix Tables E.3 and E.4 show — for Year 1 and Year 2 separately — CEO's impacts on additional measures of recidivism for the subgroups defined by time between release from prison and random assignment.

### **Other Subgroups**

As discussed in Chapter 4, the research team examined results for other subgroups, based on literature suggesting that CEO may have affected certain subgroups differently. As with the results for employment and earnings, recidivism results were analyzed for older participants compared with younger participants and for those who had a high school diploma or General Educational Development (GED) certificate at baseline compared with those who did not. Impact results for these other subgroups are generally similar to results for the full sample. Findings are presented in Appendix E.

## **Conclusion**

CEO led to reductions in recidivism in the first and second years of the follow-up period. This means that CEO continued to impact recidivism beyond the time that participants were active in the program and beyond the time that the program had impacts on employment.

Because CEO had no impact on employment in Year 2, these findings raise questions about why or how CEO reduced recidivism during that time. CEO's model is built around a package of services designed to allow staff to observe and coach participants on a regular (daily) basis. It is possible that these regular interactions with program staff, in conjunction with the work environment, led to changes in the behavior and outlook of participants, even without

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<sup>15</sup>It is not clear why CEO would lead to an increase in felony convictions in Year 2. Further analysis does not indicate that there was "delayed" recidivism for the recently released subgroup; in fact, that subgroup had a 7.8 percentage point reduction in arrests in Year 2 (which is nearly statistically significant).

the lasting impact on employment. Some evidence that staff interactions played an important role is discussed in Chapter 3. Results from the client survey show that program group members were more likely than their control group counterparts to report that a CEO staff person offered support and guidance to them (Table 3.3).

The findings from this study also suggest that there is some connection between employment and recidivism but that the connection is complicated. Even when program group members were not experiencing higher rates of employment, they were still experiencing reductions in recidivism.

Most recidivism occurs within the first year of release from prison. Because many sample members in this study had already been out of prison for a substantial amount of time before coming to CEO, Year 1 of the study period often does not correspond to the first year out of prison. It is likely that by the time many individuals enrolled in the study, they had already made it past the riskiest and most difficult period of transition. There is also some evidence that the control group in this evaluation experienced below-average recidivism compared with the larger reentry population in New York State.<sup>16</sup> These circumstances may have made it difficult for CEO to achieve positive impacts on recidivism outcomes, making those results all the more noteworthy. However, this group was also motivated enough to seek help from an employment program, suggesting that, in general, the research sample may have been more amenable to services and that this motivation may have helped CEO achieve its positive results.

The fact that CEO produced lasting impacts on recidivism is important, inasmuch as few rigorous random assignment evaluations of reentry programs have found positive effects on recidivism. Each year, government agencies spend billions of dollars on incarceration. Even small reductions in recidivism can lead to substantial monetary savings. Chapter 7 explores the costs of CEO's program services in detail and includes a brief discussion of the program's cost-effectiveness thus far.

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<sup>16</sup>As discussed early in this chapter, the research sample was less likely than New York State's larger reentry population to experience a return to prison.

## Chapter 6

# Impacts of CEO on Other Outcomes

This chapter presents the effects of the Center for Employment Opportunities (CEO) transitional job program for former prisoners returning to the community — compared with the results for the control group — on a range of noneconomic outcomes, such as educational attainment, housing, substance abuse treatment, health insurance coverage, and health. It also presents the impacts of CEO on income and formal child support. The chapter primarily uses data from the client survey, which was administered to members of the program and control groups approximately 16 to 23 months after they entered the study; random assignment occurred between January 2004 and October 2005. In addition, in order to estimate the program's effects on child support, the chapter also uses administrative records data from the New York State Division of Child Support Enforcement (CSE) of the Office of Temporary and Disability Assistance.<sup>1</sup>

Although CEO focused on providing employment services and transitional work to assist participants in gaining employment, increases in employment and earnings or a reduction in recidivism could also indirectly result in positive effects on some of the outcomes presented in this section. Yet impacts on outcomes that are tied to employment or income are not expected, since, as discussed in Chapter 4, CEO did not lead to earnings increases over the long run and people were surveyed long after the employment impacts had faded. CEO, however, did lead to statistically significant impacts on recidivism.

### Noneconomic Outcomes

- **As expected, CEO had little effect on noneconomic outcomes, such as educational attainment, living arrangements, health, and receipt of substance abuse treatment.**

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<sup>1</sup>The tables in this chapter present the effects on summary measures, while the effects on the full set of outcomes from the client survey are shown in Appendix D. Note that the survey results on child outcomes are not presented in this section but are shown in the appendix. Control group members were more likely than program group members to report having children at the survey interview (even though there were no differences between the groups at baseline). All the survey results that are related to being a parent may have been affected by this and, thus, may be unreliable.

Table 6.1 presents CEO's effects on noneconomic outcomes, such as educational attainment, housing, and health care coverage.<sup>2</sup> Similar to the baseline characteristics, the survey results show that about half of the program and control group members did not have a high school diploma or General Educational Development (GED) certificate at the time of the survey interview.<sup>3</sup> In addition, equal proportions of program and control group members (about 70 percent) reported never having been married. Compared with the baseline characteristics discussed in Chapter 1 (Table 1.1), a larger percentage of survey respondents reported being married at the time of the survey interview (about 15 percent, compared with 9 percent).

The second panel of Table 6.1 shows that, compared with the control group, program group members were more likely to report living at someone else's residence at the time of the survey interview: 70 percent of the program group, compared with 61 percent of the control group, for a statistically significant difference of 9.3 percentage points. Program group members were less likely than control group members to report living with a relative. It is not clear why CEO had an effect on living arrangements. The program did not have an effect on the percentage of respondents who moved since random assignment; about 40 percent of both research groups moved at least once. This suggests that living arrangements on release from prison are temporary, or it may indicate difficulties in maintaining housing arrangements. Among those who moved at least once since random assignment, the reasons for moving included being kicked out or being unable to pay the rent (Appendix Table D.1).

The third panel of Table 6.1 presents CEO's effects on substance abuse treatment. Although CEO did not provide treatment services, it could have affected the receipt of treatment indirectly. Note that since the survey did not collect data on substance use or abuse, it is impossible to know the percentage of the population who used drugs or alcohol after study entry. As shown, the program did not have an effect on substance abuse treatment. About half the program and control group members attended treatment since random assignment, and about one-fifth reported attending treatment at the time of the client survey. The results also show that the majority of individuals who attended drug treatment were mandated by their parole officer to do so, which may suggest that attending treatment was a condition of parole.<sup>4</sup>

The next panel of the table shows that sample members in both research groups rated their health similarly; less than 20 percent of each research group rated their health as either fair

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<sup>2</sup>Note that data were collected from individuals in the study regardless of whether they were in the community or were incarcerated at the time of the survey interview. For those who were incarcerated, some questions, such as those about living arrangements, reference the period prior to the latest commitment.

<sup>3</sup>Selected characteristics of the survey sample at baseline are presented in Appendix Table A.2.

<sup>4</sup>Conditions of Parole are rules that parolees must follow while under supervision. The conditions can be set by the parole officer or by the Board of Parole prior to the parolee's release.

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**Table 6.1**

**Impacts on Educational Attainment, Marital Status, Housing, Substance Abuse, Health, and Health Insurance Coverage**

**Center for Employment Opportunities**

Outcome	Program Group	Control Group	Difference (Impact)	P-Value
<b><u>Educational attainment (%)</u></b>				
High school diploma/GED certificate	49.0	47.3	1.7	0.643
<b><u>Marital status (%)</u></b>				
Never married	73.5	70.4	3.1	0.439
Married	15.5	15.2	0.3	0.923
Other	11.0	14.4	-3.4	0.229
<b><u>Housing (%)</u></b>				
Currently living in: <sup>a</sup>				
Own place	20.4	24.8	-4.4	0.220
Someone else's place	70.4	61.0	9.3 **	0.021
Treatment facility/transitional housing/halfway house	3.2	5.5	-2.3	0.191
Homeless/other	4.2	4.8	-0.7	0.725
Sample member currently lives with:				
Spouse/partner	34.9	30.5	4.4	0.300
Relative	59.6	67.0	-7.5 *	0.076
Nonrelative	9.1	5.0	4.1 *	0.080
Ever moved since random assignment	41.6	39.2	2.4	0.585
<b><u>Substance abuse treatment (%)</u></b>				
Received substance abuse treatment or attended self-help groups <sup>b</sup>	53.1	55.9	-2.8	0.516
Currently in drug treatment or attending self-help groups	21.8	18.9	2.9	0.407
Mandated to drug treatment by parole officer	40.7	43.2	-2.5	0.567
<b><u>Health</u></b>				
Rated own health as fair or poor (%)	17.5	19.1	-1.6	0.636
Psychological Distress Scale <sup>c</sup>	6.6	6.7	-0.1	0.778
Experienced serious psychological distress in the past month <sup>d</sup> (%)	17.3	19.2	-1.9	0.556
Went to emergency room during the past year (%)	22.8	22.4	0.5	0.899

(continued)



**Table 6.1 (continued)**

Outcome	Program Group	Control Group	Difference (Impact)	P-Value
<b>Health insurance coverage (%)</b>				
Respondent has health insurance	55.1	57.1	-2.0	0.642
Publicly funded	48.3	54.1	-5.8	0.188
Privately funded	6.9	3.1	3.8 *	0.061
Sample size (total = 531)	316	215		

SOURCE: MDRC calculations based on responses to the client survey.

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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>Subcategories may not sum to the total due to rounding or missing data.

<sup>b</sup>Includes Alcoholics Anonymous (AA) and Narcotics Anonymous (NA) groups.

<sup>c</sup>Based on the K6 scale that includes six questions about how often a respondent experienced symptoms of psychological distress during the past 30 days. The response codes (0-4) of the six items for each person are summed to yield a scale with a 0-24 range.

<sup>d</sup>A value of 13 or more for this scale is used here to define serious psychological distress.

or poor. In addition, there is no significant difference in the percentages of sample members who went to the emergency room in the year prior to the survey interview.

The final panel of Table 6.1 shows that the program and control group members were also just as likely to be covered by health insurance. However, CEO increased the percentage of program group members who were covered by private health insurance; this is somewhat surprising, because there is no indication that CEO increased the proportion of people who had employer-provided insurance. (See Chapter 4.)

### **Income and Public Assistance**

- **CEO did not have an effect on income or public assistance receipt.**

Table 6.2 shows the percentages of respondents who had different income sources during the month prior to the survey interview. Not surprisingly, the program did not have an effect on earnings and public assistance receipt. About half of both research groups had income from earnings, and about a quarter of sample members received public assistance. The low rates of public assistance receipt are not surprising, since individuals with criminal histories are subject

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**Table 6.2**

**Impacts on Income**

**Center for Employment Opportunities**

Outcome	Program Group	Control Group	Difference (Impact)	P-Value
<b><u>Individual income</u></b>				
Percentage of respondents with each income source				
Earnings	51.9	46.0	5.9	0.181
Child support	0.2	0.0	0.1	0.617
Public assistance (%)				
TANF	9.8	14.2	-4.4	0.115
Food stamps	29.0	26.6	2.5	0.521
SSI or disability	1.8	3.2	-1.4	0.290
Total individual income in prior month (\$)	775	760	15	0.901
Percentage of household income that is respondent's	56.6	63.3	-6.7	0.329
Sample size (total = 531)	316	215		

SOURCE: MDRC calculations from responses to the client survey.

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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

to a range of restrictions — including lack of access to some public benefits, depending on the type of prior criminal conviction.<sup>5</sup> Low earnings coupled with low public assistance receipt resulted in a very low total average income (less than \$800 per month) for both research groups.

<sup>5</sup>The Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996 includes a lifetime ban on eligibility for food stamps and benefits under the Temporary Assistance for Needy Families (TANF) program for anyone who receives a felony drug conviction if both the conviction and the underlying conduct occurred after August 22, 1996. However, states have the flexibility to “opt out” or to modify the ban. As of the time of publication, New York State has opted out and made benefits available without regard for prior conviction offenses. The 1996 welfare reform law also prohibits states from providing TANF assistance, food stamps, Supplemental Security Income (SSI), and public housing to anyone who is in violation of probation or parole. The duration of ineligibility is different for each program. For SSI and food stamps, the ineligibility applies “during such month” and “during any period,” respectively, that the individual has either absconded or has been out of compliance with the conditions of release (Reentry Policy Council, 2008).

The results also show that most of the individuals contributed a significant amount to the overall household income; more than half of the household income came from respondents.<sup>6</sup>

Using survey data, Box 6.1 examines how many control group members were not employed in a UI-covered job and were not incarcerated at the time of the survey interview.

### **Formal Child Support**

- **Program and control group members were just as likely to provide any formal child support during the follow-up period.**

Table 6.3 presents CEO's estimated impacts on formal child support payments, as measured through administrative records from the child support enforcement (CSE) agency. Child support outcomes are analyzed only for sample members who had a child support case with CSE prior to study entry. Note that since the sample size is very small (about 20 percent of the research sample), the results should be interpreted cautiously.

Child support payments include only financial payments to cases known to the CSE system. It is possible that sample members may have been providing informal support to children, but this information is not captured in these results.<sup>7</sup> The results show that program and control group members were just as likely to provide formal child support. Within the two-year follow-up period, about 40 percent of both research groups made at least one payment through the CSE system. (As noted above, these percentages include only sample members who had CSE cases at study entry.) Program and control group members paid, on average, about \$600 over the two-year follow-up period.

Results on informal child support and parent-child contact as measured by the client survey are presented in Appendix D.

## **Conclusion**

The economic results discussed in Chapter 4 show that CEO did not have a long-term employment effect; therefore, CEO was not expected to have an effect on income and most noneconomic outcomes. As shown, this expectation was borne out. CEO had little effect on the outcomes presented in this chapter, such as educational attainment, health, and health insurance coverage. Furthermore, CEO did not have an effect on other outcomes, such as public assistance receipt, income, and formal child support payments.

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<sup>6</sup>For results on other household members' income, see Appendix Table D.2.

<sup>7</sup>Payments made through the CSE system are made through different venues, including wage withholdings, tax levies, and others.

**Box 6.1**

**Income Sources and Housing Status for  
Control Group Members Not Employed in a UI-Covered Job and  
Not Incarcerated at the Time of the Survey Interview**

Chapter 5 shows that over 45 percent of the research sample were not working or incarcerated at the end of the Year 2 (Table 5.1). Using data from the client survey, this box examines the primary means of support and living arrangements for control group members who were not working in a UI-covered job or were not incarcerated at the time of the survey interview. The outcomes for this group are compared with the survey sample responses for the control group as a whole. This comparison examines whether this group differs from the larger population.

The results show that 24 percent of control group survey respondents not working in a UI-covered job and not incarcerated reported working at the time of the survey interview. Compared with all the control group survey respondents, they were more likely to be employed at a temporary job or in day labor or an odd job. This group's earnings made up a smaller fraction of total household income, and they relied more on food stamps and cash assistance. The total measured household income for both groups is very low, but it is much lower for those not incarcerated and not working in a UI-covered job.

Living arrangements are very similar between both groups; most were living at someone else's place and only small differences were found in the percent that were homeless or in transitional housing. However, those not incarcerated and not working in a UI job were more likely than the control survey respondents to be in drug treatment.

Outcome	Full Sample (N = 215)	Not Incarcerated, Not in UI-Covered Job (N = 88)
Currently working in the community (%)	37.5	24.4
Current or most recent job is day labor, temporary, or odd job (%)	10.7	15.7
Respondent's income source (%)		
Earnings	46.7	30.3
TANF/Safety		
Net	14.2	20.9
Food stamps	26.9	37.3
SSI or disability	3.0	6.9
Total individual income in prior month (\$)	776	499
Total household income in prior month (\$)	1,646	1,160
Living arrangements (%)		
Own place	24.9	23.2
Someone else's place	61.5	60.9
Treatment facility / transitional housing / halfway house	5.0	6.1
Homeless/other	4.5	2.7
Currently in drug treatment (%)	13.4	18.9

**The Enhanced Services for the Hard-to-Employ Demonstration**

**Table 6.3**

**Impacts on Formal Child Support Payments**

**Center for Employment Opportunities**

Outcome	Program Group	Control Group	Difference (Impact)	P-Value
<b><u>Child support payments<sup>a</sup></u></b>				
Ever made a child support payment (%)	38.6	41.6	-3.0	0.687
Average number of child support payments	8	7	1	0.637
Number of child support payments (%)				
0	61.4	58.4	3.0	0.687
1-4	15.6	17.2	-1.6	0.783
5-20	11.7	11.6	0.2	0.974
21 or more	11.3	12.9	-1.6	0.736
Average amount paid (\$)	607	612	-5	0.985
Sample size (total = 187)	104	83		

SOURCE: MDRC calculations using data from the New York State Office of Child Support Enforcement (CSE).

NOTES: Results in this table are weighted by week of random assignment and adjusted for pre-random assignment characteristics. This table includes only sample members who had a child support case with CSE prior to random assignment.

Statistical significance levels are indicated as: \*\*\* = 1 percent; \*\* = 5 percent; \* = 10 percent. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>These payments were made during the two years following random assignment.

## Chapter 7

# Costs of the Center for Employment Opportunities

The preceding chapters report on the implementation and impacts of the New York City-based Center for Employment Opportunities (CEO) program, which places former prisoners who are returning to the community in immediate, wage-paying transitional jobs during the uncertain period after release. The results show that the program was well implemented and that most program group members worked in a paid transitional job. Impact results show that CEO increased employment early in the study's follow-up period and led to reductions in convictions and incarceration during the two years following random assignment.<sup>1</sup>

This chapter presents estimates of the costs of operating the CEO program and achieving those effects. The costs of services for program group members are compared with the estimated costs of providing services to the control group both within CEO and through outside organizations.

## Methodology of the Cost Analysis

The cost analysis measures the differences in expenditures for serving the program group and the control group, and so 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. The cost analysis considers the value of all resources required for service provision. The general approach is to place dollar values on all resources used, wherever possible, either by directly measuring them or by estimating them. The cost of services was estimated for 20 months following random assignment, on average. The costs are estimated and presented as an average cost per program group member.

### Cost Components

Figure 7.1 illustrates the main cost components for both the program and the control group. It shows that the total cost of CEO for each program group member (Box C) is made up of expenditures for CEO's program services (Box A) and expenses for services received outside CEO (Box B). Box F represents the total cost that accrued for each control group member, including costs for CEO's limited job coaching and life skills services (Box D) and costs for services obtained outside CEO (Box E).

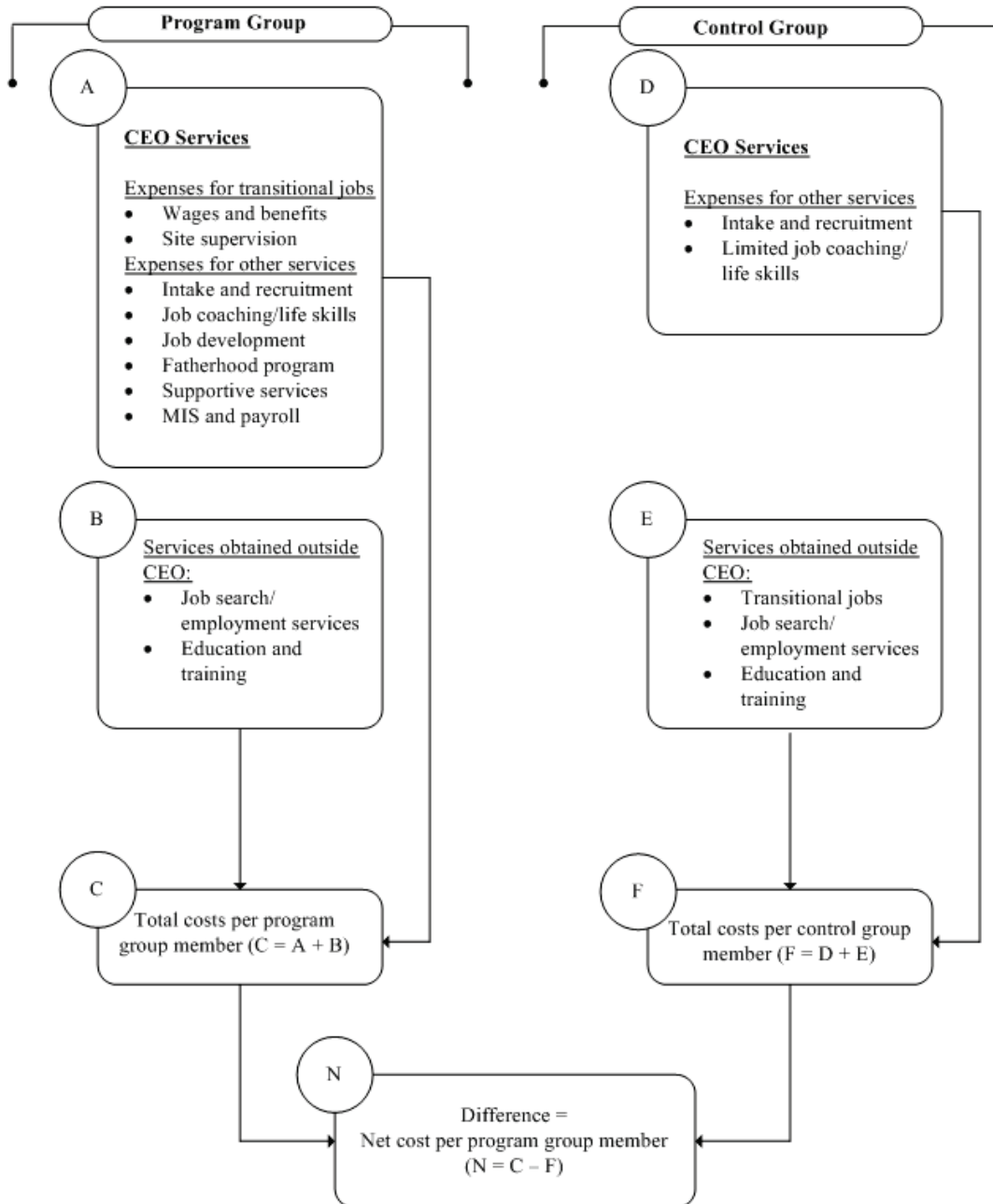
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<sup>1</sup>Random assignment occurred between January 2004 and October 2005.

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Figure 7.1

Diagram of Major Cost Components:  
Center for Employment Opportunities



The main cost components of CEO for the program group include (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. In addition, the costs for the program group include expenses incurred by other organizations for employment and training services that were provided to program group members outside CEO.

The cost of services provided to control group members represent what would have been spent on program group members in the absence of CEO. The cost components for the control group consist of expenses for (1) CEO's limited job coaching and life skills services<sup>2</sup> and (2) expenses incurred by outside agencies for providing employment and training services to control group members and, for some of them, transitional jobs.<sup>3</sup>

The *net cost* of CEO, — that is, the cost per program group member over and above the cost per control group member — is represented in Box N. The net cost is obtained by subtracting the total cost per control group member (Box F) from the total cost per program group member (Box C).

### **Data Sources**

The cost of each program component was estimated using CEO's detailed financial expenditure reports for Fiscal Year 2005 and in-depth interviews with program fiscal staff.<sup>4</sup> 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 operation as experienced by program group members. To produce cost estimates for each program component per participant 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 time 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. Cost estimates for the services are based on multiple sources. For example, college costs were estimated using tuition and fees information for New York City public

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<sup>2</sup>It was assumed that the control group would have received job search and life skills services outside CEO if CEO did not provide a basic set of these services to them.

<sup>3</sup>As noted in Chapter 3, some control group members reported working at transitional jobs (provided by organizations other than CEO).

<sup>4</sup>Fiscal Year 2005 covers the period from July 1, 2004, to June 30, 2005.



colleges.<sup>5</sup> For transitional jobs outside CEO (that is, for the transitional jobs accessed by the control group), costs were assumed to be the same as those for CEO's transitional jobs, including wages, fringe benefits, and on-site supervision. It was also assumed that control group members who worked in transitional jobs worked the same average number of weeks as the program group members who worked in CEO's transitional jobs (eight weeks).

### **Limitations of the Analysis**

Some limitations of the cost analysis should be recognized. First, this analysis is not a cost-benefit analysis and, therefore, does not account for any offsetting benefits of the program. For example, Chapter 5 shows that CEO reduced the number of days incarcerated in prison.<sup>6</sup> Given the very high expenditures by government agencies on incarceration, any reduction in prison days could translate into substantial cost savings. CEO also led to reductions in arrests and convictions during the follow-up period. Savings from reduced court costs, which also could be substantial, are not accounted for in this analysis.

There are also a number of expected benefits stemming from the transitional jobs themselves. City and state agencies contract with CEO to perform work that needs to be done. Thus, if CEO's participants were not performing these job functions, it is likely that these agencies would have had to use regular employees to do this work. In this analysis, the wages, fringe benefits, supervision, and administration of the transitional jobs are presented as part of operating costs, and the analysis does not account for the funding of these positions by the agencies or for any offsetting benefits of the labor output. The cost of the possible displacement of other workers resulting from the increased employment of program group members is also not accounted for. Finally, this analysis does not account for any nonmonetary benefits or costs of CEO's program.

## **Financial Costs of CEO**

### **Operating 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. The average operating cost

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<sup>5</sup>Estimates of the cost of General Educational Development (GED) and vocational programs were based on cost estimates for similar services provided in Philadelphia, another site in the Hard-to-Employ evaluation.

<sup>6</sup>CEO reduced the number of days in prison by 25 for each program group member who reported to CEO within three months of release from prison. On average, CEO reduced the days in prison by eight for each program group member (combining those who were released from prison within three months of enrollment and those who were not). The eight-day difference is not statistically significant.

per program group member for a specified activity was calculated by first estimating a unit cost (that is, the average operating cost per participant in that activity). This unit cost includes all staff salaries, fringe benefits, overhead, and administration costs for the activity. However, not all program group members participated in each activity. In order to obtain an average cost per program group member, the cost per participant in each component was multiplied by the participation rate in that activity.

- **The cost of providing CEO's services was about \$4,200 per program group member, including nearly \$1,000 in direct payments to participants.**

Table 7.1 presents the estimated costs of operating CEO. As shown in the table, more than half of CEO's total program cost (55 percent) was made up of direct expenses for the transitional jobs. (Direct expenses include participants' wages, fringe benefits, and supervision.) The largest single expense 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. They are responsible for directing and observing clients' work performance on the work sites as well as for providing transportation to and from the sites and for other duties.<sup>7</sup> Each work crew has a separate supervisor, and work crews generally consist of only 8 to 10 participants. Thus, the client-to-staff ratio on the work sites is very low and allows for a significant amount of daily supervision and job coaching by CEO staff. The second-largest expense was for participants' wages. The average cost for wages, including for those who worked a transitional job and those who did not, was \$887 per program group member.

Taken together, other services provided to participants were estimated to cost \$1,381 per program group member. Most of this cost was for job coaching (\$559) and job development activities (\$489). Program group members were also given about \$70 worth of supportive service payments and incentives (for example, transportation and clothing).

CEO's fatherhood program provided a set of enhanced services, including parenting classes and advocacy for participants who required help with issues involving the child support enforcement agency.<sup>8</sup> Because only a small proportion of CEO's clients enroll in the fatherhood program, it was one of the most expensive activities per participant.<sup>9</sup> However, spread over

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<sup>7</sup>Based on research that was conducted for the Hard-to-Employ project by Demetra Nightingale of the Johns Hopkins Institute for Policy Studies and John Trutko of Capital Research Corporation.

<sup>8</sup>These estimates do not include the costs of staff in the child support enforcement agency. There are two employees in that office whose job is to coordinate with CEO. One is a case manager, and one is a staff analyst in the outreach department. Conversations with the child support office indicate that case managers spend an  
(continued)

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**Table 7.1**

**Estimated Unit and Gross Costs of CEO (in 2005 Dollars)**

**Center for Employment Opportunities**

Cost Component	Average Cost per Participant in Component (\$)	Program Group Participation Rate (%)	Average Cost Program Group Member (\$)
<b><u>CEO expenditures</u></b>			
Recruitment/intake	160		160
Expenses for transitional jobs			
Transitional job wages	1,237	71.7	887
Transitional job fringe benefits <sup>a</sup>	206	71.7	148
Field/site supervision	1,849	71.7	1,326
Expenses for other services			
Job coaching/life skills	940	59.5	559
Job development	851	57.4	489
Bonding	15	30.0	5
Fatherhood component	1,435	18.0	258
Supportive services	97	71.7	70
MIS and payroll	458	78.9	362
<b>Total operating costs</b>	<b>7,249</b>		<b>4,263</b>

SOURCES: MDRC calculations from CEO's financial expenditure reports for Fiscal Year 2005 and CEO's Network for Information Gathering Evaluation and Learning (NIGEL) database.

NOTE:

<sup>a</sup>CEO incurs fringe benefit costs of 16.7 percent of day labor wages resulting from employer-paid payroll taxes.

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average of 30 minutes with each client but that those in the CEO fatherhood program are so well prepared that they require less time, estimated at 15 to 20 minutes per client.

<sup>9</sup>Only about half of the research sample reported that they have any children, and even fewer (18 percent) enrolled in fatherhood services.

participants and nonparticipants, the fatherhood program resulted in a cost of only about \$250 per program group member, because only a small proportion of the program group used those services.

CEO's management information system was estimated to cost \$362 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.

The total gross cost to operate CEO was \$4,263 per program group member. Of this total cost, nearly \$1,000 included direct payments to participants in the form of wages, incentives, and other supportive services like transportation and clothing.

### **Net Cost of CEO**

The control group in this evaluation was provided basic job search assistance and a life skills class by CEO. 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 Chapter 3 shows, many control group members — and some program group members — received employment and training services from other programs. This section presents the net cost of CEO, which 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 \$3,200 more per program group member than the cost of services for the average control group member.**

The top panel of Table 7.2 shows CEO's expenditures for services for the program and control groups. As noted above, CEO provided a limited set of services for members of the control group; they received a shorter version of the life skills class, which was taught by a job coach. The control group also received access to a resource room for job search activities. This room was staffed by a job coach who answered questions and provided assistance to participants while they conducted job search activities. Therefore, CEO's control group costs include the cost of one full-time job coach, including salary, fringe benefits, overhead, and administration.<sup>10</sup> It was estimated that the cost to serve a control group member in CEO was \$543.

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<sup>10</sup>The resource room provided computers and other resources for use by members of both research groups. The amortized cost of this equipment was included in the overhead costs, and, thus, a proportion was allocated to all participants.

**The Enhanced Services for the Hard-to-Employ Demonstration**

**Table 7.2**

**Estimated Net Costs of CEO (in 2005 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 (\$)
<b><u>CEO services</u></b>			
Recruitment/intake	160	160	0
Expenses for transitional jobs			
Transitional job wages	887	0	887
Transitional job fringe benefits <sup>a</sup>	148	0	148
Field/site supervision	1,326	0	1,326
Expenses for other services			
Job coaching/life skills	559	383	177
Job development	489	0	489
Bonding	5	0	5
Fatherhood component	258	0	258
Supportive services	70	0	70
MIS and payroll	362	0	362
<b>Total CEO costs</b>	<b>4,263</b>	<b>543</b>	<b>3,720</b>
<b><u>Services obtained outside CEO</u></b>			
Expenses for transitional jobs <sup>b</sup>			
Transitional job wages	0	200	-200
Transitional job fringe benefits	0	33	-33
Field/site supervision	0	215	-215
Expenses for other services			
Employment/job search	158	322	-164
Adult Basic Education/GED	6	5	1
College courses	212	126	86
Vocational training	641	641	0
<b>Total non-CEO costs</b>	<b>1,017</b>	<b>1,542</b>	<b>-525</b>
<b>Total CEO and non-CEO costs</b>	<b>5,280</b>	<b>2,085</b>	<b>3,195</b>

SOURCES: MDRC calculations from CEO's financial expenditure reports for Fiscal Year 2005 and CEO's Network for Information Gathering Evaluation and Learning (NIGEL) database.

NOTES:

<sup>a</sup>CEO incurs fringe benefit costs of 16.7 percent of day labor wages resulting from employer-paid payroll taxes.

<sup>b</sup>Control group costs represent transitional job participation reported by 16 percent of control group respondents on the client survey.

As mentioned above, CEO provided only a limited set of services for the control group, while program group members were offered the full range of CEO's services. Thus, it was expected that control group members would be more likely than program group members to seek out other services within the community, especially employment services. The bottom panel of Table 7.2 presents estimates of expenditures by outside organizations to provide job search and other education and training services to both research groups. As expected, expenditures for services received outside CEO were \$525 more for the control group than for the program group. Differences in expenditures for outside services are attributed mainly to costs for accessing transitional jobs and employment services from other organizations. There was little difference in costs between the two research groups for educational expenses. After accounting for costs of services received outside CEO, the net cost for program group members — that is, costs over and above the control group costs — was \$3,195 (Box N in Figure 7.1).

As noted in Chapter 3, 16 percent of control group members reported on the client survey that they worked in a transitional job. For program group members, information on participation in transitional jobs was available both from the client survey and from participant records maintained by CEO. By comparing data from these two sources, it was estimated that program group members underreported transitional work by about 46 percent in the client survey. In order to account for the possibility that control group members underreported transitional employment at the same rate, Appendix Table F.1 presents an alternative net cost analysis, which assumes that 35 percent of the control group worked in a transitional job.

## Conclusion

This analysis has shown that the net cost of CEO's program was about \$3,200 per program group member. Of this total, roughly \$1,000 went directly to program group clients in the form of wages and supportive service payments. Taken together, the majority of CEO's program costs were for participant payments and expenditures for work site supervision.

The focus of this analysis has been on the monetary *costs* associated with the CEO program. Although the analysis does not attempt to estimate the monetary *benefits* resulting from participation in CEO, it is possible to briefly explore the program's cost-effectiveness by comparing the information on estimated costs with the impacts produced thus far.<sup>11</sup>

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<sup>11</sup>A thorough exploration of cost-effectiveness should include a consideration of offsetting benefits as well as nonmonetary effects. No attempt to estimate these effects is included in this cost analysis.

When thinking about cost-effectiveness, it is important to consider different viewpoints. For example, from the government budget perspective, the CEO program costs more to operate than the cost of services received by the control group, but the CEO program also produced increases in employment as well as reductions in prison incarceration. From this perspective, the very high expenditures by government agencies on incarceration mean that any reduction in prison days could translate into substantial savings. As shown in Chapter 5, CEO program group members spent fewer days in prison than the control group members did.

Therefore, in prison days alone, it is estimated that CEO produced a cost savings between \$1,500 per program group member and as much as \$4,500 per recently released program group member.<sup>12</sup> All results in this report are presented on a per person basis. Cost savings to the government budget would be substantially higher when multiplied over the thousands of former prisoners who are served by CEO's program each year.<sup>13</sup>

The estimates discussed above likely understate the full cost savings. The program costs presented here were realized during the first 20 months of the follow-up period, and it is not expected that the program group will incur any additional costs beyond this time; but the trend in recidivism results suggests that financial benefits from CEO could continue to accrue into the future. Moreover, prison days are only one potential offsetting benefit. Other monetary benefits — such as reduced court costs and the value of labor output from the work performed by participants — have not been measured but are also likely to be considerable. Additionally, it is likely that taxpayers place value on increased employment and reduced recidivism that is not easily quantified but that should be considered.

From the perspective of the program group members, CEO led to increased employment and earnings from transitional jobs. Chapter 4 shows that, within two years, program group members earned about \$1,500 more than their control group counterparts in combined income from transitional employment and unsubsidized employment. CEO also reduced convictions and incarceration, which were not monetized in this analysis but — from the perspective of CEO participants — are likely to be important for financial as well as nonfinancial reasons.

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<sup>12</sup>This calculation is based on the Fiscal Year 2009 budget of \$969 million for approximately 14,127 inmates per day for 365 days; the daily cost of incarceration therefore equals \$969 million divided by the product of 14,127 and 365, yielding an approximate cost of \$188 per day per inmate (Horn and Martin, 2008).

<sup>13</sup>Researchers acknowledge that these savings are likely to be marginal and may not reflect true savings resulting from reduced incarceration. This estimate also assumes that the cost of incarceration for the recently released sample members is the same as for the full sample.

**Appendix A**

**Analysis of Survey Response Bias**





Appendix A assesses the reliability of impact results measured by the client survey. It also examines whether the impacts for the survey respondents can be generalized to the impacts for the research sample. The appendix first describes how the survey sample was selected. Then it discusses the response rates for the survey sample and for the program and the control groups. Next, it examines differences between survey respondents and survey nonrespondents, followed by a comparison of the two research groups among the survey respondents. Finally, it compares the impacts on employment and recidivism across the survey samples and the research sample, as calculated using administrative records.

This appendix concludes — with some caution — that the survey is reliable and that the results for the survey respondent sample can be generalized to the research sample. A comparison of the program and control groups among the survey respondents shows no systematic differences in background characteristics. Furthermore, respondents and nonrespondents did not differ in most measurable characteristics before random assignment. However, compared with the research sample, survey respondents' impacts on employment and incarceration are larger. In addition, the impacts between respondents and nonrespondents are significantly different. Therefore, the survey may produce larger impacts on outcomes linked to recidivism than might exist for the full sample.

## **Survey Sample Selection**

As noted in Chapter 1, the research sample includes 977 sample members who were randomly assigned from January 2004 through October 2005. A total of 777 individuals were selected to be interviewed; everyone who was randomly assigned from January to December 2004 and a random sample from the later cohort — those randomly assigned between January and October 2005 — were selected to be interviewed. This sample is referred to as the “fielded sample” (see Box A.1). To ensure representation of individuals across the total sample, the fielded sample has the same proportion of sample members in each group as the proportion of the sample members randomly assigned to each group. For example, since program group members account for 58 percent of the full sample, 58 percent of the fielded sample were selected from the program group. The fielded sample constitutes about 80 percent of the research sample, and it includes 451 program group members and 326 control group members.

## **Survey Response Rates**

Sample members who were interviewed for the client survey are referred to as “survey respondents,” or the “respondent sample,” while sample members who were not interviewed are known as “nonrespondents,” or the “nonrespondent sample.” A total of 531 sample members, or 68 percent of the fielded sample, completed the survey (70 percent [total = 316] of program

### Box A.1

#### Key Analysis Samples

**Research sample:** All individuals in the study who were randomly assigned from January 2004 through October 2005.

**Fielded sample:** Sample members in the research sample who were selected for the field interview.

**Respondent sample:** Sample members in the fielded sample who completed the client survey.

**Nonrespondent sample:** Sample members in the fielded sample who were not interviewed because they were not located or refused to be interviewed or because of other reasons.

group members and 66 percent [total = 215] of control group members). It was expected that a larger percentage of program group members would complete the survey, since program group members made up a larger proportion of the fielded sample.

Of the nonrespondent sample, 75 percent (185 out of 246) could not be located for the interview.<sup>1</sup> Whenever the response rate is lower than 100 percent, nonresponse bias may occur. That is, differences may exist between the respondent sample and the larger, fielded sample, owing to differences between the sample members who completed a survey and those who did not. Furthermore, the estimates may be biased if the background characteristics differ between the research groups.

### Comparison Between Respondents and Nonrespondents Within the Survey Sample

In order to examine whether there are systematic differences between those who responded to the survey and those who did not, an indicator of survey response status was created, and then multivariate analysis was used to identify which pre-random assignment characteristics are significantly related to the indicator.

Appendix Table A.1 presents selected baseline characteristics of survey respondents and nonrespondents. Overall, the two groups are very similar. There is only one statistically

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<sup>1</sup>Other respondents were not interviewed because they refused or were incapacitated, institutionalized, located after the fielding period expired, or deceased or had moved far away.

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**Appendix Table A.1**

**Selected Baseline Characteristics of Survey Respondents and Nonrespondents**

**Center for Employment Opportunities**

Characteristic	Respondent Group	Nonrespondent Group	Total
Male (%)	93.5	93.0	93.3
Age (%)			
18 to 24 years	19.5	19.0	19.3
25 to 30 years	24.2	26.6	25.3
31 to 40 years	31.6	28.3	30.1
41 years or older	24.8	26.0	25.3
Average age (years)	33.5	33.3	33.4
Citizen (%)	80.3	76.2	78.5
Race/ethnicity (%)			
White/non-Hispanic	1.6	2.8	2.1
Black/non-Hispanic	62.7	64.7	63.6
Hispanic	31.7	30.0	30.9
Other	4.0	2.4	3.3
Has any children age 18 or younger (%)	47.7	44.9	46.5
Any children under age 18 in household (%)	15.9	13.4	14.8
Education (%)			
High school diploma	9.4	12.1	10.6
GED certificate	43.1	42.6	42.8
Beyond high school diploma	2.9	5.0	3.9
None of the above	44.6	40.3	42.6
Housing status (%)			
Rents or owns home	20.6	15.8	18.4
Lives with friends or relatives	57.7	59.2	58.3
Transitional housing	10.6	11.8	11.1
Emergency housing/homeless	4.1	5.5	4.7
Other	7.1	7.8	7.4
Marital status (%)			
Married, living with spouse	9.6	7.8	8.8
Married, living away from spouse	6.0	9.7	7.7
Unmarried, living with partner	22.1	20.9	21.6
Single	62.2	61.6	62.0
Ordered to provide child support to a child under 18 (%)	18.2	21.8	19.8

(continued)

**Appendix Table A.1 (continued)**

Characteristic	Respondent Group	Nonrespondent Group	Total
Ever employed (%)	81.3	80.2	80.8
Ever employed 6 consecutive months for one employer (%)	64.4	58.1	61.5 **
UI-covered employment in the quarter prior to random assignment <sup>a</sup> (%)	13.0	13.8	13.4
UI-covered employment in the year prior to random assignment <sup>a</sup> (%)	24.4	23.2	23.9
Violated parole prior to random assignment (%)	42.0	45.6	43.6
Prior drug conviction (%)	73.1	72.9	73.0
Number of felony conviction prior to random assignment	2.6	2.7	2.6
Number of quarters employed in the three years prior to random assignment	1.4	1.4	1.4
Randomly assigned within 3 months of release (%)	42.5	44.5	43.4
Sample size	531	446	977

SOURCES: MDRC calculations using CEO Baseline Information Form data, unemployment insurance (UI) wage records from New York State, and data from 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. The significance level indicates the probability that one would be making an error in concluding that there is a difference between research groups for the variable in question.

<sup>a</sup>This measure was created using data from unemployment insurance (UI) wage records from New York State.

significant difference between them: before random assignment, respondents were significantly more likely than nonrespondents to have worked for six consecutive months for one employer. Furthermore, a multivariate regression analysis was performed to test whether or not there is an overall difference in measurable characteristics between the research groups. A 0/1 dummy indicating response to the client survey was regressed on baseline characteristics — many of which are shown in Appendix Table A.1. The results show that there are not significant differences in the baseline characteristics of respondents and nonrespondents.

## **Comparison Between the Research Groups in the Survey Respondent Sample**

Random assignment research designs minimize potential bias. There is the possibility, however, that the characteristics of the research groups will differ due to the selective nature of the survey response process. If this is true, the reliability of impact estimates for the respondent sample may be affected.

Appendix Table A.2 shows selected baseline characteristics of the program and control group members. In general, only a few statistically significant differences were found between them. The program group members were more likely than the control group members to have violated parole prior to random assignment and to have had a higher number of felony convictions. A logistic regression was also used to test whether or not there is a relationship between the sample members' background characteristics and their research status. The results of this tests show that the program group did not differ from the control group in the baseline characteristics (results not shown).

## **Comparison of Survey Respondents and the Nonrespondent, Fielded, and Research Samples**

Using administrative records data for comparison, this section discusses whether the survey respondents' impacts can be generalized to the fielded sample and the research sample. Consistency of impact findings among the samples is considered to be the best result, suggesting that impacts on measures calculated from survey responses can be generalized to the research sample. Survey results may be considered unreliable because of response bias when the impacts for survey respondents that are calculated using administrative data differ in size and direction from the results for all other samples. An unlucky sample draw, or "sampling bias," may be inferred when impacts for the respondent sample resemble results for the fielded sample but findings for both samples vary from findings for the research sample — from which the samples were drawn.

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**Appendix Table A.2**

**Selected Baseline Characteristics of Survey Respondents, by Research Group  
Center for Employment Opportunities**

Characteristic	Program Group	Control Group	Total
Male (%)	92.6	94.8	93.5
Average age (years)	33.6	33.4	33.5
Citizen (%)	81.9	77.9	80.3
Race/ethnicity (%)			
White, non-Hispanic	1.5	1.7	1.6
Black, non-Hispanic	64.0	60.9	62.7
Hispanic	31.4	32.1	31.7
Other	3.1	5.4	4.0
Has any children age 18 or younger (%)	46.0	50.3	47.7
Any children under age 18 in household (%)	14.7	17.6	15.9
Has General Educational Development (GED) certificate (%)	43.4	42.6	43.1
Housing status (%)			
Rents or owns home	18.5	23.4	20.6
Lives with friends or relatives	60.1	54.3	57.7
Transitional housing	10.7	10.4	10.6
Emergency/temporary housing	3.8	4.4	4.1
Other	6.8	7.6	7.1
Marital status (%)			
Married, living with spouse	10.1	8.9	9.6
Married, living away from spouse	5.0	7.5	6.0
Unmarried, living with partner	22.3	21.9	22.1
Single	62.6	61.7	62.2
Ever employed (%)	79.3	84.1	81.3
Ever employed 6 consecutive months for one employer (%)	63.6	65.5	64.4
UI-covered employment in the quarter prior to random assignment <sup>a</sup> (%)	13.4	12.5	13.0
Violated parole prior to random assignment (%)	45.8	36.8	42.0 **
Prior drug conviction (%)	73.9	71.9	73.1
Number of felony convictions prior to random assignment	2.7	2.4	2.6 ***
Randomly assigned within 3 months of release (%)	44.7	39.3	42.5
Sample size	316	215	531

(continued)

### Appendix Table A.2 (continued)

SOURCES: MDRC calculations using CEO Baseline Information Form data, unemployment insurance (UI) wage records from New York State, and data from 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. The significance level indicates the probability that one would be making an error in concluding that there is a difference between research groups for the variable in question.

<sup>a</sup>This measure was created using data from unemployment insurance (UI) wage records from New York State.

Appendix Table A.3 shows the adjusted means and impacts on employment and several recidivism outcomes for the report, fielded, respondent, and nonrespondent samples.<sup>2</sup> This table shows larger effects on employment and incarceration for the survey respondent sample. For instance, the impact on incarceration for the respondent sample is  $-9.6$ , but it is  $-5.9$  for the research sample and  $1.5$  for the nonrespondent sample. In general, the results show that the impacts for the respondent sample are very different from the impacts for the nonrespondent sample; the respondents are better off than the nonrespondents.<sup>3</sup> Therefore, the survey results may show larger impacts on outcomes linked to recidivism that might exist for the research sample.

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<sup>2</sup>All the impacts are regression-adjusted within each sample, to control for differences in background characteristics, prior earnings, prior employment, prior recidivism, and quarter of random assignment.

<sup>3</sup>To test whether the impacts for respondents and nonrespondents are significantly different statistically, MDRC ran a multiple regression model using multivariate analysis of variance (MANOVA). The results show that the Wilks' Lambda F-statistic is significant; therefore, the impacts are significantly different for respondents and nonrespondent



**The Enhanced Services for the Hard-to-Employ Demonstration**

**Appendix Table A.3**

**Two-Year Impacts for the Research, Fielded, Respondent, and Nonrespondent Samples**

**Center for Employment Opportunities**

Outcome (%)	Program Group	Control Group	Difference (Impact)	P-Value <sup>a</sup>
Ever employed in a UI-covered job				
Research sample	83.4	65.3	18.1 ***	0.000
Fielded sample	85.6	66.6	19.0 ***	0.000
Respondent sample	88.0	67.8	20.2 ***	0.000
Nonrespondent sample	80.5	64.4	16.1 ***	0.004
Convicted of a felony				
Research sample	6.6	7.7	-1.1	0.536
Fielded sample	6.1	6.0	0.1	0.956
Respondent sample	5.9	5.4	0.5	0.815
Nonrespondent sample	6.3	7.5	-1.2	0.711
Arrested				
Research sample	37.7	41.8	-4.1	0.192
Fielded sample	37.0	42.1	-5.1	0.143
Respondent sample	34.4	40.8	-6.4	0.133
Nonrespondent sample	44.9	44.2	0.7	0.912
Incarcerated <sup>b</sup>				
Research sample	49.5	55.4	-5.9 *	0.064
Fielded sample	49.5	55.2	-5.8	0.107
Respondent sample	46.9	56.5	-9.6 **	0.026
Nonrespondent sample	56.2	54.7	1.5	0.814
Sample size				
Research sample (total = 977)	568	409		
Fielded sample (total = 777)	451	326		
Respondent sample (total = 531)	316	215		
Nonrespondent sample (total = 246)	135	111		

SOURCE: MDRC calculations using data from unemployment insurance (UI) wage records from New York State and 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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>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 on the table): 2.7, 2.9, and 3.37.

<sup>b</sup>This measure includes both prison and jail incarcerations.

## **Appendix B**

# **Additional Impacts on Employment and Earnings**

Appendix B presents additional results showing CEO's impacts on employment and earnings in the following tables:

- B.1 Comparison of the National Directory of New Hires Database and the New York State Department of Labor Employment Data
- B.2 Quarterly Impacts on Employment and Earnings
- B.3 Comparison of Unemployment Insurance Records and Survey-Reported Employment



The Enhanced Services for the Hard-to-Employ Demonstration

Appendix Table B.1

Comparison of the National Directory of New Hires Database and the New York State Department of Labor Employment Data

Center for Employment Opportunities

Outcome (%)	Data Source						
	National Directory of New Hires				New York State DOL <sup>a</sup>		
	Program Group	Control Group	Difference (Impact)	Sample Size	Program Group	Control Group	Difference (Impact)
<b><u>CEO transitional employment</u></b>							
Quarter 1 (quarter of random assignment)	58.7	2.1	56.6 ***	534	60.7	1.2	59.5
Quarter 2	37.0	1.4	35.6 ***	624	40.5	1.0	39.6
Quarter 3	12.1	0.0	12.1 ***	788	13.1	0.0	13.1
Quarter 4	6.4	0.5	5.9 ***	973	6.4	0.5	5.9
Quarter 5	2.8	0.5	2.3 ***	973	3.7	0.5	3.2
Quarter 6	1.6	0.0	1.6 **	973	2.7	0.0	2.7
Quarter 7	2.7	0.7	1.9 **	973	2.7	0.7	1.9
Quarter 8	2.5	0.0	2.5 ***	973	2.7	0.2	2.4
<b><u>Unsubsidized employment</u></b>							
Quarter 1 (quarter of random assignment)	20.5	27.8	-7.3 **	534	21.4	28.6	-7.2
Quarter 2	32.4	34.2	-1.8	624	32.7	43.0	-10.3
Quarter 3	33.0	37.0	-4.1	788	38.1	43.5	-5.5
Quarter 4	32.1	35.0	-2.9	973	34.9	40.3	-5.5
Quarter 5	29.6	34.5	-4.9	973	31.0	39.9	-8.9
Quarter 6	30.5	32.8	-2.3	973	33.6	33.7	-0.1
Quarter 7	31.7	32.3	-0.5	973	34.3	33.7	0.6
Quarter 8	31.9	32.8	-0.8	973	32.6	36.2	-3.6

(continued)

**Appendix Table B.1 (continued)**

Outcome (%)	Data Source						
	National Directory of New Hires				New York State DOL <sup>a</sup>		
	Program Group	Control Group	Difference (Impact)	Sample Size	Program Group	Control Group	Difference (Impact)
<b><u>Total UI-covered employment</u></b>							
Quarter 1 (quarter of random assignment)	67.2	29.5	37.8 ***	534	67.8	25.9	41.9
Quarter 2	58.9	35.2	23.7 ***	624	58.8	35.2	23.6
Quarter 3	41.4	37.0	4.4	788	40.5	35.7	4.8
Quarter 4	35.5	35.5	0.0	973	34.5	34.0	0.5
Quarter 5	31.0	35.0	-3.9	973	29.9	31.5	-1.6
Quarter 6	31.4	32.8	-1.4	973	29.4	29.1	0.3
Quarter 7	33.7	32.8	0.9	973	31.2	30.1	1.1
Quarter 8	34.2	32.8	1.5	973	31.7	28.9	2.8
Sample size (total = 973) <sup>b</sup>	564	409			564	409	

SOURCES: Aggregate counts by random assignment cohort and research group were provided by the New York State Department of Labor (DOL). Individual-level data were provided by the National Directory of New Hires (NDNH).

NOTES: Results in this table are unadjusted and unweighted means for both data sources.

Statistical significance levels are indicated as: \*\*\* = 1 percent; \*\* = 5 percent; \* = 10 percent. The significance level indicates the probability that one would be making an error in concluding that there is a difference between research groups for the variable in question.

<sup>a</sup>New York State DOL provided MDRC with aggregate estimates of CEO transitional and unsubsidized employment. These estimates were compared with data from NDNH for a partial sample in Quarters 1 through 3 and for the full sample in all remaining quarters (shown above).

<sup>b</sup>Four sample members are missing Social Security numbers and could not be matched to UI data.

**The Enhanced Services for the Hard-to-Employ Demonstration**

**Appendix Table B.2**

**Quarterly Impacts on Employment and Earnings**

**Center for Employment Opportunities**

Outcome	Program Group	Control Group	Difference (Impact)	P-Value <sup>a</sup>
<b><u>Employment (%)</u></b>				
CEO transitional employment				
Quarter 1 (quarter of random assignment)	62.7	1.6	61.1 ***	0.000
Quarter 2	43.1	2.1	41.1 ***	0.000
Quarter 3	14.0	0.3	13.7 ***	0.000
Quarter 4	5.7	0.5	5.2 ***	0.000
Quarter 5	2.3	0.5	1.8 **	0.020
Quarter 6	1.3	0.0	1.4 **	0.018
Quarter 7	2.2	0.8	1.5 *	0.076
Quarter 8	2.9	0.1	2.7 ***	0.001
Years 1-2 (Q1-Q8)	69.8	3.8	66.0 ***	0.000
Unsubsidized employment <sup>b</sup>				
Quarter 1 (quarter of random assignment)	19.0	28.3	-9.2 **	0.011
Quarter 2	29.5	34.0	-4.5	0.236
Quarter 3	34.4	36.4	-2.0	0.557
Quarter 4	32.6	34.2	-1.6	0.602
Quarter 5	30.6	33.8	-3.1	0.293
Quarter 6	30.5	31.7	-1.3	0.673
Quarter 7	32.1	30.8	1.3	0.661
Quarter 8	32.5	32.3	0.2	0.952
Years 1-2 (Q1-Q8)	59.6	62.8	-3.2	0.307
Total UI-covered employment				
Quarter 1 (quarter of random assignment)	65.8	26.0	39.8 ***	0.000
Quarter 2	59.1	34.9	24.2 ***	0.000
Quarter 3	41.7	35.4	6.4 **	0.045
Quarter 4	35.8	34.6	1.2	0.689
Quarter 5	32.0	34.2	-2.2	0.468
Quarter 6	31.2	31.7	-0.5	0.867
Quarter 7	33.7	31.3	2.4	0.433
Quarter 8	35.2	32.4	2.8	0.357
Years 1-2 (Q1-Q8)	85.2	69.3	16.0 ***	0.000

(continued)

**Appendix Table B.2 (continued)**

Outcome	Program Group	Control Group	Difference (Impact)		P-Value <sup>a</sup>
<b><u>Earnings (\$)</u></b>					
Earnings from CEO transitional job					
Quarter 1 (quarter of random assignment)	371	6	364 ***		0.000
Quarter 2	374	13	361 ***		0.000
Quarter 3	107	6	101 ***		0.000
Quarter 4	37	4	32 ***		0.000
Quarter 5	21	2	18 **		0.022
Quarter 6	11	1	11		0.287
Quarter 7	12	5	6		0.344
Quarter 8	17	1	16 ***		0.008
Years 1-2 (Q1-Q8) <sup>d</sup>	949	39	910 NA		--
Unsubsidized earnings					
Quarter 1 (quarter of random assignment)	--	--	--	--	--
Quarter 2	--	--	--	--	--
Quarter 3	--	--	--	--	--
Quarter 4	1,143	1,174	-31		0.873
Quarter 5	1,227	1,174	53		0.783
Quarter 6	1,261	1,089	173		0.336
Quarter 7	1,330	1,209	122		0.588
Quarter 8	1,287	1,189	98		0.615
Years 1-2 (Q1-Q8)	--	--	--	--	--
Total earnings from UI-covered employment					
Quarter 1 (quarter of random assignment)	--	--	--	--	--
Quarter 2	--	--	--	--	--
Quarter 3	--	--	--	--	--
Quarter 4	1,180	1,178	2		0.994
Quarter 5	1,248	1,177	71		0.710
Quarter 6	1,272	1,089	183		0.307
Quarter 7	1,342	1,214	128		0.569
Quarter 8	1,304	1,190	114		0.558
Years 1-2 (Q1-Q8)	--	--	--	--	--
Sample size (total = 973) <sup>c</sup>	564	409			

(continued)

**The Enhanced Services for the Hard-to-Employ Demonstration**

**Appendix Table B.3**

**Comparison of Unemployment Insurance Records and Survey-Reported Employment**

**Center for Employment Opportunities**

Outcome (%)	Program Group	Control Group	Difference (Impact)	P-Value
Quarter of interview: survey employment/no UI employment	17.3	20.4	-3.1	0.384
Interviewed in prison or jail	12.5	14.4	-1.9	0.535
Currently working in prison or jail	6.2	8.3	-2.1	0.359
Current or most recent job is day labor, temporary, or odd job	14.4	10.8	3.6	0.227
<b><u>Among those with survey employment/no UI employment</u></b>				
Currently working in prison or jail	37.5	39.6	-2.1	0.823
Current or most recent job is day labor, temporary, or odd job	8.8	10.8	-2.0	0.751
Sample size (total = 531)	316	215		

SOURCES: MDRC calculations from unemployment insurance (UI) wage records from New York State and the client survey.

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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.





## Appendix C

# Glossary of Recidivism Outcomes and Additional Impacts on Recidivism

Appendix C presents a Glossary of Recidivism Outcomes as well as additional impacts on recidivism in the following tables:

- C.1 Additional Two-Year Impacts on Recidivism
- C.2 Impacts on Recidivism in Year 1
- C.3 Impacts on Recidivism in Year 2
- C.4 Quarterly Impacts on Arrests and Incarceration
- C.5 Quarterly Impacts on Incarceration and Employment



## Glossary of Recidivism Outcomes

### Arrests

**Sealed/unsealed arrest data:** New York State’s Division of Criminal Justice Services (DCJS) provided both types of arrest information. In general, *unsealed arrests* include those that end in conviction (about half of all arrests) or those still awaiting disposition. Arrests may become *sealed arrests* — removed from a person’s public record — for a number of reasons, including: events that are favorably disposed (that is, do not end in a conviction), some lower-level misdemeanor convictions, and some events where “youthful offender status” was granted. Thus, following adjudication, an arrest may be removed from a person’s unsealed record.

The sealed arrest information from DCJS was provided without personal identifiers, so it can be analyzed only in a limited way in this report. In Chapter 5, the arrests discussed are unsealed at the time of the data collection. In Appendix Table C.2, the first two arrest outcomes (arrest rate and number of arrests) include all arrests in an individual’s record, both sealed and unsealed.

**Months between random assignment and first arrest:** For individuals with an unsealed arrest in the follow-up period, this measure calculates the number of months between the random assignment date and the first arrest date.

### Convictions

**Convictions:** Equal to 1 if a person had a conviction with a disposition date after random assignment. The *disposition date* is the date of a sentence for conviction.

**Convicted of a felony or misdemeanor:** Conviction class is based on information provided by DCJS for a court disposition. Conviction classes include felony, misdemeanor, violation, and infraction.

**Convicted of a violent crime:** In Chapter 1, Table 1.2 (“Criminal History at the Time of Random Assignment, by Research Group”), categorization as a violent crime is based on a violent crime indicator in an individual’s DCJS record. Recidivism outcomes shown in Chapter 5 and the appendix tables categorize violent crime based on the 1994 Bureau of Justice Statistics Special Report (Langan and Levin, 2002) and on the Uniform Crime Reporting (UCR) conviction codes in the DCJS record. In the present report, violent crime may be either a felony-level or a misdemeanor-level conviction.

## **Incarceration**

**Incarceration in state prison:** Equal to 1 if a person was incarcerated for at least one day in a state prison facility during the specified period.

**Admitted to state prison:** Equal to 1 if a person was admitted to prison in the given year of follow-up. For Year 2, this measure does not include a person's continued incarceration in prison from the previous year.

**Jail incarceration:** Equal to 1 if a person was incarcerated for at least one day on Rikers Island, New York City's large jail facility. Admission status for Rikers incarceration fell into one of three categories: city sentence, detainee, or technical parole violation. This measure includes jail stays before and after adjudication.

**Incarcerated for a new crime:** Equal to 1 if a person was admitted to prison or jail with a new sentence. A return to jail or prison for a parole violation associated with a previous arrest or conviction in the follow-up period is not a new incarceration.

**Incarceration as a detainee (Rikers Island only):** An individual was given a 1 on this measure if the admission status to Rikers was classified as "detainee." These incarceration spells most frequently ended with a transfer to state prison, but individuals in the detainee category were incarcerated for a variety of reasons both before and after adjudication. Individuals incarcerated as detainees were released for the following reasons: transferred to prison, in custody of the New York Police Department, transferred to a mental hospital, paid bail, expired city sentence, dismissed case, and released on own recognizance.

## **Parole**

**Absconded/revoked from parole:** Equal to 1 if a person has a parole record with a discharge date after random assignment and the discharge reason associated with that date is "absconded" or "revoked." Note, that a person can be revoked from parole only after being found. Someone who absconded and was not found during the follow-up period remained listed as absconded.

## **Other**

**Recidivism composite measure:** An individual was given a 1 on this measure if (1) arrested, (2) convicted, or (3) incarcerated at any time during the follow-up period. This measure is not a cumulative score of recidivism. An individual could have had a 1 on any of the three outcomes, on two of the three outcomes, or on all three of them.

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**Appendix Table C.1**

**Additional Two-Year Impacts on Recidivism**

**Center for Employment Opportunities**

Outcome	Program Group	Control Group	Difference (Impact)	P-Value
<b><u>Additional arrest measures</u></b>				
Arrested (sealed and unsealed) <sup>a,b</sup> (%)	53.2	58.4	-5.1	0.106
Average number of arrests (sealed and unsealed) <sup>b</sup>	1.1	1.2	-0.2	0.132
Number of months between random assignment and first arrest (%)				
Never arrested	47.0	42.7	4.3	0.176
3 months or less	1.5	2.0	-0.5	0.600
4-6 months	2.4	1.8	0.6	0.561
7-12 months	5.8	4.7	1.0	0.499
13-18 months	7.4	6.1	1.3	0.435
19-24 months	5.8	9.0	-3.2 *	0.061
Months until first unsealed arrest, conviction, or incarceration <sup>c</sup>	9.3	10.2	-0.9	0.164
<b><u>Incarceration in state prison (%)</u></b>				
Incarcerated in state prison <sup>d</sup>	25.3	27.7	-2.4	0.399
Incarcerated for a new crime	4.2	6.8	-2.6 *	0.077
Incarcerated for a technical parole violation	17.4	15.5	1.9	0.428
Incarcerated for all other/unknown reasons <sup>e</sup>	4.4	5.7	-1.3	0.352
<b><u>Incarceration in jail (%)</u></b>				
Incarcerated in jail	49.1	53.5	-4.4	0.168
Incarcerated for a new crime	10.1	10.8	-0.7	0.718
Incarcerated for a technical parole violation	29.1	25.6	3.5	0.234
Incarcerated as a "detainee"	29.4	33.9	-4.5	0.127
<b><u>Parole outcome (%)</u></b>				
Absconded/revoked from parole	31.4	31.5	-0.1	0.984
<b><u>Reason for conviction<sup>f,g</sup> (%)</u></b>				
Violent crime	4.4	5.8	-1.5	0.309
Drug crime	15.6	19.9	-4.3 *	0.079
Property crime	14.9	18.3	-3.4	0.159
Public order crime	3.4	2.9	0.5	0.674
Other	0.6	0.5	0.1	0.871
Sample size (total = 977)	568	409		

(continued)

### Appendix Table C.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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>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.

<sup>b</sup>This measure includes both sealed and unsealed arrest events. Data on sealed events were provided to MDRC in a format that did not allow for the identification of individuals.

<sup>c</sup>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.

<sup>d</sup>Due to multiple incarcerations, subcategories are not mutually exclusive and may sum to a total greater than the "Incarcerated" measure.

<sup>e</sup>All other incarcerations, including those for technical violations while not on parole and for other unknown/unspecified reasons.

<sup>f</sup>Categories of conviction charges are based on definitions from Langan and Levin (2002). See Appendix C for charges defined as violent.

<sup>g</sup>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.

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**Appendix Table C.2**

**Impacts on Recidivism in Year 1**

**Center for Employment Opportunities**

Outcome	Program Group	Control Group	Difference (Impact)	P-Value
Arrested <sup>a</sup> (%)	21.7	22.9	-1.3	0.638
Convicted of a crime <sup>b</sup> (%)	15.5	17.3	-1.8	0.460
Convicted of a felony	1.4	3.1	-1.7 *	0.071
Convicted of a misdemeanor	11.8	12.1	-0.3	0.897
Convicted of a violent crime <sup>c</sup> (%)	1.9	1.4	0.5	0.542
Incarcerated <sup>d</sup> (%)	35.0	39.8	-4.9	0.120
Prison	11.0	14.0	-3.0	0.166
Jail	34.8	39.7	-4.8	0.120
Incarcerated for a new crime (%)	7.0	8.2	-1.2	0.474
Prison	0.8	3.0	-2.2 **	0.012
Jail	6.2	5.2	1.0	0.522
Incarcerated for a technical parole violation <sup>e</sup> (%)	19.9	22.1	-2.1	0.420
Prison	8.0	9.5	-1.5	0.412
Jail	18.6	20.3	-1.7	0.516
Total days incarcerated	37	38	-1	0.801
Prison	12	13	-1	0.583
Jail	25	25	0	0.941
Arrested, convicted, or incarcerated (%)	39.3	45.5	-6.2 *	0.053
Sample size (total = 977)	568	409		

(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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>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.

<sup>b</sup>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.

<sup>c</sup>Violent crimes are based on conviction charges defined by Langan and Levin (2002). See Appendix C for charges defined as violent.



### **Appendix Table C.2 (continued)**

<sup>d</sup>Includes all reasons for incarceration, such as sentences for new crimes, technical violations of parole, detainee (jail), and other reasons. Therefore incarcerations for new crimes and parole violations do not sum to percentage incarcerated. See Appendix Table C.1 for more details about reasons for incarceration.

<sup>e</sup>Incarceration status based on Quarter 8 after random assignment. Incarceration includes both prison and jail.

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**Appendix Table C.3**

**Impacts on Recidivism in Year 2**

**Center for Employment Opportunities**

Outcome	Program Group	Control Group	Difference (Impact)	P-Value
Arrested <sup>a</sup> (%)	22.8	27.5	-4.6 *	0.098
Convicted of a crime <sup>b</sup> (%)	20.2	26.3	-6.1 **	0.025
Convicted of a felony	5.2	4.5	0.7	0.630
Convicted of a misdemeanor	14.5	21.5	-7.1 ***	0.004
Convicted of a violent crime <sup>c</sup> (%)	2.5	4.5	-1.9	0.106
Admitted to prison or jail <sup>d</sup> (%)	29.9	33.0	-3.2	0.292
Prison	16.7	17.5	-0.8	0.733
Jail	29.6	31.2	-1.6	0.591
Incarcerated for a new crime (%)	8.4	10.8	-2.3	0.228
Prison	3.4	3.8	-0.4	0.742
Jail	5.2	7.0	-1.8	0.251
Incarcerated for a parole technical violation (%)	18.7	15.4	3.2	0.192
Prison	11.2	9.5	1.7	0.395
Jail	14.9	10.4	4.5 **	0.042
Total days incarcerated	62.9	68.6	-5.7	0.424
Prison	33.8	40.5	-6.7	0.228
Jail	29.1	28.1	1.0	0.801
Arrested, convicted, or incarcerated (%)	47.1	51.8	-4.7	0.146
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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>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.

<sup>b</sup>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.

<sup>c</sup>Violent crimes are based on conviction charges defined by Langan and Levin (2002). See Appendix C for charges defined as violent.

<sup>d</sup>Includes all reasons for incarceration, such as sentences for new crimes, technical violations of parole, detainee (jail), and other reasons. Therefore incarcerations for new crimes and parole violations do not sum to percentage incarcerated. See Appendix Table C.1 for more details about reasons for incarceration. This measure only includes new admissions to prison or jail in Year 2.

**The Enhanced Services for the Hard-to-Employ Demonstration**

**Appendix Table C.4**

**Quarterly Impacts on Arrests and Incarceration**

**Center for Employment Opportunities**

Outcome (%)	Program Group	Control Group	Difference (Impact)	P-Value
Arrested and not incarcerated				
Quarter 1 (quarter of random assignment)	0.7	0.7	0.1	0.906
Quarter 2	1.0	1.6	-0.5	0.466
Quarter 3	1.8	1.8	0.0	0.959
Quarter 4	2.5	2.2	0.2	0.820
Quarter 5	2.6	2.4	0.3	0.808
Quarter 6	2.9	3.1	-0.3	0.820
Quarter 7	2.7	2.7	0.0	0.980
Quarter 8	2.7	3.7	-1.0	0.407
Arrested and incarcerated				
Quarter 1 (quarter of random assignment)	2.1	4.0	-1.8	0.109
Quarter 2	5.0	3.3	1.6	0.221
Quarter 3	5.2	4.8	0.4	0.776
Quarter 4	5.8	5.9	-0.1	0.950
Quarter 5	4.8	7.9	-3.0 **	0.048
Quarter 6	4.5	5.1	-0.6	0.649
Quarter 7	5.6	6.3	-0.7	0.653
Quarter 8	4.3	6.3	-2.0	0.172
Not arrested and not incarcerated				
Quarter 1 (quarter of random assignment)	78.7	78.8	-0.2	0.947
Quarter 2	82.5	83.5	-1.0	0.698
Quarter 3	76.7	74.4	2.3	0.407
Quarter 4	74.7	69.3	5.4 *	0.062
Quarter 5	70.5	68.1	2.5	0.408
Quarter 6	69.7	67.6	2.1	0.486
Quarter 7	71.4	69.6	1.7	0.552
Quarter 8	72.4	68.2	4.2	0.151
Not arrested and incarcerated				
Quarter 1 (quarter of random assignment)	18.5	16.6	1.9	0.409
Quarter 2	11.5	11.6	-0.1	0.947
Quarter 3	16.2	19.0	-2.8	0.261
Quarter 4	17.0	22.5	-5.5 **	0.033
Quarter 5	22.0	21.7	0.3	0.905
Quarter 6	23.0	24.2	-1.2	0.664
Quarter 7	20.3	21.4	-1.1	0.683
Quarter 8	20.6	21.9	-1.3	0.624
Sample size (total = 977)	568	409		

(continued)

### **Appendix Table C.4 (continued)**

SOURCES: MDRC calculations using data from New York State unemployment insurance (UI) wage records and the New York State Division of Criminal Justice Services (DCJS).

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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

**The Enhanced Services for the Hard-to-Employ Demonstration**

**Appendix Table C.5**

**Quarterly Impacts on Incarceration and Employment**

**Center for Employment Opportunities**

Outcome (%)	Program Group	Control Group	Difference (Impact)	P-Value <sup>a</sup>
Employed and not incarcerated				
Quarter 1 (quarter of random assignment)	54.0	22.4	31.6 ***	0.000
Quarter 2	54.6	32.2	22.4 ***	0.000
Quarter 3	38.8	32.7	6.1 *	0.051
Quarter 4	33.2	29.0	4.2	0.158
Quarter 5	27.5	27.4	0.1	0.978
Quarter 6	27.6	25.1	2.5	0.382
Quarter 7	29.0	24.3	4.7 *	0.097
Quarter 8	29.3	26.9	2.4	0.404
Employed and incarcerated				
Quarter 1 (quarter of random assignment)	11.8	3.6	8.2 ***	0.000
Quarter 2	4.4	2.7	1.7	0.169
Quarter 3	3.2	2.8	0.4	0.696
Quarter 4	1.6	4.4	-2.8 ***	0.010
Quarter 5	3.4	2.9	0.5	0.661
Quarter 6	1.8	2.7	-0.9	0.335
Quarter 7	2.6	3.5	-0.9	0.403
Quarter 8	3.5	1.9	1.6	0.135
Not employed and not incarcerated				
Quarter 1 (quarter of random assignment)	24.2	56.5	-32.3 ***	0.000
Quarter 2	29.1	53.2	-24.1 ***	0.000
Quarter 3	40.1	44.4	-4.3	0.184
Quarter 4	44.1	44.0	0.2	0.953
Quarter 5	45.6	44.4	1.1	0.730
Quarter 6	45.1	46.7	-1.6	0.617
Quarter 7	44.9	49.1	-4.2	0.200
Quarter 8	45.9	46.3	-0.4	0.911
Not employed and incarcerated				
Quarter 1 (quarter of random assignment)	10.0	17.5	-7.5 ***	0.000
Quarter 2	11.8	11.9	0.0	0.984
Quarter 3	17.9	20.2	-2.3	0.375
Quarter 4	21.0	22.6	-1.6	0.541
Quarter 5	23.5	25.2	-1.7	0.542
Quarter 6	25.5	25.5	0.0	0.990
Quarter 7	23.5	23.1	0.4	0.887
Quarter 8	21.4	25.0	-3.7	0.177
Sample size (total = 974) <sup>b</sup>	565	409		

(continued)

### Appendix Table C.5 (continued)

SOURCES: MDRC calculations using data from New York State unemployment insurance (UI) wage records and the New York State Division of Criminal Justice Services (DCJS).

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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>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 on the table): 3.0, 3.1, 1.8, 2.8, 3.1, and 2.1.

<sup>b</sup>A total of three people were excluded from the sample because they were missing a Social Security number.



## **Appendix D**

# **Impacts on Housing, Income, Health, and Family Outcomes**

Appendix D shows the impacts that CEO had on the following outcomes.

- D.1 Impacts on Housing and Mobility
- D.2 Impacts on Income and Financial Assistance
- D.3 Impacts on Health Insurance Coverage and Health
- D.4 Impacts on Participation in Substance Abuse Treatment
- D.5 Impacts on Child Custody and Child Support
- D.6 Impacts on Child Contact and Relationships





**The Enhanced Services for the Hard-to-Employ Demonstration**

**Appendix Table D.1**

**Impacts on Housing and Mobility**

**Center for Employment Opportunities**

Outcome (%)	Program Group	Control Group	Impact	P-Value
<b><u>Current housing status</u></b>				
Currently living in:				
Own place	20.4	24.8	-4.4	0.220
Someone else's place	70.4	61.0	9.3 **	0.021
Treatment facility/transitional housing/halfway house	3.2	5.5	-2.3	0.191
Homeless/other	4.2	4.8	-0.7	0.725
Currently in public housing	31.6	31.5	0.0	0.995
Currently living in subsidized housing (Section 8)	12.6	13.0	-0.4	0.892
Sample member lives alone	13.7	13.7	0.0	0.992
Sample member currently lives with:				
Spouse/partner	34.9	30.5	4.4	0.300
Relative	59.6	67.0	-7.5 *	0.076
Nonrelative	9.1	5.0	4.1 *	0.080
Length in current home: <sup>a</sup>				
Less than 2 months	8.3	8.8	-0.5	0.840
2 to 6 months	19.9	15.4	4.6	0.192
7 to 12 months	9.4	10.3	-0.9	0.740
More than 1 year	57.1	57.6	-0.5	0.919
Name is on lease/mortgage	13.3	15.2	-1.9	0.530
Contributes to cost of rent or mortgage	70.1	65.2	4.9	0.238
<b><u>Mobility</u></b>				
Ever moved since random assignment	41.6	39.2	2.4	0.585
Number of residences:				
1	55.1	57.4	-2.3	0.604
2	27.8	25.7	2.1	0.598
3 or more	13.8	13.5	0.3	0.918
Reason for last move (%)				
Evicted/kicked out	6.0	3.5	2.6	0.175
Unable to pay rent	3.9	1.9	2.0	0.204
Found own place to live	6.6	6.7	-0.1	0.976
Moved in with family/friends	13.7	11.3	2.4	0.409
Other	12.7	14.4	-1.7	0.580
Sample size (total = 531) <sup>b</sup>	316	215		

(continued)

### **Appendix Table D.1 (continued)**

SOURCE: MDRC calculations from responses to the client survey.

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

Subcategories may not sum to the total due to rounding and missing data

Statistical significance levels are indicated as: \*\*\* = 1 percent; \*\* = 5 percent; \* = 10 percent. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

For those currently in jail/prison, all responses are based on the most recent housing status before incarceration.

<sup>a</sup>Fourteen respondents replied "no answer" to this question, and one respondent refused to respond.

<sup>b</sup>The total sample size ranges from 488 to 531 as a result of missing responses to various items.

**The Enhanced Services for the Hard-to-Employ Demonstration**

**Appendix Table D.2**

**Impacts on Income and Financial Assistance**

**Center for Employment Opportunities**

Outcome	Program Group	Control Group	Impact	P-Value
<b><u>Individual income</u></b>				
Percentage of respondents with each income source				
Earnings	51.9	46.0	5.9	0.181
Child support	0.2	0.0	0.1	0.617
Public assistance				
TANF	9.8	14.2	-4.4	0.115
Food stamps	29.0	26.6	2.5	0.521
SSI or disability	1.8	3.2	-1.4	0.290
Total individual income in prior month (\$)	775	760	15	0.901
<b><u>Other household members' income</u></b>				
Percentage with each income source				
Earnings	47.8	42.7	5.1	0.251
Child support	3.3	3.2	0.1	0.952
Public assistance				
TANF	6.7	10.6	-3.9	0.126
Food stamps	13.8	23.5	-9.7 ***	0.005
SSI or disability	13.0	15.4	-2.4	0.426
Total household income in prior month (\$)	1,756	1,550	206	0.547
Percentage of household income that is respondent's	56.6	63.3	-6.7	0.329
Sample size (total = 531)	316	215		

SOURCE: MDRC calculations from responses to the client survey.

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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

**The Enhanced Services for the Hard-to-Employ Demonstration**  
**Appendix Table D.3**  
**Impacts on Health Insurance Coverage and Health**  
**Center for Employment Opportunities**

Outcome	Program Group	Control Group	Impact	P-Value
<b><u>Health insurance coverage (%)</u></b>				
Respondent has health insurance				
Publicly funded	48.3	54.1	-5.8	0.188
Publicly funded and not on TANF or SSI	37.6	37.6	-0.1	0.986
Privately funded	6.9	3.1	3.8 *	0.061
Spouse has health insurance	37.3	36.0	1.3	0.759
<b><u>Medical hardship (%)</u></b>				
Unable to go to a doctor or hospital due to lack of money or health insurance	17.0	16.5	0.5	0.881
Unable to go see a dentist due to lack of money or health insurance	14.8	19.6	-4.9	0.127
Went to emergency room	22.8	22.4	0.5	0.899
<b><u>Health</u></b>				
Average Body Mass Index (BMI) <sup>a</sup>				
Underweight	0.0	1.2	-1.2 **	0.045
Normal weight	27.1	36.7	-9.6 **	0.019
Overweight	41.4	40.3	1.1	0.796
Obese	30.5	21.4	9.1 **	0.019
Self-rated health (%)				
Excellent health	27.1	26.3	0.7	0.850
Very good	31.5	26.6	4.9	0.227
Good health	22.1	27.1	-5.0	0.192
Fair health	15.7	15.6	0.2	0.961
Poor health	1.8	3.5	-1.7	0.198
Psychological Distress Scale <sup>b</sup>	6.6	6.7	-0.1	0.778
Experienced serious psychological distress in the past month <sup>c</sup> (%)	17.3	19.2	-1.9	0.556
Sample size (total = 531)	316	215		

(continued)

**The Enhanced Services for the Hard-to-Employ Demonstration**  
**Appendix Table D.4**  
**Impacts on Participation in Substance Abuse Treatment**  
**Center for Employment Opportunities**

Outcome (%)	Program Group	Control Group	Impact	P-Value
Received substance abuse treatment or attended self-help groups	53.1	55.9	-2.8	0.516
In substance abuse treatment since random assignment	51.9	51.7	0.1	0.974
Emergency room visit for drug or alcohol treatment	5.8	6.9	-1.1	0.632
Hospital stay for detoxification	8.2	9.3	-1.1	0.667
Outpatient treatment	37.9	42.9	-5.0	0.239
Medicinal interventions including methadone	5.5	6.8	-1.4	0.526
Residential treatment	14.1	17.1	-2.9	0.350
Other treatment	23.8	15.2	8.7 **	0.015
Attended self-help groups <sup>a</sup>	16.5	21.7	-5.2	0.124
Mandated to drug treatment by parole officer	40.7	43.2	-2.5	0.567
Currently in drug treatment or attending self-help groups	21.8	18.8	2.9	0.407
Currently in self-help	8.9	9.5	-0.6	0.802
Currently in drug treatment	18.2	13.4	4.8	0.148
Sample size (total = 531)	316	215		

SOURCE: MDRC calculations from responses to the client survey.

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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>Includes Alcoholics Anonymous (AA) and Narcotics Anonymous (NA) groups.

**The Enhanced Services for the Hard-to-Employ Demonstration**

**Appendix Table D.5**

**Impacts on Child Custody and Child Support**

**Center for Employment Opportunities**

Outcome	Program Group	Control Group	Impact	P-Value
Respondent has any children (%)	45.3	56.2	-10.9 **	0.013
<i>Has any children living apart (%)</i>	<i>71.6</i>	<i>65.0</i>		
<i>Had difficulties getting custody (%)</i>	<i>4.9</i>	<i>8.4</i>		
<i>Other parent not willing to give up custody (%)</i>	<i>3.1</i>	<i>3.1</i>		
<i>Had difficulties making visitation arrangements (%)</i>	<i>6.8</i>	<i>7.1</i>		
<b><u>Formal child support</u></b>				
<i>Required to pay child support for any children (%)</i>	<i>11.3</i>	<i>11.0</i>		
<i>Did child support court order change?</i>				
<i>Yes (%)</i>	<i>2.7</i>	<i>2.1</i>		
<i>State forgave or decreased child support debt (%)</i>	<i>1.9</i>	<i>0.5</i>		
<b><u>Informal child support</u></b>				
<i>Provided informal support to children in prior month (%)</i>	<i>8.4</i>	<i>7.3</i>		
<i>Average amount of informal support in prior month(\$)</i>	<i>14</i>	<i>9</i>		
<i>Types of informal support in prior month (%)</i>				
<i>Cash</i>	<i>8.4</i>	<i>7.3</i>		
<i>Food</i>	<i>6.4</i>	<i>3.4</i>		
<i>Clothing</i>	<i>8.4</i>	<i>4.2</i>		
<i>Something else</i>	<i>3.7</i>	<i>3.5</i>		
Sample size (total = 530)	315	215		

SOURCE: MDRC calculations based on responses to the client survey.

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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

All measures in italic type are calculated among those who were parents (total = 281) and are therefore considered nonexperimental and are not tested for statistical significance.

**The Enhanced Services for the Hard-to-Employ Demonstration**

**Appendix Table D.6**

**Impacts on Child Contact and Relationships**

**Center for Employment Opportunities**

Outcome (%)	Program Group	Control Group
<i>In past month, had contact with any child</i>	83.4	86.2
<i>Relationship between respondent and child in past month</i>		
<i>Hugged child and said that he loved child</i>	73.6	73.2
<i>Helped child with homework or school project</i>	33.8	31.9
<i>Attended child's school events</i>	23.4	25.9
<b><u>For parents that have at least one child living apart</u></b>		
<i>Frequency of visits in past month</i>		
<i>Daily</i>	13.8	11.4
<i>A couple times a week</i>	11.4	9.8
<i>Weekly</i>	10.0	18.3
<i>2-3 times in past month</i>	9.6	2.3
<i>Once in past month</i>	2.4	0.9
<i>Frequency of phone/mail contact with child in past month</i>		
<i>Daily</i>	16.7	10.1
<i>A couple times a week</i>	12.5	14.3
<i>Weekly</i>	8.5	4.8
<i>2-3 times in past month</i>	3.3	2.7
<i>Once in past month</i>	-0.1	1.3
<i>Ever baby-sat during the past month</i>	21.2	10.6
Sample size (total = 530)	315	215

SOURCE: MDRC calculations from responses to the client survey.

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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

All child contact measures include those parents who were incarcerated.

All measures in italic type are calculated among those who were parents and are therefore considered nonexperimental and are not tested for statistical significance.





## Appendix E

### **Additional Subgroup Impacts: Time Between Prison Release and Random Assignment; Age; and Educational Attainment**

Appendix E shows the impacts that CEO had on various subgroups, as defined by the titles of the following tables.

- E.1 Quarterly Impacts on Employment, by Time Between Prison Release and Random Assignment
- E.2 Quarterly Impacts on Earnings, by Time Between Prison Release and Random Assignment
- E.3 Year 1 Impacts on Recidivism, by Time Between Prison Release and Random Assignment
- E.4 Year 2 Impacts on Recidivism, by Time Between Prison Release and Random Assignment
- E.5 Two-Year Impacts on Employment, by Age
- E.6 Two-Year Impacts on Recidivism, by Age
- E.7 Impacts on Employment, by Educational Attainment
- E.8 Two-Year Impacts on Recidivism, by Educational Attainment



The Enhanced Services for the Hard-to-Employ Demonstration

Appendix Table E.1

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

Center for Employment Opportunities

Outcome (%)	Length of Time Between Prison Release and Random Assignment								H-Stars <sup>b</sup>
	Less Than 3 Months				More Than 3 Months				
	Program Group	Control Group	Difference (Impact)	P-Value	Program Group	Control Group	Difference (Impact)	P-Value <sup>a</sup>	
<b><u>CEO transitional employment</u></b>									
Quarter 1 (quarter of random assignment)	65.8	-0.7	66.6 ***	0.000	62.1	2.3	59.8 ***	0.000	
Quarter 2	47.7	-0.4	48.1 ***	0.000	41.4	2.1	39.3 ***	0.000	*
Quarter 3	15.8	0.5	15.3 ***	0.000	12.9	0.2	12.7 ***	0.000	
Quarter 4	5.8	0.0	5.8 ***	0.002	5.9	0.7	5.1 ***	0.001	
Quarter 5	1.5	0.2	1.3	0.198	2.8	0.7	2.0 *	0.083	
Quarter 6	0.6	0.0	0.6	0.360	1.7	-0.1	1.8 **	0.029	
Quarter 7	2.0	0.0	2.0 *	0.093	2.3	1.2	1.1	0.383	
Quarter 8	0.5	0.0	0.5	0.373	4.5	0.2	4.3 ***	0.002	**
Years 1-2 (Q1-Q8)	73.3	-0.6	74.0 ***	0.000	68.1	5.4	62.7 ***	0.000	**
<b><u>Unsubsidized employment<sup>c</sup></u></b>									
Quarter 1 (quarter of random assignment)	-	-	-	-	-	-	-	-	
Quarter 2	-	-	-	-	-	-	-	-	
Quarter 3	-	-	-	-	-	-	-	-	
Quarter 4	36.6	32.7	3.9	0.434	30.7	34.2	-3.5	0.389	
Quarter 5	37.2	33.7	3.5	0.489	26.6	34.1	-7.5 *	0.055	*
Quarter 6	32.2	32.6	-0.4	0.936	29.8	31.6	-1.8	0.650	
Quarter 7	37.1	29.4	7.7	0.122	28.7	31.4	-2.7	0.503	
Quarter 8	37.8	31.0	6.8	0.163	30.1	32.6	-2.5	0.538	
Years 1-2 (Q1-Q8)	64.2	61.5	2.7	0.602	55.7	62.9	-7.2 *	0.079	

(continued)

**Appendix Table E.1 (continued)**

Outcome	Length of Time Between Prison Release and Random Assignment									
	Less Than 3 Months					More Than 3 Months				
	Program Group	Control Group	Difference (Impact)	P-Value	Program Group	Control Group	Difference (Impact)	P-Value <sup>a</sup>	H-Stars <sup>b</sup>	
<b>Total UI-covered employment</b>										
Quarter 1 (quarter of random assignment)	69.9	25.9	44.0 ***	0.000	65.9	24.7	41.1 ***	0.000		
Quarter 2	65.4	31.3	34.1 ***	0.000	57.0	35.4	21.6 ***	0.000	*	
Quarter 3	48.2	34.6	13.6 ***	0.009	39.4	35.1	4.3	0.307		
Quarter 4	39.6	32.7	7.0	0.168	34.5	34.7	-0.2	0.963		
Quarter 5	37.5	33.7	3.8	0.447	28.5	34.8	-6.3	0.111		
Quarter 6	32.5	32.6	-0.1	0.978	31.1	31.5	-0.5	0.905		
Quarter 7	38.8	29.4	9.3 *	0.063	29.9	32.1	-2.2	0.592	*	
Quarter 8	38.1	31.0	7.1	0.148	34.6	32.8	1.9	0.649		
Years 1-2 (Q1-Q8)	89.4	66.7	22.7 ***	0.000	83.8	69.4	14.3 ***	0.000		
Sample size (total = 925) <sup>d</sup>	220	155			313	237				

SOURCES: MDRC calculations from the National Directory of New Hires (NDNH) database and unemployment insurance (UI) wage records from New York State and payroll data from CEO's Network for Information Gathering Evaluation and Learning (NIGEL) database.

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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>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 on the table beginning with the 'Less than 3 Months' group): 4.0887, 4.1722, 3.0841, 3.7945, 4.8697, 5.0911, 4.1909, 3.2232, 3.2419, 2.1439, 3.282, 3.8544, 4.1601, and 3.5286.

<sup>b</sup>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.

<sup>c</sup>Unsubsidized employment is estimated from NDNH data that are incomplete for Quarters 1 to 3. A comparison of aggregate estimates provided for the full sample from the U.S. Department of Labor indicates that the results from NDNH are representative of the full sample.

<sup>d</sup>Forty-eight sample members are missing a release date, and four sample members are missing a Social Security number; they therefore could not be matched to UI data.

The Enhanced Services for the Hard-to-Employ Demonstration

Appendix Table E.2

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

Center for Employment Opportunities

Outcome (\$)	Length of Time Between Prison Release and Random Assignment								H-Stars <sup>b</sup>	
	Less Than 3 Months				More Than 3 Months					
	Program Group	Control Group	Difference (Impact)	P-Value	Program Group	Control Group	Difference (Impact)	P-Value <sup>a</sup>		
<b>Earnings</b>										
CEO transitional job earnings										
Quarter 1 (quarter of random assignment)	385	-10	395 ***	0.000	378	11	368 ***	0.000		
Quarter 2	422	13	409 ***	0.000	348	12	336 ***	0.000		
Quarter 3	115	15	100 ***	0.001	102	3	99 ***	0.000		
Quarter 4	42	1	42 ***	0.005	37	6	31 **	0.012		
Quarter 5	7	0	7	0.192	29	4	25 *	0.055		
Quarter 6	18	0	18	0.465	9	-1	10 *	0.056		
Quarter 7	16	-1	17	0.206	10	8	2	0.816		
Quarter 8	4	-1	5	0.374	24	1	23 **	0.011		*
Years 1-2 (Q1-Q8)	-	-	-	-	-	-	-	-		
Unsubsidized earnings										
Quarter 1 (quarter of random assignment)	-	-	-	-	-	-	-	-		
Quarter 2	-	-	-	-	-	-	-	-		
Quarter 3	-	-	-	-	-	-	-	-		
Quarter 4	1,437	1,355	82	0.794	1,043	1,005	39	0.879		
Quarter 5	1,534	1,500	33	0.912	1,126	916	209	0.426		
Quarter 6	1,566	1,267	299	0.329	1,153	896	257	0.262		
Quarter 7	1,676	1,550	126	0.780	1,189	917	272	0.246		
Quarter 8	1,501	1,386	114	0.693	1,260	988	272	0.310		
Years 1-2 (Q1-Q8)	-	-	-	-	-	-	-	-		
Sample size (total = 925) <sup>c</sup>	220	155			313	237				

(continued)

### Appendix Table E.2 (continued)

SOURCES: MDRC calculations based on unemployment insurance (UI) wage records from New York State provided by the National Database of New Hires (NDNH) and payroll data from CEO's Network for Information Gathering Evaluation and Learning (NIGEL) database.

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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>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 on the table beginning with the "Less Than 3 Months" group): 36.6834, 48.8841, 28.951, 31.8575, and 20.6718.

<sup>b</sup>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.

<sup>c</sup>Forty-nine sample members are missing a release date, and four sample members are missing a Social Security number; they therefore could not be matched to UI data.

The Enhanced Services for the Hard-to-Employ Demonstration

Appendix Table E.3

Year 1 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								H-Stars <sup>f</sup>
	Less Than 3 Months				More Than 3 Months				
	Program Group	Control Group	Difference (Impact)	P-Value	Program Group	Control Group	Difference (Impact)	P-Value	
Arrested <sup>a</sup> (%)	20.8	24.2	-3.4	0.429	24.0	22.1	1.9	0.612	
Convicted of a crime <sup>b</sup> (%)	12.6	18.4	-5.7	0.124	18.1	17.3	0.7	0.828	
Convicted of a felony	1.4	4.8	-3.4 *	0.051	1.3	2.3	-1.1	0.366	
Convicted of a misdemeanor	9.5	11.9	-2.4	0.461	14.2	12.5	1.7	0.578	
Convicted of a violent crime <sup>c</sup> (%)	2.5	1.3	1.2	0.432	1.6	1.5	0.1	0.937	
Incarcerated (%)	32.5	42.2	-9.8 *	0.053	36.6	39.4	-2.9	0.496	
Prison	9.4	19.5	-10.1 ***	0.005	12.8	11.6	1.3	0.658	**
Jail	32.0	42.3	-10.3 **	0.041	36.6	39.1	-2.5	0.552	
Incarcerated for a new crime <sup>d</sup> (%)	7.4	10.9	-3.5	0.240	7.6	6.7	0.9	0.704	
Prison	0.5	4.9	-4.4 ***	0.005	1.1	2.1	-1.0	0.365	*
Jail	6.9	6.0	0.9	0.734	6.5	4.6	1.9	0.353	
Incarcerated for a technical parole violation (%)	17.1	22.0	-4.9	0.255	20.8	23.8	-3.1	0.393	
Prison	7.2	12.1	-4.9	0.119	9.1	8.6	0.5	0.840	
Jail	15.3	20.6	-5.3	0.204	19.7	21.7	-1.9	0.577	
Total days incarcerated	39	54	-15 *	0.077	36	29	7	0.217	**
Prison	10	20	-10 **	0.036	14	9	4	0.184	**
Jail	28	34	-5	0.390	23	20	3	0.484	
Arrested, convicted, or incarcerated (%)	34.6	46.7	-12.2 **	0.018	42.6	45.8	-3.3	0.454	
Sample size (total = 929) <sup>e</sup>	225	160			311	233			

(continued)



### Appendix Table E.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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>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.

<sup>b</sup>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.

<sup>c</sup>Violent crimes are based on conviction charges defined by Langan and Levin (2002). See Appendix C for charges defined as violent.

<sup>d</sup>Includes all reasons for incarceration, such as sentences for new crimes, technical violations of parole, detainee (jail), and other reasons. Therefore, incarcerations for new crimes and parole violations do not sum to percentage incarcerated. See Appendix Table C.1 for more details about reasons for incarceration.

<sup>e</sup>A total of 48 sample members are missing the last prison release date and are therefore missing from all outcomes in this table.

<sup>f</sup>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.

The Enhanced Services for the Hard-to-Employ Demonstration

Appendix Table E.4

Year 2 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								H-Stars <sup>f</sup>
	Less Than 3 Months				More Than 3 Months				
	Program Group	Control Group	Difference (Impact)	P-Value	Program Group	Control Group	Difference (Impact)	P-Value	
Arrested <sup>a</sup> (%)	25.8	33.7	-7.8	0.109	20.0	24.5	-4.5	0.222	
Convicted of a crime <sup>b</sup> (%)	23.1	29.6	-6.5	0.170	18.5	25.0	-6.5 *	0.072	
Convicted of a felony	11.3	4.2	7.1 **	0.020	1.6	4.5	-2.9 **	0.048	***
Convicted of a misdemeanor	13.2	25.5	-12.3 ***	0.003	16.3	19.9	-3.7	0.280	
Convicted of a violent crime <sup>c</sup> (%)	1.7	7.0	-5.3 **	0.015	3.0	2.9	0.1	0.951	**
Admitted to prison or jail <sup>d</sup> (%)	33.4	37.7	-4.3	0.399	28.2	29.9	-1.7	0.660	
Prison	21.1	19.0	2.2	0.616	15.8	16.4	-0.6	0.846	
Jail	32.9	37.5	-4.6	0.367	27.7	27.1	0.6	0.870	
Incarcerated for a new crime (%)	11.3	10.6	0.7	0.845	7.6	10.7	-3.1	0.216	
Prison	6.6	3.2	3.4	0.164	1.4	4.2	-2.8 *	0.051	**
Jail	4.6	7.4	-2.7	0.282	6.4	6.5	-0.1	0.954	
Incarcerated for a technical parole violation (%)	19.8	18.7	1.1	0.790	18.9	13.2	5.7 *	0.077	
Prison	12.7	11.3	1.5	0.681	11.7	7.8	3.9	0.124	
Jail	15.1	13.5	1.6	0.671	15.3	8.2	7.2 **	0.013	
Total days incarcerated	80	92	-12	0.356	57	54	2	0.777	
Prison	41	56	-15	0.132	33	31	2	0.793	
Jail	39	36	3	0.681	24	23	1	0.879	
Arrested, convicted, or incarcerated (%)	50.3	58.8	-8.5 *	0.099	45.9	49.2	-3.2	0.458	
Sample size (total = 929) <sup>e</sup>	225	160			311	233			

(continued)

### Appendix Table E.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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>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.

<sup>b</sup>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.

<sup>c</sup>Violent crimes are based on conviction charges defined by Langan and Levin (2002). See Appendix C for charges defined as violent.

<sup>d</sup>Includes all reasons for incarceration, such as sentences for new crimes, technical violations of parole, detainee (jail), and other reasons. Therefore, incarcerations for new crimes and parole violations do not sum to percentage incarcerated. See Appendix Table C.1 for more details about reasons for incarceration.

<sup>e</sup>A total of 48 sample members are missing the last prison release date and are therefore missing from all outcomes in this table.

<sup>f</sup>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.

**The Enhanced Services for the Hard-to-Employ Demonstration**

**Appendix Table E.5**

**Two-Year Impacts on Employment, by Age**

**Center for Employment Opportunities**

Outcome (%)	Age at Baseline								H-Stars <sup>b</sup>
	Age 29 or Younger				Age 30 or Older				
	Program Group	Control Group	Difference (Impact)	P-Value	Program Group	Control Group	Difference (Impact)	P-Value <sup>a</sup>	
CEO transitional employment									
Year 1 (Q1-Q4)	63.9	2.8	61.1 ***	0.000	73.8	1.9	71.9 ***	0.000	**
Year 2 (Q5-Q8)	6.1	2.2	3.8 *	0.069	6.5	0.8	5.7 ***	0.001	
Years 1-2 (Q1-Q8)	64.1	4.8	59.3 ***	0.000	74.0	2.4	71.6 ***	0.000	**
Total UI covered employment									
Year 1 (Q1-Q4)	80.3	59.5	20.8 ***	0.000	82.6	55.6	27.1 ***	0.000	
Year 2 (Q5-Q8)	47.1	53.2	-6.0	0.237	54.3	47.5	6.9	0.103	*
Years 1-2 (Q1-Q8)	82.8	71.9	10.9 ***	0.008	87.4	67.1	20.3 ***	0.000	*
Sample size (total = 972) <sup>c</sup>	227	167			336	242			

SOURCES: MDRC calculations from the National Directory of New Hires (NDNH) database and unemployment insurance (UI) wage records from New York State and payroll data from CEO's Network for Information Gathering Evaluation and Learning (NIGEL) database.

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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>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 on the table beginning with the "Age 29 or Younger" group): 3.9295, 4.0419, 4.4634, 2.9884, 3.0148, 3.7557, and 3.4211.

<sup>b</sup>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.

<sup>c</sup>Five sample members are missing an age and therefore could not be included in the analysis.

The Enhanced Services for the Hard-to-Employ Demonstration

Appendix Table E.6

Two-Year Impacts on Recidivism, by Age  
Center for Employment Opportunities

	Age at Baseline								H-Stars <sup>f</sup>
	Age 29 or Younger				Age 30 or Older				
	Program Group	Control Group	Difference (Impact)	P-Value	Program Group	Control Group	Difference (Impact)	P-Value	
Arrested <sup>a</sup> (%)	35.6	36.5	-0.9	0.853	39.7	45.2	-5.5	0.186	
Convicted of a crime <sup>b</sup> (%)	28.2	31.4	-3.2	0.494	32.8	42.5	-9.7 **	0.017	
Convicted of a felony	9.2	7.5	1.7	0.565	5.2	7.4	-2.2	0.306	
Convicted of a misdemeanor	17.6	22.9	-5.3	0.196	27.0	33.3	-6.3	0.107	
Convicted of a violent crime <sup>c</sup> (%)	5.7	4.4	1.3	0.559	3.4	6.9	-3.5 *	0.067	
Incarcerated <sup>d</sup> (%)	46.6	55.4	-8.8 *	0.078	52.6	55.0	-2.4	0.566	
Prison	22.3	30.3	-8.0 *	0.073	27.9	25.4	2.5	0.511	*
Jail	46.2	52.3	-6.2	0.216	52.4	53.7	-1.3	0.758	
Incarcerated for a new crime (%)	10.2	15.6	-5.4	0.114	16.9	16.6	0.4	0.907	
Prison	4.5	7.3	-2.7	0.241	4.2	5.9	-1.7	0.374	
Jail	5.7	8.4	-2.7	0.312	13.4	11.9	1.5	0.605	
Incarcerated for a technical parole violation (%)	28.2	31.4	-3.2	0.491	34.5	27.3	7.3 *	0.065	*
Prison	11.0	12.8	-1.8	0.587	22.3	17.1	5.2	0.135	
Jail	24.1	26.8	-2.7	0.535	32.8	24.9	8.0 **	0.040	*
Total days incarcerated	103	109	-6	0.711	100	104	-4	0.749	
Prison	49	61	-12	0.311	44	48	-3	0.714	
Jail	54	48	6	0.510	55	56	-1	0.900	
Arrested, convicted, or incarcerated (%)	55.0	62.1	-7.0	0.165	57.4	62.5	-5.1	0.206	
Sample size (total = 976) <sup>e</sup>	230	167			337	242			

(continued)

### Appendix Table E.6 (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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>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.

<sup>b</sup>A total of 23 convictions were found to be associated with an arrest that occurred prior to random assignment. The total includes convictions for "other" reasons, felonies, and misdemeanor crimes.

<sup>c</sup>Violent crimes as based on conviction charges defined by Langan and Levin (2002). See Appendix C for charges defined as violent.

<sup>d</sup>Includes all reasons for incarceration, such as sentences for new crimes, technical violations of parole, detainee (jail), and other reasons. Therefore, incarcerations for new crimes and parole violations do not sum to percentage incarcerated. See Appendix Table C.1 for more details about reasons for incarceration.

<sup>e</sup>Five sample members are missing an age and therefore could not be included in the analysis.

<sup>f</sup>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.

**The Enhanced Services for the Hard-to-Employ Demonstration**

**Appendix Table E.7**

**Impacts on Employment, by Educational Attainment**

**Center for Employment Opportunities**

Outcome (%)	Has High School Diploma				Does Not Have High School Diploma				H-Stars <sup>b</sup>
	Program Group	Control Group	Difference (Impact)	P-Value	Program Group	Control Group	Difference (Impact)	P-Value <sup>a</sup>	
CEO transitional employment									
Year 1 (Q1-Q4)	71.6	4.4	67.2 ***	0.000	65.9	0.8	65.1 ***	0.000	
Year 2 (Q5-Q8)	4.8	1.2	3.7 **	0.021	7.6	2.3	5.3 **	0.027	
Years 1-2 (Q1-Q8)	71.6	4.8	66.8 ***	0.000	66.4	3.0	63.3 ***	0.000	
Total UI covered employment									
Year 1 (Q1-Q4)	85.5	60.8	24.7 ***	0.000	75.9	55.6	20.3 ***	0.000	
Year 2 (Q5-Q8)	58.2	52.4	5.8	0.196	44.8	49.0	-4.2	0.411	
Years 1-2 (Q1-Q8)	89.7	73.4	16.3 ***	0.000	79.1	66.5	12.6 ***	0.007	
Sample size (total = 889) <sup>c</sup>	292	221			221	155			

SOURCES: MDRC calculations from the National Directory of New Hires (NDNH) database and unemployment insurance (UI) wage records from New York State and payroll data from CEO's Network for Information Gathering Evaluation and Learning (NIGEL) database.

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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>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 on the table beginning with the "Has High School Diploma" group): 3.3375, 3.3627, 3.7328, 2.7505, 3.7221, 3.8887, and 2.9728.

<sup>b</sup>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.

<sup>c</sup>Eighty-four sample members are missing data for educational attainment, and four sample members are missing a Social Security number; they therefore could not be matched to UI data.

**The Enhanced Services for the Hard-to-Employ Demonstration**  
**Appendix Table E.8**  
**Two-Year Impacts on Recidivism, by Educational Attainment**  
**Center for Employment Opportunities**

	Does Not Have High School Diploma				Has High School Diploma				H-Stars <sup>f</sup>
	Program	Control	Difference	P-Value	Program	Control	Difference	P-Value	
	Group	Group	(Impact)		Group	Group	(Impact)		
Arrested <sup>a</sup> (%)	33.9	42.0	-8.1	0.108	39.0	39.7	-0.7	0.873	
Convicted of a crime <sup>b</sup> (%)	29.0	38.0	-9.0 *	0.069	31.0	36.2	-5.2	0.209	
Convicted of a felony	4.6	8.9	-4.3 *	0.093	7.7	6.6	1.1	0.643	
Convicted of a misdemeanor	21.0	29.6	-8.5 *	0.059	23.2	27.8	-4.6	0.229	
Convicted of a violent crime <sup>c</sup> (%)	4.8	6.0	-1.2	0.624	4.1	5.1	-1.1	0.586	
Incarcerated <sup>d</sup> (%)	53.5	58.6	-5.1	0.316	44.8	51.1	-6.2	0.155	
Prison	23.2	29.3	-6.1	0.188	26.3	26.2	0.0	0.991	
Jail	52.9	56.5	-3.6	0.485	44.4	49.1	-4.6	0.290	
Ever incarcerated for a new crime (%)	12.3	20.1	-7.9 **	0.041	13.2	14.0	-0.9	0.778	
Prison	2.7	8.9	-6.1 ***	0.010	4.7	5.6	-0.9	0.644	*
Jail	10.0	12.2	-2.2	0.501	8.9	9.2	-0.3	0.899	
Incarcerated for a technical parole violation (%)	34.6	27.1	7.5	0.127	28.7	26.9	1.9	0.642	
Prison	14.5	15.5	-1.0	0.789	19.3	15.1	4.2	0.205	
Jail	32.3	22.6	9.7 **	0.041	25.6	23.7	1.9	0.629	
Total days incarcerated	94	113	-19	0.227	99	97	1	0.924	
Prison	46	59	-12	0.277	43	49	-6	0.543	
Jail	48	55	-7	0.418	55	48	7	0.396	
Arrested, convicted, or incarcerated (%)	58.0	63.4	-5.4	0.291	52.8	59.5	-6.6	0.134	
Sample size (total = 893) <sup>e</sup>	223	155			294	221			

(continued)



### Appendix Table E.8 (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. The significance level indicates the probability that one would incorrectly conclude that a difference exists between research groups for the corresponding variable.

<sup>a</sup>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.

<sup>b</sup>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.

<sup>c</sup>Violent crimes are based on conviction charges defined by Langan and Levin (2002). See Appendix C for charges defined as violent.

<sup>d</sup>Includes all reasons for incarceration, such as sentences for new crimes, technical violations of parole, detainee (jail), and other reasons. Therefore, incarcerations for new crimes and parole violations do not sum to percentage incarcerated. See Appendix Table C.1 for more details about reasons for incarceration.

<sup>e</sup>Eighty-four sample members are missing educational attainment; they therefore could not be included in the analysis.

<sup>f</sup>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.

**Appendix F**

**Sensitivity Tests for the Estimated Net Cost of CEO**



**The Enhanced Services for the Hard-to-Employ Demonstration**  
**Appendix Table F.1**  
**Sensitivity Analysis of Estimated Net Costs of CEO,**  
**Assuming That 35 Percent of the Control Group Worked a Transitional Job**  
**Center for Employment Opportunities**

Cost Component	Gross Cost per Program Group Member (\$)	Gross Cost per Control Group Member (\$)	Net Cost per Program Group Member (\$)
<b><u>CEO services</u></b>			
Recruitment/intake	160	160	0
Expenses for transitional jobs			
Transitional job wages	887	0	887
Transitional job fringe benefits <sup>a</sup>	148	0	148
Field/site supervision	1,326	0	1,326
Expenses for other services			
Job coaching/life skills	559	383	177
Job development	489	0	489
Bonding	5	0	5
Fatherhood component	258	0	258
Supportive services	70	0	70
MIS and payroll	362	0	362
<b>Total CEO costs</b>	<b>4,263</b>	<b>543</b>	<b>3,720</b>
<b><u>Services obtained outside CEO</u></b>			
Expenses for transitional jobs <sup>b</sup>			
Transitional job wage	0	433	-433
Transitional job fringe benefits	0	72	-72
Field/site supervision	0	464	-464
Expenses for other services			
Employment/job search	158	322	-164
Adult Basic Education/GED	6	5	1
College courses	212	126	86
Vocational training	641	641	0
<b>Total non-CEO costs</b>	<b>1,017</b>	<b>2,063</b>	<b>-1,046</b>
<b>Total CEO and non-CEO costs</b>	<b>5,280</b>	<b>2,605</b>	<b>2,674</b>

SOURCES: MDRC calculations from CEO's financial expenditure reports for Fiscal Year 2005 and CEO's Network for Information Gathering Evaluation and Learning (NIGEL) database.

NOTES:

<sup>a</sup>CEO incurs fringe benefit costs of 16.7 percent of day labor wages resulting from employer-paid payroll taxes.

<sup>b</sup>Control group costs represent transitional job participation reported by 35 percent of control group respondents on the client survey. The main analysis in Chapter 7, Table 7.2, assumes that 16.2 percent of the control group worked in a transitional job.



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## About MDRC

MDRC is a nonprofit, nonpartisan social and education policy research organization dedicated to learning what works to improve the well-being of low-income people. Through its research and the active communication of its findings, MDRC seeks to enhance the effectiveness of social and education policies and programs.

Founded in 1974 and located in New York City and Oakland, California, MDRC is best known for mounting rigorous, large-scale, real-world tests of new and existing policies and programs. Its projects are a mix of demonstrations (field tests of promising new program approaches) and evaluations of ongoing government and community initiatives. MDRC's staff bring an unusual combination of research and organizational experience to their work, providing expertise on the latest in qualitative and quantitative methods and on program design, development, implementation, and management. MDRC seeks to learn not just whether a program is effective but also how and why the program's effects occur. In addition, it tries to place each project's findings in the broader context of related research — in order to build knowledge about what works across the social and education policy fields. MDRC's findings, lessons, and best practices are proactively shared with a broad audience in the policy and practitioner community as well as with the general public and the media.

Over the years, MDRC has brought its unique approach to an ever-growing range of policy areas and target populations. Once known primarily for evaluations of state welfare-to-work programs, today MDRC is also studying public school reforms, employment programs for ex-offenders and people with disabilities, and programs to help low-income students succeed in college. MDRC's projects are organized into five areas:

- Promoting Family Well-Being and Children's Development
- Improving Public Education
- Raising Academic Achievement and Persistence in College
- Supporting Low-Wage Workers and Communities
- Overcoming Barriers to Employment

Working in almost every state, all of the nation's largest cities, and Canada and the United Kingdom, MDRC conducts its projects in partnership with national, state, and local governments, public school systems, community organizations, and numerous private philanthropies.

