Evaluation of the Re-Integration of Ex-Offenders (RExO) Program: Final Impact Report

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EXECUTIVE SUMMARY

The Reintegration of Ex-Offenders (RExO) project began in 2005 as a joint initiative of the Department of Labor, Employment and Training Administration (ETA), the Department of Justice (DOJ), and several other federal agencies. RExO aimed to capitalize on the strengths of faith-based and community organizations (FBCOs) and their ability to serve prisoners seeking to re-enter their communities following the completion of their sentences. In June 2009, ETA contracted with Social Policy Research Associates (SPR) and its subcontractors MDRC and NORC at the University of Chicago to conduct an impact evaluation of 24 RExO grantees.

The programs funded under RExO primarily provided three main types of services: mentoring, which most often took the form of group mentoring, but also included one-on-one mentoring and other activities; employment services, including work readiness training, job training, job placement, job clubs, transitional employment, and post-placement follow-up; and case management and supportive services.

Upon enrollment, a participant was typically placed in work readiness training, which ranged from only a few hours to more than 24 hours in total duration. Toward the latter part of this training, or immediately following it, a participant was typically either matched with an individual mentor or asked to participate in group mentoring activities. Surrounding these activities were regular meetings with a case manager (at least bi-weekly, and most often weekly), during which the participant's service needs were discussed, and referrals were made for any needed services. Additionally, a participant discussed potential job leads with his or her case manager (or with a job placement specialist or job developer, in a minority of cases). Although the average duration of participation in RExO was approximately twelve weeks, this varied widely across participants, and the period of intensive participation was often much shorter. Similarly, the actual services participants received varied substantially across grantees.

This report summarizes the impacts of the RExO program on offender outcomes in the two main areas of interest: labor market success and recidivism. Using a random assignment (RA) design, the evaluation created two essentially equivalent groups: a program group that was eligible to enroll in RExO and a control group that was prevented from enrolling in RExO but could enroll

in other services that were available within the community (and to which program participants also had access). A total of 4,655 participants enrolled in the study, with approximately 60 percent (N=2,804) of those being assigned to the program group and 40 percent (N=1,851) assigned to the control group.

The results in this final report are based on outcomes for these individuals in the three-year period after they enrolled into the study, with outcome measures obtained from three different data sources. The first of these was a telephone survey that asked about a range of items, but focused on labor market outcomes and recidivism. The overall response rate to this survey after three years was 64.2 percent, which represented 83.6 percent of those who had responded to a two-year survey. The second set of data used in this report was administrative data on criminal justice outcomes which were sought from each of the 18 states in which RExO grantees operated. The final data source was employment and earnings information collected from the National Directory of New Hires (NDNH), which provided objective and uniform data on employment for all study participants.

The key findings of the report are as follows:

- **There is no evidence of positive impacts of RExO on recidivism outcomes.** Across an array of measures and model specifications, analyses of administrative data on recidivism finds no evidence of impacts of RExO. Analysis of self-reported recidivism obtained from responses to the survey finds evidence of a single positive impact (on the probability of re-arrest within the three-year follow up period). However, a subsequent analysis that links the administrative data with the survey data suggests that this finding results from some form of bias in reporting on the part of program group members.

- **There is no evidence of impacts of RExO on labor market outcomes.** As with the analysis of administrative criminal justice data, no evidence of impacts was observed in the analysis of administrative employment data. Again, this was true across several outcome measures and model specifications. Also mirroring the criminal justice results, isolated positive impacts were detected in the analysis of self-reported labor market outcomes (employment probability and a survival analysis of time to first job). Unlike administrative data on recidivism, one condition in accessing the NDNH data precludes linking those data to the survey data; as a result, the possibility that true impacts were detected in the survey data analysis cannot be ruled out. However, these findings are not supported by analyses of the administrative data, and support for this hypothesis is further weakened by the finding of program group bias in reporting criminal justice outcomes.

- **There was no clear evidence that RExO had differential impacts for different subgroups.** This report examined RExO's impacts on subgroups defined by age, gender, education, number of prior convictions, time between release and enrollment in the study, date of enrollment into the study, whether the program

conducted more or less intensive screening, and whether the program focused on a work-first approach or on first providing a broader range of services before assisting participants in finding employment. Analysis of differential impacts indicated that there is no evidence of consistent differences in impacts within or across these subgroups.

- **The variety of services provided by grantees means that this is not an assessment of a single program.** Given that the services offered by grantees and their partners varied substantially, and that the point of RA varied across grantees as well, what constitutes the "program" differed from site to site. While the impact analysis examined whether effects varied by the intensity of grantee screening, or whether grantees used a "work-first" model, there were a number of other variations that could not be included in the analysis that may have led to the overall results.

- **The participants in this study—including both program and control group members—are not representative of the "average" offender returning from prison or jail.** Average recidivism among the program and control groups (as measured by arrests, convictions, and incarceration) is noticeably lower—and rates of employment are somewhat higher—than national averages for offenders recently released from state or federal prisons. This may be partially the result of the locations in which the grantees operated, but almost certainly also reflects the screening and eligibility criteria implemented by the grantees, which led to the study sample likely being more motivated or able to succeed than the "average" offender.

- **RExO grantees were providing a wide array of services to their clients, but the services may not have been of sufficient duration or intensity to impact the key outcomes.** The relatively short-term nature of the services provided may have been insufficient to produce meaningful impacts on labor market and recidivism outcomes measured over multiple years.

- **This evaluation may not provide a strong test of whether employment-based programs lower one's likelihood of recidivating.** Given that this evaluation presents no evidence that RExO had an impact on employment, the study does not provide a strong assessment of whether programs that actually increase employment affect recidivism. One can expect recidivism to be affected by employment-based programs only if those programs actually increase employment rates. Thus, a full test of the impact of employment-based programs on offender recidivism may require evaluation of a program that generates clear impacts on employment.

- **It is possible that additional services are needed for programs serving offenders to meet the many needs of their participants.** Most programs provided work readiness training, mentoring, and case management and supportive services. But offenders reported an array of additional challenges that may require services not frequently provided under RExO. It is possible that providing additional resources, including housing services, would have yielded evidence of impacts on employment and/or recidivism.

Taken together, these findings present a disappointing picture of the impact of RExO. Overall, although RExO participants reported receiving more services than did control group members, there is no evidence this translated into any impacts on recidivism. Further, there was limited evidence of impacts on employment, and the evidence that did exist was exclusively in the self-report survey data and was not supported by administrative data. Additionally, the self-reported impacts, while statistically significant, are relatively small in practical terms.

One possible reason for the lack of impacts discussed in this report is that RExO grantees may not have had sufficient resources to meet the many needs of their participants. While most programs provided work readiness training, mentoring, and case management and supportive services, these may have been insufficient to help participants deal with drug abuse, alcoholism, physical health problems, and other common challenges, including substantial child support requirements, that likely posed serious barriers to employment and the attainment of other positive outcomes. Thus, the findings may suggest the need for a more comprehensive and intensive approach that helps address the wide array of other issues present in the ex-offender population during the period immediately following release.

Additionally, it is possible that the variation in program services, and in the contrast in the level of services received by program group members, as compared to control group members, may have limited the likelihood of finding impacts of the RExO program. It is possible that if the study had focused on RExO grantees that were providing the most intensive services, or were in areas that did not have substantial alternative services available, the findings presented here would reflect more positive conclusions about the program.

I.   INTRODUCTION

The Reintegration of Ex-Offenders (RExO) program began in 2005 as a joint initiative of the Department of Labor, Employment and Training Administration (ETA), the Department of Justice (DOJ), and several other federal agencies.[1] RExO was intended to aid communities heavily affected by the challenges associated with high numbers of prisoners seeking to re-enter their communities following completion of their sentences. It did so by funding employment-focused programs that provided mentoring and supportive services to offenders returning to their communities.

Five rounds, or generations, of RExO funding were awarded, totaling more than $98 million in grants to agencies implementing the program.[2] Generation I RExO funding was awarded in 2006 to 30 organizations across the country for a two-year period. Following this, 24 of these grantees were given subsequent funding to continue operating RExO for three additional years, through March 2011. In June 2009, ETA contracted with Social Policy Research Associates (SPR) and its subcontractors, MDRC and NORC at the University of Chicago, to conduct a random assignment (RA) impact evaluation of these 24 RExO programs. This evaluation included two reports on the impacts of the program. An initial report on the impacts in the two years after study participants were randomly assigned—referred to hereafter as the Two-Year Impact Report —has already been published.[3] The present report focuses on the impacts of the program in the three years after study participants were randomly assigned.[4] This introductory chapter has five

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[1]   Initially, it was known as the Prisoner Reentry Initiative (PRI), but was renamed RExO under the Obama administration.

[2]   Additional rounds of funding have been made under the RExO funding stream, though these have variously been known by other names, such as High-Poverty High-Crime and Face Forward, among others.

[3]   Wiegand, Sussell, Valentine, and Henderson (2015)

[4]   Administrative labor outcomes were available for the fourth year following RA for some study participants; these are also assessed in this report.

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roles.[5] First, it provides an overview of the challenges faced by ex-offenders re-entering their communities and a synopsis of the research on the effectiveness of employment programs in helping ex-offenders avoid returning to prison. Second, it outlines the evaluation and its methodology, and describes the study participants and their characteristics. Third, it provides a descriptive summary of the data on which the findings detailed in this report are based. Fourth, the chapter describes the analytic methods used to examine the impacts of RExO as presented in the report. Finally, it provides a summary of findings from the Two-Year Impact Report.

Ex-Offender Re-entry into Society

Since the mid-1970s, there has been an explosion in U.S. incarceration rates, with the result being that the United States now incarcerates nearly 500 of every 100,000 residents.[6] This is roughly four times the rate of the next highest country among peers of the United States, and more than five-and-one-half times the median of those peers.[7] In absolute terms, more than 1.5 million people were incarcerated in state and federal prisons in 2012, and more than 637,000 were released. The total number of people who were confined in the adult criminal justice system during 2012 rises to approximately 2.3 million if one includes those incarcerated in local jails.[8] Nearly all of the growth in the incarceration rate has been driven by changes that increase the likelihood that an offender receives a prison sentence, rather than by any actual increase in crime or improved policing.[9] Regardless of the reason, however, the end result is that large numbers of individuals in the United States either are or have been imprisoned, and large numbers of prisoners are released each year.

Once released, ex-offenders face daunting obstacles to successful re-entry, including difficulties with finding jobs, housing, and services for substance abuse or mental health problems; huge child support arrears; and challenges in reintegrating into their families. Moreover, they are concentrated in a relatively small number of urban neighborhoods that experience high rates of poverty and other social problems. Given these challenges, it is not surprising that rates of recidivism are very high. The most recent national statistics show that more than two-thirds of ex-offenders are rearrested and nearly half are reincarcerated within three years of release, most

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[5] The contents of this chapter are drawn heavily from Chapter I of the Two-Year Impact Report.

[6] Raphael and Stoll (2013). The number of prisoners per 100,000 hovered around 100 between 1925 and 1975. After 1975, the rate increased dramatically, reaching its peak of more than 500 per 100,000 in 2006.

[7] Raphael (2014). "Peer countries" refers to Canada, Mexico, and the 15 original members of the European Union.

[8] Carson and Golinelli (2013)

[9] Raphael (2014)

commonly for violations of parole conditions or drug possession.[10] Viewed in this context, efforts aimed at reducing recidivism are critical.

Although the relationship between crime and work is complex, it seems feasible that securing employment, and thus the ability to earn income, is important for a successful transition from prison to the community. However, finding and keeping employment is difficult for many ex-offenders. Aside from the potential stigma caused by their prison sentences, a large proportion of ex-offenders also faced substantial employment barriers prior to their sentences due to low levels of educational attainment, poor performance in what schooling they did complete, limited prior work experience, health problems, and personal characteristics (such as substance abuse issues) that are not viewed favorably by employers.[11] While prior research has provided mixed results, it is clear that for most individuals, prison worsens labor market prospects that were already poor prior to incarceration.[12]

Unfortunately, there is little reliable evidence about whether employment reduces recidivism or which types of employment services, if any, are effective for ex-offenders. Despite a long history of research in the criminal justice field, there have been very few rigorous studies of employment-focused re-entry models.[13] However, the flurry of interest in re-entry during the past five to ten years, likely triggered by the surge in prison populations described above, has spurred several recent non-experimental studies. Among these is the Serious and Violent Offender Reentry Initiative evaluation, which found modest improvements in outcomes for adult program recipients but no differences among youth participants.[14] These studies have produced very useful findings, but their non-experimental nature leaves important questions. Because most experts agree that personal motivation is a key factor in explaining why some ex-offenders end up back in prison and others do not, there is some concern that ex-offenders who choose to participate in programs may be different from those who do not, and it is very difficult to measure or control for motivation in a non-experimental evaluation.

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[10]   Durose, Cooper, and Snyder (2014)

[11]   Raphael (2014)

[12]   For a fuller discussion of this, see Wiegand et al. (2015).

[13]   Drake, Aos, and Miller (2009) conducted a thorough meta-analysis of all English-language evaluations of prisoner re-entry and crime-abatement programs, identifying 545 such evaluations. Of these, less than five percent were random assignment studies.

[14]   Lattimore and Visher (2009). Other non-experimental studies in recent years have examined Texas's Project RIO, San Diego's Second Chance program, Ready4Work, and others.

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A few recent experimental studies of employment-based programs serving offenders have been completed, however. In 2004, a random-assignment 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 ex-offenders, was initiated as part of the U.S. Department of Health and Human Services' Hard-to-Employ project. CEO provides transitional employment, in combination with a five-day pre-employment class, and other supportive services. Results from this study showed that CEO produced a large increase in employment over the first three quarters after random assignment, driven by the transitional jobs provided by the program, but virtually no evidence of a difference in employment after this point for the remainder of the three-year follow-up period. Despite this latter finding, there was evidence of a statistically significant decrease in several measures of recidivism, including an overall measure of whether the individuals were ever arrested, convicted, or incarcerated. Effect sizes were largest for those who were randomly assigned to the program within three months of their release from prison.[15]

Similarly, in 2006, the Joyce Foundation developed the Transitional Jobs Reentry Demonstration (TJRD), a four-site random assignment study of transitional jobs programs for recently released ex-offenders. Ex-offenders interested in participating in this project were randomly assigned either to a program or control group, and were followed for a two-year period after their entry into the study. Results from this experimental study were less promising. Much like the CEO study, there was evidence of a short-term increase in employment, driven by transitional jobs, but this effect had largely vanished by the end of a year. In contrast to the CEO evaluation, however, there was no evidence of impacts on multiple measures of recidivism during the two-year follow-up period.[16]

Current research findings on the effects of employment-based programs targeting ex-offenders are thus somewhat mixed. While the relatively recent quasi-experimental studies of employment-focused programs have suggested there are some modest gains in employment and reductions in recidivism for offenders, concerns about selection bias and differences in the levels of motivation between the program and comparison groups render these results uncertain. Recent experimental evaluations have found relatively little evidence of an effect on employment for former offenders, but in at least one case (CEO), the program did reduce recidivism among offenders, particularly those who had been released shortly before enrolling in the study. Both

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[15]   Redcross et al. (2012). There was also evidence that those randomly assigned within three months of release had better employment outcomes, even after the initial effect driven by transitional jobs. Because these effects did not appear until well after random assignment, however, it is unclear whether they might have been a direct effect of the program itself.

[16]   Jacobs (2012)

recent experimental studies focused on programs that utilized a transitional employment model, which is only one potential approach to increasing employment among hard-to-serve populations.[17] Thus, the evaluation of RExO provides a valuable new perspective on the ability of employment-focused programs to increase employment and earnings and decrease recidivism, not only because it examines the impacts of 24 additional programs, but also because RExO provided an employment-focused approach to serving offenders that did not utilize a transitional employment model.

Design of the Evaluation

The RExO evaluation measured the effects of program participation[18] on ex-offenders' employment, earnings, recidivism, and other outcomes using a RA design. RA establishes two equivalent groups—a program group and a control group—and enables the evaluation team to compare the outcomes of the members of the two groups and to estimate the impact of the program. Critically, the RA design is intended to eliminate the effect of unobserved factors, such as motivation. This evaluation was based on three primary research questions:

- What were the impacts of the RExO grantees' programs on ex-offenders' labor market and recidivism outcomes?

- What were the programs' impacts by key subgroups (e.g., those segregated by age, gender, educational attainment, criminal justice history prior to entering the study, etc.)?

- How did grantees implement the various aspects of RExO, including the provision of employment-centered services and mentoring?

Between January and December 2010,[19] approximately 60 percent of all eligible applicants were assigned to the program group and provided access to RExO services, while the remaining 40 percent were assigned to the control group. To be eligible to receive RExO services, each ex-offender had to meet the following requirements:

- be at least 18 years of age or older;

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[17] Additionally, the control groups in both the CEO and TJRD studies were assigned to a program that provided job readiness training and job search assistance. Thus, the treatment contrast in these studies was that program group members had access to transitional employment and control group members did not, but control group members did receive some level of employment services.

[18] Technically, the impact study assessed the effects of the *intent* to provide program services to participants, rather than program participation itself. For ease of presentation, however, the term "program participation" is used in this report.

[19] Two of the 24 grantees continued to enroll participants through January 2011 in an effort to increase their enrollments.

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- have been convicted as an adult and imprisoned pursuant to an Act of Congress or a state law;

- have been incarcerated for a minimum of 120 days;

- have enrolled in the RExO program within 180 days of release from a prison, jail, or a halfway house (though sites were allowed to enroll up to 10 percent of participants whose time after release exceeded 180 days);

- not have been convicted of a sex-related offense; and

- not have had a violent crime as her/his most recent offense.[20]

The members of the control group were prohibited from receiving RExO services during the intake period and for a period of 12 months following that time, but were able to seek out and receive any other services in their communities for which they were eligible (and to which program group members also had access).[21] This means that this study is a comparison not between RExO and a true no-treatment control group, but rather between RExO and whatever other services were available to and accessed by control group members.

A critical decision, both from a design standpoint and from the perspective of the grantees, was when in the release/re-entry cycle assignment to the program or control group would occur. All of the grantees had well-established intake and enrollment procedures and were justifiably concerned about how an RA process would affect these procedures or add burden to their workload.

At nearly all sites, established assessment and screening procedures were the key point of articulation with RA. These procedures were designed to ensure that potential participants (1) were eligible, (2) were suitable for the program, and, in some sites, (3) demonstrated a level of engagement or commitment to participating fully in the program. The level of intensity of these procedures varied substantially across sites, so that in some sites a potential participant needed only to meet the basic eligibility criteria and express interest in participating before being

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[20]   Initially, all RExO participants were required not to have been convicted of any violent offense in the past. During the intake period, however, ETA allowed grantees to enroll individuals who had been convicted of a violent offense, provided that their most recent offense was not violent. This change, which expanded the pool of eligible study participants, was intended to support grantees in meeting their target enrollments. As shown in Table I-2, however, the percentage of study participants who had violent offenses in the past remained very low, despite this change.

[21]   For all but a handful of control group participants, this 12-month ban on receiving services amounted to a lifetime ban, because only two of the 24 grantees in the study received subsequent RExO funding to continuously provide services beyond March 2011. (Several others have subsequently received funding to serve ex-offenders through other DOL grants.)

enrolled in the study, while in other sites potential participants underwent multiple assessments and were required to participate in multiday workshops before they were enrolled.

The existence of screening and assessment procedures raised a fundamental tension for grantees vis-à-vis RA, because they did not want to have to turn away potential clients (i.e., those assigned to the control group) after already having had significant face-to-face contact with them, but they also did not want to enroll clients who they believed were not appropriate for their programs. The first consideration suggested conducting RA earlier in the customer flow process, and the second suggested conducting RA later. Grantees and their partners ultimately expressed the greatest comfort at different points along this continuum.

The fact that these choices varied had important ramifications for the evaluation. First, it required the study team to develop different RA procedures to fit each grantee. Second, it could potentially affect the analysis, because it had implications for the percentage of program group members who would receive the full dose of RExO services. The earlier the point of RA, the more individuals who would be assigned to the program group and not end up receiving substantial services from the grantee (because they did not fully engage in the program). This could dilute any impacts of the program because the study design required that estimates of program effects be generated from outcomes averaged across all individuals who were randomized to the program group, not just those who actually went on to receive services.

Ultimately, grantees established three distinct types of RA procedures:

- **Pre-Release RA** *(Model 1)*. One of the 24 grantees opted to implement RA while potential participants were still incarcerated. Thus, many of its participants were assigned prior to release, and then needed to make contact with the grantee upon their release to receive program services.

- **Post-Release RA**. In the remaining sites, RExO staff members did not meet one-on-one with potential participants until after release, though they may have provided orientation sessions to groups of individuals who were still incarcerated. Grantees developed two different versions of this general approach:

  - **RA Concurrent with Intake** (*Model 2*). Fifteen grantees enrolled potential participants after an initial orientation to the program (which occurred pre- or post-release, depending on the grantee) and after determining eligibility. For this group of grantees, study intake procedures—informing potential participants about the study, securing their consent, and randomly assigning them—were designed to take place either at the intake and orientation meeting or shortly thereafter.

  - **RA After Screening** (*Model 3*). Eight sites enacted various screening procedures (such as assessments or required attendance at specific

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workshops) that potential participants had to undergo prior to being enrolled in the program. These activities and workshops were designed to assess participant commitment to and suitability for the program. These sites felt that the appropriate timing of RA was after some or all of these screening steps had occurred. Though they informed participants prior to screening activities that there was a possibility that they may not be enrolled in the program, intake procedures did not begin until after screening occurred and they had determined which candidates were suitable. Several of these grantees experienced difficulties early in the intake period with low numbers of enrollees, in part because they were screening out a substantial number of clients. Thus, over time, many of them relaxed their screening procedures in order to ensure they could enroll a sufficient number of participants into the study and their programs.[22]

Nearly all grantees adopted RA procedures that required potential participants to come to their offices at least once in order to learn about the program and the study and complete relevant paperwork. An important advantage of enabling grantees to have some contact with potential participants prior to the point of RA was that it was expected to increase the likelihood that a high percentage of the program group actually went on to enroll in the program. At the same time, the procedure was also expected to ensure that all potential participants received some service from the grantee. Especially for grantees that implemented Model 3, members of the control group received at least an assessment and, in a few cases, several days of a workshop or counseling. At these sites, then, members of the control group received at least a portion of the "treatment" itself.[23]

Study Participants

ETA established a recruitment target of 200 participants for each grantee. In an effort to balance the statistical power needs of the study with grantees' preference to serve more participants than were turned away, ETA and the evaluation team decided that 60 percent of participants (or 120 participants per grantee) would be assigned to the program group and 40 percent (80 participants per grantee) to the control group. Table I-1 displays the number of participants enrolled at each

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[22]   Such changes in screening procedures could affect the presence or size of impacts observed, because they may change the pool of participants entered into the study. A test of this possibility is described in subsequent chapters.

[23]   Many grantees viewed this tradeoff positively, because it meant that they were not fully denying service to anyone. In each case, care was taken to ensure that the program group would have access to more services.

site. Grantees enrolled a total of 4,655 individuals into the study; of these, 2,804 (60.2 percent) were assigned to the program group and 1,851 (39.8 percent) were assigned to the control

Table I-1:

Number of Participants at Each Grantee

Location	Grantee Name	Program	Control	Total
Baltimore, MD	Episcopal Community Services of Maryland	121	80	201
Baton Rouge, LA	Church United for Community Development	110	75	185
Boston, MA	Span, Inc.	111	72	183
Chicago, IL	Safer Foundation	68	44	112
Cincinnati, OH	Talbert House	125	83	208
Dallas, TX	Urban League of Greater Dallas & North Central Texas	123	81	204
Denver, CO	The Empowerment Program	131	86	217
Des Moines, IA	The Directors' Council	120	79	199
Egg Harbor, NJ	Career Opportunity Development	120	79	199
Fort Lauderdale, FL	OIC of Broward County	120	80	200
Fresno, CA	Fresno Career Development Institute	117	74	191
Hartford, CT	Community Partners in Action	109	70	179
Kansas City, MO	Connections to Success	89	59	148
New Orleans, LA	Odyssey House Louisiana	120	82	202
Philadelphia, PA	Connection Training Services	155	105	260
Phoenix, AZ	Arizona Women's Education and Employment, Inc.	120	79	199
Pontiac, MI	Oakland Livingston Human Services Agency	86	55	141
Portland, OR	SE Works	123	81	204
Sacramento, CA	Mexican American Addiction Program, Inc.	127	82	209
San Antonio, TX	Goodwill Industries	123	81	204
San Diego, CA	Metro United Methodist Urban Ministry	123	82	205
Seattle, WA	People of Color Against AIDS Network	119	77	196
St. Louis, MO	St. Patrick Center	119	80	199
Tucson, AZ	Primavera Foundation	125	85	210
	Total	**2,804**	**1,851**	**4,655**

SOURCE: Random assignment system data

NOTE: Each program was given a small number (no more than five) of wild cards—individuals who were not enrolled into the study but were automatically enrolled into the program. A total of 71 individuals were designated as wild cards.

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group.[24] The first participants were enrolled into the study in late January 2010, when one grantee began implementing RA. The remaining grantees implemented RA between February 1 and April 1, 2010. Grantees generally continued enrolling individuals into the study through the end of December 2010.[25]

The average number of study participants across grantees was 194. As can be seen in Table I-1, 12 of the grantees achieved their target of 200 participants, including three that exceeded this target by at least 10 participants. An additional six enrolled at least 190 participants. Only three grantees enrolled fewer than 150 participants, either because they implemented intensive screening procedures or because they struggled to recruit sufficient numbers of participants during the intake period.

Table I-2 displays the key baseline characteristics for both the program and control groups. There are a few minor differences in characteristics between the two groups. Specifically, a member of the control group was more likely to be between 25 and 34 years old at the time of RA, while a member of the program group was more likely to be between 45 and 54 years old. Further, members of the program group were somewhat less likely to be on parole than members of the control group, and somewhat more likely to be on some other form of supervision. Generally, however, the characteristics were similar between the two groups, which is the expected outcome when assignment to the groups is done randomly. These similarities provide some assurance that the program and control groups were essentially equivalent.

To provide further evidence for the equivalence between these groups, the evaluation team also employed logistic regression. This analysis regressed study group membership (i.e., program or control group) on each of the individual characteristics shown in Table I-2. None of the individual characteristics reached conventional levels of statistical significance, and an overall chi-square test of the regression model was also not statistically significant. Both of these findings suggest there is no meaningful difference between the program and control groups.

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[24]   The total number of individuals randomly assigned was 4,661. One additional person was randomly assigned to the program group, but was subsequently determined to be ineligible for the program. This individual was removed from the total numbers shown here. Additionally, five individuals, all members of the control group, asked to be removed from the study. Thus, the final sample for the study is 4,655.

[25]   Two grantees continued enrolling individuals into the study through January 2011, in an effort to reach their target of 200 participants. In contrast, one grantee ceased enrolling participants once it exceeded its target of 200 participants, so as not to turn any further program applicants away.

Table I-2:

Baseline Characteristics of the Program and Control Groups[1]

Characteristic	Program Group	Control Group
Age		
18–19	1.6	1.1
20–24	12.8	11.8
25–34	32.2	35.5**
35–44	29.0	29.4
45–54	21.1	18.3***
55+	3.4	3.8
Gender		
Male	80.6	81.7
Female	19.4	18.3
Race/Ethnicity		
White	33.1	32.1
Black	50.9	52.1
Asian	0.9	0.8
Hawaiian/Pacific Islander	0.6	0.3
Native American	2.8	2.3
No Race Recorded	13.1	13.7
Hispanic	17.9	17.2
Education		
8th Grade or Less	3.4	3.6
Some High School	42.6	43.7
High School Diploma/GED	42.2	41.5
Some College	9.9	9.0
College Graduate+	1.6	2.0
Post-Release Status		
Probation	28.2	27.5
Parole	49.7	52.6**
Other Form of Correctional Supervision	8.4	6.4***
None	13.7	13.6
Type of Institution		
Federal Prison	11.2	11.1
State Prison	67.5	68.1
County or City Jail	21.3	20.9
Other Characteristics		
Disability	6.2	5.7
Non-Violent Offender	93.5	93.1
Employed at Entry	3.4	3.7
Average Number of Months Since Most Recent Release	0.95	1.03

SOURCE: Random assignment system data

NOTE: Statistical significance levels are indicated as follows: *** = $p < .01$; ** = $p < .05$; * = $p < .1$.

[1] All figures shown are percentages, with the exception of the final row, which reflects an average.

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Data Collection

Four types of data were collected for this report: (1) qualitative data gathered from an implementation study; (2) administrative data on criminal justice outcomes of participants; (3) administrative data on employment and earnings through the National Directory of New Hires database; and (4) follow-up survey data to learn about the status of all study participants at both two and three years after study entry. These four data collection efforts are described below.

Implementation Study

Collection of data on the services provided by the 24 RExO grantees, as well as their implementation and structure, was a critical component of the evaluation. These data allowed the evaluation team to contextualize the impact results in three important ways. Specifically, they allowed the evaluators to:

- identify and compare the services provided to program group members and the limited services available to members of the control group;

- identify variations in the overall quality of services that might be expected to affect overall impacts of the program; and

- describe differences in the contextual factors at play in the communities in which the grantees were operating, including the differences in alternative services available to study participants across these communities.

During the intake period, the evaluation team visited each of the 24 grantee sites twice. The first of these visits occurred between April and June 2010, involved three days on site, and entailed learning about grantees' organizational structures, services, and partners, and the alternative services available in grantee communities. During the second round of visits, which occurred between September and December 2010, evaluation staff members spent two days on site. The first day focused on documenting any changes or modifications made to the program since the initial visit. The second day focused on a more involved documentation of alternative services available to offenders in each community, such as those from American Job Centers and other community-based organizations.

Data for the implementation study were obtained through four primary sources: (1) reviews of written program materials; (2) semi-structured interviews with staff members, administrators of grantee organizations and partner programs, representatives from alternatives to RExO within grantee communities, and employers; (3) on-site observations of grantee and partner program operations; and (4) group discussions with program participants and reviews of their case files. Anticipating that each grantee would have a different set of partners and different collaborative arrangements, this multipronged approach permitted flexibility in adapting data collection activities to circumstances and helped minimize the burden on grantees. Because evaluation

team members used previously developed discussion guides and checklists for each potential data collection activity, they obtained comparable information across all the sites, and across respondents within a given site. A summary of the key findings from the implementation study is included in Chapter II of this report.[26]

Administrative Data on Criminal Justice Outcomes/Events

Administrative data on criminal justice outcomes serve as the primary source of information on recidivism for study participants. These data were collected from agencies in the states in which the RExO programs operated. Because there are several ways to define recidivism, data were collected on a range of outcomes for each study participant, including arrests, convictions, and incarceration.

As will be described in Chapter IV, these data have been used to create a variety of recidivism measures, including whether an individual was arrested, convicted, or incarcerated following RA, the number of such events that occurred, the time it took until the first event (arrest, conviction, or incarceration), and the duration of time spent incarcerated since RA. Data were also collected for each study participant for the period before RA; these data were used to (1) describe the sample in terms of participants' criminal histories, (2) increase the precision of impact estimates by using these as covariates in the analysis, and (3) identify subgroups of participants with different criminal histories for analysis.

Not all states in which RExO programs were located provided the evaluation team all requested data. Thus, arrest and conviction data were obtained from the criminal history repositories in 16 of the 18 states in which RExO programs operated, covering 21 of the 24 RExO sites and 86.4 percent of all study participants. In addition, data on incarceration in state prisons were provided by the department of corrections in 16 of the 18 states, covering 21 of the 24 RExO sites[27] and 87.2 percent of all study participants.

The advantage of these data is that they provide a uniform and objective repository of criminal justice outcomes and, as such, provide the evaluation with information on the full population of study participants, notwithstanding the missing data described above. However, because criminal justice data were obtained only from the state in which an individual was randomly

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[26]   A more detailed summary of the findings from the implementation study can be found in Leshnick et al. (2012).

[27]   In addition, two grantees recruited heavily from their local jail population, and so jail admission records were obtained in two states.

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assigned, data on arrest, conviction, and incarceration in states outside of the one in which an individual was randomly assignment occurred were not available for the analysis. Given this fact, it is possible that the analysis of administrative data understates the overall level of recidivism by members of either or both the treatment and control groups. These data were first collected from state criminal justice agencies in the spring of 2011.[28] Subsequent extracts were collected in 2012 and in 2015.

Administrative Data on Employment and Earnings

Administrative data on employment and earnings were collected using data from the National Directory of New Hires (NDNH) and were meant to cover a three-year period following RA for each participant. These data include records from three separate files. The first of these is the new hire (or W-4) file, which records the date on which an individual was hired into a job covered by the NDNH, as well as the state in which the individual was hired. The second file is the quarterly wage file, which records quarterly earnings for individuals as well as the state in which those wages were earned. The third file includes unemployment insurance (UI) records, and identifies the state in which any UI claims were paid and the specific amount of the benefit paid.

The data in these files allows for an analysis of the impacts of the RExO program on whether an individual found employment following entry into the study, as well as their earnings in terms of both wages and income in the form of UI benefits. Additionally, because the new hire data provided a hiring date, they allowed for analysis of the time it took study participants to find employment after entering the study. As with the administrative data on criminal justice, the advantage of the NDNH data is that they provide a uniform and objective source of data on employment and earnings outcomes and, because they contain data from all states (in contrast to administrative criminal justice data), they provided the evaluation with information on covered employment, regardless of the state in which it occurred.[29]

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[28]   The initial extract was intended to provide data for participants prior to their enrollment into the study, in an effort to compare the program and control groups and identify potential subgroups for analysis. Because not all states provided data for each request, the number of files actually received from each state varied.

[29]   These data overcome two limitations associated with collecting UI data directly from states. First, because NDNH data include the new hire file, they can provide an exact hire date for individuals, thereby allowing for analysis of the time it took for participants to find employment. Second, NDNH quarterly wage data cover all states, which overcomes the limitation presented by use of state UI data in which outcomes are not measured for individuals who find employment in a state other than the one in which they enrolled in the study.

One limitation of the NDNH data is that they do not cover all employment—they omit those who are self-employed, or working "under the table." Another limitation is that NDNH data were not collected for the entire three-year period following random assignment. Due to delays in obtaining initial approval to collect the data, the earliest data available cover records beginning in July 2010, or a few months after the earliest enrollees entered the study. Additionally, there is a gap in the data between the fourth quarter of 2012 and the second quarter of 2013. Thus, the data were available for only some participants to examine impacts of employment in the second year after entry into the study, but were not available entirely for any participants for the third year after RA. The NDNH data were originally requested in November 2012. Due to delays in securing agreement to obtain the data, the initial data file was provided in March 2015. A second file was obtained in July 2015.

Follow-Up Survey

A follow-up survey was administered to study participants at two separate points: approximately two years after entrance into the study, and again approximately three years after entry.[30] The survey was primarily administered using computer-assisted telephone interviewing/personal interviewing (CATI/CAPI) technology.

The survey instrument was divided into nine substantive sections:

- Background[31]
- Current Housing Situation
- Current Employment
- Recidivism
- Services Received[32]
- Employment History
- Household Income
- Health and Substance Abuse
- Child Support

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[30] The initial survey had been scheduled to be administered 12 months following entry into the study. Because of substantial delays in getting approval for the survey, this period was changed to a two-year follow-up.

[31] Because these questions referred to information that was static by the time of respondents' entry into the study, they were asked only during the first wave of the survey.

[32] Information about these services was only collected during the initial wave of the survey.

The survey data suffer from some limitations. First, the fact that not all study participants responded introduces the possibility of non-response bias. Second, both recall error and a desire not to self-report on criminal justice activity that may be viewed negatively may have affected the results, particularly if one group (i.e., the program group) felt more obligated to report positive outcomes, whether because they received services through RExO or for some other reason.

Nevertheless, the survey did offer the advantage of enabling the evaluation team to use participants' responses to measure outcomes for which there were no readily available administrative data. For example, the survey provided the only means of measuring the number and types of services both program and control group members accessed following their entry into the study,[33] the overall household income of study participants, the health or substance abuse issues they have experienced, and any obligations they had for making child support payments.

Additionally, in cases in which administrative data were available, the survey allowed evaluation staff members to corroborate the findings from the analysis of those data. Unlike administrative records, survey-derived criminal justice outcomes cover activities that occurred in states other than the one in which an individual was randomly assigned. The survey also provided a useful measure of recidivism for respondents from states that did not or could not provide administrative data.

The survey effort attempted to reach all 4,655 study participants. Although not all respondents completed the survey exactly three years after they were enrolled into the study, each was asked about the three-year period following their enrollment. Ultimately, 2,995 participants completed the three-year survey, yielding a response rate of 64.3 percent.[34] There was slight variation between program and control group response rates; the response rate for program group members was 65.9 percent, and for control group members the rate was 62.7 percent.[35]

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[33] The RExO grantees did utilize a standardized management information system (MIS), which recorded some information about the services they provided to participants. However, there was substantial variation in the ways in which grantees used this MIS, and in the thoroughness of the data. Further, the MIS did not include any service information for control group members. As a result, these data do not provide reliable indicators of service receipt, and can be viewed only as rough estimates of services received for program group members.

[34] This total represents 83.6 percent of the 3,581 individuals who responded to the Two-Year survey.

[35] There was no difference between program and control group members with respect to the average amount of time between RA and the date they completed the survey.

Brief Overview of Analytic Methods

The primary statistical methods used in this report are straightforward. For each of the outcomes of interest, a mean has been calculated within the program group and within the control group, and the difference between these means has been calculated as well. Because the data come from a randomized trial, these differences provide an unbiased estimate of the treatment effect. To reduce the possibility of bias from survey non-response, analyses using outcomes measured by the survey data include a set of post-stratification weights.[36] These weights—which were derived from observable characteristics measured at the time of each participant's entry into the study—had the effect of making the sample more broadly representative of the original study population. To assess whether these differences are statistically meaningful, the fourth column in each table presents the probability values from tests of the hypothesis that these differences are equal to zero. For each of the analyses presented in this report, the probability values shown are those derived from models that include regression adjustment on pre-random assignment characteristics, which improves the precision of the estimates.

A number of additional statistical models were estimated as part of the analysis, including models that employed criminal history covariates and incorporated hierarchical analyses to account for the fact that study participants were "nested" within grantees. For analyses relying on survey data, unweighted models were also estimated. In general, these models provided very similar results and led to effectively the same conclusions as the simpler models described above. Thus, because they are more readily understood by a general audience, the chapters in this report present the results from the simpler models. The additional models, along with a detailed description of their calculation and meaning, are presented in the Technical Appendix to this report.

Finally, measures of the elapsed time to job acquisition and the elapsed time to first arrest are calculated in this report. While the tables display the mean values for the program and control groups, and the differences between them, the statistical analyses of the differences between these values are performed using survival analysis, which is a more appropriate method for analyzing this type of data. Although the results of the analyses are discussed in the chapters, the technique and the reasoning for using the technique are described in detail in the Technical Appendix.

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[36] The weights included an adjustment for non-response on two dimensions: grantee and study group (i.e., program or control). All other measured variables showed similar response rates across the categories and thus were not included in the post-stratification weights.

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Subgroup Analysis

In addition to exploring differences between the overall program and control groups, there may also be important differences in the program's impacts across different subgroups. In other words, the program may be more or less effective for some subgroups than it is for others. Given these possibilities, the report examines impacts for eight key subgroups in addition to analyzing impacts for the full sample. Four of these subgroups (defined by age, gender, education, and number of prior convictions) are based on demographic characteristics of the participants, two are based on the time at which the participants enrolled in the study, and the final two are based on specific programmatic choices made by the grantees operating RExO.

The first subgroup partition splits older and younger offenders (comparing those aged 27 and older to those younger than 27) because prior work has suggested that re-entry programs may be more effective for those age 27 years and older.[37] The second subgroup partition splits offenders by gender, because the criminal behavior of women and men often differs significantly, and prior studies have suggested the need for gender-appropriate re-entry programs.[38] The third subgroup partition compares impacts between three distinct groups: those without a high school diploma or GED, those with a GED, and those with a high school diploma or higher. This analysis was included because it seemed likely that RExO may have had differing impacts for those whose prior educational achievement made them more or less likely to find employment. The fourth subgroup analysis compared results for sample members with three or fewer prior convictions to those with four or more prior convictions, based on prior work suggesting that longer criminal histories predict recidivism (and therefore could potentially affect labor market outcomes).[39]

The first of the subgroup partitions based on the timing of participants' entrance into the study separated the sample between those randomly assigned within three months of release and those assigned following a longer interval. This reflects research that has shown that early access to program services may be an important factor for re-entry program effects.[40] The second subgroup analysis in this category compared impacts for those randomized prior to October 2010 to impacts for those randomized at a later date. This partition was chosen based on findings derived from the implementation study showing that grantees whose funding was expected to

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[37] Uggen and Staff (2001)

[38] Bloom, Owen, and Covington (2003)

[39] Visher (2003)

[40] Redcross et al. (2012)

expire by December 2010 were winding down and were therefore understaffed as the grant period was coming to a close.

Finally, as noted above, two other subgroup analyses were based on differential programmatic choices made by the grantees. The first divided the grantees into two groups based on whether they conducted extensive screening prior to enrollment (described as Model 3, above) or not (Models 1 and 2). The final subgroup analysis divided participants based on the programmatic emphasis of the grantees serving them. Specifically, two-thirds of the programs focused on stable employment as the immediate goal for ex-offenders, which meant that participants received work readiness training and job leads immediately after enrollment. The remaining one-third of the grantees focused on providing essential supportive services first, before participants were referred for jobs, which primarily meant that programs made sure that participants were stable in their housing situations and were able to pass a drug test before being referred for jobs.

Findings for the subgroup analyses must be interpreted cautiously, for two primary reasons. First, statistical power is lessened when a full sample is divided into subgroups, meaning that effects, even if they occur, are less likely to be detected. Second, the number of statistical tests performed overall increases with the number of subgroups analyzed, and making these multiple comparisons greatly increases the concern that spurious findings of statistically significant impacts will be found by chance, as is discussed below.[41] For these reasons, it is often most helpful to interpret the findings of subgroup analyses as exploratory, rather than confirmatory.[42] In the context of this study, this means that if analyses for a given subgroup show no evidence of effects—or, conversely, show consistently strong effects—across the different outcomes, this finding should be treated as the basis for a hypothesis for future investigation, rather than as a central finding.

Multiple Comparisons

There are many ways to measure the critical outcomes—such as employment, earnings, and recidivism—related to this study's key research questions. Thus, as with many evaluations of social programs, this report presents estimates of impacts for a large number of different outcomes. The simultaneous estimation of the effect of a program on several outcomes can lead to an increase in the probability of type I errors—i.e., concluding that the program had a

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[41] For a fuller discussion of this issue, see Schochet (2008).

[42] This approach is discussed in Bloom and Michalopoulos (2010).

significant effect on some outcome, when in fact it did not. This is because each individual comparison is subject to statistical uncertainty, and conducting multiple comparisons multiplies the likelihood that one will spuriously find a result that appears significant. One of the most preferred ways to address the multiple comparisons problem is to limit the number of outcomes and subgroups to be analyzed,[43] which this report does when examining labor market and recidivism outcomes.[44]

Findings from the Two-Year Impact Report

The previously released Two-Year Impact Report produced a number of key findings concerning impacts in the two years following participants' entry into the study. Key among these findings were that:

- **RExO significantly increased the number and types of services received.** Program group members reported having received, on average, a wider array of services than control group members, particularly work readiness training and support services. Few program or control group participants received any form of vocational training designed to enhance their skills in in-demand industries, however.

- **RExO significantly increased self-reported employment within both the first and second years after RA.** These increases were small (between 2.6 and 3.5 percentage points), but statistically significant. In addition, RExO significantly reduced the length of time between RA and self-reported first employment. Administrative data were unavailable for the Two-Year Impact Report, however, so these differences were all generated using self-reported data on employment and earnings.

- **RExO had no effect on recidivism in the two years following RA.** Using both administrative data and survey data, program group members were no less likely to have been convicted of a crime or incarcerated than control group members. While results from the survey indicate that RExO reduced the arrest rate among program group members in the first and second years after RA, the administrative data showed no such effect. Analyses of this discrepancy suggested this difference was driven by either recall bias or otherwise inaccurate reporting on the part of program group members.

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[43] Schochet (2008)

[44] Other means for addressing the issue are statistical in nature. However, as will be shown below, because the standard statistical tests indicate there are virtually no impacts, this report does not include such statistical treatment for multiple comparisons.

- **There was no clear evidence that RExO had differential impacts for different subgroups.** There were no clear patterns that indicated a particular subgroup experienced greater or lesser impact from RExO than any other subgroups.

Remainder of the Report

The remaining chapters of this report provide and discuss the results of the analyses. Before turning to a discussion of the impact analyses, Chapter II summarizes the findings from the implementation study. Chapter III presents the results of the impact analysis for employment and earnings. This chapter first describes the results of analyses of the administrative data obtained from the NDNH, and subsequently summarizes similar analyses using survey data to explore whether the RExO program affected participants' employment and earnings outcomes in the three-year period following their entry into the study. Chapter IV presents similar analyses focusing on recidivism. This chapter similarly begins by describing the results of analyses of the state-level criminal justice administrative data, and subsequently describes similar analyses using survey data. The final chapter of the report, Chapter V, summarizes the findings from each of the main chapters and describes their implications for understanding the overall impact of the RExO program.

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II. FINDINGS FROM THE IMPLEMENTATION STUDY

In addition to the impact study, which is the primary focus of this report, the evaluation also included an implementation study, which focused on the implementation and operations of the 24 RExO grantees. This chapter draws on those findings to provide valuable context for the results of the impact analyses described in subsequent chapters of the report.

Over the course of five years of grant funding, the 24 Generation I RExO grantees made significant strides in implementing their programs. They successfully mobilized community partners to participate in program activities, leveraged existing organizational resources to strengthen their RExO programs, and provided employment, case management and mentoring services, as well as other supportive services, to thousands of ex-offenders. These grantees also successfully implemented the RA study, enrolling 4,655 ex-offenders into program or control groups, thereby contributing substantially to the understanding of the impacts of workforce-based re-entry programs for ex-offenders. The following sections summarize the key findings from the implementation study, presented along the primary dimensions of interest for the study.

Community Context

RExO programs operated in diverse community contexts that revealed an array of challenges for ex-offenders.

- **Ex-offenders, in both the program and control groups, faced myriad barriers to employment and reintegration.** They confronted barriers such as substance abuse and low levels of education that impeded their ability to find work and successfully reintegrate into society. Furthermore, ex-offenders confronted employer biases in hiring practices that limited their employment opportunities. To the extent that ex-offenders found work, opportunities came from within industries that offered low-skill, low-pay jobs, and the employers tended to be smaller, local employers who often had experience with ex-offenders.

- **The economic downturn that occurred during the study period placed additional pressures on ex-offenders.** Unemployment rates in grantee communities were high. According to grantee staff, employers that previously hired ex-offenders subsequently had a large and overqualified pool of candidates

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vying for the few available jobs, and they were less willing to hire individuals with criminal backgrounds. In addition, cuts to state and local budgets reduced other services that could help ex-offenders smoothly re-enter society. These exogenous factors may have decreased the likelihood that study participants could obtain employment.

Grantee Administration

Overall, grantees represented a diverse group of organizations, such as local, regional, and national non-profits, faith-based and community organizations, and community health organizations. Many of these organizations were large and resource-rich, while others were small. Several key findings emerged concerning grantee administration.

- **The lead agencies were well-established organizations prior to receiving the RExO grant.** The vast majority of the lead agencies had been in operation for decades; most had existed for well over 20 years prior to receiving the RExO grant. The long duration of the grantees' presence in their communities appears to have influenced their ability to leverage community partnerships and reach a large number of eligible applicants.

- **The lead agencies offered many other services and programs in addition to RExO.** Lead agencies offered anywhere from three to more than 30 programs, including RExO. As a result, in some cases, RExO programs leveraged existing resources to provide wraparound services, such as housing, substance abuse counseling and treatment, and others.

- **In general the grantees were not large, employing an average of approximately six full-time-equivalent positions (FTEs) to manage and deliver services.** Most of the staff members were case managers, followed by administrative staff and employment services staff. Programs hired staff with diverse backgrounds and characteristics, some of whom had long histories of working with ex-offenders and other disadvantaged populations.

- **RExO programs were highly valued within grantee organizations.** Even when RExO programs accounted for only a small portion of their overall operating budgets, grantees considered the programs extremely valuable. At many grantee sites, RExO influenced how other program services were structured. Staff members at several sites, for example, decided to incorporate work readiness training, started under RExO, into other programs they offered.

Intake/Recruitment/Assessment

When compared to their enrollment in the prior four years of operation, grantees experienced a decline in enrollment during the year-long intake period, in part because of the RA study and also due to an overall decline in funds from previous years. To address this challenge, grantees

worked closely with the study team liaisons to strengthen their outreach and recruitment efforts through a variety of specific strategies.

- **Grantees intensified their outreach and recruitment efforts to reach a larger pool of applicants.** To address the dip in enrollment numbers, grantees reached out to partners to increase referrals and develop concrete recruitment strategies to identify applicants. Some of the more noteworthy outreach strategies included making presentations in prisons and at halfway houses and local shelters, as well as co-locating staff at probation/parole offices.

- **Grantees adjusted their intake and enrollment procedures in order to implement the RA process.** Both at the beginning of RA and subsequently, some grantees moved enrollment (and therefore RA itself) forward in their intake and enrollment process, engaging in fewer steps to screen candidates for suitability and willingness to participate. A few grantees experienced some difficulties with these changes, while others managed to conduct adequate suitability screening that occurred in a more limited fashion than it had previously.

- **Overall, grantees were able to meet the recruitment targets established for the evaluation.** Half the grantees met or exceeded the recruitment target of 200 participants, either with their pre-existing strategies or by adopting some of the efforts described above. A few grantees did experience continued recruitment difficulties and, despite their best efforts, did not meet the recruitment target.

RExO Services

A number of core services were available to RExO participants, including case management, employment services, and mentoring. These services were usually available in-house. At sites with limited capacity and expertise, however, some services were delivered by contracted partners. In general, grantees paid special attention to participants' multiple needs and barriers to design their service plans, and they leveraged community resources and support from their parent organizations to augment the service mix.

- **Case management support was a core strength of the RExO program.** Case managers served as an important glue that connected participants to essential services that could help them succeed. Recognizing the value of case managers, programs invested heavily in them, and hired more than 60 FTEs across the 24 grantees. Having a sufficient number of case managers ensured that caseloads were kept low and case managers could provide individualized support.

- **Work readiness training was a prominent feature within the participants' service mix.** Nearly all programs offered work readiness training as the core training activity. The intensity and duration of this training varied widely by grantee, but the content of this training was very similar across grantees, typically including résumé development, interviewing skills, and job search strategies.

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- **There was a wide range of mentoring approaches among ReXO grantees.**
  Approximately two-thirds (62.5 percent) of program participants engaged in
  mentoring activities. Grantees offered a mix of individual and group mentoring
  activities, though the vast majority focused primarily on group mentoring. Group
  mentoring consisted of support groups, social events, and supplemental work
  readiness and life skills training. Program staff noted that ex-offenders preferred
  group mentoring because it did not require consistent participation.

- **Grantees offered a diverse mix of services that were intensive and
  comprehensive.** The core services that made up the ReXO program—case
  management, employment services, and mentoring—allowed participants some
  flexibility in choosing services in an attempt to improve their skills and meet their
  needs. In addition, the overall funding levels allowed grantees to offer a level of
  intensity and comprehensiveness not easily found in their communities, at least in
  a single location. Combined with services grantees were able to leverage from
  partners, ReXO offered participants a relatively comprehensive package of
  supports.

- **Relatively little vocational training or job development was provided.** While
  a number of grantees ostensibly offered vocational or other forms of training
  designed to enhance participants' job skills, fewer than one in five program group
  members received such services. Grantees also did not have well developed job
  development or even job placement functions, as these functions were typically
  lower priorities than case management and other services. While most grantees
  provided job listings or identified potential employment opportunities, there were
  few cases in which grantees worked directly with employers to identify upcoming
  openings and refer participants to those openings.

ReXO Partnerships

Establishing partnerships was key to the programs' attempt to provide wraparound services.
Grantees used formal and informal mechanisms to successfully link with a wide range of
partners.

- **Grantees relied on a network of partnerships to fill gaps in their capacity to
  address the needs of program participants**. Some relationships were formal
  and contractual, while others were informal. Through partnerships, grantees were
  able to offer a range of services that attempted to address the broad needs of their
  clients, increase the likelihood of future funding by expanding their capacity,
  strengthen their standing in the community, and enrich their own services by
  coming to know better their own clients.

- **Grantees relied on one of three formal sub-grantee partnership models to
  deliver core ReXO services.** Six grantees had tightly coordinated sub-grantee
  relationships, defined by co-located staff, frequent opportunities for information-
  sharing, and other systems designed to increase transparency and communication.
  Another six had sub-grantees that operated somewhat independently of the
  grantee and one another. Half of the grantees used no sub-grantees.

- **Grantees leveraged various informal partnerships to supplement RExO services.** Grantees relied on less formal partnerships, which could range from loose attachments to quite strong relationships, to provide services such as substance abuse treatment, housing services, mental health services, health care, transportation, etc.

- **Partners shared common practices, goals, and objectives with the RExO program**. Some of the strongest partnerships included those that maintained frequent contact, had a history of collaboration, were bound by memoranda of understanding or contracts, and were located physically close to one another.

- **Partnerships with criminal justice organizations strengthened RExO services**. These partnerships included those with probation and parole offices, transitional or halfway houses, and the courts. When strong, these partners could refer participants to RExO, enhance communication and coordination with program staff, and enhance participants' access to additional services.

Alternatives to RExO

An analysis of the available alternative programs in the communities surrounding the 24 grantees indicates that RExO programs generally operated in areas in which an array of alternative services were also available to returning offenders.

- **Grantee communities offered many alternatives to RExO.** In every grantee community, ex-offenders could find some combination of work readiness training, job search and placement assistance, and case management services for ex-offenders through providers other than the RExO grantee. With some limitations, these services appeared to be accessible to ex-offenders and of roughly comparable or only slightly lesser quality, as assessed by the site visitors collecting the information, than those offered by grantees. Although this could pose a barrier to identifying impacts from the program, survey data indicate that program group members were much more likely to have *actually received* these services than control group members.

- **Mentoring services were less frequently available than employment services and case management**. Mentoring services were the exception to the general rule of available services, and appeared to be much less available in grantee communities other than through the RExO program. Survey data confirm that program group members were much more likely to report having received mentoring services than were control group members, though the mentoring program group members received was often not individual or lengthy in duration, as described below.

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Implications for the Impact Analysis

These findings suggest that grantees achieved a number of significant accomplishments and also experienced a range of challenges throughout the life of the grant. Several of these challenges may have implications for the impact analysis presented in subsequent chapters of this report.

- **The RA study appeared to have affected recruitment and enrollment.** Grantee staff noted that referral partner staff and applicants may have been afraid of being turned away from receiving services (as a result of possible assignment to the control group) and, as a result, fewer of them referred potential participants to or sought services at RExO programs. Subsequently, enrollment numbers decreased, requiring grantees to develop intentional and focused recruitment efforts to reach their enrollment goals. These efforts may have altered to some degree the nature of who was being served by RExO, which could have affected the likelihood of observing impacts of the program.

- **Grantees experienced difficulty implementing the mentoring program.** Mentoring services were new to most grantees, and they were difficult to implement. One of the most challenging aspects of the mentoring program was recruiting volunteer mentors, many of whom were reluctant to work with ex-offenders. Because of this challenge, the vast majority of programs offered group mentoring services rather than individual mentors. This approach resembled many other services already available through the programs, such as peer support groups and workshops on work readiness training. This form of mentoring appeared to be much less intensive, and thus may have been less effective in assisting participants find employment or avoid recidivating.

- **Participants' barriers to employment posed a serious challenge to placing them in jobs.** Grantee staff members noted repeatedly that barriers—such as lack of education, lack of work experience, unstable or transitional housing, transportation limitations, restrictions on movement due to terms of parole, and substance abuse issues, among others—presented the largest barriers to participants finding employment. While program and control group members should have experienced these challenges similarly, they may have contributed to poorer employment outcomes overall than if they could have been overcome more successfully.

- **Many employers were reluctant to hire ex-offenders.** A major recurring challenge for the RExO programs was that employers were hesitant to hire ex-offenders due to their criminal backgrounds. Grantee staff members felt that this perspective came from prejudice and lack of interaction with ex-offenders. As RExO job developers continued to work with employers to open opportunities for RExO participants, they frequently combated biases about ex-offenders' skills and work ethic. As with the barriers described above, this should have affected program and control group members equally, but may have depressed overall employment outcomes for study participants.

- **Many grantees were unable to find alternative funding sources to replace RExO by the time the program was slated to end.** The funding climate was

difficult for programs serving ex-offenders. Programs thus explored multiple options, such as local, state, and federal funding, that would sustain some of the RExO services. While some grantees were successful, others were still struggling to find new funders when their RExO funding ceased. Many staff left the RExO program before it ended, securing more stable employment elsewhere. This meant that, as the program neared the end of intake, there were often many fewer staff to serve participants than had been employed earlier. This could have affected the quality or intensity of services, and thereby affected the size of any impacts produced by the program. The impact analysis presented in subsequent chapters explores this possibility.

- **The impact analysis was not an analysis of a single program model.** Given that the services offered by grantees and their partners varied substantially—and the point of RA varied across grantees, as well—what constituted the "program" varied across sites. As a result, the analysis of impacts was of the RExO funding stream rather than a single program model. Of specific interest is the variation in the level of screening grantees did prior to enrollment. Those who screened potential participants more intensively may have enrolled program group participants with different characteristics than those who screened less intensively. This may have affected the size of impacts observed across these two types of grantees. The impact analysis presented in subsequent chapters explores this possibility.

- **The service contrast varied substantially across sites, which may have affected the impact analysis.** Because the services provided by RExO grantees and those available through alternative providers varied across the 24 sites, the contrast in services between program and control group members likely varied as well. Depending on the extent of this contrast, it may have implications for the impact analysis. Though this was known prior to the onset of RA, the implementation study confirmed that this service contrast indeed differed markedly across sites.

Conclusion

This chapter provided important context for subsequent chapters by presenting information about the labor market situation in the communities in which RExO programs operated, the major barriers facing ex-offenders, the organizational features of the programs, the services available through the programs, and the services available in the community. The chapter also discussed strategies that grantees used to leverage the resources available through key partners to deliver services to program participants. By examining barriers at intake and determining the most appropriate service plans at the outset, grantees often attempted to refer participants to supportive services, such as substance abuse treatment, and provide work readiness training immediately after enrollment. These programmatic features likely influenced the pathways that ex-offenders took as they sought new opportunities.

Several important challenges remained for program participants, however. The end of the RExO grant meant that some programs were unable to sustain their program models at full capacity (or at all). Thus, in the vast majority of sites, RExO services were scaled back significantly—or ended—due to decreased funding. Grantee staff also reported that the national recession affected participants' employment outcomes. This particular challenge likely exacerbated the relative weakness of RExO programs in their focus on job development. Too often, RExO programs did not provide adequate support to the job development function—in both the training available to job developers and the staff support—to ensure that this function was adequately funded. Future funding for programs of this type could better emphasize the role of job developers to ensure that ex-offenders get the support they need to compete with other job applicants in the marketplace.

The subsequent chapters of this report explore the extent to which the program had impacts on offenders' recidivism and employment outcomes, as well as whether any of the key variations in program structure, services, and stability affected these impacts. A Two-Year Impact Report has already been submitted; the remainder of this report focuses on the impacts of the program in the three-year period after participants enrolled into the study.

III. IMPACTS ON EMPLOYMENT AND EARNINGS

One of the key objectives of RExO was to improve the labor market outcomes of program participants. This is important in its own right, as well as because employment and/or higher earnings may serve as protective factors against future recidivism.[45] This chapter examines the degree to which RExO accomplished this objective by analyzing the effect of the program on participants' labor market outcomes during the three years following random assignment (RA).

A key findings described in the Two-Year Impact Report was that self-reported labor market outcomes were somewhat better for program group participants than for control group participants. Specifically, 71.3 percent of ex-offenders in the program group found some form of employment in the first year following RA, compared with 67.9 percent of ex-offenders in the control group. Similarly, 68.0 percent of program group members worked at some point during the second year following RA, compared with 65.4 percent of control group members. Each of these differences was statistically significant. Additionally, among program group members who ever found work, the average time to first job acquisition was 133.9 days; among comparable control group members, the average time was more than three weeks longer, at 157.1 days. And, finally, the difference in the measure of total annual income from all sources was both practically and statistically significant: Program group members reported an average total income in the year after RA of $10,998, which was almost 10 percent higher than the control group average of $10,115.

One concern with these results, however, is that they are self-reported. At the time of the Two-Year Impact Report, no independent administrative data were available to explore whether there was reporting, recall, or non-response bias that led to these findings, rather than any actual differences in labor market outcomes. This concern was exacerbated by the fact that the impact on some self-reported recidivism measures was found to be the result of differential misreporting on the part of program group participants. Hence, the earlier report concluded that providing

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[45]   Redcross et al. (2012)

parallel analyses of labor market impacts using more objective administrative data would be critical in examining the impacts of RExO on these outcomes.

This chapter overcomes that earlier limitation by including administrative data on employment and earnings. Specifically, this chapter presents results from analysis of data obtained from two separate sources: (1) administrative data from the National Directory of New Hires (NDNH); and (2) data from the follow-up survey of program and control group members. (Each of these is described in more detail in Chapter I.) Findings from the analyses of these separate datasets are presented below in two separate sections.

The chapter begins with a description of the results from analyses of the impact on employment and earnings using NDNH data. Following this are similar analyses that rely on self-reported employment and earnings drawn from the survey. These discussions present general results using relatively simple models that summarize the main findings. The Technical Appendix at the end of this report presents a series of statistical models that elaborate upon the results presented in this chapter. Within each section, the observed impacts on employment and earnings for the full sample are first summarized, and then the impacts for key subgroups of interest are examined.

Impacts Based on Administrative Data

As described in Chapter I, the evaluation team obtained data from the NDNH to provide a uniform and independent source of employment and earnings data for program and control group participants. The advantage of these data, relative to the survey data described below, is that they are not subject to reporting or recall bias; instead, they are measured identically across all study participants. A disadvantage of these data in this case, however, is that they are not available for the entire three-year period following enrollment into the study. Additionally, some employment, such as self-employment, is not covered by data in the NDNH. As described in Chapter I, due to delays in obtaining initial approval to collect the data, the earliest data available cover records beginning in July 2010, or a few months after the earliest enrollees entered the study. Additionally, there is a gap in the data between the fourth quarter of 2012 and the second quarter of 2013. Thus, the data are available to examine impacts of employment in the second year after entry into the study for some participants, but not for any participants for the entire third year after employment. Given these gaps, the discussion in this chapter focuses on employment, wages, and earnings for three discrete time periods: (1) the first year after RA

(Year 1); (2) the second year after RA for those for whom full data are available (Year 2);[46] and (3) the fourth year after RA (Year 4).

Impacts for the Full Sample

For each of the three time periods described above, the evaluation team identified four separate employment and earnings measures on which to focus: (1) whether participants were employed in the time period; (2) Unemployment Insurance benefits paid; (3) participant earnings; and (4) total income (i.e., the sum of earnings and UI benefits paid during the period). In combination with the three separate time periods, this yielded a total of 12 outcomes of primary interest for comparison of impacts. Table III-1 presents the results of this comparison for each of these 12 outcomes across the full sample.

As can be seen in Table III-1, none of the key outcomes of interest differed between the program and comparison groups in a statistically significant way. Thus, taken altogether, these results provide no evidence that RExO had any impact on employment or earnings in the several years following RA. Further, given that these data include an analysis of impacts in the first two years following RA, which were the source of apparent impacts using self-reported survey data, these results undermine the earlier conclusion that RExO had impacts on labor market outcomes in the year or two after RA. Though the NDNH data cannot be linked to the survey data to make a side-by-side comparison, the results shown in Table III-1 suggest that the impacts described in self-report data in the Two-Year Impact Report may have been driven not by any actual differences in employment rates, but instead by inaccurate self-reporting of labor market outcomes by program and control group members or by non-response bias within the survey results.

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[46]   Full two-year employment data are available for approximately 81 percent of the overall sample. The remaining sample members enrolled in the study after the third quarter of 2010, and thus do not have records for the full two-year period after RA.

Table III-1:
Program and Control Group Means for Key Labor Market Outcomes, Administrative Data

Outcome	Program	Control	Difference	P-value
Employed in Year 1 (%)	39.7	38.9	0.8	0.640
Employed in Years 1-2 (%)	53.5	54.2	-0.7	0.337
Employed in Years 1-4 (%)	71.0	73.6	-2.6	0.066*
UI Benefits in Year 1 ($)	307	286	21	0.811
UI Benefits in Year 2 ($)	381	342	39	0.370
UI Benefits in Year 4 ($)	272	240	32	0.441
Participant Earnings in Year 1 ($)	5,977	5,797	180	0.860
Participant Earnings in Year 2 ($)	5,299	5,040	259	0.999
Participant Earnings in Year 4 ($)	8,215	8,400	-185	0.495
Total Income in Year 1 ($)	6,284	6,083	201	0.832
Total Income in Year 2 ($)	5,680	5,382	298	0.894
Total Income in Year 4 ($)	8,487	8,639	-152	0.539

NOTE: Sample sizes are as follows:
Year 1 outcomes: 2,260 (program group) and 1,465 (control group)
Year 2 outcomes: 1,839 (program group) and 1,189 (control group)
Year 4 outcomes: 2,204 (program group) and 1,435 (control group)
Samples exclude individuals with *no* record of employment at any time in NDNH, because NDNH administrators only return data for records where a match occurs. These cases were distributed across the program and control groups in proportions identical to the group allocation during randomization (60/40). Sensitivity analyses in which zero values were imputed for nonmatching cases (thus yielding a total sample size equal to 4,655, the full study sample size) did not qualitatively alter the finding of no evidence of significant differences between the program and control groups.

Statistical significance levels are indicated as follows: *** = $p < .01$; ** = $p < .05$; * = $p < .1$
Minor variations in the values of the difference may exist due to rounding.

Impacts for Subgroups

Despite the fact that there are no apparent differences between program and control groups in the full sample, these data may mask important differences in impacts across key subgroups of interest. This could occur, for example, if RExO increased employment for some groups, while decreasing it for others, thereby offsetting each other. As described in Chapter I, the evaluation team identified eight subgroups of interest, including four defined by demographic characteristics,[47] two based on the time at which participants entered the study, and two based on

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[47]   Only seven subgroups are presented in the analysis using NDNH data. This is because the data on number of prior convictions could not be included in a pass-through file, and therefore could not be linked to the employment and earnings data obtained from NDNH.

differential programmatic approaches across grantees. Tables III-2 through III-8 present the results for comparisons of the key labor market outcomes across these subgroups.

Although the data in these tables clearly indicate that certain subgroups had better outcomes than others (for example, men had higher employment rates than did women), there is virtually no evidence of differences in impacts across these subgroups in an interacted model.[48] In other words, there are differences in outcomes across the subgroups, but there is no evidence of differences in impacts within subgroups.[49] Hence, there is no evidence that RExO had an impact on any of the subgroups included in this analysis.

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[48]   Analyses of the differential impacts across subgroups were conducted using models in which the treatment indicator was interacted with a subgroup indicator; the significance of the interaction term is the test for significant subgroup differences. (Lowenstein et al., 2014).

[49]   There is one minor exception to this rule. Program group members enrolling in the study more than three months after being released received significantly greater UI benefits in the second year after RA, as compared to those enrolled within three months of release. Given the number of comparisons and the absence of a clear explanation as to why this would be, however, this finding seems best explained as occurring by random chance.

Table III-2:

Impacts on Labor Market Outcomes, Administrative Data, by Age

	Younger than 27 Years				27 Years and Older			
	Program	**Control**	**Difference**	**P-value**	**Program**	**Control**	**Difference**	**P-value**
Employed in Year 1 (%)	40.7	40.5	0.2	0.835	36.1	32.5	3.6	0.325
Employed in Years 1–2 (%)	54.6	55.1	-0.4	0.545	49.4	50.6	-1.2	0.544
Employed in Years 1–4 (%)	71.4	72.9	-1.5	0.288	69.3	76.2	-6.9	0.080*
UI Benefits in Year 1 ($)	355	337	17	0.876	129	81	47	0.608
UI Benefits in Year 2 ($)	419	378	40	0.357	246	202	45	0.943
UI Benefits in Year 4 ($)	295	276	19	0.706	188	96	92	0.133
Participant Earnings in Year 1 ($)	6,468	6,173	295	0.677	4,150	4,302	-152	0.710
Participant Earnings in Year 2 ($)	5,738	5,350	388	0.819	3,704	3,833	-130	0.734
Participant Earnings in Year 4 ($)	8,526	8,726	-200	0.625	7,065	7,088	-23	0.759
Total Income in Year 1 ($)	6,823	6,511	312	0.662	4,279	4,384	-105	0.750
Total Income in Year 2 ($)	6,157	5,728	429	0.716	3,950	4,035	-85	0.746
Total Income in Year 4 ($)	8,821	9,002	-180	0.649	7,252	7,183	69	0.853

NOTES: Sample sizes in Year 1: 2,951 (under 27) and 774 (27 and older).
Total sample size for the following years is:
Year 2 outcomes: 2,388 (age 27+) and 640 (less than 27)
Year 4 outcomes: 2,884 (age 27+) and 755 (less than 27)
Samples exclude individuals with *no* record of employment at any time in NDNH, because NDNH administrators only return data for records where a match occurs. These cases were distributed across the program and control groups in proportions identical to the group allocation during randomization (60/40). Sensitivity analyses in which zero values were imputed for nonmatching cases (thus yielding a total sample size equal to 4,655, the full study sample size) did not qualitatively alter the finding of no evidence of significant differences between the program and control groups.

Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1

Minor variations in the values of the difference may exist due to rounding.

Table III-3:

Impacts on Labor Market Outcomes, Administrative Data, by Gender

	Female				Male			
	Program	**Control**	**Difference**	**P-value**	**Program**	**Control**	**Difference**	**P-value**
Employed in Year 1 (%)	46.4	43.5	2.9	0.197	38.0	38.0	0.1	0.945
Employed in Years 1–2 (%)	55.6	58.7	-3.1	0.771	52.9	53.3	-0.4	0.431
Employed in Years 1–4 (%)	76.6	79.8	-3.2	0.916	69.6	72.4	-2.8	0.070*
UI Benefits in Year 1 ($)	37	342	28	0.620	294	274	19	0.907
UI Benefits in Year 2 ($)	405	293	112	0.467	379	339	41	0.433
UI Benefits in Year 4 ($)	258	247	11	0.760	278	230	49	0.259
Participant Earnings in Year 1 ($)	5,124	4,428	696	0.096*	6,201	6,098	103	0.737
Participant Earnings in Year 2 ($)	4,145	4,012	133	0.790	5,610	5,309	302	0.950
Participant Earnings in Year 4 ($)	6,744	7,303	-559	0.450	8,599	8,679	-80	0.615
Total Income in Year 1 ($)	5,495	4,770	725	0.081*	6,496	6,373	123	0.752
Total Income in Year 2 ($)	4,550	4,306	244	0.676	5,990	5,647	343	0.960
Total Income in Year 4 ($)	7,002	7,550	-548	0.438	8,878	8,909	-31	0.683

NOTES: Sample sizes for Year 1: 719 (female) and 2,976 (male).
Total sample size for the following years is:
Year 2 outcomes: 576 (female) and 2,426 (male)
Year 4 outcomes: 703 (female) and 2,906 (male)
Samples exclude individuals with *no* record of employment at any time in NDNH, because NDNH administrators only return data for records where a match occurs. These cases were distributed across the program and control groups in proportions identical to the group allocation during randomization (60/40). Sensitivity analyses in which zero values were imputed for nonmatching cases (thus yielding a total sample size equal to 4,655, the full study sample size) did not qualitatively alter the finding of no evidence of significant differences between the program and control groups.

Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1
Minor variations in the values of the difference may exist due to rounding.

Table III-4:

Impacts on Labor Market Outcomes, Administrative Data, by Educational Attainment

	Program	Control	Difference	P-value
No GED/HS Diploma				
Employed in Year 1 (%)	35.7	33.8	2.0	0.510
Employed in Years 1–2 (%)	48.5	51.3	-2.8	0.406
Employed in Years 1–4 (%)	67.9	70.6	-2.7	0.420
UI Benefits in Year 1 ($)	261	211	50	0.695
UI Benefits in Year 2 ($)	266	233	33	0.799
UI Benefits in Year 4 ($)	235	195	40	0.616
Participant Earnings in Year 1 ($)	4,753	5,289	-536	0.272
Participant Earnings in Year 2 ($)	4,491	5,267	-776	0.224
Participant Earnings in Year 4 ($)	6,839	7,225	-386	0.665
Total Income in Year 1 ($)	5,014	5,499	-486	0.304
Total Income in Year 2 ($)	4,758	5,500	-743	0.243
Total Income in Year 4 ($)	7,075	7,420	-346	0.697
GED				
Employed in Year 1 (%)	44.0	40.1	3.9	0.249
Employed in Years 1–2 (%)	57.2	55.3	2.0	0.561
Employed in Years 1–4 (%)	70.1	73.7	-3.6	0.227
UI Benefits in Year 1 ($)	129	269	-139	0.021**
UI Benefits in Year 2 ($)	373	373	0	0.835
UI Benefits in Year 4 ($)	207	179	29	0.700
Participant Earnings in Year 1 ($)	6,932	6,147	785	0.342
Participant Earnings in Year 2 ($)	5,552	4,172	1,381	0.063*
Participant Earnings in Year 4 ($)	7,521	8,310	-789	0.462
Total Income in Year 1 ($)	7,061	6,416	646	0.456
Total Income in Year 2 ($)	5,926	4,545	1,381	0.069*
Total Income in Year 4 ($)	7,729	8,489	-760	0.481
HS Diploma or Higher				
Employed in Year 1 (%)	41.4	43.2	-1.8	0.456
Employed in Years 1–2 (%)	54.8	58.4	-3.7	0.188
Employed in Years 1–4 (%)	73.6	76.2	-2.6	0.238
UI Benefits in Year 1 ($)	423	320	103	0.281
UI Benefits in Year 2 ($)	444	336	108	0.261
UI Benefits in Year 4 ($)	347	313	34	0.633
Participant Earnings in Year 1 ($)	5,877	5,752	125	0.857
Participant Earnings in Year 2 ($)	5,363	5,701	-339	0.508
Participant Earnings in Year 4 ($)	9,364	9,312	52	0.972
Total Income in Year 1 ($)	6,300	6,072	228	0.679
Total Income in Year 2 ($)	5,806	6,037	-231	0.650
Total Income in Year 4 ($)	9,711	9,625	86	0.993

NOTES: Sample sizes: 1,010 (no GED/HS diploma), 997 (GED), and 1,448 (HS diploma or greater).
Sample sizes for Year 2 and Year 4 outcomes are slightly reduced.
Statistical significance levels are indicated as follows: *** = $p < .01$; ** = $p < .05$; * = $p < .1$

Table III-5:

Impacts on Labor Market Outcomes, Administrative Data, by Timing of RA (Relative to Release from Prison)

	Early Assignment				Late Assignment			
	Program	**Control**	**Difference**	**P-value**	**Program**	**Control**	**Difference**	**P-value**
Employed in Year 1 (%)	40.2	39.5	0.7	0.807	38.1	38.4	-0.3	0.907
Employed in Years 1–2 (%)	54.5	53.9	0.5	0.804	48.8	56.4	-7.6	0.070*
Employed in Years 1–4 (%)	71.5	73.9	-2.5	0.158	68.8	73.3	-4.5	0.163
UI Benefits in Year 1 ($)	296.4	292.3	4.1	0.904	353.4	244.4	109.0	0.472
UI Benefits in Year 2 ($)	338	359	-21	0.889	562	216	346	0.025**
UI Benefits in Year 4 ($)	272	231	42	0.533	304	248	56	0.401
Participant Earnings in Year 1 ($)	6,099	6,081	18	0.671	5,743	4,964	780	0.294
Participant Earnings in Year 2 ($)	5,462	5,298	164	0.809	4,945	4,334	612	0.512
Participant Earnings in Year 4 ($)	8,272	8,733	-460	0.305	8,249	7,307	942	0.274
Total Income in Year 1 ($)	6,395	6,373	22	0.688	6,097	5,208	889	0.251
Total Income in Year 2 ($)	5,800	5,657	143	0.796	5,507	4,550	957	0.281
Total Income in Year 4 ($)	8,545	8,963	-418	0.332	8,553	7,555	998	0.244

NOTES: Sample sizes: 2,863 (early assignment) and 771 (late assignment).
Total sample size for the following years is:
Year 2 outcomes: 2,348 (early assignment) and 603 (late assignment)
Year 4 outcomes: 2,798 (early assignment) and 753 (late assignment)
Samples exclude individuals with *no* record of employment at any time in NDNH, because NDNH administrators only return data for records where a match occurs. These cases were distributed across the program and control groups in proportions identical to the group allocation during randomization (60/40). Sensitivity analyses in which zero values were imputed for nonmatching cases (thus yielding a total sample size equal to 4,655, the full study sample size) did not qualitatively alter the finding of no evidence of significant differences between the program and control groups.

Statistical significance levels are indicated as follows: *** = $p < .01$; ** = $p < .05$; * = $p < .1$
Minor variations in the values of the difference may exist due to rounding.

Table III-6:

Impacts on Labor Market Outcomes, Administrative Data, by Timing of RA (Relative to Program Schedule)

	Pre-October Assignment				Post-October Assignment			
	Program	**Control**	**Difference**	**P-value**	**Program**	**Control**	**Difference**	**P-value**
Employed in Year 1 (%)	34.2	33.3	0.8	0.647	53.5	52.2	1.3	0.637
Employed in Years 1–2 (%)	51.9	51.8	0.1	0.539	64.8	69.4	-4.6	0.536
Employed in Years 1–4 (%)	67.9	70.2	-2.3	0.165	78.3	81.5	-3.2	0.275
UI Benefits in Year 1 ($)	335	296	39	0.888	237	261	-23	0.823
UI Benefits in Year 2 ($)	405	343	62	0.216	218	341	-123	0.380
UI Benefits in Year 4 ($)	269	227	42	0.487	280	269	12	0.732
Participant Earnings in Year 1 ($)	6,238	6,327	-89	0.644	5,330	4,532	798	0.335
Participant Earnings in Year 2 ($)	5,247	5,202	46	0.524	5,660	3,976	1,685	0.198
Participant Earnings in Year 4 ($)	8,602	8,742	-141	0.603	7,293	7,606	-313	0.623
Total Income in Year 1 ($)	6,574	6,624	-50	0.663	5,568	4,793	775	0.324
Total Income in Year 2 ($)	5,652	5,544	108	0.662	5,878	4,317	1,561	0.244
Total Income in Year 4 ($)	8,871	8,970	-99	0.647	7,573	7,875	-302	0.644

NOTES: Sample sizes for Year 1: 2,641(pre-October assignment) and 1,084 (post-October assignment).
Total sample size for the following years is:
Year 2 outcomes: 2,641 (pre-October) and 387 (post-October)
Year 4 outcomes: 2,555 (pre-October) and 1,084 (post-October)
Samples exclude individuals with *no* record of employment at any time in NDNH, because NDNH administrators only return data for records where a match occurs. These cases were distributed across the program and control groups in proportions identical to the group allocation during randomization (60/40). Sensitivity analyses in which zero values were imputed for nonmatching cases (thus yielding a total sample size equal to 4,655, the full study sample size) did not qualitatively alter the finding of no evidence of significant differences between the program and control groups.

Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1
Minor variations in the values of the difference may exist due to rounding.

Table III-7:

Impacts on Labor Market Outcomes, Administrative Data, by Program Emphasis

	Employment				Supportive Services			
	Program	**Control**	**Difference**	**P-value**	**Program**	**Control**	**Difference**	**P-value**
Employed in Year 1 (%)	38.9	38.5	0.4	0.873	41.6	39.8	1.8	0.646
Employed in Years 1–2 (%)	53.5	53.2	0.4	0.651	53.4	56.4	-3.0	0.292
Employed in Years 1–4 (%)	71.1	72.5	-1.3	0.390	70.6	76.1	-5.4	0.036**
UI Benefits in Year 1 ($)	306	278	27	0.803	308	302	6	0.949
UI Benefits in Year 2 ($)	376	365	11	0.753	394	291	103	0.254
UI Benefits in Year 4 ($)	275	216	59	0.205	266	291	-25	0.566
Participant Earnings in Year 1 ($)	6,358	6,148	210	0.911	5,136	5,030	107	0.793
Participant Earnings in Year 2 ($)	5,851	5,439	412	0.804	4,024	4,145	-121	0.571
Participant Earnings in Year 4 ($)	8,729	8,623	107	0.944	7,089	7,913	-824	0.241
Total Income in Year 1 ($)	6,664	6,426	238	0.884	5,445	5,332	114	0.787
Total Income in Year 2 ($)	6,227	5,804	423	0.774	4,418	4,436	-18	0.755
Total Income in Year 4 ($)	9,005	8,839	166	0.975	7,355	8,204	-849	0.226

NOTES: Sample sizes for Year 1: 2,560 (employment emphasis) and 1,165 (supportive services emphasis).
Total sample size for the following years is:
Year 2 outcomes: 2,105 (employment emphasis) and 923 (supportive services)
Year 4 outcomes: 2,497 (employment emphasis) and 1,142 (supportive services)
Samples exclude individuals with *no* record of employment at any time in NDNH, because NDNH administrators only return data for records where a match occurs. These cases were distributed across the program and control groups in proportions identical to the group allocation during randomization (60/40). Sensitivity analyses in which zero values were imputed for nonmatching cases (thus yielding a total sample size equal to 4,655, the full study sample size) did not qualitatively alter the finding of no evidence of significant differences between the program and control groups.

Statistical significance levels are indicated as follows: *** = $p < .01$; ** = $p < .05$; * = $p < .1$
Minor variations in the values of the difference may exist due to rounding.

Table III-8:

Impacts on Labor Market Outcomes, Administrative Data, by Random Assignment Model

	Concurrent RA				RA after Screening			
	Program	**Control**	**Difference**	**P-value**	**Program**	**Control**	**Difference**	**P-value**
Employed in Year 1 (%)	37.7	36.6	1.1	0.535	43.5	43.2	0.3	0.859
Employed in Years 1–2 (%)	50.7	52.7	-2.0	0.227	58.8	56.8	1.9	0.994
Employed in Years 1–4 (%)	69.7	72.2	-2.5	0.146	73.3	76.1	-2.7	0.216
UI Benefits in Year 1 ($)	362	318	44	0.568	205	227	-21	0.798
UI Benefits in Year 2 ($)	399	314	85	0.181	350	394	-45	0.707
UI Benefits in Year 4 ($)	305	237	68	0.135	212	245	-32	0.397
Participant Earnings in Year 1 ($)	5,207	5,233	-25	0.737	7,400	6,825	575	0.635
Participant Earnings in Year 2 ($)	4,343	4,328	16	0.499	7,068	6,324	744	0.679
Participant Earnings in Year 4 ($)	7,222	7,978	-756	0.094*	10,029	9,146	884	0.444
Total Income in Year 1 ($)	5,570	5,551	19	0.822	7,606	7,052	554	0.658
Total Income in Year 2 ($)	4,742	4,642	100	0.682	7,417	6,718	699	0.714
Total Income in Year 4 ($)	7,527	8,215	-688	0.129	10,242	9,391	851	0.478

NOTES: Sample sizes for Year 1: 2,413 (concurrent enrollment) and 1,312 (enrollment after screening).
Total sample size for the following years is:
Year 2 outcomes: 1,959 (concurrent) and 1,069 (enrollment after screening)
Year 4 outcomes: 2,497 (concurrent) and 1,142 (enrollment after screening)
Samples exclude individuals with *no* record of employment at any time in NDNH, because NDNH administrators only return data for records where a match occurs. These cases were distributed across the program and control groups in proportions identical to the group allocation during randomization (60/40). Sensitivity analyses in which zero values were imputed for nonmatching cases (thus yielding a total sample size equal to 4,655, the full study sample size) did not qualitatively alter the finding of no evidence of significant differences between the program and control groups.

Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1
Minor variations in the values of the difference may exist due to rounding.

Impacts Based on Survey Data

The administrative data drawn from NDNH are based on quarterly data, and do not cover all forms of employment. As such, they provide an independent and objective source of employment and earnings information, but offer only a very blunt view; in other words, one cannot measure periods of employment or the wages earned in any given employment using the NDNH data. The follow-up survey data, however, include this finer detail, and thus the next section describes the results of similar analyses of impacts on employment and earnings using data from this survey.

As described in Chapter I, the survey was initially administered approximately two years after RA. Results from this survey were described in the Two-Year Impact Report. The survey was administered again approximately three years after RA. The sampling frame for the survey included all 4,655 study participants. Ultimately, 2,995 individuals completed the three-year survey, yielding a response rate of 64.3 percent. This represented 83.6 percent of those who responded to the two-year survey (N=3,581).

Impacts for the Full Sample

Because of concerns about making multiple comparisons (described in Chapter I), and to provide a concise summary of labor market performance, the evaluation team selected in advance from the many measures available (based on the questions about employment and earnings asked in the follow-up survey)[50] a set of seven core measures of employment and earnings. Taken together, these measures provide a relatively complete picture of labor market performance:

1. Whether or not the individual worked at all in the three years following RA.

2. Whether or not the individual worked at all in the *third* year following RA.

3. Elapsed time to acquisition of first job.

4. Total days worked during the evaluation period.

5. Average hourly wages at the first job obtained following RA.

6. Average hourly wages at the job most recently obtained following RA.

7. Total personal income in the third year following RA.

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[50]   Among the several dozen additional *potential* labor market measures that were not included in this analysis are: measures of non-wage benefits (was participation in a health or dental plan or retirement plan offered or accepted?); measures of job performance (was a promotion received, or a future promotion possible?); and alternative measures of earnings and job acquisition.

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It should be noted that the wage measures (numbers 5 and 6) are necessarily computed only for study participants who actually found work. Because this implies a partitioning or selection of the sample on a post-RA attribute (employment), the difference between program and control group means does not provide an unbiased estimate of the treatment effect. Hence, results for these measures are intended to be suggestive rather than definitive.

Table III-9 describes the effect of RExO on these seven labor market outcomes. Contrary to the results described above for the administrative data, there are some significant differences between program and control group members in these self-reported data. Specifically, program group members reported being employed at some point in the three years following RA more frequently than did control group members (a difference of 3.2 percentage points). A similar difference between program and control group members was reported for being employed in the third year after RA—a difference of 3.0 percentage points, which approached conventional levels of statistical significance.

Given the discrepancy between the administrative data and the survey data, it is not entirely clear whether RExO may have had some small impact on employment. However, the survey data are self-reported, and are known only for a sample of the overall population (fewer than two-thirds of the overall population responded to the three-year survey); the NDNH data are objective and at least theoretically available for the entire sample, but do not cover all types of employment. Hence, three possible explanations for the difference in conclusions from the administrative data and survey data are that, (1) the survey data suffer from either reporting or recall bias on the part of program group members, (2) non-response bias in who completed the three-year survey contributed to this discrepancy, or (3) the survey is picking up sources of employment that the NDNH misses, and program group members have more employment of these types than do control group members.

Because the NDNH data cannot be linked in any way to the survey data, it is impossible to test this hypothesis directly. Given that comparisons between survey and administrative data in the Two-Year Impact Report yielded the clear conclusion that reporting or recall bias led to differences in impacts observed across these two types of data, however, similar biases in the survey data on employment would explain the differences observed in the current labor market data. Thus, despite the seemingly positive results for employment shown in Table III-9, there is no consistent or widespread evidence that RExO had an impact on the overall labor market outcomes for participants.

Table III-9:

Program and Control Group Means for Key Labor Market Outcomes, Survey Data

	Program	Control	Difference (Impact)	Hazard Ratio (Impact)	P-Value
Employment					
Any Job (Years 1–3) (%)	89.1	85.9	3.2		0.008***
Any Job (Year 3) (%)	67.0	64.0	3.0		0.056*
Days to First Job (#)	142.2	137.8	4.4		0.864
Survival Analysis				1.143	0.003***
Total Days Employed (#)	475.5	457.3	18.2		0.207
Total Days Employed (excluding those with no employment) (#)	615.6	614.8	0.8		0.744
Compensation					
Wage at First Job ($)	10.82	10.47	0.35		0.220
Wage at Last Job ($)	13.17	13.54	-0.37		0.495
Total Income ($)	11,783	11,139	644		0.172

NOTES: Sample sizes: 1,145 (control group) and 1,814 (program group).

Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1

Impacts for Subgroups

As with the administrative data, there is a possibility that a lack of evidence of clear impacts on employment and earnings for the overall sample masks important differences within this sample. This section therefore assesses the impacts of RExO on labor market outcomes for the eight different partitions of the sample, as described above. Complete descriptions of these subgroups and the reasons for selecting them can be found in Chapter I. Results of the analyses of the seven primary labor market outcomes for the subgroups are displayed in Tables III-10 through III-16.

As with the administrative data, there are some differences across the subgroups in overall labor market outcomes. There is little evidence, however, that the magnitude of the impacts differs across these subgroups. Specifically, while the percentage of participants who obtained employment in the first three years after RA varied from 83 to 89 percent across subgroups, the relative difference between program and control group members across these subgroups was not statistically significant.

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Table III-10:

Impacts on Labor Market Outcomes, Survey Data, by Age

	Younger than 27 Years					27 Years and Older				
	Program	Control	Difference (Impact)	Hazard Ratio (Impact)	P-value	Program	Control	Difference	Hazard Ratio (Impact)	P-value
Employment										
Any Job (Years 1–3) (%)	88.8	86.5	2.3		0.066*	90.1	82.9	7.2		0.018**
Any Job (Year 3) (%)	67.1	64.3	2.7		0.096*	66.9	62.7	4.2		0.284
Days to First Job (#)	130.8	123.3	7.5		0.731	188.6	205.8	-17.2		0.609
Survival Analysis				1.102	0.056*				1.358	0.003***
Total Days Employed (#)	474.8	469.2	5.5		0.622	478.5	403.6	74.9		0.023**
Total Days Employed (excluding those w/ no employment) (#)	626.3	625.0	1.3		0.801	574.8	566.4	8.3		0.611
Compensation										
Wage at First Job ($)	10.99	10.66	0.33		0.324	10.09	9.77	0.32		0.466
Wage at Last Job ($)	13.49	13.71	-0.22		0.712	11.85	12.74	-0.89		0.376
Total Income ($)	12,315	11,442	873		0.126	9,399	9,727	-328		0.901

NOTES: Sample sizes: 2,429 (over 27) and 566 (27 and under).

Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1

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Table III-11:

Impacts on Labor Market Outcomes, Survey Data, by Gender

	Female					Male				
	Program	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**	**Program**	**Control**	**Difference**	**Hazard Ratio (Impact)**	**P-value**
Employment										
Any Job (Years 1–3) (%)	87.3	84.2	3.1		0.216	89.5	86.2	3.3		0.018**
Any Job (Year 3) (%)	69.2	65.1	4.0		0.278	66.7	63.4	3.2		0.101
Days to First Job (#)	166.7	133.5	33.2		0.377	134.6	138.8	-4.2		0.812
Survival Analysis				1.067	0.528				1.165	0.003***
Total Days Employed (#)	471.0	475.8	-4.8		0.904	478.9	449.7	29.2		0.133
Total Days Employed (excluding those w/ no employment) (#)	619.0	624.6	-5.6		0.818	617.9	609.7	8.2		0.644
Compensation										
Wage at First Job ($)	9.63	8.70	0.93		0.034**	11.17	10.94	0.22		0.453
Wage at Last Job ($)	12.56	12.44	0.12		0.992	13.35	13.78	-0.43		0.422
Total Income ($)	10,753	9,719	1,033		0.207	12,122	11,480	642		0.259

NOTES: Sample sizes: 641 (female) and 2,333 (male).

Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1

Table III-12:

Impacts on Labor Market Outcomes, Survey Data, by Number of Prior Convictions

	3 or Fewer					4 or More				
	Program	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**	**Program**	**Control**	**Difference**	**Hazard Ratio (Impact)**	**P-value**
Employment										
Any Job (Years 1–3) (%)	91.9	87.6	4.3		0.015**	86.5	83.6	2.9		0.148
Any Job (Year 3) (%)	70.8	66.4	4.3		0.087*	63.4	60.3	3.1		0.189
Days to First Job (#)	166.4	205.0	-38.6		0.165	118.4	70.6	47.8		0.326
Survival Analysis				1.139	0.053*				1.170	0.033**
Total Days Employed (#)	499.4	468.5	30.9		0.176	452.8	431.9	20.9		0.446
Total Days Employed (excluding those w/ no employment) (#)	633.8	629.6	4.2		0.726	594.9	591.6	3.3		0.862
Compensation										
Wage at First Job ($)	10.90	10.64	0.27		0.510	10.84	10.36	0.49		0.125
Wage at Last Job ($)	12.80	13.92	-1.12		0.127	13.84	13.44	0.40		0.503
Total Income ($)	13,151	12,231	920		0.207	10,522	10,337	186		0.810

NOTES: Sample sizes: 1,316 (3 or fewer convictions) and 1,257 (four or more convictions).

Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1

Table III-13:

Impacts on Labor Market Outcomes, Survey Data, by Timing of RA (Relative to Release from Prison)

	Early Assignment					Late Assignment				
	Program	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**	**Program**	**Control**	**Difference**	**Hazard Ratio (Impact)**	**P-value**
Employment										
Any Job (Years 1–3) (%)	89.1	85.2	3.9		0.010**	88.9	87.7	1.2		0.486
Any Job (Year 3) (%)	67.7	62.1	5.5		0.008***	65.3	70.0	-4.7		0.269
Days to First Job (#)	136.5	122.3	14.2		0.626	156.4	176.7	-20.3		0.715
Survival Analysis				1.174	0.002***				1.002	0.983
Total Days Employed (#)	486.8	457.2	29.6		0.148	431.3	454.4	-23.1		0.604
Total Days Employed (excluding those w/ no employment) (#)	620.1	621.6	-1.6		0.956	606.1	594.1	12.0		0.672
Compensation										
Wage at First Job ($)	10.99	10.63	0.36		0.279	10.31	10.06	0.25		0.641
Wage at Last Job ($)	13.30	13.36	-0.06		0.860	12.63	14.18	-1.55		0.162
Total Income ($)	12,025	11,412	613		0.326	11,097	9,965	1,132		0.231

NOTES: Sample sizes: 2,319 (early assignment) and 596 (late assignment).

Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1

Table III-14:

Impacts on Labor Market Outcomes, Survey Data, by Timing of RA (Relative to Program Schedule)

	Pre-October Assignment					Post-October Assignment				
	Program	Control	Difference (Impact)	Hazard Ratio (Impact)	P-value	Program	Control	Difference	Hazard Ratio (Impact)	P-value
Employment										
Any Job (Years 1–3) (%)	89.4	85.9	3.5		0.011**	88.1	85.9	2.2		0.381
Any Job (Year 3) (%)	67.8	63.3	4.4		0.014**	65.1	65.8	-0.7		0.815
Days to First Job (#)	156.1	137.3	18.8		0.564	103.6	138.9	-35.3		0.638
Survival Analysis				1.175	0.003***				1.095	0.286
Total Days Employed (#)	476.2	455.3	20.9		0.159	473.5	462.1	11.3		0.873
Total Days Employed (excluding those w/ no employment) (#)	617.8	619.2	-1.5		0.740	609.8	604.6	5.2		0.980
Compensation										
Wage at First Job ($)	10.72	10.77	-0.05		0.940	11.12	9.74	1.38		0.003***
Wage at Last Job ($)	13.22	13.48	-0.26		0.729	13.04	13.69	-0.65		0.429
Total Income ($)	11,715	11,144	571		0.243	11,970	11,127	843		0.476

NOTES: Sample sizes: 2,164 (pre-October assignment) and 831 (post-October assignment).

Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1

Table III-15:

Impacts on Labor Market Outcomes, Survey Data, by Program Emphasis

	Employment					Supportive Services				
	Program	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**	**Program**	**Control**	**Difference**	**Hazard Ratio (Impact)**	**P-value**
Employment										
Any Job (Years 1–3) (%)	89.6	86.1	3.5		0.019**	88.0	85.5	2.5		0.177
Any Job (Year 3) (%)	69.0	64.8	4.2		0.036**	63.0	62.4	0.6		0.611
Days to First Job (#)	125.9	157.9	-32.0		0.186	177.0	95.7	81.4		0.177
Survival Analysis				1.158	0.010**				1.114	0.164
Total Days Employed (#)	494.9	470.1	24.9		0.200	434.8	430.6	4.1		0.692
Total Days Employed (excluding those w/ no employment) (#)	637.0	623.5	13.6		0.412	570.1	596.0	-26.0		0.529
Compensation										
Wage at First Job ($)	10.74	10.53	0.21		0.397	11.01	10.36	0.65		0.354
Wage at Last Job ($)	12.97	13.54	-0.57		0.439	13.60	13.54	0.06		0.979
Total Income ($)	12,201	11,444	757		0.185	10,855	10,471	385		0.558

NOTES: Sample sizes: 1,992 (employment emphasis) and 1,003 (supportive services emphasis).

Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1

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Table III-16:

Impacts on Labor Market Outcomes, Survey Data, by Random Assignment Model

	Concurrent RA					RA after Screening				
	Program	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**	**Program**	**Control**	**Difference**	**Hazard Ratio (Impact)**	**P-value**
Employment										
Any Job (Years 1–3) (%)	87.9	84.8	3.1		0.030**	91.2	87.9	3.3		0.147
Any Job (Year 3) (%)	66.3	63.2	3.1		0.099*	68.4	65.6	2.8		0.393
Days to First Job (#)	141.1	150.7	-9.6		0.863	144.1	115.1	29.0		0.418
Survival Analysis				1.156	0.011**				1.112	0.168
Total Days Employed (#)	469.4	433.7	35.7		0.045**	486.5	500.1	-13.7		0.536
Total Days Employed (excluding those w/ no employment) (#)	622.3	597.6	24.8		0.169	604.3	644.2	-39.9		0.194
Compensation										
Wage at First Job ($)	10.76	10.66	0.10		0.883	10.92	10.17	0.75		0.030**
Wage at Last Job ($)	12.98	13.81	-0.83		0.207	13.52	13.06	0.46		0.390
Total Income ($)	11,322	10,610	713		0.168	12,594	12,082	513		0.615

NOTES: Sample sizes: 1,941 (concurrent enrollment) and 1,054 (enrollment after screening).

Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1

There are two exceptions to this general rule. The first is that program group members assigned on or after October 1, 2010, observed significantly greater impacts on their wages at the first jobs they obtained than did those assigned prior to October 1, 2010. As described in the Two-Year Impact Report, this is driven almost exclusively by the much lower wage earned by control group members enrolling after this date.

The second exception is that program group members enrolling within 90 days of their release from incarceration observed significantly greater impacts on their self-reported employment in the third year after RA. This is intriguing, especially in light of prior research that has shown that enrolling within 90 days of release increases employment and reduces recidivism.[51] Given that this is a relatively isolated finding among a large number of comparisons, however, it does not seem particularly compelling as support for the need to enroll participants soon after their release.

Summary

Evidence described in this chapter provides mixed support for the notion that RExO had beneficial impacts on the labor market outcomes of participants. Using administrative data from the NDNH, there were no significant differences between program group members and control group members on any of the available labor market outcomes. However, the analysis of survey data finds small but statistically significant impacts on the probability of ever finding employment. The most likely explanation for these discrepancies is some form reporting or recall bias on the part of program group members, or non-response bias among survey completers. However, an alternative explanation that cannot be ruled out is that program group members were more likely than control group members to obtain jobs not covered under NDNH— specifically self-employment or "under-the table" employment.[52] This possibility notwithstanding, there was ultimately no clear and consistent evidence of an impact on employment or earnings. The subsequent chapter will explore the extent to which RExO had impacts on participants' recidivism.

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[51] Redcross et al. (2012)

[52] The study team views this explanation with some skepticism. In the next chapter, the analysis of recidivism outcomes reveals a similar pattern, in which an effect (on the probability of arrest) is detected in the survey data, but not corroborated by the analysis of administrative data. Subsequent calculations show that program group members with "true" arrests in the administrative data were more likely to misreport those events than were control group members. While not conclusive, evidence of misreporting of criminal justice outcomes reduces the overall level of confidence in this alternative explanation.

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IV.   IMPACTS ON CRIMINAL JUSTICE OUTCOMES

This chapter describes the effect of the RExO program on participant recidivism during the three-year period following RA. Two sets of data were available for this purpose. The first of these comprised administrative data on arrests, convictions, and incarceration, which were collected from each state (or locality) in which the RExO program operated. The second source of data was the follow-up survey, which contained questions about study participant involvement in the criminal justice system following random assignment.

Findings from the analyses of these separate datasets are presented below in two separate sections. These discussions present general results using relatively simple models that summarize the main findings. The Technical Appendix at the end of this report presents a series of statistical models that elaborate upon the results presented in this chapter. To assist with understanding the specific meanings of some terms in this chapter, a glossary of terms used to describe recidivism outcomes is provided at the end of the chapter.

Impacts Based on Administrative Data

As described in Chapter I, the evaluation team collected criminal justice data from agencies in the states in which the RExO grantees operated (although some data are missing for some sites). This section presents results of analyses of these data to estimate the effect of RExO on the recidivism rates of program participants. These analyses align closely with those presented in the Two-Year Impact Report, but contain an additional, third year of outcomes. Additionally, administrative data for some individuals that were missing from the analyses presented in the Two-Year Impact Report were subsequently obtained and are included here.

Impacts for the Full Sample

Tables IV-1 and IV-2 present results of an analysis of the impact of RExO on several measures of recidivism, including those defined by arrest, conviction, and subsequent incarceration in prison or jail. Table IV-1 contains impact results for outcomes calculated over the entire three-

year interval following RA, while Table IV-2 contains results for outcomes calculated for the third year only.

Results in both tables indicate that there is no evidence that RExO significantly altered participants' criminal justice outcomes. For example, the first row in Table IV-1 shows that 53 percent of the ex-offenders in the program group were re-arrested in the three-year period following RA, compared to 54 percent of those in the control group. The difference between these values was not statistically significant. For the majority of outcomes in both Table IV-1 and Table IV-2, similar results are observed—an observed impact estimate that is close to zero and not statistically significant. Both tables show no evidence that RExO had a significant impact on either the probability of reincarceration (in either prison or jail), or on the total number of days incarcerated following RA. For some outcomes, the program group had slightly higher average values, while for other outcomes, the control group did. This pattern is consistent with the hypothesis that RExO did not affect participant recidivism.

One set of outcomes did generate impact estimates that were statistically significant: conviction outcomes generated using only data from the third year following random assignment. Table IV-2 shows that program group members were slightly *more* likely to be convicted in that period. For example, 18.2 percent of program group members were convicted of a crime in the third year, compared to only 15.1 percent of control group members. This difference (3.1 percentage points) is statistically significant (p = 0.04). Program group members also appear to be more likely to be convicted of felonies (impact estimate = 2.0 percentage points; p = 0.062) and public order crimes (impact estimate = 2.2 percentage points; p = 0.016).

If viewed in isolation, these findings might be worrying, as they suggest that rather than reducing recidivism as intended, participation in RExO actually led to an increase in future criminal justice involvement. When viewed in the context of other study findings, however, that conclusion does not appear warranted. None of the other classes of outcomes (arrests or prison/jail re-incarceration) showed significant differences between the two groups. While the two-year administrative data impact analysis showed a statistically significant difference, in that program group members were more likely to be convicted of a felony, the analysis of conviction outcomes measured over the entire three-year period did not. Finally, impact estimates from the analysis of survey data (not shown) do not corroborate this finding. The absence of a consistent pattern of differences suggests that these results are an artifact of statistical chance, rather than a measurement of a true impact.

TABLE IV-1:

Three-Year Impacts on Recidivism, Administrative Data, Full Sample

	Program	Control	Difference	P-value
Arrested (%)	53.1	54.0	-0.9	0.335
Convicted of a Crime (%)	36.4	33.5	2.9	0.285
Convicted of a Felony (%)	21.0	18.4	2.6	0.113
Convicted of a Misdemeanor (%)	16.7	15.3	1.4	0.694
Convicted of a Violent Crime (%)	5.6	5.8	-0.2	0.457
Convicted of a Property Crime (%)	12.4	11.2	1.2	0.213
Convicted of a Drug Crime (%)	12.8	10.8	2.0	0.343
Convicted of a Public Order Crime (%)	16.2	14.8	1.4	0.402
Admitted to Prison (%)	34.8	33.8	1.0	0.736
Admitted to Prison for a New Crime (%)	16.6	17.1	-0.5	0.410
Admitted to Prison for a Parole/Probation Violation (%)	18.2	16.6	1.6	0.333
Admitted to Prison for Other Reason/Reason Unknown (%)	2.6	3.1	-0.5	0.573
Total Days Incarcerated in Prison (#)	134.3	134.3	-0.1	0.874
Admitted to Jail (%)	59.7	58.2	1.5	0.882
Admitted to Jail for a New Crime (%)	25.5	22.4	3.0	0.517
Admitted to Jail for a Parole/Probation Violation (%)	18.2	16.8	1.4	0.864
Admitted to Jail for Other Reason (%)	34.1	37.8	-3.6	0.442
Total Days Incarcerated in Jail (#)	138.7	123.8	14.8	0.669
Arrested, Convicted, or Admitted to Prison (%)	65.2	66.7	-1.4	0.260

NOTES: Sample size are as follows:

　　　Arrest and conviction outcomes: 2,165 (program group) and 1,382 (control group).
　　　Prison outcomes: 1,873 (program group) and 1,194 (control group).
　　　Jail outcomes: 182 (program group) and 120 (control group).
　　　Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1

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TABLE IV-2:

Third-Year Impacts on Recidivism, Administrative Data, Full Sample

	Program	Control	Difference	P-value
Arrested (%)	26.9	25.4	1.5	0.715
Convicted of a Crime (%)	18.2	15.1	3.1	0.040**
Convicted of a Felony (%)	9.3	7.3	2.0	0.062*
Convicted of a Misdemeanor (%)	7.1	6.5	0.6	0.773
Convicted of a Violent Crime (%)	2.3	2.5	-0.2	0.586
Convicted of a Property Crime (%)	5.5	4.6	1.0	0.293
Convicted of a Drug Crime (%)	5.6	4.4	1.2	0.198
Convicted of a Public Order Crime (%)	6.8	4.6	2.2	0.016**
Admitted to Prison (%)	11.6	11.2	0.4	0.743
Admitted to Prison for a New Crime (%)	6.4	6.7	-0.3	0.743
Admitted to Prison for a Parole/Probation Violation (%)	3.5	2.8	0.8	0.226
Admitted to Prison for Other Reason/Reason Unknown (%)	1.2	1.1	0.1	0.884
Total Days Incarcerated in Prison (#)	43.5	46.2	-2.7	0.453
Admitted to Jail (%)	37.4	33.5	3.8	0.385
Admitted to Jail for a New Crime (%)	12.8	9.5	3.3	0.334
Admitted to Jail for a Parole/Probation Violation (%)	15.9	12.5	3.4	0.563
Admitted to Jail for Other Reason (%)	19.0	21.2	-2.2	0.649
Total Days Incarcerated in Jail (#)	44.0	33.4	10.6	0.180
Arrested, Convicted, or Admitted to Prison (%)	38.6	36.9	1.7	0.638

NOTES: Sample size are as follows:

   Arrest and conviction outcomes: 2,165 (program group) and 1,382 (control group).
   Prison outcomes: 1,873 (program group) and 1,194 (control group).
   Jail outcomes: 182 (program group) and 120 (control group).
   Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1

As discussed in the Two-Year Impact Report, it is possible that the RExO program might have *delayed* recidivism without affecting overall rates of recidivism—this could happen, for example, if program and control group members were re-arrested at the same aggregate rates, but program group members' arrests occurred later in the observation period. If true, this would be an important finding, because such delays would generate cost savings related to the administration of justice. No differences were found in the analysis presented in the Two-Year Impact Report in the *failure curves* for re-arrest and reincarceration in state prison—i.e., the percentage of program and control group members who experienced either outcome, month-by-month, in the first two years of the analysis period. Figures IV-1 and IV-2 below extend those

Figure IV-1:
Failure Curves for Arrests, Administrative Data
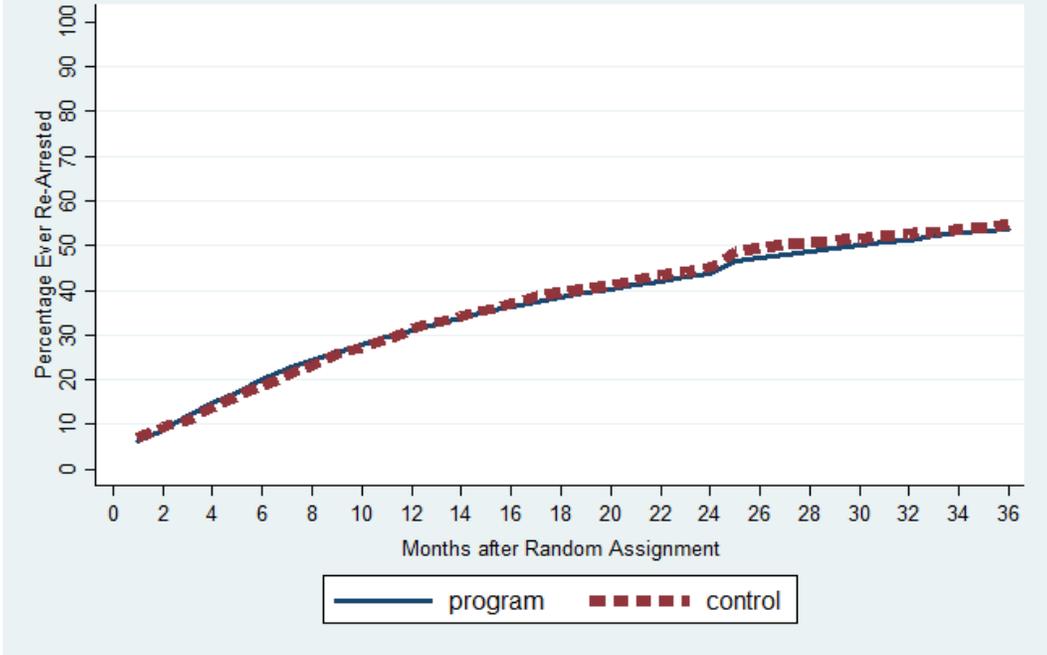
Figure IV-2:
Failure Curves for State Prison Incarceration, Administrative Data
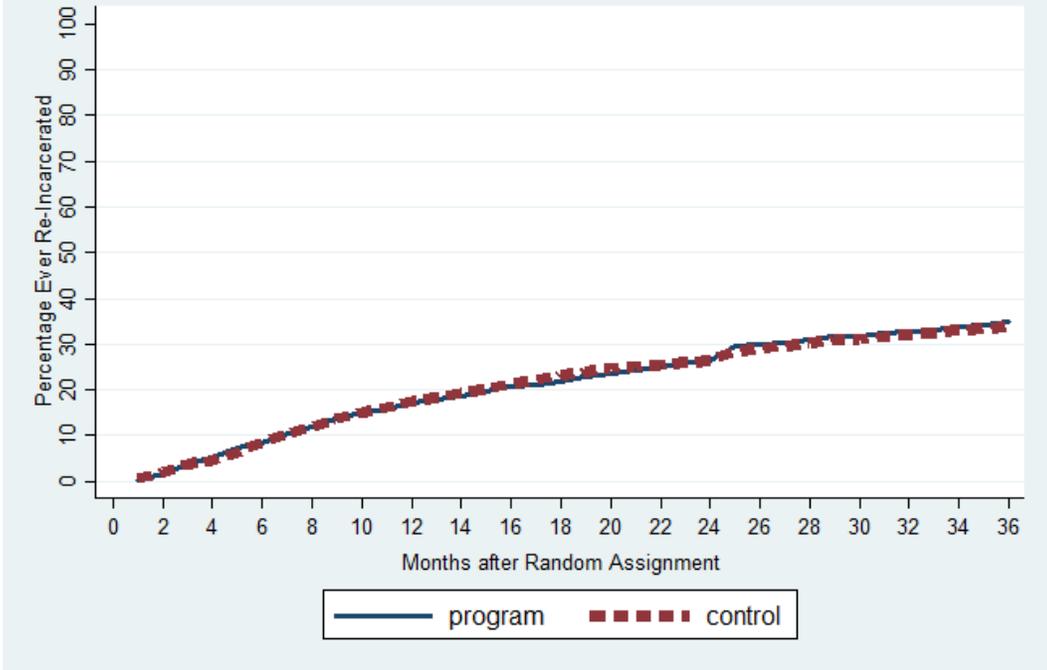
analyses to include the third year of data. The results are qualitatively identical to those reported in the Two-Year Impact Report. Specifically, for every month in the three-year study period, for

both arrest and prison reincarceration, outcomes for program and control group members are statistically indistinguishable.

Impacts for Subgroups

This section describes the results of analyses of impacts by subgroup. These analyses used the same subgroup partitions described above, including those based on: age (27 years and older vs. under 27); gender; education (no high school degree or GED vs. GED only vs. high school diploma or higher); number of prior convictions (three or fewer vs. four or more); timing of program assignment relative to October 2010 (enrolled in the study before October 1 vs. on or after October 1); and timing of participant RA relative to release (randomly assigned before release or within 90 days of release vs. randomly assigned 90 or more days after release); the primary emphasis of the grantee's program model (a "work-first approach vs. first providing stabilizing and supportive services) and the level of screening that the grantee used prior to enrollment (limited screening vs. more intensive screening). The results of these analyses are shown in Tables IV-3 through IV-10.

A review of these results indicates some instances of significant differences in RExO's impacts by subgroup. These results were derived from models in which the treatment indicator was interacted with an indicator for subgroup membership; the test on differences in impacts is the test on the coefficient of the interaction term. For participants younger than 27 years (Table IV-3), RExO participation led to higher rates of re-arrest, reconviction, and the composite measure of any arrest, conviction, or incarceration in prison. For example, 33.3 percent of program group members who were younger than 27 years old were re-arrested in the three-year follow-up period, compared with 25.3 percent of control group members in the same age group. The difference was more pronounced for the conviction outcome: 22.6 percent of program group members in this age group were convicted in the follow-up period, compared with only 13.1 percent of control group members. Both differences were statistically significant; however, as discussed in the next section, neither finding was corroborated in the analysis of survey data.

There was statistically significant variation in the effect on the total days incarcerated in prison relative to participant education level. The estimated impacts were -8.7 days for those without a high school degree, -3.3 days for those with a GED, and +10.2 days for those with a high school degree or greater level of education. None of these individual estimates were themselves statistically significant, however, and the overall impact estimate for this outcome—i.e., the impact for the full sample—was also not significant. As such, there is no compelling evidence to suggest that RExO was more or less effective depending on participant education level.

TABLE IV-3:
Three-Year Impacts on Recidivism, Administrative Data, by Age

	Younger than 27 Years				27 Years and Older			
	Program	**Control**	**Difference**	**P-value**	**Program**	**Control**	**Difference**	**P-value**
Arrested (%)$^{\neq}$	33.3	25.3	8.1	0.038**	25.3	25.4	-0.2	0.449
Convicted of a Crime (%)$^{\neq}$	22.6	13.1	9.6	0.002***	17.1	15.7	1.4	0.509
Admitted to Prison (%)	12.4	8.7	3.7	0.189	11.5	11.8	-0.4	0.757
Admitted to Prison for a New Crime (%)	7.8	6.1	1.7	0.476	6.4	7.1	-0.7	0.488
Admitted to Prison for a Parole/Probation Violatio (%)	3.8	2.6	1.2	0.528	3.4	2.8	0.6	0.338
Total Days Incarcerated in Prison (#)	59.7	64.3	-4.6	0.268	39.6	41.8	-2.3	0.531
Arrested, Convicted, or Admitted to Prison (%)$^{\neq}$	46.6	36.5	10.1	0.017**	36.5	37.0	-0.5	0.429

NOTES: For outcomes marked with an inequality sign ($\neq$), the differences between subgroup effects in an interacted model were statistically significant.

Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1

Sample size are as follows:

Arrest and conviction outcomes: 2,827 (age 27+) and 720 (less than 27).

Prison outcomes: 2,472 (age 27+) and 595 (less than 27).

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TABLE IV-4:
Three-Year Impacts on Recidivism, Administrative Data, by Number of Prior Convictions

	3 or Fewer Prior Convictions				4 or More Prior Convictions			
	Program	**Control**	**Difference**	**P-value**	**Program**	**Control**	**Difference**	**P-value**
Arrested (%)	18.1	17.8	0.3	0.723	34.9	32.9	1.9	0.519
Convicted of a Crime (%)	11.4	9.0	2.5	0.110	24.4	21.3	3.1	0.179
Admitted to Prison (%)	9.3	8.9	0.4	0.809	13.6	13.4	0.2	0.932
Admitted to Prison for a New Crime (%)	5.2	5.3	-0.1	0.892	7.4	8.0	-0.6	0.659
Admitted to Prison for a Parole/Probation Violation (%)	3.1	2.6	0.6	0.492	3.8	2.9	0.9	0.304
Total Days Incarcerated in Prison (#)	34.5	37.7	-3.3	0.435	51.2	54.2	-3.1	0.656
Arrested, Convicted, or Admitted to Prison (%)	29.4	27.8	1.6	0.714	45.9	45.1	0.8	0.826

NOTE: Statistical significance levels are indicated as follows: *** = $p < .01$; ** = $p < .05$; * = $p < .1$
      Sample size are as follows:

        Arrest and conviction outcomes: 1,723 (3 or fewer convictions) and 1,824 (4+ convictions).
        Prison outcomes: 1,442 (3 or fewer convictions) and 1,625 (4+ convictions).

TABLE IV-5:
Three-Year Impacts on Recidivism, Administrative Data, by Gender

	Female				Male			
	Program	**Control**	**Difference**	**P-value**	**Program**	**Control**	**Difference**	**P-value**
Arrested (%)	23.3	17.5	5.8	0.186	27.6	26.9	0.7	0.840
Convicted of a Crime (%)	16.9	12.4	4.5	0.211	18.5	15.6	2.8	0.099*
Admitted to Prison (%)	7.2	4.2	3.0	0.170	12.6	12.6	0.0	0.948
Admitted to Prison for a New Crime (%)	3.3	3.1	0.2	0.924	7.1	7.4	-0.3	0.740
Admitted to Prison for a Parole/Probation Violation (%)	2.2	1.3	0.9	0.372	3.9	3.1	0.8	0.283
Total Days Incarcerated in Prison (#)	16.5	22.2	-5.7	0.329	49.4	50.8	-1.4	0.641
Arrested, Convicted, or Admitted to Prison (%)	31.3	27.4	4.0	0.404	40.1	38.6	1.4	0.846

NOTE: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1

Sample size are as follows:

Arrest and conviction outcomes: 588 (female) and 2,959 (male).
Prison outcomes: 526 (female) and 2,541 (male).

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TABLE IV-6:

Three-Year Impacts on Recidivism, Administrative Data, by Timing of Random Assignment

(Relative to Program Schedule)

	Pre-October Assignment				Post-October Assignment			
	Program	**Control**	**Difference**	**P-value**	**Program**	**Control**	**Difference**	**P-value**
Arrested (%)	27.3	25.7	1.6	0.810	25.9	24.6	1.3	0.765
Convicted of a Crime (%)	19.1	16.1	3.1	0.097*	16.0	13.0	3.0	0.260
Admitted to Prison (%)	12.1	11.8	0.3	0.891	10.7	10.1	0.6	0.758
Admitted to Prison for a New Crime (%)	6.8	7.6	-0.8	0.495	5.5	4.8	0.7	0.675
Admitted to Prison for a Parole/Probation Violation (%)	3.6	2.9	0.7	0.410	3.4	2.5	0.9	0.339
Total Days Incarcerated in Prison (#)	44.4	46.7	-2.3	0.544	41.7	45.1	-3.5	0.672
Arrested, Convicted, or Admitted to Prison (%)	40.6	39.3	1.3	0.858	34.2	32.1	2.1	0.532

NOTE: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1

Sample size are as follows:

Arrest and conviction outcomes: 2,461 (pre-October) and 1,086 (post-October).
Prison outcomes: 2,056 (pre-October) and 1,011 (post-October).

TABLE IV-7:

Three-Year Impacts on Recidivism, Administrative Data, by Timing of Random Assignment

(Relative to Release from Prison)

	Early Assignment				Late Assignment			
	Program	**Control**	**Difference**	**P-value**	**Program**	**Control**	**Difference**	**P-value**
Arrested (%)	27.1	26.2	0.9	0.995	26.1	23.2	2.9	0.485
Convicted of a crime (%)	18.3	14.9	3.4	0.059*	17.8	15.7	2.1	0.381
Admitted to prison (%)	11.6	11.2	0.4	0.838	11.9	11.4	0.5	0.680
Admitted to Prison for a New Crime (%)	6.3	6.6	-0.3	0.786	6.7	7.0	-0.3	0.996
Admitted to Prison for a Parole/Probation Violation (%)	3.2	2.8	0.4	0.536	4.4	2.6	1.8	0.148
Total Days Incarcerated in Prison (#)	46.6	48.0	-1.4	0.692	34.5	41.5	-7.1	0.340
Arrested, Convicted, or Admitted to Prison (%)	38.7	38.1	0.6	0.950	38.2	34.0	4.2	0.303

NOTE: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1
       Sample size are as follows:

         Arrest and conviction outcomes: 2,624 (early assignment) and 923 (late assignment).
         Prison outcomes: 2,224 (early assignment) and 823 (late assignment).

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TABLE IV-8:

Three-Year Impacts on Recidivism, Administrative Data, by Random Assignment Model

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	Concurrent RA				RA after Screening			
	Program	**Control**	**Difference**	**P-value**	**Program**	**Control**	**Difference**	**P-value**
Arrested (%)	28.2	25.9	2.3	0.355	24.4	24.4	0.1	0.651
Convicted of a Crime (%)	20.5	16.2	4.3	0.021**	13.9	13.0	0.9	0.676
Admitted to Prison (%)	11.5	11.3	0.2	0.920	11.9	11.0	0.9	0.545
Admitted to Prison for a New Crime (%)	5.9	5.7	0.2	0.834	7.4	8.7	-1.3	0.507
Admitted to Prison for a Parole/Probation Violation (%)	3.0	2.9	0.1	0.912	4.7	2.6	2.1	0.045**
Total Days Incarcerated in Prison (#)	41.5	44.1	-2.6	0.580	47.5	50.5	-3.0	0.599
Arrested, Convicted, or Admitted to Prison (%)	39.2	37.0	2.2	0.405	37.3	36.8	0.5	0.840

NOTE: Statistical significance levels are indicated as follows: *** = $p < .01$; ** = $p < .05$; * = $p < .1$
   Sample size are as follows:

   Arrest and conviction outcomes: 2,314 (concurrent) and 1,233 (enrollment after screening).
   Prison outcomes: 2,053 (concurrent) and 1,014 (enrollment after screening).

TABLE IV-9:
Three-Year Impacts on Recidivism, Administrative Data, by Program Emphasis

	Employment				Supportive Services			
	Program	**Control**	**Difference**	**P-value**	**Program**	**Control**	**Difference**	**P-value**
Arrested (%)	26.5	25.8	0.7	0.976	27.6	24.8	2.9	0.660
Convicted of a Crime (%)	18.7	16.8	1.9	0.326	17.4	12.4	5.0	0.045**
Admitted to Prison (%)	12.5	12.7	-0.1	0.938	9.8	8.4	1.4	0.364
Admitted to Prison for a New Crime (%)	6.8	7.9	-1.1	0.394	5.6	4.4	1.2	0.441
Admitted to Prison for a Parole/Probation Violation (%)	4.0	3.5	0.4	0.467	2.6	1.2	1.3	0.109
Total Days Incarcerated in Prison (#)	42.6	46.5	-3.9	0.399	45.3	45.5	-0.3	0.994
Arrested, Convicted, or Admitted to Prison (%)	39.1	38.9	0.3	0.984	37.6	33.7	3.9	0.501

NOTE: Statistical significance levels are indicated as follows: *** = $p < .01$; ** = $p < .05$; * = $p < .1$

   Sample size are as follows:

   Arrest and conviction outcomes: 2,267 (employment focused) and 1,280 (supportive services).
   Prison outcomes: 2,040 (employment focused) and 1,027 (supportive services).

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TABLE IV-10:

Three-Year Impacts on Recidivism, Administrative Data, by Educational Attainment

	Program	Control	Difference	P-value
No GED/HS Diploma				
Arrested (%)	27.2	25.9	1.4	0.956
Convicted of a Crime (%)	17.9	14.8	3.0	0.134
Admitted to Prison (%)	11.6	11.3	0.3	0.760
Admitted to Prison for a New Crime	6.3	7.2	-0.8	0.568
Admitted to Prison for a Parole/Probation Violation (%)	3.3	2.2	1.2	0.136
Total Days Incarcerated in Prison (#)$^{\neq}$	42.3	50.9	-8.7	0.159
Arrested, Convicted, or Admitted to Prison (%)	37.3	36.9	0.4	0.978
GED				
Arrested (%)	29.6	28.5	1.1	0.931
Convicted of a Crime (%)	20.3	18.6	1.7	0.631
Admitted to Prison (%)	12.4	13.0	-0.6	0.837
Admitted to Prison for a New Crime (%)	7.2	7.6	-0.4	0.880
Admitted to Prison for a Parole/Probation Violation (%)	3.4	3.8	-0.4	0.792
Total Days Incarcerated in Prison (#)$^{\neq}$	49.4	52.8	-3.3	0.794
Arrested, Convicted, or Admitted to Prison (%)	42.1	41.0	1.1	0.911
HS Diploma or Higher				
Arrested (%)	22.8	20.8	2.1	0.762
Convicted of a Crime (%)	16.2	11.5	4.7	0.140
Admitted to Prison (%)	10.7	8.6	2.0	0.380
Admitted to Prison for a New Crime	5.5	4.5	0.9	0.593
Admitted to Prison for a Parole/Probation Violation (%)	4.0	2.6	1.4	0.321
Total Days Incarcerated in Prison (#)$^{\neq}$	37.3	27.0	10.2	0.199
Arrested, Convicted, or Admitted to Prison (%)	36.3	31.3	5.0	0.430

NOTES: For outcomes marked with an inequality sign ($\neq$), the differences between subgroup effects in a fully interacted model were statistically significant.

Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1

Sample size are as follows:

Arrest and conviction outcomes: 1,618 (no HS), 1,062 (GED) and 867 (HS degree or higher).
Prison outcomes: 1,396 (no HS), 983 (GED) and 688 (HS degree or higher).

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Impacts Based on Survey Data

Assessing the effect of RExO on recidivism using administrative data, as described above, has some limitations. Because some states in which RExO operated did not provide data, outcomes for some participants were not observed. In addition, for each ex-offender, data were retrieved only from the specific state in which RA occurred. Thus, participant recidivism in states other than the state of RA was not measured. For these reasons, results generated using the administrative data may not provide a complete picture of recidivism among study participants, or of the impact of the RExO program on recidivism. Fortunately, data from the follow-up survey present an alternative means of measuring recidivism and the impact of RExO that may help to overcome some of the limitations of the administrative data.

This section describes the results of analyses of the effect of the RExO program on criminal justice outcomes using measures constructed from the follow-up survey of study participants. The survey (described more fully in Chapter I) was first administered approximately two years after RA, and the summary of impact estimates from that survey was presented in the Two-Year Impact Report. The survey was administered a second time, approximately three years following RA; impact estimates from that survey are summarized in this report.

The sampling frame for the survey included all 4,655 study participants. Ultimately, 2,995 individuals completed the three-year survey, yielding a response rate of 64.3 percent. This represented 83.6 percent of those who responded to the two-year survey (N=3,581).

Impacts for the Full Sample

Table IV-11 shows the three-year impacts of the RExO program on measures of recidivism drawn from the self-reported survey responses. Although the survey asked a wide range of questions about recidivism, the analysis in this section describes a subset of these questions that mirror as much as possible the outcomes used in the administrative data analysis described above.

Analysis of participant self-reported recidivism produces a conclusion that is generally consistent with the findings of both the Two-Year Impact Report and the analysis of three-year administrative data: RExO did not appear to significantly affect ex-offender recidivism. For most measures of recidivism, there were no significant differences between the program and control groups.

The results from the full sample analysis of survey data are slightly at odds with the administrative data analysis in one respect, however: In the survey data, there is a difference between the program and control groups in arrest rates following RA. This difference was also apparent in the analysis of two-year survey data. As the first row of Table IV-11 shows, 42.7 percent of the individuals in the program group and 46.6 percent of the individuals in the control group reported being arrested within the three-year period following random assignment; this difference was statistically significant (p = 0.026). One possible explanation for the differential results between survey and administrative data is that the survey data include participants from every grantee, and thus are not missing data from any of the states in which RExO operated. It is therefore possible that a significant effect is being identified as a result of this shift in coverage.

A more likely explanation, however, is bias in the survey responses. In the Two-Year Impact Report, the study team described a test of this hypothesis in which the administrative data and the survey data were linked in order to identify cases in which the two datasets did not agree. That analysis revealed that program group members who had been re-arrested in the study period were more likely to misreport that status (i.e., to report that they had not been re-arrested) than were control group members. This suggested that the statistically significant differences in post-RA arrest rates (as measured by the two-year survey data) were most likely an artifact of differential recall, social desirability bias, or survey nonresponse bias.

A replication of that analysis with the three-year data reaches an identical conclusion: program group members with "true" arrests in the administrative data were less likely than control group members with "true" arrests to report having been arrested in the survey. While the difference is not statistically significant, it does suggest that the small but statistically significant effects observed in the survey arrest data are at least partly the result of recall or social desirability bias. This diminishes support for the hypothesis that this is a genuine impact of RExO. Thus, as in the analysis of administrative data, the overall conclusion of the analysis of three-year survey data is that there is no clear support for the hypothesis that the RExO program affected recidivism.

TABLE IV-11:
Program and Control Group Means for Key Criminal Justice Market Outcomes, Three-Year Survey Data

	Program	Control	Difference (Impact)	Hazard Ratio (Impact)	P-Value
Arrested (%)	42.7	46.6	-3.9		0.026**
Survival analysis: time to first arrest				0.930	0.151
Prison Admission (%)	47.2	49.6	-2.4		0.168
Total Days Incarcerated (#)	152.5	157.0	-4.5		0.569
Total Days Incarcerated, Excluding Those with No Incarceration (#)[a]	335.2	327.7	7.5		0.706

NOTES: Sample sizes are 1,814 (program group) and 1,145 (control group).

Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1

[a] Results for this outcome are calculated only for study participants who were incarcerated following RA. Because post-RA incarceration is itself correlated with treatment status, the experimental design no longer guarantees equivalence between treatment and control groups within this subset. This result should therefore be interpreted as suggestive rather than as a true impact estimate.

Impacts for Subgroups

As with the analysis of recidivism based on administrative data, the survey data analysis also examined impact analyses across subgroups. Tables IV-12 through IV-19 display results for the eight subgroups described in Chapter I.

As can be seen in these tables, the finding from the full survey sample that arrest rates are slightly lower for program group members than for control group members persists in these subgroup analyses. These tables also make clear that the levels of recidivism vary qualitatively across the different subgroups. For example, consistent with prior research on offenders, arrest and incarceration rates for those ages 27 and older are much lower than for those under 27 years of age (a difference of 10 to 20 percentage points).

These relative differences in levels of recidivism across subgroups do not, however, indicate that RExO had a differential impact on the subgroups. None of the estimates of subgroup differences in impacts were statistically significant in a fully interacted model; this was true for all outcomes and for all of the subgroup categories. Additionally, the finding from the analysis of administrative data above—that younger offenders in the program group were more likely to recidivate than were younger offenders in the control group—is not replicated in the analysis of survey data.

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Table IV-12:

Impacts on Criminal Justice Outcomes, Three-Year Survey Data, by Age

	Younger than 27					Older than 27				
	Program	Control	Difference (Impact)	Hazard Ratio (Impact)	P-value	Program	Control	Difference (Impact)	Hazard Ratio (Impact)	P-value
Arrested (%)	57.5	58.5	-1.0		0.823	39.2	43.9	-4.7		0.018**
Survival analysis: time to first arrest				1.004	0.972				0.909	0.100
Prison Admission (%)	64.2	60.2	3.9		0.324	43.2	47.2	-4.0		0.046**
Total Days Incarcerated (#)	229.2	227.2	1.9		0.920	134.3	141.3	-7.0		0.441
Total Days Incarcerated, Excluding Those with No Incarceration (#)[a]	366.6	383.9	-17.4		0.761	324.0	311.4	12.7		0.467

NOTES: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1

Sample size are as follows:

    Arrest and conviction outcomes: 2,398 (age 27+) and 561 (less than 27).

    Prison outcomes: 2,410 (age 27+) and 561 (less than 27).

[a] Results for this outcome are calculated only for study participants who were incarcerated following RA. Because post-RA incarceration is itself correlated with treatment status, the experimental design no longer guarantees equivalence between treatment and control groups within this subset. This result should therefore be interpreted as suggestive rather than as a true impact estimate.

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Table IV-13:

Impacts on Criminal Justice Outcomes, Survey Data, by Number of Prior Convictions

	3 or Fewer					4 or More				
	Program	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**	**Program**	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**
Arrested (%)	40.2	41.0	-0.7		0.819	45.6	51.9	-6.3		0.020**
Survival analysis: time to first arrest				1.022	0.784				0.867	0.053*
Prison Admission (%)	44.2	43.7	0.6		0.816	50.6	55.7	-5.1		0.057*
Total Days Incarcerated (#)	133.3	144.8	-11.5		0.428	171.7	172.2	-0.6		0.890
Total Days Incarcerated, Excluding Those with No Incarceration (#)[a]	315.1	340.5	-25.4		0.306	349.7	323.7	26.0		0.260

NOTES: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1
Sample size are as follows:
Arrest and conviction outcomes: 1,300 (3 or fewer) and 1,244 (4+ convictions).
Prison outcomes: 1,306 (3 or fewer) and 1,248 (4+ convictions).

[a] Results for this outcome are calculated only for study participants who were incarcerated following RA. Because post-RA incarceration is itself correlated with treatment status, the experimental design no longer guarantees equivalence between treatment and control groups within this subset. This result should therefore be interpreted as suggestive rather than as a true impact estimate.

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Table IV-14:

Impacts on Criminal Justice Outcomes, Survey Data, by Gender

	Female					Male				
	Program	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**	**Program**	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**
Arrested (%)	31.9	33.6	-1.7		0.690	45.7	50.4	-4.7		0.021**
Survival analysis: time to first arrest				0.939	0.633				0.927	0.167
Prison Admission (%)	36.6	38.6	-2.0		0.619	50.3	52.9	-2.7		0.183
Total Days Incarcerated (#)	76.7	99.2	-22.5		0.189	172.7	174.2	-1.5		0.841
Total Days Incarcerated, Excluding Those with No Incarceration (#)[a]	218.2	262.3	-44.1		0.204	356.4	341.5	14.9		0.380

NOTES: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1

Sample size are as follows:

Arrest and conviction outcomes: 637 (female) and 2,322 (male).

Prison outcomes: 641 (female) and 2,330 (male).

[a] Results for this outcome are calculated only for study participants who were incarcerated following RA. Because post-RA incarceration is itself correlated with treatment status, the experimental design no longer guarantees equivalence between treatment and control groups within this subset. This result should therefore be interpreted as suggestive rather than as a true impact estimate.

Table IV-15:

Impacts on Criminal Justice Outcomes, Survey Data, by Timing of RA (Relative to Program Schedule)

	Pre-October Assignment					Post-October Assignment				
	Program	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**	**Program**	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**
Arrested (%)	43.5	47.0	-3.6		0.065*	40.5	45.4	-4.9		0.223
Survival analysis: time to first arrest				0.935	0.258				0.927	0.439
Prison Admission (%)	47.5	49.1	-1.6		0.377	46.4	50.7	-4.4		0.343
Total Days Incarcerated (#)	154.4	163.7	-9.3		0.317	147.3	140.9	6.4		0.555
Total Days Incarcerated, Excluding Those with No Incarceration (#)[a]	337.6	347.3	-9.8		0.577	328.7	283.2	45.5		0.145

NOTES: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1.

Sample size are as follows:

Arrest and conviction outcomes: 2,137 (pre-October) and 822 (post-October).

Prison outcomes: 2,145 (pre-October) and 826 (post-October).

[a] Results for this outcome are calculated only for study participants who were incarcerated following RA. Because post-RA incarceration is itself correlated with treatment status, the experimental design no longer guarantees equivalence between treatment and control groups within this subset. This result should therefore be interpreted as suggestive rather than as a true impact estimate.

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Table IV-16:

Impacts on Criminal Justice Outcomes, Survey Data, by Timing of RA (Relative to Release from Prison)

	Early Assignment					Late Assignment				
	Program	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**	**Program**	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**
Arrested (%)	44.1	48.0	-3.9		0.049**	37.9	45.3	-7.4		0.154
Survival analysis: time to first arrest				0.923	0.157				0.910	0.420
Prison Admission (%)	48.4	50.7	-2.3		0.223	43.8	48.3	-4.5		0.570
Total Days Incarcerated (#)	159.9	168.6	-8.7		0.404	126.7	130.6	-3.9		0.864
Total Days Incarcerated, Excluding Those with No Incarceration (#)[a]	343.9	342.6	1.3		0.926	296.2	285.2	11.0		0.780

NOTES: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1
Sample size are as follows:
Arrest and conviction outcomes: 2,309 (early assignment) and 592 (late assignment).
Prison outcomes: 2,316 (early assignment) and 596 (late assignment).
[a] Results for this outcome are calculated only for study participants who were incarcerated following RA. Because post-RA incarceration is itself correlated with treatment status, the experimental design no longer guarantees equivalence between treatment and control groups within this subset. This result should therefore be interpreted as suggestive rather than as a true impact estimate.

Table IV-17:

Impacts on Criminal Justice Outcomes, Survey Data, by Random Assignment Model

	Concurrent Enrollment					Enrollment after Screening				
	Program	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**	**Program**	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**
Arrested (%)	39.8	46.1	-6.4		0.007***	48.0	47.4	0.6		0.978
Survival analysis: time to first arrest				0.888	0.064*				1.010	0.900
Prison Admission in Three Years Following RA(%)	45.2	49.5	-4.2		0.087*	50.8	49.8	1.0		0.906
Total Days Incarcerated (#)	142.7	159.0	-16.3		0.189	170.3	153.4	16.9		0.458
Total Days Incarcerated, Excluding Those with No Incarceration (#)[a]	328.6	335.6	-7.0		0.797	345.8	313.9	31.9		0.394

NOTES: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1

    Sample size are as follows:

        Arrest and conviction outcomes: 1,911 (concurrent) and 1,048 (enrollment after screening).

        Prison outcomes: 1,921 (concurrent) and 1,050 (enrollment after screening).

[a] Results for this outcome are calculated only for study participants who were incarcerated following RA. Because post-RA incarceration is itself correlated with treatment status, the experimental design no longer guarantees equivalence between treatment and control groups within this subset. This result should therefore be interpreted as suggestive rather than as a true impact estimate.

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Table IV-18:

Impacts on Criminal Justice Outcomes, Survey Data, by Program Emphasis

	Employment					Supportive Services				
	Program	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**	**Program**	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**
Arrested in Three Years Following RA (%)	40.8	45.8	-5.0		0.014**	46.6	48.1	-1.5		0.814
Survival analysis: time to first arrest				0.897	0.081*				1.008	0.923
Prison Admission in Three Years Following RA(%)	45.1	47.9	-2.8		0.139	51.6	53.0	-1.3		0.834
Total Days Incarcerated (#)	153.3	159.4	-6.0		0.496	150.8	152.0	-1.3		0.990
Total Days Incarcerated, Excluding Those with No Incarceration (#)[a]	347.9	341.3	6.6		0.815	311.2	301.4	9.8		0.720

NOTES: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1

Sample size are as follows:

Arrest and conviction outcomes: 1,964 (employment focused) and 995 (supportive services).

Prison outcomes: 1,975 (employment focused) and 996 (supportive services).

[a] Results for this outcome are calculated only for study participants who were incarcerated following RA. Because post-RA incarceration is itself correlated with treatment status, the experimental design no longer guarantees equivalence between treatment and control groups within this subset. This result should therefore be interpreted as suggestive rather than as a true impact estimate.

Table IV-19:

Impacts on Criminal Justice Outcomes, Survey Data, by Educational Attainment

	Program	Control	Difference (Impact)	Hazard Ratio (Impact)	P-Value
No GED/HS Degree					
Arrested in Three Years Following RA (%)	42.9	51.0	-8.1		0.007***
Survival analysis: time to first arrest				0.843	0.027**
Prison Admission in Three Years Following RA(%)	47.6	53.6	-6.0		0.058*
Total Days Incarcerated (#)	163.7	178.3	-14.5		0.549
Total Days Incarcerated, Excluding Those with No Incarceration (#)[a]	359.6	347.8	11.9		0.509
GED					
Arrested in Three Years Following RA (%)	50.8	49.4	1.4		0.817
Survival analysis: time to first arrest				1.092	0.334
Prison Admission in Three Years Following RA(%)	54.6	52.4	2.2		0.647
Total Days Incarcerated (#)	174.2	171.3	2.9		0.975
Total Days Incarcerated, Excluding Those with No Incarceration (#)[a]	327.2	332.5	-5.2		0.633
HS Degree+					
Arrested in Three Years Following RA (%)	35.3	38.6	-3.2		0.200
Survival analysis: time to first arrest				0.883	0.213
Prison Admission in Three Years Following RA(%)	40.2	42.1	-2.0		0.457
Total Days Incarcerated (#)	118.0	118.2	-0.2		0.795
Total Days Incarcerated, Excluding Those with No Incarceration (#)[a]	305.2	290.5	14.7		0.806

NOTES: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1
Sample size are as follows:

Arrest and conviction outcomes: 1,265 (no HS), 781 (GED) and 913 (HS degree or higher).
Prison outcomes: 1,271 (no HS), 783 (GED) and 917 (HS degree or higher).

[a] Results for this outcome are calculated only for study participants who were incarcerated following RA. Because post-RA incarceration is itself correlated with treatment status, the experimental design no longer guarantees equivalence between treatment and control groups within this subset. This result should therefore be interpreted as suggestive rather than as a true impact estimate.

Summary

The analyses of the effect of RExO participation on participant criminal justice outcomes presented in this chapter yield an identical conclusion to that of the Two-Year Impact Report: There is little support for the hypothesis that the RExO program reduced recidivism among participants. With the exception of an isolated finding in the subgroup analyses (that younger program group members who participated in RExO were *more* likely to recidivate than were control group members in the same age group), the administrative data provided no evidence whatsoever of any impacts of RExO. The survey data suggested a possible effect on arrest rates,

but no effect on any other measure of recidivism. Subsequent analyses linking the survey and administrative data indicate, however, that the most likely explanation for the difference in reported arrest rates is some form of reporting bias, rather than a true program impact. Although the two-year analysis of survey data suggested that RExO delayed re-arrest to a greater degree among those with a high school diploma, that finding did not persist in either administrative data analysis (i.e., the two-year and three-year studies) or in the three-year survey data analysis. Thus, the overall conclusion of the analysis of criminal justice data is that there is no evidence that RExO had a real effect on participants' recidivism.

Glossary of Recidivism Outcomes

Admission to prison. Admission to state prison for any reason.

Admission to prison for a new crime. Admission to state prison with a new sentence following a conviction for a new crime.

Admission to prison for a technical parole violation. Admission to prison after a parolee has violated a condition of his or her parole from a previous incarceration. Conditions of parole may include reporting to a parole officer, abstaining from drugs and alcohol, participating in substance abuse treatment, attending anger management classes, or a number of other conditions. Depending on severity, a violation of one or more of these rules may lead to revocation of parole, resulting in a return to prison. Technical rule violations are not usually preceded by an arrest or conviction.

Arrests. Unsealed arrests. Depending on state rules for the sealing of arrest records, data may include arrests that did not lead to conviction.

Conviction. A disposition of a guilty verdict, whether by trial or plea. Some convictions may be related to arrests that occurred prior to random assignment.

Felony or misdemeanor convictions. Convictions with felony or misdemeanor charges. For each conviction date, only the charge with the highest class—in order of felony, misdemeanor, and other—is included.

Violent, property, drug, or public order convictions. Convictions with charges in the given crime category.[2] Crimes were categorized as follows:

- **Violent crime:** Homicide, manslaughter, kidnapping, sexual assault, robbery, assault, extortion, and other crimes against the person.
- **Property crime:** Arson, burglary, larceny, stolen vehicles, fraudulent activities, stolen property, damage to property, smuggling, and other property offenses.
- **Drug crime:** Drug trafficking, drug possession, and other drug offenses.
- **Public order crime:** Weapons offenses, traffic offenses, nonviolent sex offenses, obscenity, family offenses, commercialized sex offenses, obstructing the police or the judiciary, bribery, disturbing the public peace, invasion of privacy, and other public order crimes.

NOTES: Connecticut prison data included admissions to jail.

[2] Crimes were categorized based on the 1994 Bureau of Justice Statistics Special Report classifications (see Langan and Levin, 2002).

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V.   SUMMARY AND CONCLUSIONS

This report represents the culmination of a six-year effort to evaluate the RExO program. It describes the impacts of the program on employment, earnings, and recidivism in the three-year period after participants enrolled in the study, thereby updating and extending findings from the previously published Two-Year Impact Report.

The evaluation included 24 RExO grantees operating in 18 states. A total of 4,655 participants enrolled in the study; approximately 60 percent (N=2,804) of those were assigned to the program group and 40 percent (N=1,851) were assigned to the control group.

The evaluation team followed these individuals for a three-year period following enrollment into the study, using three primary types of data to measure outcomes: (1) a telephone survey of study participants; (2) data from the NDNH; and (3) administrative data on criminal justice outcomes obtained from the states in which RExO grantees operated.

Primary Results

The results of the study, described in greater detail in earlier chapters, provide little evidence that RExO had any impact on employment, earnings, or recidivism. Across both the survey and administrative data, and across several different measures for each key outcome of interest, there were no differences between program and control group members.

The few isolated differences between program and control groups that did emerge largely occurred in the self-reported survey data and not in the administrative data. Even in those cases, the differences were so infrequent that they could be expected by random chance alone, rather than because of any actual effect of the program.[53] Further, where self-reported data can be compared with objective administrative data (which is possible for many of the recidivism

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[53]   The primary results for employment and criminal justice outcomes from the administrative and survey data sources (Tables III-1, III-9, IV-1, and IV-11) contain a total of 45 statistical tests. With $\alpha = 0.10$, the number of significant results to be expected by chance alone is 4.5. Five such results were observed.

measures), the difference between results from the two different datasets appears to be driven by biased recall or reporting on the part of program group members. Specifically, program group members whose administrative data showed an arrest, conviction, or incarceration, were more likely than control group members to under-report such incidents in the survey data.

A similar comparison is not possible with the administrative and survey-based employment outcomes, because NDNH data cannot be re-merged with other data sources. This leaves open the possibility that the survey-data based finding of a small but significant impact on employment outcomes is in fact a true result being driven by jobs not covered by NDNH (e.g. self-employment or "under the table" work). However given the observed tendency of program group members to misreport arrest data more frequently than control group members, an equally plausible explanation for the positive results in the survey-derived employment outcomes is some form of bias.

These findings are largely consistent with those presented in the Two-Year Impact Report, which found no differences in recidivism between the two groups, but did find some small impacts on employment and total income. As in the current report, the earlier report demonstrated that the few impacts that were identified in recidivism after two years in the study were driven by some form of bias in the survey data. Because the data were not yet available, the Two-Year Impact Report did not include analyses of NDNH administrative data on employment and earnings. The small impacts on labor market outcomes described in the earlier report were based on an analysis of self-report data alone. The present report cannot corroborate these earlier findings, because the NDNH data did not reveal any evidence of an impact of the program in either of the first two years after participants enrolled into the study, or in the fourth year after enrolling.[54]

Unlike the administrative data on criminal justice outcomes, the requirements for accessing NDNH data prevent post hoc matching with individual self-report data. As a result this report cannot definitively determine whether the small impacts observed for the survey-derived employment and total income outcomes (both here and in the Two-Year Impact Report) are the result of some form of bias (recall, reporting, or survey non-response). However, given the problems in self-reported data on recidivism, described above, it seems plausible that similar biases in the self-reported employment data are the cause of the small impacts described in the Two-Year Impact Report. In any case, the results based on administrative data on employment and earnings presented in this report clearly indicate that there is no evidence of an impact of RExO.

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[54]   As described in Chapters I and III, there is a gap in the available NDNH data, such that the study cannot assess employment in the third year after enrollment using these data.

In addition to the broader conclusion that there is no clear evidence of an impact of RExO on employment, earnings, or recidivism, there is also no evidence that different subgroups were impacted any differently by the program. Across eight different subgroups that were included in these analyses, different subgroups achieved different levels on given outcomes, but there were no consistent statistically significant differences in the impacts observed on these outcomes.

Conclusions

In addition to the overall lack of impacts of the program on the key outcomes of interest—described above—the evaluation is able to draw a number of additional conclusions about RExO. These include findings concerning the implementation of the program, its operation, and the challenges faced by participants, as well as about RExO's strategies to overcome these challenges. There are also potential lessons learned concerning why RExO had no observable impact on its participants.

This study provides a rigorous test of RExO's impact on employment and recidivism, because there was a clear contrast in the number of services received by program and control group members. Given the strong evidence, as described in the Two-Year Impact Report, that program group members received more services than control group members, there was a clear service contrast between program and control group members. The study thus provides a rigorous assessment of the extent to which these additional services had impacts on employment and recidivism.

The variety of services provided by grantees means that this is not an assessment of a single program. Given that the services offered by grantees and their partners varied substantially, and the point at which RA occurred varied across grantees as well, what constitutes "the program" varied across sites. While the analysis examined whether impacts varied based on the intensity of grantee screening or whether grantees used a "work-first" model, there were a number of other variations that could not be included in the analysis that may have led to the disappointing overall results. An alternative approach to the present study—where a particularly promising program model is identified and implemented consistently and with high intensity across all sites in the study, and then impacts are assessed—might yield different results. This would limit the theoretical possibility that positive results from sites with stronger programs might be watered down by including results for programs implementing weaker models or services.

The participants in this study—including both program and control group members—are not reflective of the "average" offender returning from prison or jail. The rates for each of the recidivism outcomes of interest (arrests, convictions, and incarceration) are noticeably lower and the rates of employment are somewhat higher among the RExO sample members as

compared to national averages for newly released ex-offenders.[55] This may be partially the result of the locations in which the grantees operated, but almost certainly also reflects the screening and eligibility criteria implemented for the study. Whether the program would have been more effective had it served a population more like the "average" ex-offender cannot be answered by this study.

RExO grantees were providing a wide array of services to their clients, but the services may not have been of sufficient duration or intensity to impact key outcomes. Program group members reported having received more services than did control group members across nearly all measures of service receipt. However, these services may have been insufficient to meet the broad array of needs that ex-offenders have. For example, nearly one-third of all survey respondents reported having been in a substance abuse treatment program at some point following RA, and one-fourth of all respondents reported physical health issues that limited their work or other activities. These and other challenges may have created serious barriers to employment and the attainment of other positive outcomes, but RExO grantees rarely provided services directly addressing them. It is quite possible that a more comprehensive and intensive approach that helps address a wide array of other issues would have greater impact for ex-offenders seeking to return to their communities.

This evaluation does not provide a strong test of whether employment-based programs lower one's likelihood of recidivating. Given that RExO had no significant impact on employment, the study does not provide a strong assessment of whether programs that actually increase employment affect recidivism. One can expect recidivism to be affected by employment-based programs only if they produce significant differences in employment rates. Thus, a full test of the impact of employment-based programs on ex-offender recidivism may require evaluation of a program that generates clear impacts on employment.

It is likely that additional services are needed for programs serving ex-offenders to meet the many needs of their participants. As described above, most RExO programs provided some form of work readiness training, mentoring, and case management and supportive services, though in many cases several of these services were not particularly intensive or long-lasting. But ex-offenders reported an array of additional challenges that may require services not frequently provided under RExO. Further, RExO staff reported that they did not have sufficient funds to address the many needs of their participants, and participants identified numerous services to which they had little or no access. Chief among these were housing services, which

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[55]   Durose et al. (2014)

ReXO was prohibited from paying for. It is possible that providing additional resources, including housing services, would yield impacts on employment and/or recidivism.

The lengthy recession that continued well into the follow-up period may have affected results. The economic recession that began in late 2007 officially ended in June 2009.[56] Its effects, particularly in the communities in which ReXO grantees were operating, have lasted much longer, however, and continued well into the follow-up period that is covered by this report. This may have led to greater difficulty for study participants in finding employment. While the RA design ensured that program and control group members faced similar economic circumstances, it is possible that the fact that there were so few jobs available led to lower overall employment outcomes, thereby depressing an impact of ReXO that might have been observed under better economic times.[57]

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[56] See http://www.nber.org/cycles.html

[57] The evidence of whether program effects differ based on economic characteristics is mixed. For example, Greenberg, Michalopoulos, and Robins (2003) found no difference in the magnitude of program impacts based on the unemployment rate, while Lechner and Wunsch (2009) found the size of program impacts did vary based on this factor. However, neither of these studies focused on ex-offenders, whose employment rate may, for a number of reasons, be more or less affected by the unemployment rate than the broader population.

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APPENDIX A: TECHNICAL APPENDIX—METHODS FOR DATA ANALYSIS

This technical appendix serves three purposes. First, it provides a detailed explanation of the methods that were used to estimate the effects of the RExO program on study participant outcomes, as described in the body of the report. Second, it describes an additional statistical technique, hierarchical linear modeling (HLM), which here serves to account for clustering at the program level. And third, it presents results from a series of sensitivity analyses that demonstrate how results vary depending on the choice of statistical model.[58]

Description of Methods Used

The experimental design of this study ensured that unbiased estimates of the effect of the RExO program on outcomes of interest could be obtained through relatively straightforward procedures. Because assignment to the program and control groups was conducted randomly, *by design* none of the unobserved factors that affect study outcomes should be correlated with assignment. In the general case, this often means that a simple comparison of the means of an outcome (for example, the percentage of program and control group members who found work in the one-year period following random assignment) will suffice as a statistical procedure for evaluating the effects of the program. In the main chapters of the report, the point estimates provided for program and control groups reflect this relatively simple comparison. However, additional methods can be used in an experimental context to improve the precision of the statistical analysis and to better fit the data being analyzed. In the subsections that follow, four such methods are described.

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[58] An additional relevant technical consideration is the problem of multiple comparisons. The multiple comparisons problem arises when researchers conduct many hypothesis tests using a single sample (as in the present study). In such cases, individual significance tests should be adjusted in such a way that they can be considered jointly, rather than individually. Because the ultimate qualitative conclusions of this report are that RExO did not significantly impact either the employment or criminal justice outcomes of program participants, formal adjustment for multiple comparisons is omitted from this technical appendix.

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Regression Adjustment

Modeling the data as a comparison of the differences in the control and program group means of some outcome Y_i is analogous to a regression of that outcome on an indicator variable T_i, denoting program status. Such models can often be improved by the addition of covariates. Although the experimental design theoretically ensures that estimates of the effect of the program are unbiased, adding other variables to the statistical model may improve the precision of the estimates of the treatment effect. This can occur when the covariates in question are themselves correlated with the outcome; when this is the case, their addition reduces the amount of error in the model.[59]

For regression adjustment for both survey data and administrative criminal justice data, covariates were partitioned into two distinct groups, both of which are in general thought to be correlated with labor market outcomes and with the likelihood of recidivism. The first group of covariates was socio-demographic, and included race and ethnicity, gender, age, and pre-experiment educational status. This group also included the unemployment rate in the local labor market at the approximate time of enrollment. Because not all enrollees entered the labor market immediately after assignment, the models used a forward-shifted three-month average of unemployment rates. For example, if an individual was randomly assigned at the Chicago, Illinois, site in April 2010, the unemployment variable was calculated as the average of the unemployment rates for Chicago for April, May, and June of that year.

The second group of covariates relates to prior criminal histories, and included the total number of prior arrests, the number of prior felony arrests, the number of prior violent and drug-related arrests, the number of prior convictions, and the total time incarcerated prior to RA.

While demographic covariates were available for all study participants, the research team was not able to obtain arrest and conviction data for ex-offenders in Louisiana and Ohio, and not able to obtain state prison data for ex-offenders in Illinois, Louisiana, and Michigan. Inclusion of these variables could potentially alter the statistical models in two significant ways: On the one hand, estimates might be made more precise because inclusion of these variables reduces the overall level of uncertainty in the models; on the other hand, missing data forces case-wise deletion in regression modeling. This can both alter point estimates (by excluding blocks of

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[59]   Kling et al. (2004)

participants) and reduce the precision of estimates (by reducing sample size). The fact that this missingness is completely determined by state precludes the possibility of multiple imputation.

Regression adjustment for administrative employment data followed a similar pattern, except that many of the criminal justice covariates were not available for analysis. As a condition of access to the NDNH data, the records were returned stripped of identifiers, and the study team was prohibited from attempting to merge that dataset with other records. As a result, only the covariates sent with the original pass-through file were available for analysis. Because the study team sought to obtain NDNH data concurrent with efforts to obtain administrative criminal justice data, many of the criminal justice covariates used for the analysis of non-NDNH data (e.g., total number of prior arrests or total number of prior convictions) were not available to be included in the NDNH pass-through file, as they had not yet been obtained. Thus, the sole criminal justice covariate used for NDNH data analysis was an indicator equal to 1 if the individual was a violent offender.

Given the program group indicator and these two groups of covariates, there are three logical regression models. The first approach is to simply regress the outcome on the program group indicator: With $i = 1 \dots n$ denoting individuals, Y_i denoting an individual level outcome, and T_i denoting individual level program status, equation (1) below defines this model:

$$Y_i = \beta_0 + \beta_1^1 T_i + \varepsilon_i \tag{1}$$

Here, β_1^1 is an estimate of the mean difference between the program and control groups. The subscript denotes which parameter in the regression model is estimated; the superscript denotes that this is the first of three different estimates of the treatment effect. Finally, e_i denotes an error term assumed to be uncorrelated with the remaining parameters.

This model can be extended, either by adding demographic covariates or by adding demographic and criminal history covariates simultaneously. With $\boldsymbol{D}$ denoting the previously described demographic attributes and $\boldsymbol{C}$ denoting criminal justice attributes, covariate adjustment alters the model to be either equation (2) or (3):

$$Y_i = \beta_0 + \beta_1^2 T_i + \boldsymbol{D}_i' \alpha + \varepsilon_i \tag{2}$$

$$Y_i = \beta_0 + \beta_1^3 T_i + D_i' \alpha + C_i' \delta + \varepsilon_i \tag{3}$$

Ordinarily, the experimental design of this study would guarantee that all three estimates of the treatment effect ($\beta_1^1, \beta_1^2, \text{and } \beta_1^3$) are unbiased. To the extent that D (demographic covariates) and C (criminal justice covariates) are correlated with the outcomes, estimates from equation (3) will generally be preferred. This is because of the ability of such covariates to improve the statistical precision of the estimates of the treatment effect.

As previously mentioned, however, the research team was unable to obtain the full set of criminal justice covariates from Louisiana, Illinois, Michigan, and Ohio, meaning that equation (3) would omit all observations from those states. This introduces concerns that point estimates would be altered because results would be subset to offenders from states where criminal histories were available. As a result, for the non-NDNH analyses, equation (2) is the preferred specification. For the NDNH analysis, equation (3) is the preferred specification, because the only available criminal justice covariate—violent offender status—was available for all cases. (In this Technical Appendix, results from each of these specifications are presented as sensitivity analyses.)

In general, the point estimates in the main chapters were derived from models following the form of equation (1). The p-values were derived from models following the form of equation (2) for all survey data analyses and for analyses of criminal justice outcomes derived from administrative sources. For the analyses of outcomes derived from NDNH data, p-values were derived from models following the form of equation (3).[60]

Logistic Regression

For most binary outcomes, tables in this report contain p-values from models using logistic regression (and average marginal effects) instead of ordinary least squares (OLS). This is because the properties of OLS are such that it is not the best linear unbiased estimator (BLUE) for binary outcomes. Estimates from logistic regression are more precise, meaning they generally have lower variance.

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[60]   All survey results were generated following the application of post-stratification weights for survey non-response.

Survival Analysis

Chapters III and IV described results of a survival analysis of the time to job acquisition, or time to first arrest, as a complement to models that used indicator variables for those events during discrete time frames, such as the first year following random assignment. The indicator variable approach has many benefits; it is straightforward, commonly used, and easy to interpret. However, it also entails the loss of potentially important information. For example, if offender A obtains employment on the first day following random assignment and offender B obtains employment on the 365th day, the measure would treat both individuals as having achieved an identical outcome in the labor market. In addition, if offender C obtains employment on the 366th day following randomization, he or she will be assigned a 0 rather than a 1, even though the labor market outcomes of individuals B and C are much more similar than those of individuals A and B.

Also known as "time-to-event analysis," survival analysis is an alternative statistical technique that does not impose the loss of information described above. It has been widely used to study the effects of interventions on both recidivism[61] and employment.[62] Instead of modeling *whether* individuals were able to find employment within a discrete time period, survival analysis models the duration of time that elapses before employment is found, if ever. In the example above, this approach preserves the subtle distinctions between individuals A, B, and C. As such, survival analysis potentially allows for a more complete understanding of how RExO participation influenced the time it took for participants to find employment. The tradeoff for this gain is that interpretation of survival analysis results is less straightforward than interpretation of results produced using binary outcome measures.

The key output of a survival analysis is typically the hazard ratio. The "hazard" is the probability of the occurrence of an event—often denoted as a "failure"—at a given point in time, conditional on that event not having already occurred.[63] In this technical appendix, the event in question is either job acquisition or arrest. The hazard ratio for the program group indicator is the ratio of the hazards for the program and control groups, respectively, holding other variables constant.

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[61]   Hepburn and Albonetti (1994)

[62]   Dolton and O'Neill (1996)

[63]   Survival analysis was created by medical and demographic researchers seeking to model time until death (Cox, 1972). Because of these origins, the terminology associated with the method has a somewhat negative character: events (in this analysis, the acquisition of employment) are generically referred to as "failures," and a standard parameter of interest is the "hazard ratio."

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Using job acquisition as an example, a hazard ratio of 1 would indicate no difference in the hazards of the program and control groups; a hazard ratio greater than 1 would indicate that program group members who had not yet found work had a higher average probability of finding work than did control group members who had also not yet found work. A hazard ratio of less than 1 would indicate the reverse. The survival analyses in this study were performed using Cox proportional hazard models. This model is thought to be more flexible than other survival models because it does not require distributional assumptions about the baseline hazard rate.[64]

Hierarchical Linear Modeling

Estimates of treatment effects can also be made more precise by accounting for the hierarchical nature of the data. The participants in this study were not drawn randomly from the entire population of eligible ex-offenders in the United States; rather, they were clustered within 24 unique grantees. It is possible—indeed likely—that independent of the program status, the observed values of the outcome variables used here would be correlated within these clusters (i.e., within grantee). Accounting for this correlation reduces the overall degree of residual "noise" in statistical models, allowing for greater precision in the estimates of the treatment effect. This is analogous to the precision gains derived from covariate adjustment described above. Because explicit estimation of individual grantee effects on outcome variables is not a research goal, the random effects (RE) specification is preferred to fixed effects (FE).[65]

This analysis used hierarchical linear modeling (HLM)[66] to account for the grouped nature of the data. This is because, in addition to allowing for RE estimation, HLM permits exploration of the possibility of heterogeneous treatment effects. For each of the outcomes in this study, the primary question of interest was whether the RExO program affected that outcome in a way that was both statistically and practically significant. Given such an estimated effect, and given that all estimates in this study were averaged across individuals grouped within program sites, an important ancillary question was whether the effect also varied across sites—a heterogeneous treatment effect.

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[64]   For a more complete discussion of the Cox proportional hazard model, see Rabe-Hesketh and Skrondal (2012).

[65]   The RE specification is preferred to the FE specification because it is more statistically efficient. However, RE models assume that treatment status and cluster status are uncorrelated. In non-experimental settings, violation of this assumption can lead to biased estimates (and therefore are one reason to prefer FE models). In the present study, however, the random assignment of program status guarantees that this assumption is satisfied.

[66]   Raudenbush and Bryk (2002)

With greater sample size, or with large effect sizes, direct estimation of site-level effects using fixed effects would be possible. However, this study had both relatively small sample sizes per grantee and relatively small effect sizes. As a result, the study was not adequately powered to directly estimate treatment effects at each of the 24 sites. Both of these are reasons to prefer the random effects approach that underlies HLM over fixed effects.

HLM extends the regression adjusted model (equation 2) described above as follows. With $j = 1 \ldots m$ denoting the 24 RExO sites and removing the superscript on the treatment effect parameter, equation (2) is re-specified as

$$Y_{ij} = \beta_{0j} + \beta_1 T_{ij} + D_i' \alpha + \varepsilon_{ij} \tag{4}$$

Here, the intercept term (β_{0j}) is subscripted in j, meaning that it is allowed to vary by site. This variation is modeled as deviations from an overall mean intercept:

$$\beta_{0j} = \gamma_{00} + U_{0j} \tag{5}$$

Substituting (5) into (4) yields

$$Y_{ij} = \gamma_{00} + \beta_1 T_{ij} + D_i' \alpha + U_{0j} + \varepsilon_{ij} \tag{6}$$

The site-specific intercepts are not estimated directly; instead they are assumed to follow a normal distribution. This framework permits estimation of the intraclass correlation coefficient (ICC)—the share of residual variation that exists at the group level, e.g.:

$$ICC = \frac{var(U_{0j})}{var(\varepsilon_{ij}) + var(U_{0j})} \tag{7}$$

When the ICC is large, it means that much of the residual variation in the outcome measure exists at the group (grantee) level. Conversely, a small ICC indicates that the outcomes of study participants within individual grantees are not strongly correlated.

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Sensitivity Analyses

This section presents sensitivity analyses for impact results for four key outcomes discussed in Chapters III and IV—one criminal justice outcome and one labor market outcome from the survey dataset, and one of each from the administrative data. Table A-1 contains results for the effect of RExO on the probability of any employment in the first year following random assignment, as measured in the NDNH data. Table A-2 contains results for the effect on the probability of any employment in Years 1–3, as measured in the survey data. Table A-3 presents estimates for the probability of any arrest in Years 1–3 as measured in the administrative criminal justice data, and Table A-4 presents estimates for this same outcome, as measured in the survey data. These measures correspond to key outcomes discussed in Chapters III and IV.

Each results table in this section is structured as follows: Model 1 compares the means of the outcomes for the program and control groups. Model 2 extends Model 1 by adding regression adjustment for demographic covariates. Model 3 adds criminal justice covariates to Model 2. Model 4 adds a hierarchical linear model (HLM) framework, which introduces random effects at the site level and also allows for investigation of the degree to which outcomes varied across sites. For outcomes from the survey data, all models are weighted to account for unit non-response. The first row in each table contains the point estimates (treatment effect), followed by Z-statistics in parentheses in the second row.

Point estimates presented in Chapters III and IV were derived from Model 1—the raw difference in means. P-values reported in Chapter III for outcomes derived from administrative data were obtained using variants of Model 3. P-values for outcomes derived from the survey data were obtained using variants of Model 2, which includes demographic covariates but not criminal justice covariates. This is because—as discussed in Chapter I, as well as above in this appendix—full criminal justice histories were not obtained for all individuals who completed the survey. Inclusion of these covariates in models for survey data outcomes thus introduces a potential for point estimates that do not generalize to the full sample (because cases without these covariates are omitted from these models).

Tables A1–A4 reveal that the results presented in the main body of this report are robust to choice of statistical model. Neither the inclusion of covariates nor the switch to the HLM framework qualitatively alters point estimates or levels of statistical significance. Similar tables were produced for other key outcomes discussed in the main body of this report, and the results of those sensitivity analyses were identical to the results below, in that the main findings were not qualitatively altered by any of the alternative specifications. These tables are omitted from this report for brevity, but are available on request.

TABLE A-1:

RExO Effects on the Probability of Employment, Administrative Data, Year 1

Any Job in Year 1	Model 1	Model 2	Model 3	Model 4
Treatment Effect	0.008	0.008	0.008	0.008
	(0.505)	(0.467)	(0.469)	(0.511)
Demographic Covariates	(no)	(yes)	(yes)	(yes)
Criminal Justice Covariates	(no)	(no)	(yes)	(yes)
Hierarchical Linear Modeling	(no)	(no)	(no)	(yes)
(ICC)				0.055
Observations	3,725	3,455	3,455	3,455

NOTES: Z-statistics are in parentheses.

All models present average marginal effects calculated after logit regression. HLM model estimated with random intercept at program level.

Statistical significance levels are indicated as follows: *** p<0.01, ** p<0.05, * p<0.1.

TABLE A-2:

RExO Effects on the Probability of Employment, Survey Data, Years 1–3

Any Job in Years 1–3	Model 1		Model 2		Model 3		Model 4	
Treatment Effect	0.032	**	0.033	***	0.039	***	0.034	***
	(2.507)		(2.596)		(2.758)		(2.805)	
Demographic Covariates	(no)		(yes)		(yes)		(yes)	
Criminal Justice Covariates	(no)		(no)		(yes)		(no)	
Hierarchical Linear Modeling	(no)		(no)		(no)		(yes)	
(ICC)							0.032	
Observations	2,995		2,974		2,397		2,974	

NOTES: Z-statistics are in parentheses.

      All models present average marginal effects calculated after logit regression. HLM model estimated with random intercept at program level.

Statistical significance levels are indicated as follows: *** p<0.01, ** p<0.05, * p<0.1.

TABLE A-3:

RExO Effects on the Probability of Arrest, Administrative Data, Years 1–3

Arrested in Years 1–3	Model 1	Model 2	Model 3	Model 4
Treatment Effect	-0.009	0.005	-0.010	-0.007
	(-0.554)	(0.299)	(-0.659)	(-0.484)
Demographic Covariates	(no)	(yes)	(yes)	(yes)
Criminal Justice Covariates	(no)	(no)	(yes)	(yes)
Hierarchical Linear Modeling	(no)	(no)	(no)	(yes)
(ICC)				0.157
Observations	3,859	3,547	3,547	3,547

NOTES: Z-statistics are in parentheses.

All models present average marginal effects calculated after logit regression. HLM model estimated with random intercept at program level.

Statistical significance levels are indicated as follows: *** p<0.01, ** p<0.05, * p<0.1.

TABLE A-4:

RExO Effects on the Probability of Arrest, Survey Data, Years 1–3

Any Arrest in Years 1–3	Model 1		Model 2		Model 3		Model 4	
Treatment Effect	-0.039	**	-0.041	**	-0.034	*	-0.044	**
	(-2.048)		(-2.229)		(-1.721)		(-2.402)	
Demographic Covariates	(no)		(yes)		(yes)		(yes)	
Criminal Justice Covariates	(no)		(no)		(yes)		(no)	
Hierarchical Linear Modeling	(no)		(no)		(no)		(yes)	
(ICC)							0.025	
Observations	2,980		2,959		2,384		2,959	

NOTES: Z-statistics are in parentheses.

All models present average marginal effects calculated after logit regression. HLM model estimated with random intercept at program level.

Statistical significance levels are indicated as follows: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

APPENDIX B: REFERENCES

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