SOCIAL POLICY RESEARCH
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Evaluation of the Re-Integration of Ex-Offenders (RExO) Program: Two-Year Impact Report

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The Authors

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 just following it, a participant was usually matched with an individual mentor, or was 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 than this average.

This report summarizes the impacts of the RExO program on offender outcomes in four areas: service receipt, labor market success, recidivism, and other outcomes. 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. 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 report are based on outcomes for these individuals in the two-year period after they enrolled into the study, and draws upon two sources of data to measure outcomes. The first of these was a telephone survey that asked about a range of items, including service receipt, labor market outcomes, recidivism, health and mental health, substance abuse, housing, and child support issues. The overall response rate to this survey was 76.9 percent. The second set of data used in this report was administrative data on criminal justice outcomes obtained from each of the 18 the states in which RExO grantees operated.

Key findings can be summarized as follows:

- **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. Program group members were more likely to participate in job clubs or job readiness classes and to receive vocational training, job search assistance, referrals to job openings, and help with resume development and filling out job applications. Program group members were also more likely to report participating in mentoring sessions and to declare that there was someone from a program who went out of their way to help them and to whom they could turn for advice on personal or family issues. Despite these differences, it is important to note that the program primarily provided work readiness training and support services; fewer than one in five RExO participants (and one in seven control group members) received any form of vocational or other forms of training designed to enhance their skills in in-demand industries.
- **The economic downturn placed additional pressures on ex-offenders.** Unemployment rates in grantee communities were high. Data gathered as part of the evaluation's implementation study indicated that employers that previously hired ex-offenders subsequently had an abundant and overqualified pool of candidates vying for fewer jobs and were less willing to hire individuals with criminal backgrounds, potentially affecting study participants' ability to find and retain employment. In addition, cuts to state and local budgets as a result of the economic downturn reduced other services that could help ex-offenders smoothly re-enter society.
- **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. At any given point following random assignment, program group members who had not yet found work were about 11 percent more likely to do so in the next time period than were control group members who had also not yet found work. However, there were no differences between the study groups in the total number of days employed in the two-year period following RA.
- **RExO had no effect on reported hourly wages, but did increase total reported income from all sources.** There were no differences between the study groups in their reported hourly wages at either the first job obtained after RA or at

their current or most recent job, but program group members reported higher average total income from all sources. It is not clear whether this higher average income is due to program group members working more total hours than control group members, obtaining more non-wage income, or some other reason, but program group members reported receiving approximately eight percent more income than control group members.

- **RExO had no effect on recidivism.** 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 (in the first and second years after RA) among program group members, the administrative data found no such effect. Analyses of this discrepancy suggest this difference is driven by either recall bias or otherwise inaccurate reporting on the part of program group members.
- **There was little evidence that RExO affected an array of other outcomes.** RExO had no effect on self-reported mental health, substance abuse, housing, and child support. There was some evidence that RExO may have affected health outcomes, as program group members were less likely to report having made any visits to the emergency room (a difference of 4.2 percentage points) or that their physical health limited their work or activities in the most recent month (a difference of 4.7 percentages points). Given that RExO grantees only rarely provided services directly to address these issues, it is perhaps not surprising that there are no clear effects in these areas.

Taken together, these findings present a mixed picture of the impact of RExO. On the one hand, it is clear that RExO increased the number and types of services received by program group members, and that it improved the self-reported labor market outcomes of participants as well. But there is little evidence this translated into any impacts on recidivism. Further, the impacts on employment, while statistically significant, are quite small in practical terms.

One additional finding is that 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, and date of enrollment into the study. Although there were a few instances in which the impacts were significantly different between subgroups, the number of these cases never exceeded the number one would expect based on statistical chance. Further, to the extent there were any differences among subgroups, they appeared to be driven more by changes in screening procedures used by grantees (several RExO grantees broadened their applicant pools toward the end of the intake period in an effort to reach their targeted enrollment), or by recall or reporting bias among members of the control group.

One possible reason for the somewhat modest results 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 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.

A final impact report is scheduled to be submitted in Summer 2015, and will focus on impacts in the three-year period following RA. This final report will include data similar to those reported in this report, but will add data for a third year following RA. Additionally, the final report will include administrative data on employment and earnings, which will allow for an analysis of the extent to which recall or other response bias in the survey results may have affected the estimates of impact on labor market outcomes. If the administrative data analysis provides results consistent with the analysis of survey data, the joint finding will provide solid evidence that RExO positively impacts participants' labor market outcomes. Further, despite the lack of impacts on recidivism described in this report, the final report will examine whether differences in recidivism emerge in the third year after RA.

I. INTRODUCTION

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.[1] RExO was intended to aid primarily urban communities heavily affected by the challenges associated with high numbers of prisoners seeking to re-enter their communities following the completion of their sentences. It does so by funding employment-focused programs that include mentoring and capitalize on the strengths of faith-based and community organizations (FBCOs). RExO built on several earlier Federal reentry initiatives, mostly emanating from DOJ or ETA, including Weed and Seed, the Serious and Violent Offender Reentry Initiative (SVORI), the Reentry Partnership Initiative, and, most directly, Ready4Work.

Five rounds, or generations, of RExO funding have been awarded, totaling more than $98 million in grants to agencies implementing the program. 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 the NORC at the University of Chicago to conduct a random assignment (RA) impact evaluation of these 24 RExO grantees and their partners. Under this contract, the results of the impact study are to be reported twice, first based on data from two years of study and then again after three years of data collection. This is the first of those two reports.[2]

This introductory chapter has four roles. First, it provides an overview of the challenges faced by ex-offenders reentering 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

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

[2] The second report, examining impacts over the three-year period following random assignment, will be completed in Summer 2015.

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. Finally, the chapter describes the analytic methods used to examine the impacts of RExO as presented in the 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 U.S. now incarcerates nearly 500 of every 100,000 U.S. residents.[3] This rate of incarceration is roughly four times the rate of the next highest country among peers of the U.S., and more than five and a half times the median of those peers.[4] 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.[5] 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.[6] Regardless of the reason, however, the end results are that larger and larger numbers of individuals in the U.S. either are or have been imprisoned and large numbers of prisoners are released each year.

Once released, ex-offenders face daunting obstacles to successful reentry, including difficulties with finding jobs, housing, and services for substance abuse or mental health problems; huge child support arrears; and challenges in reintegrating with 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 commonly for violations of parole conditions or drug possession.[7] Viewed in this context, efforts aimed at reducing recidivism are critical.

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3 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.

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

5 Carson and Golinelli (2013)

6 Raphael (2014)

7 Durose, Cooper, and Snyder (2014)

Although the relationship between crime and work is complex, many experts believe that stable employment 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 sentence, a large proportion of ex-offenders 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.[8] While it is difficult to isolate the impact of incarceration on labor market outcomes, several studies have found that the likelihood of finding employment, and the total number of weeks worked during the studies' follow-up period, is lower for those who have been incarcerated.[9] Other studies have documented that earnings also are lower for individuals who have spent time in prison than for similar individuals who have not.[10] Finally, a number of studies have shown that employers are quite reluctant to hire ex-offenders, particularly African Americans, those with violent offenses, and those who were recently released.[11] Indeed, one study of California employers has shown that more than a third of those surveyed would "definitely not accept" an applicant with a criminal record,[12] thereby substantially limiting the available pool of employers for this population. In summary, for most individuals prison worsens labor market prospects that were already poor prior to incarceration.

Of course, the fact that ex-offenders tend to struggle in the labor market and frequently end up back in prison does not necessarily mean that improving their employment outcomes will reduce recidivism. In other words, the relationship between low employment and high recidivism is not necessarily causal and, in fact, most offenders are employed at the time of arrest.[13] But there are both theoretical arguments and empirical evidence to support the notion that crime is linked to unemployment, low earnings, and job instability.[14] Legitimate employment may reduce the economic incentive to commit crimes, and also may connect ex-offenders to more positive social

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8 Raphael (2014)

9 Apel and Sweeten (2010); Raphael (2007)

10 Western, Kling, and Weiman (2001)

11 Holzer, Raphael, and Stoll (2007); Pager (2007)

12 Institute for Research on Labor and Employment (2007)

13 According to the Bureau of Justice Statistics (BJS), between 57 percent and 76 percent of state prison inmates (depending on educational attainment) had wage income in the month prior to admission. Between 48 percent and 70 percent reported that they were working full-time. See James (2004).

14 Bernstein and Houston (2000); Solomon et al. (2006); Sampson and Laub (2005)

networks, role models, and daily routines. Moreover, many prisoners identify finding a job as one of their highest post-release priorities.[15] It is therefore reasonable to hypothesize that interventions that boost employment and earnings among ex-offenders may also lead to reductions in recidivism.

Recent Research on Recidivism and Employment Interventions

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, including some experimental evaluations,[16] there have been very few rigorous studies of employment-focused reentry models. For example, one meta-analysis of the effects of community employment programs on recidivism among persons who have previously been arrested, convicted, or incarcerated found only eight such studies that used random assignment designs, and several of those studies did not specifically target ex-offenders. The authors noted that "this systematic review…is hampered by inadequate contemporary research."[17] This meta-analysis also found the programs that were included in the analysis produced no overall impact on recidivism.

Several of the most important studies of employment-focused programs for ex-offenders, including the National Supported Work Demonstration, the Living Insurance for Ex-offenders project, and the Temporary Aid Research Project, were conducted in the 1970s.[18] After these studies produced generally discouraging results, there was a long hiatus in research on ex-offenders. However, the flurry of interest in reentry during the past five to ten years, likely triggered by the surge in prison populations described above, spurred a new round of studies.

Among these recent studies are a number of non-experimental ones. The multi-site Serious and Violent Offender Reentry Initiative (SVORI) evaluation found modest improvements in outcomes for adult program recipients and no differences among youth participants.[19] Other non-experimental studies in recent years have examined Texas's Project RIO, San Diego's Second

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15 Visher and Lattimore (2008)

16 Drake, Aos, and Miller (2009), for example, conducted a thorough meta-analysis of all English-language evaluations of prisoner reentry and crime-abatement programs, identifying 545 such evaluations. Of these, less than five percent were experimental evaluations (i.e., employing random assignment).

17 Visher, Winterfield, and Coggeshall (2005)

18 See, e.g., Manpower Demonstration Research Corporation (1980); Rossi, Berk, and Lenihan (1980); Maller and Thornton (1978)

19 Lattimore and Visher (2009)

Chance program, Ready4Work, and others. 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.

Experimental studies of employment-based programs serving offenders have been launched, 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. The CEO provides transitional employment, in combination with a five-day preemployment 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 difference in employment after this point for the remainder of the three-year follow-up period. Despite this latter finding, there was 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.[20]

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. Offenders interested in participating in this project were randomly assigned either to a program or a control group and 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 a short-term increase in employment, driven by the transitional jobs, but these effects had largely vanished by the end of a year. However, in contrast to the CEO evaluation, there were no clear impacts on multiple measures of recidivism during the two-year follow-up period.[21]

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

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

[21] Jacobs (2012)

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 treatment and comparison groups render these results uncertain. Recent experimental evaluations have found relatively little effect on employment for former offenders, but in at least one case (CEO), the program did have an effect in reducing recidivism among offenders, particularly those who had been released shortly before enrolling in the study. Both 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.[22] 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 by examining 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 measures the effects of program participation[23] on ex-offenders' employment, earnings, recidivism, and other outcomes using a random assignment (RA) design. RA establishes two equivalent groups—a program group and a control group—and enables the research team to compare the outcomes of their members and estimate the impact of the program. Critically, the RA design is intended to eliminate the effect of unobserved factors such as motivation. The evaluation is based on three primary research questions:

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

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[22] 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.

[23] Technically, the impact study assesses 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.

Between January and December 2010,[24] 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 a 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.
- 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.
- Enroll in the RExO program within 180 days of release from a prison, jail, or 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.
- Not have had a violent crime as her/his most recent offense.[25]

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.[26] 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/reentry cycle the actual point of RA would occur. All of the grantees had well-established intake and enrollment procedures and were justifiably concerned about how a RA process would affect these procedures or add burden to their workload.

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

[25] 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 Chapter I, the percentage of study participants who had a violent offense in the past remained very low despite this change.

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

In 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 deemed 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 enrolled in the study, while in other sites potential participants underwent multiple assessments and were required to participate in multi-day 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 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 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 would be assigned to the program group who did 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 RA design requires that estimates of program effects be generated from outcomes that are 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 site 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 workshops) that potential participants had to undergo prior to being enrolled in the program. These grantees devised these activities and workshops as a way 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 these grantees 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).[27]

Nearly all grantees therefore adopted RA procedures that required potential participants to come to the grantees' offices at least once, to learn about the program and the study, and to 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 increased the likelihood that a high percentage of the program group actually went on to enroll in the program. At the same time, the procedure ensured 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. In these sites, then, members of the control group received at least a portion of the "treatment" itself.[28]

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

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27 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. This possibility is tested in subsequent chapters.

28 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 receive more services.

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: In addition, 71 individuals were designated as wild cards. Each program was given a small number (no more than five) of these wild cards, who were not enrolled into the study, but automatically enrolled into the program.

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. The first participants were enrolled into the study in late January 2010, when one grantee implemented 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.[29]

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 group (see Table I-1).[30] The average number of study participants across grantees was 194.2. As can be seen in the table, 12 of the grantees achieved their target of 200 participants, including 3 that exceeded this target by at least 10 participants. An additional 6 enrolled at least 190 participants. Only 3 grantees enrolled fewer than 150 participants.

Table I-2 displays the key characteristics for both the program and control groups. As can be seen in this table, there are a few minor differences in characteristics between the program and control 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 are similar between the two groups, which is the expected outcome when assignment to the groups is done at random. These similarities provide some assurance that the program and control groups are 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 also was not statistically significant. Both of these findings suggest there is no meaningful difference between the program and control groups.

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[29] 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.

[30] 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.

Table I-2:

Characteristics of the Program and Control Groups

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 and Ethnicity		
White (%)	33.1	32.1
Black (%)	50.9	52.1
Asian (%)	0.9	0.8
Hawaiian/Pac. 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.

Data Collection

Three 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;[31] and (3) a follow- up survey to learn about the status of all study participants at both two and three years after study entry. These three 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 allow the evaluation team to contextualize the impact results in three important ways. Specifically, they allow the evaluators to

- identify and compare the services provided to program group members and the standard services that were available to ex-offenders in the control group;
- identify variations in the overall quality of services that may 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 are 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 from 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

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31 Administrative data on employment and earnings are also being collected for the evaluation through the National Directory of New Hires database. These data were not available for this report, but will be included in the Final Impact Report (which will include outcomes for the three-year period after RA), which will be submitted in Summer 2015.

arrangements, this multi-pronged 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.[32]

Administrative Data on Criminal Justice

Administrative data on criminal justice serve as the primary source of data 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 are 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 have been obtained from the criminal history depositories in 16 of the 18 states in which the RExO programs operated; because more than one program operated in some of these states, these data cover 21 of the 24 RExO sites. These data thus include 87.2 percent of all study participants. In addition, data on incarceration in state prisons were provided by the department of corrections in 15 of the 18 states, covering 20 of the 24 RExO sites.[33] These data therefore include 85.8 percent of all study participants.

The advantage of these data is that they provide a uniform and objective source of data on criminal justice outcomes and, as such, provide the evaluation with information on the full population of study participants. However, because criminal justice data were obtained only from the state in which an individual was randomly assigned, a sample member arrested, convicted, or admitted to prison in a state outside of the one in which he or she was randomly assigned did not have criminal justice information available for the analysis. Given this fact, it is

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

[33] In addition, jail admission records were obtained for two grantees in two states, because these grantees recruited heavily from their local jail population.

possible that the analysis of administrative data understates the overall level of recidivism because it misses criminal activity that occurs outside the state in which an individual was randomly assigned.

These data were collected from state criminal justice agencies beginning in the Spring of 2011.[34] Subsequent extracts were collected in 2012 and 2013.[35]

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.[36] The survey was primarily administered using Computer-Assisted Telephone Interviewing/Personal Interviewing (CATI/CAPI) technology.

The survey instrument was divided into nine sections, capturing information on a number of items within each of these sections:[37]

- Background[38]
- Current Housing Situation
- Current Employment
- Recidivism
- Employment and Education Activities/Services Received
- Employment History

[34] 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 that was made, the number of files actually received from each state varied.

[35] A final request for data collection—to cover the full three-year period following RA—is currently underway. Results of the analysis of these data will be available in the longer-term Impact Report, to be completed in Summer 2015.

[36] 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. Approximately twelve percent of respondents were not located during the initial wave of survey administration, but were located during the second wave. These individuals completed a "combined" survey that first asked them about their experiences in the two years following entry into the study and then immediately asked them about their experiences between the second and third years after study entry.

[37] There were two additional sections in the survey. One of these was used to refer respondents to the time at which they entered the study, using a series of prompts to remind them of that date, to ensure that their responses were properly anchored to the appropriate time period. The second additional section was at the end, and asked respondents for updated contact information, to facilitate subsequent follow-up efforts.

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

- Household Income
- Health and Substance Abuse
- Child Support

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

Nevertheless, the survey data do offer a number of advantages. Chief among these advantages is that the survey data enable the evaluation team to measure (through participants' responses) outcomes for which there are no readily available administrative data. For example, the survey provides the only means of measuring the number and types of services both program and control group members accessed following their entry into the study,[39] the overall household income of study participants, the health or substance abuse issues they have experienced, and any obligations they have for making child support payments.

Additionally, the survey data have some advantages even in cases in which administrative data are available. In particular, they allow evaluation staff members to corroborate the findings from the data on criminal justice, potentially increasing confidence in any conclusions drawn from those data. In addition, survey respondents might report criminal justice activities that occurred in states other than the one in which they were randomly assigned. Also, the survey data provide useful measures of recidivism data for respondents from those states that did not or could not provide administrative data.

The survey sample included all 4,655 study participants. Although not all respondents completed the survey exactly two years after they were enrolled into the study, each was asked about the two-year period following their enrollment. Ultimately, 3,581 participants completed the two-year survey, yielding a response rate of 76.9 percent.[40] There was slight variation between

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[39] 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 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.

[40] Several of the respondents had been reincarcerated by the time of the follow-up interview. The evaluation team therefore sought permission from the institutions in which they were housed to conduct interviews with these respondents. In many cases, this permission was granted, but the approval process was a lengthy one. Because of this, the time at which they were interviewed was close to the end of the three-year follow-up period. These individuals therefore completed a combined survey, in which they were first asked about the two-year period

program and control group response rates; the response rate for program group members was 78.6 percent, and for control group members the rate was 74.4 percent.[41]

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 for the outcome within the program group and for the outcome in the control group, and the difference between these means has been calculated as well. Because the data come from a randomized trial, this difference provides 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.[42] 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. Thus, the first two columns in each of the tables presented in this report provide the mean values for the program and control groups on the outcomes of interest, while the third column displays the differences between these. 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.[43] 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. As a result, the chapters in this report present the results from the simpler models

following their entry into the study, and then were asked about the third year after RA. Of the total final sample, 2,966 individuals (or 63.7 percent) completed the two-year survey and 615 (13.2 percent) completed the combined survey.

41 There was no difference between program and control group members in the average amount of time between RA and the date they completed the survey.

42 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.

43 In Chapters III and IV, some tables include an additional column that shows the probability value for hazard models, which utilize survival analysis. This analysis is described briefly in this chapter, and more fully in the Technical Appendix.

because they are more readily understood by a general audience. 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 of this elapsed time for the program and control groups, and the differences between them, the statistical analyses of the difference between them is performed using survival analysis, which is an alternative and 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 it are described in detail in the Technical Appendix.

Subgroup Analysis

Impact estimates derived from the full sample may mask important and policy-relevant differences in impacts across subgroups of participants. If there are impacts observed for the full sample, they may be driven by a single subgroup which experienced very large effects, while other subgroups experienced little to no effect. Alternatively, in situations in which there are no impacts among the full sample, there may be important impacts among key subgroups which are not observable within the full sample, particularly if the subgroup experiencing the impact is relatively small in size. Given these possibilities, the report examines impacts for several 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, and the remaining two are based on the time at which the participants enrolled in the study.

The first subgroup partition splits older and younger offenders (comparing those aged 27 and older to those younger than 27). This subgroup analysis was chosen because prior work has suggested that reentry programs may be more effective for those age 27 years and older.[44] 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 gendered reentry programs.[45] The third subgroup partition, based on educational achievement, 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. This subgroup analysis was included because it seemed likely that REXO may have differing impacts for those whose prior educational achievement made them more or less likely to find employment. The fourth subgroup analysis

[44] Uggen (2000); Uggen and Staff (2001)

[45] Bloom, Owen, & Covington (2003)

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).[46]

The final two subgroup analyses examined in this report are based on the timing of participants' entrance into the study. The first of these has to do with the length of time between a participant's release from prison or jail and their enrollment into the study. As described above, research has shown that early access to program services may be an important factor for re-entry program effects.[47] To explore the extent to which RExO had differential impacts based on the timing of access to services, the evaluation team partitioned the sample by the timing of random assignment relative to release from prison and compared those randomly assigned within three months of release to those randomly assigned following a longer interval.

The final subgroup analysis 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. Specifically, through implementation study site visits, the evaluation team learned that, because the RExO grantees were funded through March 2011, enrollment was generally expected to end by December 2010 and that all services other than follow-up would end shortly thereafter. In anticipation of these changes, staff members at many RExO grantees were unsure as to whether their positions would be sustainable into 2011 and began to leave the RExO program to take other positions, either with the organizations operating RExO or elsewhere. This resulted in substantial understaffing at many RExO grantees during the latter months of enrollment. The evaluation team realized this during the second round of site visits and grew concerned that participants enrolling during this later period would experience something less than the full array of services offered to those who enrolled earlier. The final subgroup analysis therefore provides a test for whether this change in the nature of the treatment might affect the impacts of that treatment.

Findings for the subgroup analysis must be interpreted cautiously, for two primary reasons. First, any power concerns that may exist in the main analysis (i.e., having sufficient sample size to detect effects that may not be large) are greatly exacerbated when the sample is divided into subgroups. Second, the number of statistical tests performed overall increases with the number of subgroups analyzed; making these multiple comparisons greatly increases the concern that spurious findings of statistically significant impacts are likely to be found by chance, as is

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46 Visher (2003)

47 Redcross et al. (2012)

discussed below.[48] For these reasons, it is often most helpful to interpret the findings of the subgroup analyses as exploratory, rather than confirmatory.[49] In the context of this study, this means that if analyses for a given subgroup show no 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 of this study.

Multiple Comparisons

There are many ways in which 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. This raises a statistical issue known as the multiple comparisons concern. The multiple comparisons concern is that simultaneous estimation of the effect of a program on several outcomes can lead to an increase in the probability of type I errors—concluding that the program had a significant effect on some outcome when in fact it did not. This is because each individual comparison is subject to statistical uncertainty (one sets the value for statistical significance at a certain threshold, meaning there is some known chance that one will conclude a result is significant when it is due instead to statistical "chance."). Conducting multiple comparisons multiplies the likelihood that one will spuriously find a result that appears significant. Although a number of techniques have been developed to address the multiple comparisons concern, none of these can completely eliminate the concern when examining a number of different outcomes. One of the most preferred ways to limit the multiple comparisons problem is to limit the number of outcomes and subgroups to be analyzed,[50] which this report does when examining labor market and recidivism outcomes. Other means for addressing the issue are statistical in nature. The three most common of these statistical approaches, and their implications for the conclusions drawn in this report, are described in detail in the Technical Appendix.

Remainder of the Report

The remaining chapters of this report provide and discuss the results of the impact analyses. Chapter II describes the impact of RExO on the services received by program and control group members. Chapter III presents results for employment and earnings impacts. These analyses rely upon the survey data to explore whether the RExO program affected its participants'

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48 For a fuller discussion of this issue, see Schochet (2008).

49 This approach is discussed in Bloom and Michalopoulos (2010).

50 Schochet (2008)

employment and earnings outcomes in the two-year period following their entry into the study. Chapter IV presents similar analyses focusing on recidivism. This chapter includes analyses of both administrative data and survey data. Chapter V includes impact analyses for other outcomes based on survey data, including physical and mental health, substance abuse, housing, and child support issues. The final chapter of the report 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. SERVICES RECEIVED BY PROGRAM AND CONTROL GROUPS

In order to examine the impact of RExO services, the evaluation must be able to compare the outcomes of participants who received these services to a similar or identical group of participants who did not. Ideally, nearly all those who were in the program group both enrolled in RExO and received services from the program, so that they could receive whatever benefit there was from these services, and no one in the control group received services equivalent to those in RExO. It is not likely, however, that this ideal situation was attained. While careful attention was given to ensure that those in the control group did not enroll in RExO programs, they did have access to other services in their communities, and thus may have sought and received services similar to those in RExO from alternative providers. In addition, it is possible that some program group members did not receive RExO services. To the extent that these deviations from the ideal occurred, the service contrast between program and control group members was reduced, along with the likelihood of observing impacts of RExO. Thus, one critical aspect of the evaluation is to determine whether program group members actually did receive more RExO (or RExO-like) services than control group members.

This chapter summarizes the data that the evaluation uses to make this determination. It begins by drawing upon findings from the implementation study to provide brief summaries of the services offered by RExO programs and the types of services offered by alternative providers in the same communities. The next section draws upon survey data to identify the number and types of services actually received by both program and control group members, examining the extent to which there is a true service contrast between these two groups. The final section of the chapter explores whether there is variation in service receipt among the key subgroups identified in Chapter I.

RExO Program Services

As described in Chapter I, RExO sought to reduce recidivism in urban communities with significant numbers of returning ex-offenders by helping these individuals find and prepare for employment.[51] To realize this goal, the grant targeted adults at risk of re-arrest but with reasonable potential to enter the workforce.

Grantees received referrals from a variety of sources, including state and federal prisons, county jails, probation and parole officers, judges, halfway houses, and other community agencies. Several grantees conducted extensive outreach efforts to recruit ex-offenders. These efforts included making presentations at community events or probation/parole offices, developing public service announcements, and creating flyers to be placed in areas that offenders were likely to frequent.

The programs funded under RExO primarily provided the following three main types of services:

- **Mentoring.** This most often took the form of group mentoring, but it also included one-on-one mentoring and other activities in which participants were connected to others in a supportive environment.

- **Employment services**. These services consisted of work readiness, job training, job placement, job clubs, transitional employment,[52] and post-placement follow-up.

- **Case management and supportive services**. These varied services included transitional housing; referrals for substance abuse, health, or mental health services; assistance with court issues, including paying restitution and court fees; securing driver's licenses or other needed documents; and providing incentives such as bus tokens and payments for achieving key milestones.

Some programs also offered educational services, such as basic skills remediation and GED preparation and testing, but these were uncommon.[53]

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. Data from the Management Information System (MIS) used by RExO grantees indicate that nearly all program group members (82.3 percent) did, in fact, receive this type of training.[54] Toward the latter part of this

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51 Department of Labor, Employment and Training Administration (2005)

52 Only approximately six percent of program group members were placed in transitional employment, and approximately half of all these placements were associated with a single grantee that provided such employment as a key focus of its program.

53 Data from the RExO MIS system indicate fewer than five percent of program group members received educational services directly from a RExO grantee.

54 Although these figures represent the best available programmatic data, grantees varied in the degree to which they accurately entered information on services received. Thus, as noted in Chapter I, these and other MIS data should be viewed as approximate estimates rather than as exact.

training, or just following it, a participant was usually matched with an individual mentor, or was asked to participate in group mentoring activities. Data from the MIS indicate that 62.5 percent of all program group members received some form of mentoring service during their participation in the program. 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 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 12 weeks, this varied widely across program participants, and the period of intensive participation was often much shorter than this average.

Two-thirds of the programs (16) 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 third of the grantees focused on the provision of essential supportive services first, before participants were referred for jobs, which meant primarily 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.

Most grantees lacked the capacity to provide the full slate of required RExO services or to provide all the programs and services necessary to meet the many needs of the ex-offender population. In order to fill some of these gaps, they reached out to other programs and service providers in the community to form partnerships. This often increased the effective capacity of the grantees, strengthened their standing in the community, and allowed them to provide a richer array of services.

Alternative Providers and their Services

In addition to gathering information from, and about, the RExO grantees, the implementation study also identified alternative providers offering similar services within the communities in which RExO operated. These providers were identified in several ways: (1) tapping the knowledge of RExO staff members and participants, (2) conducting web-based searches of available services, and (3) asking providers of alternative services for information about additional providers in the area (i.e., using a "snowball" technique). While these efforts cannot be considered to provide exhaustive lists of all services in the communities, they do provide a summary of RExO participants' and staff members' perceptions of the availability and general accessibility of the alternative services in these areas. The evaluation team was therefore confident of having a reasonably thorough overview of the types of services that were available to members of the control group, who were unable to access RExO services, as well as to those in the program group seeking additional services outside of RExO.

In nearly all the RExO communities, there were multiple other agencies providing services to ex-offenders that could serve as alternatives to the services provided by RExO. In total, the evaluation team identified 97 providers across the 24 grantee communities that offered at least one service similar to a core RExO service, though as noted this should not be taken as an exhaustive accounting. Each grantee community had between two and eight such providers, and services similar to each of the three core RExO services were available through some combination of alternative providers in every grantee community, with many communities having more than one of each.

In addition to being fairly readily available, alternative provider services, with a few exceptions, were also viewed as being generally accessible to ex-offenders in these communities. Both staff members and program participants noted that alternative provider services generally were visible to the ex-offender population, were located where ex-offenders could reach them relatively easily, used eligibility criteria that left them sufficiently open to those eligible for RExO, and had sufficient capacity to serve control group members. There may be reasons why RExO participants and staff members could have known more about the availability and accessibility of these services than control group members, but the general perception was that such services were relatively easy to find and access.

Within most communities, the quality of alternative provider services was roughly similar to or slightly lower than the quality of similar services offered by the RExO program. The assessment of service quality was based on measurements of the intensity of the services offered and on the views of participant and staff respondents within grantee communities. The evaluation team found that core services offered by alternative providers were generally slightly less intense than the same services offered by the grantee organizations within the same communities.

In addition to the services provided by these "alternative" providers, the implementation study examined the services ex-offenders could receive either prior to release from prison or after release under supervision by a probation or parole officer. Pre-release services were available to some degree in all grantee communities, and they often included a slate of services similar to what ex-offenders find in post-release programs (though they generally were substantially less intensive). In contrast, supervision by probation and parole officers tended to involve monitoring more than service delivery, especially in times of tighter budgets, as was the case for nearly all sites during this evaluation.

Hence, control group members likely had access to services that were similar in nature and number to those provided by the RExO grantees, but perhaps were slightly less intensive than those offered through RExO. Whether control group members accessed these services at the same rates that program group members did is thus a critical question.

Differences in Service Receipt for Members of the Program and Control Groups

Given that the very design of the study involves randomly assigning participants either to be enrolled in RExO or to be prohibited from enrolling in the program, one would expect this variable to have sizable impacts on participants' actual service receipt. Although those assigned to the control group were informed they could seek out any alternative services in their community for which they were eligible, and many of them undoubtedly did so, the assumption underlying the design of the evaluation was that members of the program group would receive a greater number of (and more intensive) services than members of the control group (and thus the evaluation could determine whether these services had any impact on key outcomes). If control group members actually received nearly the same services as program group members, one would not expect there to be impacts on other outcomes, such as labor market success and recidivism. One component of the survey therefore focused on the extent to which study participants actually received several types of services, including employment, education, mentoring, and anti-recidivism services. Specifically, the survey asked respondents to identify whether they actually received any of these services in the two-year period after they had enrolled into the study.[55]

Impacts for the Full Sample

Table II-1 displays a comparison of program and control group means calculated from the full sample of participants for these various service receipt questions. As can be seen in this table, on nearly every dimension, program group members were far more likely to have received the services than were control group members. Although there can be concerns about making multiple comparisons, the pattern of results is consistent and widespread, indicating that group membership had a clear impact on service receipt.

Program group members were much more likely to have received employment-focused services, such as participation in job clubs or job readiness classes, vocational training, job search assistance, referrals to job openings, and help with resume development and filling out job

[55] Note that a small number of participants were randomized *before* they were released from prison. For these individuals, the period of analysis is the period following release, not the period following randomization. This is the case for all subsequent time-specific analyses.

Table II-1:

Impacts of Group Membership on Service Receipt

Service Received	Program	Control	Difference	P-value
Work Readiness				
Job Club/Job Readiness Training (%)	72.5	51.2	21.3	0.000***
Number of Days in Job Readiness Training†	57.9	53.0	4.9	0.170
Vocational Training				
Vocational training (%)	17.8	13.2	4.5	0.000***
Number of weeks of vocational training†	13.7	20.2	-6.5	0.000***
Received vocational certification/credentials (%)†	77.8	72.1	5.7	0.149
Job Search/Interviewing Assistance/Resume Prep				
Independent job search (%)	47.3	33.8	13.5	0.000***
Job search assistance (%)	40.0	22.6	17.4	0.000***
Referred to job opening by program (%)	39.6	29.5	10.1	0.000***
Advice about job interviewing (%)	70.2	64.8	5.4	0.004**
Advice from program on answering employers' questions about criminal history (%)	71.2	62.5	8.7	0.000***
Advice about behavior at job from program (%)	67.0	58.7	8.3	0.000***
Contact information about jobs in the community (%)	52.1	45.6	6.5	0.001***
Resume assistance (%)	73.8	67.6	6.2	0.001***
Assistance with job applications (%)	65.6	57.4	8.1	0.000***
Education Services				
Adult Basic Education/GED classes (%)	11.1	10.9	0.2	0.872
GED, high school, or other degree/diploma instruction (%)	5.2	5.2	0.1	0.948
Took college courses for credit (%)	14.6	14.1	0.5	0.675
Mentoring				
Participated in formal mentoring (%)	22.8	10.0	12.8	0.000***
Had person (from program) to turn to for advice on family/personal issues (%)	59.3	50.7	8.6	0.000***
Had a mentor or guide (from program) (%)	52.2	40.8	11.4	0.000***
Helpfulness of mentor in helping to avoid crime††	1.3	1.3	0.0	0.617
Pre-Release/Parole Referrals				
Referred while incarcerated to agencies/organizations for help finding a job (%)	34.6	30.6	4.0	0.014**
Referred by parole or probation officer to agency/organization for help finding job (%)	39.6	35.8	3.8	0.049**
Other Services				
Helpfulness of employment services††	1.5	1.9	-0.3	0.000***
Participated in other employment-related programs (%)	12.2	9.2	3.1	0.004***
Number of weeks in employment-related program†	13.8	14.0	-0.2	0.937

SEE NOTES AT END OF TABLE

Service Received	Program	Control	Difference	P-value
There was a person (from program) who went out of their way to help (%)	62.5	53.2	9.2	0.000***
Sessions offering counseling or advice to former offenders (%)	49.8	42.1	7.8	0.000***
Help dealing with child support enforcement system (%)	9.9	7.3	2.7	0.017**

SOURCE: Two-year follow-up survey of participants

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

The third column ("Difference") displays the percentage point or raw difference between the first and second columns. This convention is followed in all similarly structured tables in the remainder of the report.

Probability values in this table are regression-adjusted for pre-random assignment characteristics.

† These items were only asked for those participants who received the given service; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.

†† These items indicate self-reported helpfulness of the service (on a scale of 1 [very helpful] to 4 [not helpful]) and are coded such that lower scores indicate a more favorable rating. They also were only asked for those participants who received the given service, and thus the comparisons are non-experimental in nature.

applications. Program group members were also more likely to have received advice from program staff members on a number of topics, including about job interviewing, how to answer questions about their criminal history, and how to behave in an employment setting.

Additionally, program group members were much more likely to have participated in mentoring sessions, and to declare that there was someone from a program that went out of their way to help them and to whom they could turn for advice on personal or family issues. They also were more likely to have participated in sessions offering counseling or other support. Finally, program group members were more likely to report that a program had provided them with help dealing with the child support enforcement system. The helpfulness of the employment services received was also rated more favorably by program group members, though this was only asked of those who had received these services so this comparison is non-experimental in nature, and thus cannot be considered an unbiased estimate of the effect of RExO.

The one set of services that show no impact of RExO are educational services, including receiving basic educational instruction, receiving a high school diploma or GED, and taking college courses for credit. This is consistent with the findings from the implementation study that these services were offered only infrequently by RExO grantees.

There is one exception to the general result that program group members received significantly more services. Among those who received vocational training, control group members on average received 6.5 more weeks of this training than program group members. This may reflect the relatively short-term nature of many of the RExO program interventions. Additionally, this item was only asked of those who reported receiving vocational training. As such, this comparison is non-experimental in nature and thus may not provide an unbiased estimate of the impact of RExO.

In short, then, it is clear that the RExO program had a substantial impact on the number and types of services study participants received during the two-year follow-up period. The survey did not ask about which programs provided respondents with these services, so it is not certain that all of these differences are due to services that RExO grantees directly provided to program group members. It is possible that some (or even many) of the services were provided by other programs, though even in these cases they may have been accessed as a result of a referral from the RExO grantee. But what is certain is that program group members received significantly more services than did control group members. This is an important finding, because to the extent that RExO services have an effect on key outcomes, such as labor market success and recidivism, it will only be apparent if program group members actually received more of these services than did control group members. The data in this chapter demonstrate there was a clear treatment contrast between the study groups.

Impacts for Subgroups

As described in Chapter I, impacts for the full sample may mask important or policy-relevant differences in these impacts across key subgroups. This section therefore examines the extent to which there are differences in impacts across the six subgroup partitions described in Chapter I.

Because of the number of services potentially received, and the number of subgroups, individual results are not shown in this chapter, and are instead displayed as part of Appendix B. As can be seen in this Appendix, there are some differences in the number or types of services received by the various subgroups of interest. Specifically, employment services were rated as more helpful by younger participants (those under 27) than by older ones. Additionally, males were more likely to have received vocational training than were females. And males, those enrolled in the study after October 1, and those under 27 were more likely to have participated in group counseling sessions. Aside from these fairly isolated findings, however, there is little evidence that RExO had a differential impact across subgroups in the specific types or number of services they received. Indeed, overall, the number of statistically significant findings in this subgroup analysis (18) is exactly the number one would expect to find based purely on statistical chance. Thus, while it is very clear that RExO increased the services received by the program group overall, there is not clear and consistent evidence to suggest that certain subgroups experienced this increase differently than others.

III. IMPACTS ON EMPLOYMENT AND EARNINGS

One of the key objectives of the RExO program is to improve the labor market outcomes of participants. This is important both in its own right and because it is thought that employment and/or higher earnings may serve as protective factors against future recidivism.[56] This chapter examines the degree to which RExO accomplished this objective by analyzing the effect of RExO on participants' labor market outcomes during the two-year period following random assignment (RA). To perform this analysis, the chapter relies on data compiled from the follow-up survey of program and control group members (described in more detail in Chapter I).[57] After summarizing observed impacts on employment and earnings for the full sample, it examines the impacts for key subgroups of interest.

Impacts for the Full Sample

Because of concerns about making multiple comparisons (described in Chapter I)[58] 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)[59] a set of seven core measures of employment and earnings. Taken together, these measures provide a relatively complete picture of labor market performance:

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56 Redcross et al. (2012)

57 The final impact report will also include results from an analysis of administrative data on earnings and employment obtained from the National Directory of New Hires (NDNH). These data were not available for inclusion in this initial impact report.

58 As described in Chapter I, the multiple comparisons concern is that simultaneous estimation of the effect of a program on several outcomes can lead to an increase in the probability of type I errors. Limiting the number of outcomes and subgroups to be analyzed is one of the most preferred ways to limit the multiple comparisons problem (Schochet, 2008). This issue and the ways in which the current analysis dealt with it are described in detail in the Technical Appendix.

59 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.

1. Whether or not the individual worked at all in the first year following RA.
2. Whether or not the individual worked at all in the *second* 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 one-year period beginning one year following RA.

It should be noted that the wage measures (numbers 5 and 6) are necessarily computed only for those 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-1 presents the results of the analysis of the effect of RExO on these labor market outcomes. The results show evidence of a statistically significant effect of RExO on participant labor market outcomes, across several measures. In the program group, 71.3 percent of ex-offenders found some form of employment in the first year following random assignment, compared with 67.9 percent of ex-offenders in the control group—a difference of 3.5 percentage points.[60] Similarly, 68.0 percent of program group members worked at some point during the second year following random assignment, compared with 65.4 percent of control group members. While smaller than the first-year effect, the difference (2.6 percentage points) was also statistically significant.

Evidence of the beneficial effect of the RExO program was also evident in a comparison of the elapsed time to first job acquisition. 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—157.1 days. Because of the nature of this measure, it was subjected to survival analysis. This analysis, using Cox proportional hazard models (with regression adjustment and post-stratification weighting), yields a hazard ratio of 1.111. This means that at any given point following random assignment, treatment group members *who had not yet found work* were about 11 percent more likely to do so in the next time

60 The difference of 0.1 is due to rounding.

period than were control group members *who had also not yet found work.*[61] This result was also statistically significant.

Table III-1:

Program and Control Group Means for Key Labor Market Outcomes

Outcomes	Program	Control	Difference (Impact)	Hazard Ratio (Impact)	P-value
Employment					
Worked at all in 1st year following RA (%)	71.3	67.9	3.5		0.025**
Worked at all in 2nd year following RA (%)	68.0	65.4	2.6		0.057*
Days to first job†	133.9	157.1	-23.2		
Survival analysis				1.111	0.006***
Total days employed in analysis period	286.7	274.3	12.4		0.148
Total days employed in analysis period† (excluding those with no employment)	419.0	419.6	-0.6		0.873
Compensation and Benefits					
Hourly wage at first job ($)†	10.66	10.42	0.23		0.347
Hourly wage at most recent job ($)†	12.75	12.95	-0.20		0.761
Total income from all sources ($)	10,998	10,115	883		0.031**

SOURCE: Two-year follow-up survey of study participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. This process is described in detail in the Technical Appendix.

Mean values and differences in this table are weighted to account for survey non-response. P-values are similarly weighted and also regression adjusted for pre-random assignment characteristics.

†Results for these outcomes are calculated only for those study participants who found work following random assignment. Because post-RA employment is itself correlated with treatment status, the experimental design no longer guarantees equivalence between treatment and control groups (within this subset). These results should therefore be interpreted as suggestive rather than as true impact estimates.

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 of $10,998, which is almost 10% higher than the control group average of $10,115.

The evaluation did not reveal an impact for all of the measures analyzed. There was no significant impact on the total days employed during the study period (whether or not those who had ever worked are excluded), and there was no apparent effect on wages.[62]

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[61] The main advantage of survival analysis over a comparison of the difference in means is that the former approach incorporates information from those study participants who *never* found work, while the latter necessarily excludes those individuals who were not able to find employment.

[62] As noted, the wage outcome results are generated following a partitioning of the sample on a post-RA attribute (employment) and thus do not provide unbiased estimates of the effect of RExO on wages.

There are several possible explanations for why a significant effect is observed for total income but not for wages: It is possible that program group members worked more total hours than did control group members. It is also possible that the RExO intervention increased the ability of program participants to acquire non-wage income. Finally, it is possible that an effect on wages in fact did exist, but was not identified in this analysis because of the selection issue discussed above.

Nevertheless, the totality of the evidence provides fairly strong support for the conclusion that the RExO program had a beneficial effect on participants' self-reported labor market outcomes, with statistically significant improvements seen for four of the seven key measures of labor market performance. Although in most cases the impacts are not very large, they are consistent across multiple measures, which in combination with the statistical assessment provides substantial evidence that RExO did positively affect its participants' self-reported labor market outcomes.

Impacts for Subgroups

This section assesses the impacts of RExO on labor market outcomes for six different partitions of the sample, which create a total of 13 subgroups. Complete descriptions of these subgroups and the reasons for selecting them can be found in Chapter I. Results of the analysis of the seven primary labor market outcomes for the subgroups are displayed in Tables III-2 through III-7.

For most of the subgroups analyzed, the effects seen in the main analysis persisted. Within subgroups, program group members were in general more likely to find work (and find work sooner) and had higher average incomes—matching the findings of the main analysis. These differences were not present in all subgroups, and in several cases were present but not statistically significant, perhaps because differences that were statistically meaningful in the full sample are not so when the sample is partitioned and therefore reduced in size.

While there are clear differences across the subgroups in the overall labor market outcomes obtained, there is little evidence that the magnitude of the impacts differs across these subgroups. Specifically, while the percentage of participants who obtained employment in the first and second years after RA varied from 62 to 74 percent across subgroups, the relative difference between program and control group members across these subgroups was not statistically significant.[63] Thus, the size of the impacts does not appear to vary significantly when the sample is divided into subgroups.

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[63] The analyses of the differential impacts across subgroups were conducted using fully interacted models (Lowenstein et al., 2014).

There is one exception to this generalization, however. For the subgroup partition based on time of enrollment (i.e., enrolled before October 1 vs. October 1 or later), significantly greater impacts were observed for one subgroup relative to the other. Program group members who enrolled in the study *after* October 1 observed greater impacts from RExO (compared to control group members enrolling during the same time period) than did those who enrolled prior to October 1.

Interestingly, this effect went in the opposite direction of what was predicted. Because the implementation study found that the RExO grantees were generally understaffed during the latter months of enrollment, the evaluation team developed the hypothesis that the grantees may have been unable to provide the full complement of services to their participants during the final months of study enrollment, thereby reducing the likelihood of RExO having a positive impact on labor market outcomes.

Examining the data for this subgroup analysis (Table III-2), it is clear that the primary difference among the subgroups is that control group members who enrolled in the study after October 1 had worse outcomes than any of the other subgroup samples. Given that RA occurred throughout the enrollment period and should have ensured the general equivalence of the groups, this finding may indicate that the grantees enrolled a more challenging population during the latter months of the RExO grant, perhaps in an effort to meet their target number of participants. Despite this, RExO grantees were able to assist their participants during this period to achieve similarly high employment outcomes, indicating that there was no drop-off in the level of services during the latter period of enrollment. This finding cannot be verified with the available data, however, and thus should be considered speculative.

Table III-2:

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

	Timing of Random Assignment									
	Pre-October Assignment					Post-October Assignment				
Outcome	**Program**	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**	**Program**	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**
Any job (1-year) (%)≠	71.1	68.7	2.4		0.201	71.9	65.8	6.1		0.042**
Any job (2-year) (%)	68.4	65.7	2.7		0.169	67.1	64.7	2.4		0.434
Days to first employment	135.0	150.2	-15.2			131.2	173.8	-42.6		
Survival analysis				1.135	0.006***				1.054	0.466
Total days employed	291.3	278.7	12.6		0.271	275.0	263.4	11.6		0.559
Total days employed (excluding those w/ no employment) †	421.7	427.8	-6.1		0.596	412.1	399.6	12.5		0.509
Wage at first job ($)†,≠	10.39	10.74	-0.35		0.355	11.40	9.68	1.73		0.002***
Wage at last job ($)†	12.95	12.85	0.10		0.839	12.27	13.20	-0.93		0.194
Total income ($)	11,093	10,352	741		0.205	11,156	9,537	1,619		0.050*

SOURCE: Two-year follow-up survey of study participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. This process is described in detail in the Technical Appendix.

Mean values and differences in this table are weighted to account for survey non-response. P-values are similarly weighted and also regression adjusted for pre-random assignment characteristics.

†Results for these outcomes are calculated only for those study participants who found work following random assignment. Because post-RA employment is itself correlated with treatment status, the experimental design no longer guarantees equivalence between treatment and control groups (within this subset). These results should therefore be interpreted as suggestive rather than as true impact estimates.

Sample sizes are 2,551 (pre-October cohort) and 1,030 (post-October cohort).

For outcomes marked with an inequality sign (≠), the difference between subgroup effects in a fully interacted model were statistically significant.[64]

[64] See Lowenstein et al. (2014) for a description of this method.

Table III-3:

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

	Timing of Random Assignment									
	Early Assignment					Late Assignment				
Outcome	**Program group**	**Control group**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**	**Program group**	**Control group**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**
Any job (1-year) (%)	72.6	68.3	4.3		0.016**	65.9	66.6	-0.7		0.848
Any job (2-year) (%)	68.4	65.2	3.2		0.088*	66.3	66.2	0.1		0.970
Days to first employment	126.7	152.2	-25.5			165.3	173.9	-8.7		
Survival analysis				1.099	0.030**				1.082	0.364
Total days employed	294.9	275.9	18.9		0.084*	257.0	269.3	-12.4		0.516
Total days employed (excluding those w/ no employment) †	423.5	419.7	3.8		0.733	407.4	414.8	-7.4		0.744
Wage at first job ($)†	10.80	10.54	0.26		0.501	10.27	10.16	0.11		0.988
Wage at last job ($)†	12.93	13.21	-0.28		0.529	11.86	12.09	-0.13		0.929
Total income ($)	11,571	10,303	1,268		0.026**	9,696	9,645	51		0.954

SOURCE: Two-year follow-up survey of study participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

Mean values and differences in this table are weighted to account for survey non-response. P-values are similarly weighted and also regression adjusted for pre-random assignment characteristics.

†Results for these outcomes are calculated only for those study participants who found work following random assignment. Because post-RA employment is itself correlated with treatment status, the experimental design no longer guarantees equivalence between treatment and control groups (within this subset). These results should therefore be interpreted as suggestive rather than as true impact estimates.

Sample sizes are 2,785 (early assignment) and 704 (late assignment).

Table III-4:

Impacts on Labor Market Outcomes, by Age

	Age									
	Under 27					27 and older				
Outcome	**Program**	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**	**Program**	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**
Any job (1-year) (%)	71.9	69.0	2.9		0.100	69.1	62.9	6.2		0.100
Any job (2-year) (%)	68.2	65.9	2.3		0.198	67.2	63.4	3.8		0.317
Days to first employment	124.4	147.8	-23.5			166.0	195.5	-29.5		
Survival analysis				1.083	0.064*				1.253	0.011**
Total days employed	290.4	283.3	7.1		0.520	271.8	235.2	36.6		0.064*
Total days employed (excluding those w/ no employment) †	432.6	429.2	3.4		0.759	369.6	375.4	-5.8		0.788
Wage at first job ($)†	10.83	10.67	0.16		0.687	9.96	9.47	0.50		0.308
Wage at last job ($)†	13.04	13.07	-0.03		0.939	11.69	12.46	-0.77		0.412
Total income ($)	11,642	10,702	940		0.090*	8,949	7,567	1,382		0.109

SOURCE: Two-year follow-up survey of study participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

Mean values and differences in this table are weighted to account for survey non-response. P-values are similarly weighted and also regression adjusted for pre-random assignment characteristics.

†Results for these outcomes are calculated only for those study participants who found work following random assignment. Because post-RA employment is itself correlated with treatment status, the experimental design no longer guarantees equivalence between treatment and control groups (within this subset). These results should therefore be interpreted as suggestive rather than as true impact estimates.

Sample sizes are 2,882 (under 27) and 699 (27 and older).

Table III-5:

Impacts on Labor Market Outcomes, by Number of Prior Convictions

	Number of Prior Convictions									
	3 or Fewer					4 or more				
Outcome	**Program**	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**	**Program**	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**
Any job (1-year) (%)	72.0	69.0	3.0		0.214	71.0	64.9	6.2		0.013**
Any job (2-year) (%)	71.9	69.3	2.7		0.262	63.3	61.6	1.7		0.498
Days to first employment	131.1	159.0	-27.9			127.2	154.1	-26.9		
Survival analysis				1.061	0.301				1.168	0.010**
Total days employed	297.6	277.8	19.8		0.204	278.3	266.2	12.1		0.486
Total days employed (excluding those w/ no employment) †	427.5	423.7	3.8		0.794	412.6	414.0	-1.3		0.932
Wage at first job($)†	10.59	10.41	0.17		0.709	10.95	10.48	0.47		0.259
Wage at last job($)†	12.81	12.47	0.34		0.447	12.92	13.27	-0.35		0.586
Total income($)	11,425	10,865	560		0.439	11,240	9,421	1,819		0.017**

SOURCE: Two-year follow-up survey of study participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

Mean values and differences in this table are weighted to account for survey non-response. P-values are similarly weighted and also regression adjusted for pre-random assignment characteristics.

†Results for these outcomes are calculated only for those study participants who found work following random assignment. Because post-RA employment is itself correlated with treatment status, the experimental design no longer guarantees equivalence between treatment and control groups (within this subset). These results should therefore be interpreted as suggestive rather than as true impact estimates.

Sample sizes are 1,584 (3 or fewer convictions) and 1,519 (4 or more convictions). Conviction data not available for all states.

Table III-6:

Impacts on Labor Market Outcomes, by Gender

	Gender									
	Female					Male				
Outcome	Program	Control	Difference (Impact)	Hazard Ratio (Impact)	P-value	Program	Control	Difference (Impact)	Hazard Ratio (Impact)	P-value
Any job (1-year) (%)	69.1	68.2	1.0		0.788	71.9	67.5	4.4		0.015**
Any job (2-year) (%)	70.8	65.6	5.2		0.142	67.4	65.0	2.4		0.188
Days to first employment	139.6	157.6	-18.0			132.0	156.4	-24.5		
Survival analysis				1.090	0.320				1.117	0.010**
Total days employed	288.2	288.0	0.1		0.999	287.6	270.0	17.6		0.095*
Total days employed (excluding those w/ no employment) †	425.9	425.5	0.4		0.987	418.9	417.1	1.8		0.870
Wage at first job($)†	9.17	8.79	0.38		0.416	11.07	10.83	0.24		0.498
Wage at last job($)†	12.00	11.64	0.36		0.843	12.97	13.27	-0.30		0.573
Total income($)	9,151	8,584	567		0.426	11,662	10,458	1,205		0.035**

SOURCE: Two-year follow-up survey of study participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

Mean values and differences in this table are weighted to account for survey non-response. P-values are similarly weighted and also regression adjusted for pre-random assignment characteristics.

†Results for these outcomes are calculated only for those study participants who found work following random assignment. Because post-RA employment is itself correlated with treatment status, the experimental design no longer guarantees equivalence between treatment and control groups (within this subset). These results should therefore be interpreted as suggestive rather than as true impact estimates.

Sample sizes are 731 (female) and 2,823 (male).

Table III-7:

Impacts on Labor Market Outcomes, by Educational Attainment

Outcome	Program	Control	Difference (Impact)	Hazard Ratio (Impact)	P-value
No GED/HS Diploma					
Any job (1-year) (%)	68.3	63.0	5.3		0.038**
Any job (2-year) (%)	66.4	61.9	4.5		0.080*
Days to first employment	150.0	171.2	-21.2		
Survival analysis				1.127	0.045**
Total days employed	266.6	249.2	17.4		0.324
Wage at first job($)†	10.30	10.38	-0.07		0.723
Wage at last job($)†	12.58	12.50	0.08		0.859
Total income($)	10,097	8,443	1,655		0.011**
GED					
Any job (1-year) (%)	73.5	70.2	3.3		0.280
Any job (2-year) (%)	67.6	63.7	3.9		0.224
Days to first employment	130.4	147.7	-17.3		
Survival analysis				1.056	0.463
Total days employed	292.2	293.8	-1.5		0.931
Wage at first job($)†	10.90	10.04	0.86		0.076*
Wage at last job($)†	12.40	12.53	-0.13		0.841
Total income($)	10,868	11,086	-217		0.816
HS Diploma					
Any job (1-year) (%)	73.9	72.3	1.6		0.554
Any job (2-year) (%)	70.7	71.4	-0.7		0.813
Days to first employment	115.5	147.7	-32.2		
Survival analysis				1.146	0.046**
Total days employed	292.2	293.8	-1.5		0.931
Wage at first job($)†	10.92	10.76	0.16		0.963
Wage at last job($)†	13.30	13.80	-0.50		0.486
Total income($)	12,709	11,434	1,275		0.183

SOURCE: Two-year follow-up survey of study participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

Mean values and differences in this table are weighted to account for survey non-response. P-values are similarly weighted and also regression adjusted for pre-random assignment characteristics.

†Results for these outcomes are calculated only for those study participants who found work following random assignment. Because post-RA employment is itself correlated with treatment status, the experimental design no longer guarantees equivalence between treatment and control groups (within this subset). These results should therefore be interpreted as suggestive rather than as true impact estimates.

The HS diploma category includes a small number of offenders with post-HS education.

Sample sizes are 1,529 (no GED/diploma), 952 (GED) and 1,100 (HS diploma).

For outcomes marked with an inequality sign (≠), the difference between subgroup effects in a fully interacted model were statistically significant.

Twelve percent of the sample reported their level of education as being “some college” or higher. This fraction was too small for meaningful subgroup analysis specific to this group; instead these individuals were consolidated with individuals who reported receipt of a HS diploma and analyzed as a single group.

Summary

Evidence described in this chapter indicates that RExO had beneficial impacts on the labor market outcomes of participants, though the impacts were generally small. Program group members were more likely to find work and had higher average incomes than did control group members. These differences persist across several different measures and within several different subgroups.

One limitation of these analyses is that they are derived solely from responses to follow-up surveys of program and control group members. If program group members consciously or unconsciously altered their responses to questions about labor market outcomes (because they knew they had received elevated levels of services and did not want to disappoint interviewers, for example), this would bias the results, and the survey data do not allow an assessment of the extent to which this occurred. While random assignment helps to ensure that program and control group members are similar at the point of random assignment, it cannot rule out that differences between them in the likelihood of mis-reporting outcomes emerge after RA. Thus, it is possible that the positive results described in this chapter are driven by some level of response bias in the survey data.

The final impact report will supplement the survey analysis by estimating the effect of RExO on participant labor market outcomes using administrative data. Information from the National Directory of New Hires (NDNH)—which the evaluation team is in the process of obtaining—will enable an examination of how RExO affected participants' earnings and employment using an objective and independent source of data. Should this analysis confirm the general findings from this chapter, it would provide additional support that RExO had positive impacts on a number of labor market outcomes.

IV. IMPACTS ON CRIMINAL JUSTICE OUTCOMES

This chapter explores the extent to which the RExO program helped to reduce recidivism among participants during the two-year period following RA. Whereas a single data set (responses to the follow-up survey) was the basis for the analysis of employment outcomes, two sets of data are available to examine recidivism among participants in this study. The first of these is administrative data on arrests, convictions, and incarceration, which were collected from each state (or locality) in which the RExO program operated. The second comes from the follow-up survey, which asked respondents several questions about their involvement in the criminal justice system since being enrolled into the study.

Because these two sets of data are different and required different analyses, findings on RExO's impact on recidivism are discussed below in two separate sections, the first based on the administrative data and the second on the survey data. As with the analysis of employment and earnings discussed in the previous chapter, these discussions present general results using relatively simple models that summarize the main thrust of the findings. The Technical Appendix at the end of this report presents a series of statistical models that extend and elaborate upon the results presented in this chapter. To assist with understanding the specific meanings of some terms used in this chapter, a glossary of terms used to describe recidivism outcomes is provided at the end of the chapter.

Impacts on Recidivism 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 (though data are missing for some sites across data sources).[65] These data were analyzed to determine the extent to which RExO had an impact on the recidivism rates of program participants.

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65 No arrest and conviction data were received from Louisiana or Ohio; thus the analyses of these data were restricted to participants from the remaining 21 grantees. Furthermore, no prison incarceration data were received from Illinois, Louisiana, or Michigan; thus analyses of these data were restricted to participants from the remaining 20 grantees.

Impacts for the Full Sample

Table IV-1 shows the two-year impacts of the RExO program on key measures of recidivism, including arrest, conviction, state prison incarceration, and local jail incarceration. The final row of the table reports a composite measure of recidivism based on whether a sample member had any recidivism event during the two-year follow-up period, whether it was an arrest, a conviction, an admission to prison, or any combination of the three.[66,67]

The data in Table IV-1 indicate that the RExO programs did not have statistically significant impacts on key measures of recidivism within the two years following random assignment. As the table shows, 42 percent of the individuals in the program group and 43 percent of those in the control group were arrested during the two-year follow-up period, and about one-quarter of the individuals in both groups were convicted; in both cases the differences were not statistically significant. Table IV-1 also shows that there were no significant differences between the groups in the type of crime for which recidivists were convicted.

Table IV-1 further shows that RExO had no significant impact on state prison or jail incarceration during the two-year follow-up period. During this time, about 25 percent of sample members were admitted to state prison. Most of these admissions were for parole or probation violations (about 13 percent), with a slightly smaller percentage (about 10 percent) representing new sentences.[68] Sample members spent an average of 76 total days in prison during the follow-up period. There also are no significant impacts on admissions to jail, regardless of admission reason, nor on total days spent in jail. For the two sites for which jail data were available, slightly more than half of the individuals in each group were admitted to jail, where they spent an average total of about 50 days over the course of the follow-up period.

Only one statistically significant difference between program and control group members emerged from the analysis. Program group members were significantly *more* likely to be convicted of a felony than control group members (13.2 percent compared with 11.4 percent). Although this difference in outcomes suggests that RExO had an impact contrary to that which was intended, it is uncertain that this should be construed as strong evidence of a true impact of the program because it was the only significant difference between program group and control

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66 Because jail data were only available for 2 of the 24 sites, they are not included in the composite measure.

67 The concern about multiple comparisons raised in chapter III is also present here, and thus one could have chosen to limit the number of comparisons in this analysis as well. However, as will be shown, the lack of statistically significant findings in these data that may be subject to concerns about multiple comparisons led to a decision to provide results for all possible outcomes.

68 Some prison incarceration records were missing on type of admission.

group recidivism outcomes that emerged among many such comparisons. As such, it is quite possible that it is solely an effect of the multiple comparisons concern.

Table IV-1:
Two-Year Impacts on Recidivism Using Administrative Data: Full Sample

Outcome	Program Group	Control Group	Difference (Impact)	P-Value
-				
Arrested (%)	42.0	43.2	-1.2	0.395
Convicted of a crime[a] (%)	25.1	24.0	1.1	0.409
Convicted of a felony	13.2	11.4	1.8	0.087*
Convicted of a misdemeanor	11.5	10.9	0.6	0.535
Conviction categories[b] (%)				
Convicted of a violent crime	3.3	3.2	0.2	0.748
Convicted of a property crime	8.1	7.6	0.4	0.602
Convicted of a drug crime	7.8	6.6	1.2	0.156
Convicted of a public order crime	10.9	11.1	-0.2	0.825
Admitted to prison (%)	24.7	25.0	-0.3	0.823
Admitted to prison for a new crime	9.5	10.3	-0.8	0.421
Admitted to prison for a parole/probation violation	13.2	12.5	0.7	0.493
Admitted to prison for other reason/reason unknown	3.9	3.9	0.0	0.463
Total days incarcerated in prison	76	76	0	0.946
Admitted to jail[c] (%)	53.4	53.7	-0.4	0.944
Admitted to jail for a new crime	15.7	16.9	-1.1	0.785
Admitted to jail for a parole/probation violation	21.4	21.6	-0.3	0.949
Admitted to jail for other reason	36.0	35.7	0.4	0.938
Total days incarcerated in jail	51	44	8	0.400
Arrested, convicted, or admitted to prison (%)	48.9	50.8	-1.9	0.204
Sample size, arrest and conviction outcomes (total = 4,060)	2,447	1,613		
Sample size, prison outcomes (total = 4,014)	2,417	1,597		
Sample size, jail outcomes (total = 340)	206	134		

SOURCE: Calculations based on administrative data from criminal justice agencies in each state

NOTES: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1.
Results in this table are regression-adjusted for pre-random assignment characteristics.
Subcategories may sum to more than the total due to multiple arrests, convictions, or prison admissions per person during the follow-up period.

[a]Each conviction date is counted only as a single event. If there were multiple convictions on the same date, only the most serious conviction is recorded in the analysis. Some convictions may have been associated with an arrest that occurred prior to random assignment. These convictions are counted in the analysis as occurring after random assignment.

[b]The categorization of charges is based on definitions from Langan and Levin (2002).

[c]Jail data were only available for two states--New Jersey and Michigan.

Even if the RExO program did not reduce recidivism over the two-year follow-up period as a whole, it is possible that it could have delayed recidivism during the time in which program group members were actively participating. Any delays in recidivism—especially in incarceration—produced by the program may yield substantial cost savings, even if overall recidivism rates within the two-year period were no different between the study groups. If, for example, the program group was incarcerated, on average, one month later than the control group, the savings in state prison costs would be substantial, despite both groups having similar overall incarceration rates.

Two analyses were conducted to test the hypothesis that RExO delayed recidivism. Figure IV-1 shows the results of the first analysis, which determined the percentage of sample members in each group who were arrested or incarcerated at least once as of a given month. This percentage is termed the *failure rate*. The point estimates show that program group members were arrested slightly sooner in the first eight months after random assignment than were control group members, though this difference does not reach statistical significance. After Month 8, the two groups had nearly identical failure rates. The point estimates for incarceration show a similar pattern of nearly identical failure rates for the two groups.

The second analysis divided the data by follow-up time period, calculating the impacts of RExO during the first year following random assignment separately from the impacts during the second year. The results, presented in Table IV-2, do not show significant impacts on key measures of recidivism during either the first year or the second year, nor significant differences between the two years. In both Year 1 and Year 2, about one-third of sample members were arrested, convicted, or admitted to state prison at least once. Considering the findings from both analyses, it must be concluded that RExO did not delay recidivism.

Overall, the rates for each of the outcomes of interest (arrests, convictions, and incarceration) are noticeably lower among the RExO study sample members (both treatments and controls) as compared to national averages for newly released offenders. According to the most recent national study of recidivism, 60 percent of inmates released in 2005 were arrested within two years of release, 36 percent were convicted, and 43 percent were returned to prison.[69]

The RExO study participants and the national sample of inmates released in 2005 have several dissimilarities that may account for their differences in conviction and prison admission rates. To begin with, there is very likely some selection bias, as people who sought out reentry services, or who met the multiple screening criteria used by many of the RExO grantees, may be more motivated to avoid recidivating than the general population of released offenders. Therefore one

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[69] Durose, et al. (2014)

might expect RExO sample members, who all signed up for participation in reentry services, to be less likely to recidivate than members of the national sample. In addition, RExO's proportion of women (19%) was nearly twice that of the national sample (11%); because women are less likely to recidivate, it is conceivable that RExO's larger proportion of women would translate into a lower rate of recidivism. Furthermore, the RExO sample only includes some of the states that were included in the national study, and it is possible that recidivism rates in these states differ from those in other states. No matter the reason, it is clear that the sample of RExO participants has substantially better recidivism outcomes than the "average" offender returning from prisons or jails. Given the lack of impacts found for most outcomes, this cannot be explained by the efficacy of the program.

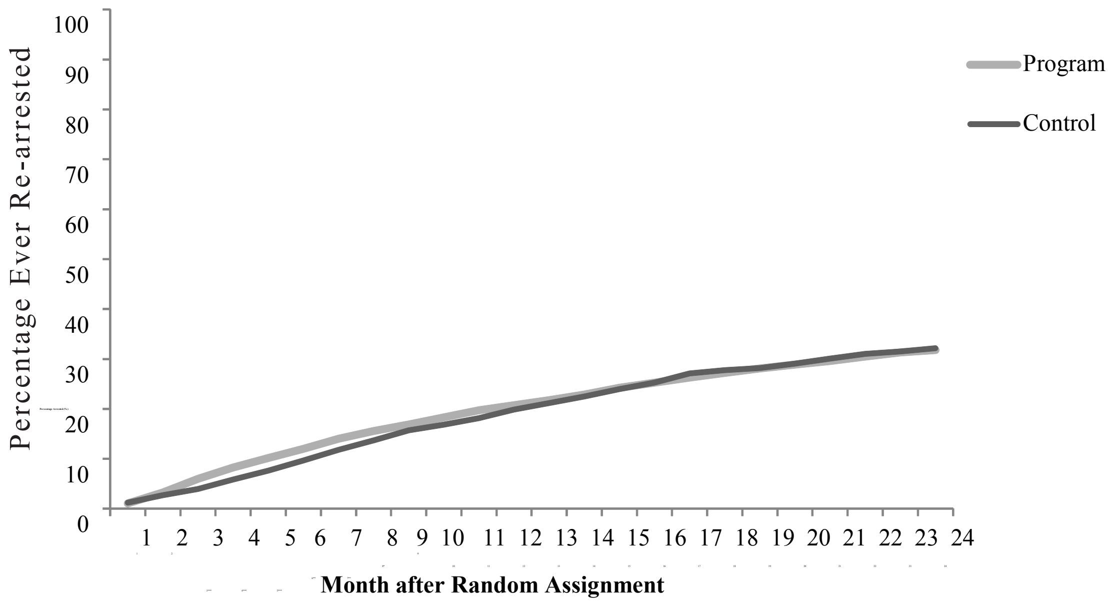
Impacts for Subgroups

This section describes the results of analyses of impacts by subgroup. These analysis use the same subgroup partitions described in the previous chapter; they are based on age (age 27 years and older vs. under 27), gender, education (no high school degree or GED, GED only, high school diploma), and number of prior convictions (three or fewer vs. four or more). The results of these analyses are shown in Tables IV-3 through IV-6.

A review of these results indicates that there were no statistically significant differences in RExO's impacts by subgroup. While the subgroups did differ in recidivism rates, there was no evidence of different *impacts* between subgroups. For example, recidivism levels were higher for men than for women, but the analysis did not reveal a difference in the impact of RExO between these two groups. Similarly, although sample members age 27 and older had lower rates of recidivism than their younger counterparts, there was no difference in the impacts RExO had across the age groups.

Figure IV-1:
Failure Curves for Arrest and State Prison Incarceration, Administrative Data

IV-1A:
Arrest Failure Curve



IV-1B:
State Prison Incarceration Failure Curve

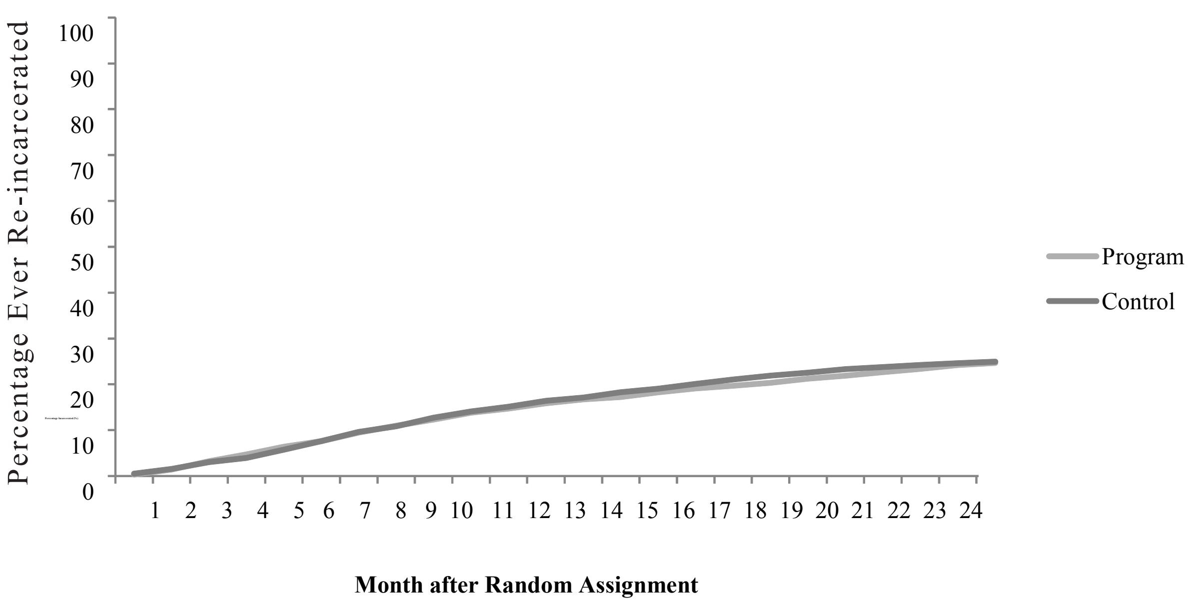


Table IV-2:
Impacts on Recidivism Using Administrative Data, by Year: Full Sample

Outcome	Program Group	Control Group	Difference (Impact)	P-Value
Year 1				
Arrested (%)	27.8	27.6	0.2	0.911
Convicted of a crime[a] (%)	13.3	12.6	0.7	0.480
Convicted of a felony	5.7	5.4	0.4	0.602
Convicted of a misdemeanor	6.0	5.9	0.1	0.848
Admitted to prison (%)	15.9	16.4	-0.6	0.615
Admitted to prison for a new crime	4.4	5.1	-0.7	0.290
Admitted to prison for a parole/probation violation	10.0	10.0	0.0	0.961
Total days incarcerated in prison	40	39	1	0.720
Arrested, convicted, or admitted to prison (%)	34.8	36.0	-1.1	0.442
Year 2				
Arrested (%)	26.5	26.8	-0.3	0.830
Convicted of a crime[a] (%)	16.0	15.7	0.3	0.801
Convicted of a felony	8.3	6.9	1.5	0.089*
Convicted of a misdemeanor	6.7	6.6	0.1	0.853
Admitted to prison (%)	12.1	12.6	-0.5	0.642
Admitted to prison for a new crime	5.4	5.6	-0.3	0.714
Admitted to prison for a parole/probation violation	4.6	4.4	0.2	0.762
Total days incarcerated in prison	36	37	-1	0.848
Arrested, convicted, or admitted to prison (%)	34.0	34.2	-0.2	0.898
Sample size, arrest and conviction outcomes (total = 4,060)	2,447	1,613		
Sample size, prison outcomes (total = 4,014)	2,417	1,597		

SOURCE: Calculations based on administrative data from criminal justice agencies in each state

NOTES: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1.
Results in this table are regression-adjusted for pre-random assignment characteristics.
Subcategories may sum to more than the total due to multiple arrests, convictions, or prison admissions per person during the follow-up period.

[a]Each conviction date is counted only as a single event. If there were multiple convictions on the same date, only the most serious conviction is recorded in the analysis. Some convictions may have been associated with an arrest that occurred prior to random assignment. These convictions are counted in the analysis as occurring after random assignment.

Table IV-3
Impacts on Recidivism Using Administrative Data, by Gender: Full Sample

	Gender							
	Male				Female			
Outcome	Program Group	Control Group	Difference (Impact)	P-Value	Program Group	Control Group	Difference (Impact)	P-Value
Arrested (%)	33.3	34.1	-0.8	0.569	24.0	24.8	-0.7	0.813
Convicted of a crime (%)	22.3	21.1	1.1	0.394	16.0	16.4	-0.3	0.901
Admitted to prison (%)	26.4	27.0	-0.5	0.722	16.3	16.2	0.1	0.981
Admitted to prison for a new crime	10.2	10.5	-0.3	0.785	6.0	9.9	-3.9	0.050*
Admitted to prison for a parole/probation violation	14.0	13.9	0.2	0.895	9.4	5.9	3.5	0.090*
Total days incarcerated in prison	82	81	1	0.800	47	59	-12	0.165
Arrested, convicted, or admitted to prison (%)	46.1	47.6	-1.5	0.366	30.7	34.3	-3.6	0.310
Sample size, arrest and conviction outcomes (total = 4,032)	1,985	1,323			446	278		
Sample size, prison outcomes (total = 3,987)	1,954	1,312			447	274		

SOURCE: Calculations based on administrative data from criminal justice agencies in each state

NOTES: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1.
Subcategories may sum to more than the total due to multiple arrests, convictions, or incarcerations per person during the follow-up period.
The H-statistic was calculated to assess whether the difference in impacts between the subgroups is statistically significant. None of the comparisons between the subgroups was significant at the .1 level.

Table IV-4
Impacts on Recidivism Using Administrative Data, by Age: Full Sample

	Age							
	Under 27				27 and older			
Outcome	**Program Group**	**Control Group**	**Difference (Impact)**	**P-Value**	**Program Group**	**Control Group**	**Difference (Impact)**	**P-Value**
Arrested (%)	39.4	40.7	-1.3	0.674	29.6	30.6	-1.0	0.469
Convicted of a crime (%)	26.6	25.4	1.2	0.686	19.8	19.3	0.6	0.659
Admitted to prison (%)	28.9	29.0	-0.1	0.963	23.4	24.4	-1.0	0.501
Admitted to prison for a new crime	12.4	15.9	-3.5	0.158	8.7	9.2	-0.5	0.595
Admitted to prison for a parole/probation violation	12.6	11.6	1.0	0.663	13.3	12.9	0.4	0.721
Total days incarcerated in prison	86	90	-4	0.686	73	74	-1	0.910
Arrested, convicted, or admitted to prison (%)	58.4	57.9	0.5	0.867	34.5	35.3	-0.8	0.761
Sample size, arrest and conviction outcomes (total = 4,060)	478	314			1,969	1,299		
Sample size, prison outcomes (total = 4,014)	477	300			1,940	1,297		

SOURCE: Calculations based on administrative data from criminal justice agencies in each state

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

Subcategories may sum to more than the total due to multiple arrests, convictions, or incarcerations per person during the follow-up period.

The H-statistic was calculated to assess whether the difference in impacts between the subgroups is statistically significant. None of the comparisons between the subgroups was significant at the .1 level.

Table IV-5
Impacts on Recidivism Using Administrative Data, Educational Attainment: Full Sample

Outcome	Program Group	Control Group	Difference (Impact)	P-Value
No GED/ HS Diploma				
Arrested (%)	29.0	30.4	-1.3	0.483
Convicted of a crime (%)	19.8	20.9	-1.1	0.549
Admitted to prison (%)	26.5	26.9	-0.4	0.860
Admitted to prison for a new crime	11.0	11.8	-0.7	0.632
Admitted to prison for a parole/probation violation	14.1	12.8	1.3	0.400
Total days incarcerated in prison	72	72	0	0.971
Arrested, convicted, or admitted to prison (%)	44.1	46.3	-2.2	0.351
GED				
Arrested (%)	37.3	36.9	0.4	0.877
Convicted of a crime (%)	24.9	22.6	2.3	0.348
Admitted to prison (%)	28.1	28.5	-0.4	0.869
Admitted to prison for a new crime	10.2	11.6	-1.4	0.456
Admitted to prison for a parole/probation violation	15.5	15.5	0.0	0.982
Total days incarcerated in prison	80	84	-3	0.685
Arrested, convicted, or admitted to prison (%)	48.7	49.1	-0.4	0.899
High School Diploma				
Arrested (%)	30.8	32.2	-1.4	0.626
Convicted of a crime (%)	21.0	16.9	4.0	0.105
Admitted to prison (%)	18.3	16.8	1.5	0.546
Admitted to prison for a new crime	7.1	6.7	0.3	0.849
Admitted to prison for a parole/probation violation	8.5	8.2	0.4	0.846
Total days incarcerated in prison	70	61	9	0.307
Arrested, convicted, or admitted to prison (%)	38.2	39.9	-1.7	0.597
Sample size, arrest and conviction outcomes (total = 3,745)	2,282	1,463		
Sample size, prison outcomes (total = 3,727)	2,272	1,455		

SOURCE: Calculations based on administrative data from criminal justice agencies in each state

NOTES: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1.
Subcategories may sum to more than the total due to multiple arrests, convictions, or incarcerations per person
during the follow-up period.
The H-statistic was calculated to assess whether the difference in impacts between the subgroups is statistically significant. None of the comparisons between the subgroups was significant at the .1 level.

Table IV-6
Impacts on Recidivism Using Administrative Data, Number of Prior Convictions : Full Sample

	Number of Prior Convictions							
	3 or Fewer				4 or More			
Outcome	**Program Group**	**Control Group**	**Difference (Impact)**	**P-Value**	**Program Group**	**Control Group**	**Difference (Impact)**	**P-Value**
Arrested[b] (%)	21.6	22.2	-0.6	0.694	43.0	42.1	0.9	0.675
Convicted of a crime[c] (%)	12.5	11.3	1.2	0.407	31.2	28.8	2.4	0.235
Admitted to prison (%)	17.9	18.9	-1.0	0.543	32.3	32.7	-0.4	0.865
Admitted to prison for a new crime	7.6	8.2	-0.6	0.632	10.6	12.2	-1.6	0.287
Admitted to prison for a parole/probation violation	9.3	10.0	-0.7	0.605	18.6	16.5	2.1	0.229
Total days incarcerated in prison	56	60	-4	0.488	96	93	4	0.609
Arrested, convicted, or admitted to prison (%)	31.8	33.7	-1.9	0.338	56.0	56.3	-0.3	0.900
Sample size, arrest and conviction outcomes (total = 4,060)	1,206	766			1241	847		
Sample size, prison outcomes (total = 3,807)	1,146	724			1147	790		

SOURCE: Calculations based on administrative data from criminal justice agencies in each state

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

Subcategories may sum to more than the total due to multiple arrests, convictions, or incarcerations per person during the follow-up period.

The H-statistic was calculated to assess whether the difference in impacts between the subgroups is statistically significant. None of the comparisons between the subgroups was significant at the .1 level.

[b]Each arrest date is counted only as a single event. If there are multiple crimes or charges on the same date, only the most serious charge is recorded in the analysis. Some convictions may have been associated with an arrest that occurred prior to random assignment. These convictions are counted in the analysis as occurring after random assignment. Total includes convictions for felony, misdemeanor, and other crime classes.

Impacts on Recidivism Based on Survey Data

The analysis of recidivism using administrative data, described above, is limited in some respects because some states in which RExO operated did not provide data. In addition, even among offenders for whom data are available, this information only covers the specific state in which random assignment occurred. For these reasons, results generated using the administrative data may not provide a fully accurate picture of recidivism among study participants and 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.

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 in Chapter I) was administered approximately two years after the point of random assignment, so the data cover approximately the same time period as the administrative data that served as the basis for the analysis described in the previous section.

As described above, the survey was fielded to all 4,655 study participants. Although not all respondents completed the survey exactly two years after they were enrolled into the study, each was asked about the two-year reference period following their enrollment into the study. Ultimately, 3,581 participants completed the two-year survey, yielding a response rate of 76.9 percent. Although the survey asked a wide range of questions about recidivism, the analysis in this section is based on data from a subset of these questions so that it can mirror as much as possible the analyses of the administrative data described above.

Impacts for the Full Sample

Table IV-7 shows the two-year impacts of the RExO program on measures of recidivism, drawn from the self-reported survey responses. A review of these findings indicates that they are generally consistent with the conclusions drawn from the administrative data analysis: there is no strong evidence to support the hypothesis that the RExO program reduced recidivism. Specifically, for most measures of recidivism, including those measuring convictions, parole violations, incarceration, and the time to first arrest, there were no significant differences between the program and control groups.

However, the results from the analysis of survey data are slightly at odds with the administrative data analysis in one respect; there is a difference between the program and control groups in arrest rates following RA. As the first row of Table IV-7 shows, 18.4 percent of the individuals in the program group and 20.5 percent of the individuals in the control group reported being arrested within the 1-year period following random assignment. At the two-year mark, these figures were 36.9 and 41.0 percent, respectively. These differences are both statistically significant. It is possible that this difference is a meaningful one, perhaps driven by the fact that the survey data include participants from every grantee, and thus are not missing data from any of the states in which RExO operated.

But a more likely explanation is that there is some differential recall bias among respondents that leads to the statistically significant difference observed. To test this possibility, the administrative data were linked with the survey data to identify cases in which the two datasets do not agree. If the administrative data are limited only to those who responded to the survey, arrests rates in the first year after RA are slightly lower than were reported for the full sample (26.0 percent for survey respondents versus 27.1 percent for the full sample), indicating there is some small bias in who responded to the survey: those who were arrested were less likely to respond to the survey.

But the results shown in Table IV-7 suggest even lower arrest rates than this (19.2 percent of survey respondents reported being arrested compared to the 26.0 percent that had an arrest in the administrative data). Thus, there seems to be clear underreporting of arrests among survey respondents. Examining the data further, program group members were somewhat more likely to underreport (or mis-report) arrests than were control group members. Specifically, 54.5 percent of program group members who had an arrest in the administrative data self-reported not having been arrested, compared to only 50 percent of the control group. While this difference is not statistically significant, it does suggest that the small but statistically significant effects observed in the survey arrest data are more likely to be the result of recall errors or a desire to report more positive results than actually occurred than they are evidence of a genuine impact of RExO.

Overall, then, there appears to be no real support for the hypothesis that RExO affected recidivism.

Impacts for Subgroups

As for the analysis of recidivism based on administrative data, the evaluation team performed several analyses of the impact by subgroup based on the survey data. Tables IV-8 through IV-13 display results for the six subgroups described in Chapter I.

As can be seen in these tables, the general survey-data-based finding 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 somewhat 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 ten to twenty percentage points).

Table IV-7:

Impacts on Recidivism Using Survey Data, by Year: Full Sample

Outcomes	Program	Control	Difference (Impact)	Hazard Ratio (Impact)	P-value
Arrested					
Arrested (1-year) (%)	18.4	20.5	-2.0		0.093*
Arrested (2-year) (%)	36.9	41.0	-4.1		0.007**
Number of times arrested	1.7	1.7	0.0		0.926
Elapsed days to 1st arrest	310.9	311.0	-0.1		
Survival Analysis				0.913	0.137
Convicted of a Crime					
Charged with new crime (%)	24.3	25.8	-1.5		0.204
Convicted of a crime (%)	17.9	19.6	-1.7		0.155
Number of times convicted	1.3	1.3	-0.0		0.841
Parole Violations					
Violated parole (%)	26.5	27.5	-1.1		0.291
Parole revoked (%)	17.5	17.3	0.2		0.850
Admitted to Prison					
Incarcerated (%)	42.5	44.3	-1.9		0.173
Total time incarcerated (days)	123.3	127.1	-3.7		0.540
Total time incarcerated, excluding those with no incarceration (days) †	301.2	296.7	4.5		0.742

SOURCE: Two-year follow-up survey of study participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

Unless otherwise noted, values are for the 2-year analysis period.

The number of times arrested, number of times convicted, and elapsed days to 1st arrest are all limited only to those individuals with at least 1 relevant event—for example, 1.7 is the average number of arrests (among those with at least 1 arrest) in the treatment and control groups.

† These results are limited only to those study participants with nonzero incarceration. Because incarceration is measured post-RA, the experimental design no longer guarantees equivalence between treatment and control groups (within this subset). These results should therefore be interpreted as suggestive rather than as true impact estimates.

But these relative differences in levels of recidivism across subgroups do not indicate that RExO had a differential impact between the subgroups. With one exception, there is no relative difference in recidivism between program and control groups across the subgroup categories. Only for the education subgroup is there a differential impact among the subgroups. Specifically, RExO increased the time to (self-reported) first arrest among program group members with a high school diploma as compared to those without a diploma. In other words, RExO had a greater beneficial effect on time to first arrest for those with a high school diploma than for those with something less than a diploma. It is unclear why RExO may have had such a differential impact for those with somewhat more education. Given this is the only finding within the subgroup analysis, it may be that this result is spurious.

Summary

Overall, the analyses presented in this chapter have not provided substantial support for the hypothesis that RExO affected participants' recidivism outcomes. The administrative data provided no evidence whatsoever of any impacts of RExO. The survey data suggested there may be some effect on arrest rates, but no effect on any other measure of recidivism. Subsequent analyses linking the survey and administrative data indicate that the most likely explanation for the difference in reported arrest rates is some form of reporting bias, rather than a true impact of the program. The survey data do indicate that RExO delayed re-arrest to a greater degree among those with a high school diploma, but showed no other subgroup differences in recidivism rates. This result, too, may be affected by recall or other response bias among those in the program group. Thus, the general conclusion arising from the recidivism data is that, overall, RExO had little to no effect on study participants' recidivism.

Table IV-8:

Impacts on Criminal Justice Outcomes, by Age

	Age									
	Under 27					27 and older				
Outcome	**Program**	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**	**Program**	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**
Arrested (1-yr) (%)	28.8	27.9	0.9		0.815	15.8	18.7	-3.0		0.036**
Arrested (2-yr) (%)	50.4	54.2	-3.8		0.294	33.3	37.8	-4.5		0.012**
Survival Analysis				0.892	0.109				0.970	0.800
Incarcerated (%)	58.5	57.4	1.1		0.843	38.0	41.1	-3.1		0.089*
Total days incarcerated	108.7	115.8	-7.1		0.530	186.2	181.2	5.1		0.821
Total days incarcerated, excluding those w/ no incarceration) †	322.5	319.4	3.1		0.862	292.3	289.0	3.4		0.814

SOURCE: Two-year follow-up survey of study participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

Unless otherwise noted, values are for the 2-year analysis period.

† This result is limited only to those study participants who were incarcerated at some point following random assignment. Because incarceration is measured post-RA, the experimental design no longer guarantees equivalence between treatment and control groups (within this subset). These results should therefore be interpreted as suggestive rather than as true impact estimates.

Sample sizes are 2,882 (under 27) and 699 (27 and older).

Table IV-9:

Impacts on Criminal Justice Outcomes, by Number of Prior Convictions

Outcome	Number of Prior Convictions									
	3 or Fewer					4 or more				
	Program group	Control group	Difference (Impact)	Hazard Ratio (Impact)	P-value	Program group	Control group	Difference (Impact)	Hazard Ratio (Impact)	P-value
Arrested (1-yr) (%)	17.6	19.0	-1.3		0.493	19.5	23.7	-4.3		0.045**
Arrested (2-yr) (%)	34.7	37.3	-2.5		0.301	38.7	44.9	-6.2		0.013**
Survival Analysis				0.950	0.590				0.848	0.070*
Incarcerated (%)	39.0	39.4	-0.3		0.820	44.9	49.4	-4.5		0.076*
Total days incarcerated	108.5	114.3	-5.8		0.759	139.6	143.9	-4.3		0.867
Total days incarcerated (excluding those w/ no incarceration) †	286.8	294.4	-7.6		0.588	315.5	300.4	15.1		0.393

SOURCE: Two-year follow-up survey of study participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

Unless otherwise noted, values are for the 2-year analysis period.

† This result is limited only to those study participants who were incarcerated at some point following random assignment. Because incarceration is measured post-RA, the experimental design no longer guarantees equivalence between treatment and control groups (within this subset). These results should therefore be interpreted as suggestive rather than as true impact estimates.

Sample sizes are 1,584 (3 or fewer convictions) and 1,519 (4 or more convictions).

Table IV-10:

Impacts on Criminal Justice Outcomes, by Gender

	Gender									
	Female					Male				
Outcome	**Program**	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**	**Program**	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**
Arrested (1-yr) (%)	11.4	15.9	-4.5		0.080*	20.1	21.9	-1.8		0.260
Arrested (2-yr) (%)	25.2	31.9	-6.8		0.047**	39.7	43.7	-4.0		0.038**
Survival Analysis				0.714	0.028**				0.953	0.469
Incarcerated (%)	29.8	36.5	-6.7		0.048**	45.3	46.5	-1.3		0.511
Total days incarcerated	62.3	82.3	-20.0		0.175	139.1	140.9	-1.8		0.837
Total days incarcerated (excluding those w/ no incarceration) †	211.1	225.5	-14.4		0.787	313.5	310.7	2.9		0.732

SOURCE: Two-year follow-up survey of study participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

Unless otherwise noted, values are for the 2-year analysis period.

† This result is limited only to those study participants who were incarcerated at some point following random assignment. Because incarceration is measured post-RA, the experimental design no longer guarantees equivalence between treatment and control groups (within this subset). These results should therefore be interpreted as suggestive rather than as true impact estimates.

Sample sizes are 731 (female) and 2,823 (male).

Table IV-11:

Impacts on Criminal Justice Outcomes, by Timing of Random Assignment (Relative to Program Schedule)

	Timing of Random Assignment									
	Pre-October Assignment					Post-October Assignment				
Outcome	Program	Control	Difference (Impact)	Hazard Ratio (Impact)	P-value	Program	Control	Difference (Impact)	Hazard Ratio (Impact)	P-value
Arrested (1-yr) (%)	18.2	20.7	-2.6		0.083*	18.9	19.7	-0.8		0.777
Arrested (2-yr) (%)	36.6	41.3	-4.6		0.012**	36.8	40.0	-3.1		0.342
Survival Analysis				0.894	0.126				0.980	0.863
Incarcerated (%)	41.4	43.7	-2.3		0.190	43.8	45.1	-1.3		0.757
Total days incarcerated	126.4	132.8	-6.4		0.619	118.8	116.4	2.4		0.823
Total days incarcerated (excluding those w/ no incarceration) †	309.0	309.4	-0.4		0.911	280.7	265.2	15.5		0.636

SOURCE: Two-year follow-up survey of study participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

Unless otherwise noted, values are for the 2-year analysis period.

† This result is limited only to those study participants who were incarcerated at some point following random assignment. Because incarceration is measured post-RA, the experimental design no longer guarantees equivalence between treatment and control groups (within this subset). These results should therefore be interpreted as suggestive rather than as true impact estimates.

Sample sizes are 2,551 (pre-October cohort) and 1,030 (post-October cohort).

Table IV-12:

Impacts on Criminal Justice Outcomes, by Timing of Random Assignment (Relative to Release from Prison)

	Timing of Random Assignment									
	Early Assignment					Late Assignment				
Outcome	**Program**	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**	**Program**	**Control**	**Difference (Impact)**	**Hazard Ratio (Impact)**	**P-value**
Arrested (1-yr) (%)	19.7	21.4	-1.7		0.270	13.5	19.2	-5.7		0.080*
Arrested (2-yr) (%)	38.5	42.3	-3.8		0.039**	30.6	40.0	-9.4		0.022**
Survival Analysis				0.929	0.281				0.809	0.150
Incarcerated (%)	43.3	46.1	-2.7		0.133	37.6	40.3	-2.7		0.781
Total days incarcerated	129.3	138.0	-8.7		0.345	104.4	101.2	3.2		0.535
Total days incarcerated (excluding those w/ no incarceration) †	304.2	306.1	-1.9		0.917	282.6	256.0	26.6		0.531

SOURCE: Two-year follow-up survey of study participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

Unless otherwise noted, values are for the 2-year analysis period.

† This result is limited only to those study participants who were incarcerated at some point following random assignment. Because incarceration is measured post-RA, the experimental design no longer guarantees equivalence between treatment and control groups (within this subset). These results should therefore be interpreted as suggestive rather than as true impact estimates.

Sample sizes are 2,785 (early assignment) and 704 (late assignment).

Table IV-13:

Impacts on Criminal Justice Outcomes, by Educational Attainment

Outcome	Program	Control	Difference (Impact)	Hazard Ratio (Impact)	P-value
No GED/HS Diploma					
Arrested (1-year) (%)	19.1	21.4	-2.3		0.360
Arrested (2-year) (%)	37.3	44.1	-6.8		0.018**
Incarcerated (2-year) (%)	43.2	46.5	-3.3		0.335
Survival Analysis≠				0.893	0.229
Total days incarcerated	127.9	138.8	-10.8		0.642
Total days incarcerated (excluding those w/ no incarceration) †	303.8	306.3	-2.6		0.991
GED					
Arrested (1-year) (%)	21.8	21.7	0.1		0.981
Arrested (2-year) (%)	44.2	43.3	1.0		0.833
Incarcerated (2-year) (%)	48.5	46.7	1.8		0.629
Survival Analysis≠				1.121	0.316
Total days incarcerated	148.1	144.6	3.5		0.916
Total days incarcerated (excluding those w/ no incarceration) †	308.9	314.1	-5.3		0.813
HS Diploma[a]					
Arrested (1-year) (%)	14.3	18.2	-3.9		0.034**
Arrested (2-year) (%)	29.2	34.8	-5.6		0.013**
Incarcerated (2-year) (%)	34.7	38.9	-4.2		0.055*
Survival Analysis≠				0.724	0.008***
Total days incarcerated	97.5	100.5	-3.0		0.812
Total days incarcerated (excluding those w/ no incarceration) †	285.2	263.7	21.5		0.477

SOURCE: Two-year follow-up survey of study participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

Unless otherwise noted, values are for the 2-year analysis period.

† This result is limited only to those study participants who were incarcerated at some point following random assignment. Because incarceration is measured post-RA, the experimental design no longer guarantees equivalence between treatment and control groups (within this subset). These results should therefore be interpreted as suggestive rather than as true impact estimates.

[a] This category includes a small number of offenders with some college or a college degree.

Sample sizes are 1,529 (no GED/diploma), 952 (GED) and 1,100 (HS diploma).

For outcomes marked with an inequality sign (≠), the difference between subgroup effects in a fully interacted model were statistically significant.

Twelve percent of the sample reported their level of education as being “some college” or higher. This fraction was too small for meaningful subgroup analysis specific to this group; instead these individuals were consolidated with individuals who reported receipt of a HS diploma and analyzed as a single group.

Glossary of Recidivism Outcomes

Admissions to prison. Admissions to state prison for any reason.

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

Admissions to prison for a technical parole violation. Admissions to prison after a parolee has violated a condition of his 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 its severity, a violation of these rules may lead to the 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, the data may include arrest that did not lead to a conviction.

Conviction – a disposition of guilty, whether by trial or plea. Some convictions may have been related to an arrest 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: CT 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).

V. IMPACTS ON OTHER OUTCOMES

This chapter explores the extent to which the RExO program had impacts on several outcomes of potential interest other than employment and recidivism. These include physical and mental health, substance abuse, housing, and child support payments. Since the grantees rarely offered direct services to address these other outcomes, there is no clear hypothesis concerning why RExO would impact them. However, it is possible that, by virtue of being in the RExO program, participants may have decided to address other issues in their lives and therefore received referrals for these services or otherwise sought them out on their own. Each of the analyses in this chapter relies on self-reported survey data, as no administrative data on these topics were available.

Impacts for the Full Sample

Results using the full sample of participants for each of the outcome areas are displayed in Tables V-1 through V-4. In general, very few statistically significant differences between the program and control groups appear in any of the results. In the areas of physical and mental health, as reported in Table V-1, members of the program group were slightly less likely to have visited an emergency room or urgent care facility, made fewer such visits on average, and were less likely to report that their physical health limited their work activities during the previous month.

In the area of substance abuse outcomes, as reported in Table V-2, program group members were somewhat more likely to report having been in treatment within the last month (a difference of 5.2 percentage points) and were much more likely to report having been in some other form of treatment for substance abuse issues within the last month (a difference of 10.7 percentage points).[70]

[70] Both of these comparisons involve only the portion of the sample that reported being in any treatment since they enrolled in the study; as described in prior chapters, this partitioning of the sample renders the comparisons non-experimental in nature and the results may not provide an unbiased estimate of the impact of RExO.

As shown in Table V-3, members of the control group were more likely to report living with a partner, and Table V-4 shows that program group members were more likely to report giving food to a parent or guardian of their child in the most recent six months.

Table V-1:

Impacts on Physical and Mental Health Outcomes

Outcome	Program	Control	Difference	P-value
Physical Health				
Needed to go to a doctor or hospital but lacked money or insurance (%)	39.0	39.6	-0.7	0.686
Needed to see a dentist but lacked money or insurance (%)	50.3	51.2	-0.9	0.588
Any visits to emergency room/urgent care (%)	47.9	52.1	-4.2	0.016**
Number of visits to emergency room/urgent care†	3.0	3.4	-0.4	0.030**
Number of those visits that were for emergencies and not routine care†	2.6	2.9	-0.2	0.165
General state of health[a]	2.7	2.7	-0.1	0.137
Physical health limited type of work or activities during last month (%)	23.2	27.8	-4.7	0.002***
How much physical health interfered with normal work[b,]†	3.7	3.7	0.0	0.730
Mental Health				
Emotional problems limited type of work or activities during last month (%)	19.2	20.6	-1.5	0.288
How much emotional problems interfered with normal work[b] †	3.6	3.7	-0.1	0.521

SOURCE: Two-year follow-up survey of study participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

† These items were only asked of a subset of participants; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.

[a] This item was rated on a scale from 1 (“Excellent”) to 5 (“Poor”).

[b] These items were rated on a scale from 1 (“Not at All”) to 5 (“Extremely”).

Table V-2:
Impacts on Substance Abuse Outcomes

Substance Abuse Outcome	Program	Control	Difference	P-value
In Substance Abuse Treatment At Any Point Since RA				
In treatment program for substance abuse at any point (%)	30.1	31.3	-1.2	0.435
Treatment was mandated/condition of parole (%)†	70.0	74.3	-4.3	0.121
In self-help groups, such as Alcoholics Anonymous or Narcotics Anonymous (%)†	70.0	60.9	9.1	0.106
Treatment Within the Most Recent Month†				
In any treatment programs during last month (%)	32.9	27.7	5.2*	0.067
In detoxification during last month (%)	15.1	16.9	-1.9	0.669
In outpatient drug free program in last month (%)	50.2	41.5	8.7	0.136
In medicinal treatment (i.e., Methadone) program during last month (%)	10.5	10.3	0.2	0.959
In residential program during last month (%)	35.4	39.7	-4.3	0.444
In other type of treatment during last month (%)	20.3	9.6	10.7	0.007***
Substance Use in Most Recent Month				
Used any illegal drugs or prescription drugs without prescription during last month (%)	9.5	9.9	-0.4	0.716
Frequency of drug use during last month[a]	2.2	2.2	-0.1	0.457
Number of days had 5 or more drinks in a row within a couple hours during last month	1.2	1.2	0.0	0.892

SOURCE: Two-year follow-up survey of study participants

NOTES: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1.
Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.
† These items were all only asked for those participants who reported being in treatment at any point since RA; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.
[a] This item was rated on a scale from 1 (“Every Day”) to 4 (“Once or Twice”).

Table V-3:
Impacts on Housing Outcomes

Current Housing Status	Program	Control	Difference	P-value
Living in public housing (%)	6.1	5.6	0.5	0.578
Living in Section 8 housing (%)	2.7	2.8	-0.1	0.876
Days at current residence	1,158.7	1,097.5	61.2	0.264
Contributing to rent/cost (%)	59.8	60.7	-0.9	0.377
Living with partner (%)	24.2	27.3	-3.1	0.026**
Living with children (%)	22.0	21.0	1.0	0.794
Living with parents (%)	22.3	22.5	-0.2	0.982
Living with other family (%)	23.2	21.0	2.2	0.141
Living with friends (%)	9.1	9.9	-0.8	0.526

SOURCE: Two-year follow-up survey of study participants

NOTES: Statistical significance levels are indicated as follows: *** = $p < .01$; ** = $p < .05$; * = $p < .1$.
Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.
Other than "Days at Current Residence," all figures shown in the first three columns are percentages.

Impacts for Subgroups

Given the lack of any clear hypothesis for why RExO may have affected these other outcomes, and the large number of outcomes and subgroups of interest (resulting in a total of 264 comparisons across the subgroups), results for the subgroup analyses on these other outcomes are shown in Appendix B. Although there are a few significant differences between the subgroups in the impacts observed, this is to be expected given the large number of tests performed. Indeed, the number of significant findings in these tests (26) is the exact number that one would expect to find purely by chance, given the statistical thresholds used in the analysis. Hence, there is not convincing evidence that RExO had greater (or lesser) impacts for any particular subgroup on these other outcomes.

Table V-4:

Impacts on Child Support Outcomes

Outcome	Program	Control	Difference	P-value
Child Support Outcomes				
Required by court to pay child support for children living away from home (%)	30.3	31.1	-0.8	0.667
Number of children required to pay child support for†	1.8	1.9	0.0	0.536
Approximate total amount provided, excluding child support required by court($)†	684	905	-220	0.132
Number of children this support covered†	1.9	1.8	0.0	0.682
Concerns about owing child support affected willingness to accept job offers (%) †	11.2	13.0	-1.8	0.447
Child Support Enforcement System				
Currently required to pay child support through the child support enforcement system(%)	85.2	87.4	-2.2	0.380
Paid child support through the child support enforcement system during last month [a,†]	1.9	1.8	0.0	0.821
Amount paid through child support enforcement system during last month($)†	287	274	13	0.583
Gave money directly to parent or guardian instead of going through child support system (%)†	1.6	1.6	0.0	0.352
Assistance to Parent/Guardian of Child				
Gave money to parent or guardian during last six months (%)	94.2	89.5	4.7	0.135
Gave food to parent or guardian during last six months (%)	63.7	50.4	13.3	0.017**
Gave clothing to parent or guardian during last six months (%)	76.3	78.2	-1.9	0.689
Gave something else to parent or guardian during last six months (%)	40.5	35.0	5.6	0.307

SOURCE: Two-year follow-up survey of study participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

† These items were only asked of a subset of participants; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.

a This item was rated on a three-point scale, with 1="Yes", 2="Some of it", and 3="No".

Summary

The overall summary of the results reported in these tables is that the RExO program had virtually no impact on the physical and mental health, substance abuse, housing, or child support outcomes of study participants. This is not particularly surprising, given that RExO programs generally provided few or no services that were related to these issues. In some cases, grantees required participants to test negative for substance use, but even in these cases they rarely provided the substance abuse testing and treatment services themselves. In most cases they referred participants to these services at other providers in the community—and control group members potentially had access to many of the same services.

Although for seven outcomes the differences between program and control groups reach conventional levels of statistical significance, this result must be interpreted relative to the fact that 43 total outcomes were analyzed. With 43 tests, one would expect that between 4 and 5 of them would reach conventional levels of significance by statistical chance alone. Taken together, however, the three statistically significant differences reported in the area of physical health may merit a closer look at the possible impacts of RExO on participants' health. Although the RExO grantees did not provide any health-related services, it is possible that other features of the program produced a slightly positive impact on physical health. Overall, however, the results in this chapter do not provide much support to suggest that RExO affected the physical and mental health, substance abuse, housing or child support outcomes of participants.

Similarly, a few of the subgroup analyses were statistically significantly different, indicating that impacts varied across these subgroups. However, the number of significant results was exactly equal to the number one would expect purely by statistical chance. Thus, there does not appear to be strong support to suggest RExO's impacts on these other outcomes differed meaningfully across the subgroups examined in this report.

VI. CONCLUSION

This report summarizes the impacts of the Reintegration of Ex-Offenders (RExO) program on offender outcomes in four areas: service receipt, labor market success, recidivism, and other outcomes. This evaluation spanned 24 RExO grantees operating in eighteen states. These grantees had been in operation for approximately three years at the time the evaluation began, providing primarily work readiness training and other workforce services, mentoring, case management, and supportive services to offenders returning to their communities from state or federal prisons or local jails. 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 any other services. 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 evaluation team followed these individuals for a two-year period[71] following enrollment into the study, using two primary types of data to measure outcomes. The first of these was a telephone survey that included the entire sample of study participants and asked about a range of items, including service receipt, labor market outcomes, recidivism, physical and mental health, substance abuse, housing, and child support issues. The overall response rate to this survey was 76.9 percent. The second set of data used in this report was administrative data on criminal justice outcomes obtained from the states in which RExO grantees operated.

Primary Results

The results of the study, as described in this report, indicate there was a clear difference in service receipt between those in the program and control groups. Self-reported data on service receipt drawn from the follow-up survey indicate that program group members were much more likely to have received a wide range of employment-focused services. Among the specific

[71] As noted in prior chapters, the evaluation also followed study participants for a third year following RA. Results for the three-year follow-up will be available in a Final Report for the evaluation, to be submitted in Summer 2015.

services that program group members were more likely to report having received were participation in job clubs or job readiness classes, vocational training, job search assistance, referrals to job openings, and help with resume development and filling out job applications. Program group members were also more likely to report having received advice from program staff on a number of topics, including job interviewing skills, how to answer questions about their criminal history, and how to behave in an employment setting. Additionally, program group members were much more likely to have participated in mentoring sessions, and to declare that there was someone from a program who went out of their way to help them and to whom they could turn for advice on personal or family issues. They also were more likely to have participated in sessions offering counseling or other support. Finally, program group members were more likely to report that a program had provided them with help dealing with the child support enforcement system. The helpfulness of the employment services received was also rated more favorably by program group members.

The only set of services included in the survey that showed no impact of RExO were educational services, including receipt of basic educational instruction, receipt of a high school diploma or GED, and taking college courses for credit. This is consistent with the findings from the implementation study that less than five percent of study participants received these services directly from RExO grantees.

Thus, while control group members were able to seek out and access non-RExO services from alternative providers in their communities, the service receipt data make clear that there was a significant treatment contrast between program and control group members. These differences were not only statistically meaningful, they were also in some instances large in practical terms, ranging from a 3.1 percentage point difference to more than 21 percentage points, with most being greater than 10 percentage points. Thus, there is strong evidence that RExO increased the overall level of service uptake among participants. The critical question is whether this higher level of service receipt resulted in improved criminal justice and labor market outcomes.

Estimates of the effect of RExO on these and other outcomes are mixed. The data on participants' labor market outcomes (which are self-reported) indicate that a higher percentage of program group members reported having any employment in the first year after RA (a difference of 3.5 percentage points) and in the second year after RA (a difference of 2.6 percentage points). Program group members also reported finding their first job after RA more quickly than did those in the control group. Specifically, at any given point following random assignment, treatment group members who had not yet found work were about 11 percent more likely to do so in the next time period than were control group members who had also not yet found work. However, there were no differences between the study groups in the total number of days employed in the two-year period following RA.

Data on compensation and benefits display a similarly mixed pattern: there were no differences between the study groups on the hourly wage received either at the first job obtained after RA or at the current or most recent job held, but program group members reported higher average total income from all sources. It is not clear whether this higher average income is due to program group members working more total hours than control group members, obtaining more non-wage income, or some other reason, but program group members reported receiving approximately eight percent more income than control group members.

Overall, then, RExO appears to have an effect on self-reported employment. This effect is somewhat small in practical terms (e.g., 2.6 percentage-point difference for having worked at all in the second year after RA), but statistically meaningful.

One concern with these impact estimates is that they rely exclusively on survey data, and thus are dependent on study participants accurately recalling the information asked for, and truthfully reporting their actual outcomes. Ideally, these data could be compared to administrative data on employment and earnings, which do not suffer from the potential issues posed by poor recall or a desire to present more positive outcomes than were actually achieved. While the final impact report for this evaluation will include these data, they were not available for inclusion in this report. As a result, the survey data on labor market outcomes represent the best estimate currently available for the impacts of RExO on this critical area of interest.

In contrast, here are two sets of data available to assess RExO's impact on recidivism. The survey asked respondents to report if they had been arrested, convicted, or incarcerated in the two years since enrolling in the study. Similarly, administrative data on these topics were collected from the eighteen states in which REXO grantees operated.

In general, both sets of data indicate that the RExO program did not have a significant impact on recidivism. The administrative data revealed no differences in recidivism between the program and control groups. This lack of an effect was consistent across different measures of recidivism, including those defined by arrests, convictions, and incarceration in prisons or jails. Self-reported data on recidivism obtained from the survey mostly mirrored this finding, with no statistically significant differences between program and control group members on convictions or incarceration. There are differences in the survey results between the study groups in the rates of arrest, with a lower percentage of program group members reporting being arrested in the first year after RA (a difference of two percentage points), and in the second year after RA (a difference of 4.1 percentage points). However, comparing each survey response with the corresponding administrative data reveals this difference to be driven by either recall bias or otherwise inaccurate reporting on the part of program group members. Hence, the overall data on recidivism suggest that RExO had no real effect on the subsequent criminal justice outcomes of study participants.

In addition to labor market and recidivism outcomes, which were primary areas of interest for the evaluation, the survey also collected information on a number of other outcomes, including those related to physical and mental health, substance abuse, housing, and child support. Given that RExO grantees only infrequently provided services designed to address these issues, it is perhaps unsurprising that there were few differences between the study groups on these measures. There was some evidence that RExO may have affected health outcomes, as program group members were less likely to report having made any visits to the emergency room (a difference of 4.2 percentage points) or that their physical health limited their work or activities in the most recent month (a difference of 4.7 percentages points). Most other areas of health and mental health revealed no differences between the study groups. Further, there were almost no other significant impacts on other outcomes, including substance abuse, housing, and child support issues. Hence, the general conclusion from this analysis is that RExO had no apparent effects on these other outcomes.

Conclusions

A number of conclusions about the impact of RExO on participants can be drawn from the results for the first two years after RA.

The participants in this study—including both program and control group members—had more positive outcomes than 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 offenders.[72] This cannot be explained by the efficacy of the program, given the lack of impacts found for most outcomes. Rather, it seems likely that the screening and eligibility criteria used by the program and its grantees led to a selected subset of offenders participating in this study, suggesting that the results from this study cannot be taken as representative of the general offender population. It is unclear whether the impacts of the program would be different with a more representative sample of offenders.

RExO grantees were effective in providing an array of services to their clients. Program group members were much more likely to report having received services, across nearly all measures of service receipt. These individuals were also more likely to rate these services as being more helpful.

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[72] Durose, et al. (2014)

Despite the sizable difference in services received, the impact of RExO on key outcomes is less clear. There is strong evidence that program group members received more services than control group members. Though these differences were clear and consistent across a range of services, they may not have been large enough to translate into large differences on recidivism or employment. In any event, evidence for positive effects on the key outcomes is mixed. There are no apparent differences in recidivism outcomes, and only relatively small (in practical terms) impacts on employment.

There is some evidence that recall or response bias in the survey data may have contributed to estimates of impacts. One concern in using survey data is that there could be recall issues or other forms of response bias that affect the impact estimates in unknown ways. Although the use of RA theoretically eliminates any systematic bias between the program and control groups *at the time of* RA, it cannot completely eliminate the possibility for such bias emerging *after* RA. This could happen, for instance, if members of the program group felt compelled to report better outcomes than they actually achieved, for fear of casting a negative light on the program. Administrative data can help to provide a check on this. The only outcome area for which both survey and administrative data were available for this report was recidivism. Although results using these two sets of data largely agree (showing no impact on most measures of recidivism), the survey data suggest that program group members were significantly less likely to be re-arrested within one and two years after RA, while the administrative data display no such difference. Analyses comparing the two data sets across the respondents reveal that this difference is driven generally by program group members being more likely to "mis-report" that they were not arrested when administrative data indicate they were. This suggests that there is some bias among program group members to report better outcomes than they actually achieved. This means that impact estimates relying solely on survey data need to be viewed cautiously and, ideally, tested against administrative data on the same measures.[73]

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.[74] But its effects, particularly in the communities in which RExO grantees were operating, lasted for much longer, 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 ensures that program and control group members faced similar economic circumstances, it is

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73 While this report does test survey responses against administrative data on measures of recidivism, the final Impact Report for the evaluation will also include such a comparison on measures of employment and earnings.

74 http://www.nber.org/cycles.html

possible that the fact there were so few jobs available led to lower overall employment outcomes, thereby depressing an impact of RExO that might be observed under better economic times.[75]

Despite the difficult economic circumstances, RExO had an impact on self-reported employment outcomes. Even in the difficult economic conditions in which RExO grantees operated, they did manage to improve the self-reported employment outcomes of program group members. Though these differences were small in real terms, they persisted through both the first and second full years after RA. Program group members also obtained their first jobs more quickly after RA than did control group members.

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, and date of enrollment into the study. Analysis of differential impacts indicated that there were a few subgroups for which the impact of RExO was greater than it was for others. For example, the impact of RExO on labor market outcomes was greater for those who enrolled later in the intake period (October 1, 2010 or later) than for those who enrolled earlier. Additionally, RExO seemed to increase the time to first arrest for those with a high school diploma, as compared to those who had not received a diploma. But the former of these findings is driven more by poorer outcomes among the control group (perhaps confirming that RExO grantees broadened their applicant pools toward the end of the intake period in an effort to reach their targeted enrollment, a finding noted as part of the Implementation Report for this evaluation), and the latter may be driven by recall or reporting bias among survey respondents. Thus, overall, there is little evidence that RExO had differential impacts across the various subgroups examined in this analysis.

This evaluation may not provide a strong test of whether employment-based programs lower one's likelihood of recidivating. Though RExO appeared to have had a statistically significant impact on employment, the fact that this impact was quite small likely makes it difficult to detect related differences in recidivism. This is because one can expect recidivism to be affected by employment-based programs only if they produce practically significant differences in employment rates. Thus, a full test of the impact of employment-based programs on offender recidivism may require evaluation of a program that generates impacts on employment larger than those of the RExO intervention.

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[75] The evidence as to 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. Neither of the studies, however, 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.

It is possible that RExO grantees did not have 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 meet the broad array of needs that offenders have. The fact that nearly one-third of all survey respondents reported having been in a substance abuse treatment program at some point following RA indicates widespread issues with drug abuse and alcoholism. Similarly, approximately one-fourth of all respondents reported physical health issues that limited their work or other activities. These and other challenges may provide serious barriers to employment and the attainment of other positive outcomes, but RExO grantees rarely provided services directly addressing them. Thus, the findings may suggest the need for a more comprehensive and intensive approach that helps address a wide array of other issues present in the ex-offender population.

This report has summarized the findings from an analysis of the impacts of RExO in the two years after participants enrolled into the study. A final impact report is scheduled to be submitted in Summer 2015, and will focus on impacts in the three-year period following RA. This final report will include data similar to those reported here, but will also include data for an additional year following RA. Additionally, the final report will incorporate administrative data on employment and earnings, which will allow for an analysis of the extent to which recall or other response bias in the survey results may have affected the estimates of impact on labor market outcomes. If the administrative data analysis provides results consistent with the analysis of survey data, the joint finding will provide solid evidence that RExO positively impacts participants' labor market outcomes.

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APPENDIX A: TECHNICAL APPENDIX - METHODS FOR SURVEY DATA ANALYSIS

This Technical Appendix serves three purposes: The first is to provide a detailed explanation of the methods that were used in this report to estimate the effects of the RExO program on study participant outcomes. The second is to describe additional statistical models that extend those used in this report, and to present results from those models. Two such extensions are discussed: Hierarchical Linear Modeling (HLM), which accounts for clustering at the program level; and Generalized Linear Model (GLM) transformations, which accounts for skew in income data. The third purpose is to address the multiple comparisons problem, which occurs when more than one test of statistical significance is performed using a single dataset. This appendix describes this problem in detail, describes three methods for adjusting results to account for it, and presents the results of those adjustments.

Description of Methods Used

The experimental design of this study ensures that unbiased estimates of the effect of the RExO program on outcomes of interest may 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 that found work in the 1-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 next section, three such methods are described.

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.[76]

For regression adjustment, 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 is socio-demographic, and includes race and ethnicity, gender, age, and pre-experiment educational status. This group also includes 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 use a forward-shifted three month average of unemployment rates. For example, if an individual was randomly assigned at the Chicago, IL site in April, 2010, the unemployment variable is 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 includes 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 offenders in Louisiana and Ohio, and not able to obtain state prison data for offenders from Illinois, Louisiana, and Michigan. Inclusion of these variables will potentially alter the statistical models in two significant ways: On the one hand, estimates may be made more precise because their inclusion reduces the overall level of uncertainty in the models. On the other hand, all cases from states with missing data will be dropped, which can both alter point estimates (by excluding blocks of 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.

[76] Kling et al. (2004).

Given the program group indicator and these two groups of covariates, there are three logical regression models. The first is to simply regress the outcome on the program group indicator: With $i = 1 \ldots n$ 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.

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):

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

$$Y_i = \beta_0 + \beta_1^3 T_i + \boldsymbol{D}_i'\boldsymbol{\alpha} + \boldsymbol{C}_i'\boldsymbol{\delta} + \varepsilon_i \tag{3}$$

Ordinarily, the experimental design of this study would guarantee that all three estimates of the treatment effect ($\beta_1^1, \beta_1^2,$ and β_1^3) are unbiased. To the extent that $\boldsymbol{D}$ (demographic covariates) and $\boldsymbol{C}$ (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. However, as previously mentioned, the research team was unable to obtain the full set of criminal justice covariates from Louisiana, Illinois, Michigan and Ohio, meaning that equation (3) will omit all observations from those states. This introduces concerns that point estimates will be altered because results are subset to offenders from states where criminal histories were available. As a result equation (2) is the preferred specification, though in this Technical Appendix results from each of these specifications are presented. The point estimates in the main chapters were in general derived from models following the form of equation (1); the p-values were derived from models following the form of equation (2).[77]

Logistic Regression

[77] In both cases estimates were generated following the application of post-stratification weights for survey non-response.

For most binary outcomes, tables contain p-values from models using logistic regression (and average marginal effects) instead of ordinary least squares (OLS).[78] 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 in general have lower variance.

Survival Analysis

Chapter II described results of a survival analysis of the time to job acquisition as a complement to models that used indicator variables for job acquisition 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[79] and employment.[80] 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. Survival analysis potentially allows for a more complete understanding of how RExO participation influences the ability of participants to find employment. The tradeoff for this gain is that interpretation of survival analysis results can be less straightforward than interpretation of analyses 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 a "failure"— at a given point in time, conditional

78 For the analysis of administrative data on criminal justice outcomes, results are presented using OLS, but logistic regression was used to conduct sensitivity analysis to confirm that results did not differ between the two types of models.

79 Hepburn and Albonetti (1994).

80 Dolton and O'Neill (1996).

on that event not having already occurred. [81] In this example, the event in question is job acquisition. 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. A hazard ratio of 1 therefore 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.[82]

Additional Statistical Models

The next section of this appendix describes additional statistical methods used to analyze the impacts of RExO on offender outcomes. There are three such methods; all three are applied only to the survey data set. They are (1) Hierarchical Linear Modeling (HLM), which accounts for the grouped nature of the data; (2) use of the Generalized Linear Model (GLM) to account for skew in the distribution of earnings outcomes; and (3) multiple comparison adjustment, which reviews different techniques for interpreting the results discussed in the chapters in a way that accounts for the fact that multiple statistical tests are conducted simultaneously.

Hierarchical Linear Modeling: Methods

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 will 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

[81] Survival analysis was created by medical and demographic researchers seeking to model the 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."

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

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).[83]

This analysis uses Hierarchical Linear Modeling (HLM)[84] 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 is 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 are averaged across individuals grouped within program sites, an important ancillary question is whether the effect also varies across sites—a heterogeneous treatment effect. With greater sample size, or with large effect sizes, direct estimation of site-level effects using fixed effects would be possible. However, this study has both relatively small sample sizes per grantee and relatively small effect sizes. As a result, the study is not adequately powered to directly estimate treatment effects at each of the 24 sites. In addition, doing so would greatly exacerbate concerns about multiple comparisons (discussed in detail below). 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

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

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

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

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

[84] Raudenbush and Bryk (2002).

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.:

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

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.

HLM can be extended to allow the estimate of the treatment effect (as well as the estimate of the intercept) to vary randomly across sites. To accomplish this, (4) is re-expressed as follows:

$$Y_{ij} = \beta_{0j} + \beta_{1j}T_{ij} + \boldsymbol{D}_i'\boldsymbol{\alpha} + \varepsilon_{ij} \qquad (8)$$

Here both the intercept and the treatment effect parameters are subscripted in j. As with the intercepts, the effect of the program is now modeled as variation around an overall mean effect, e.g.

$$\beta_{1j} = \gamma_{10} + U_{1j} \qquad (9)$$

Simultaneously substituting equations (5) and (9) into equation (8) yields:

$$Y_{ij} = \gamma_{00} + \gamma_{10}T_{ij} + \boldsymbol{D}_i'\boldsymbol{\alpha} + U_{0j} + U_{1j}T_{ij} + \varepsilon_{ij} \qquad (10)$$

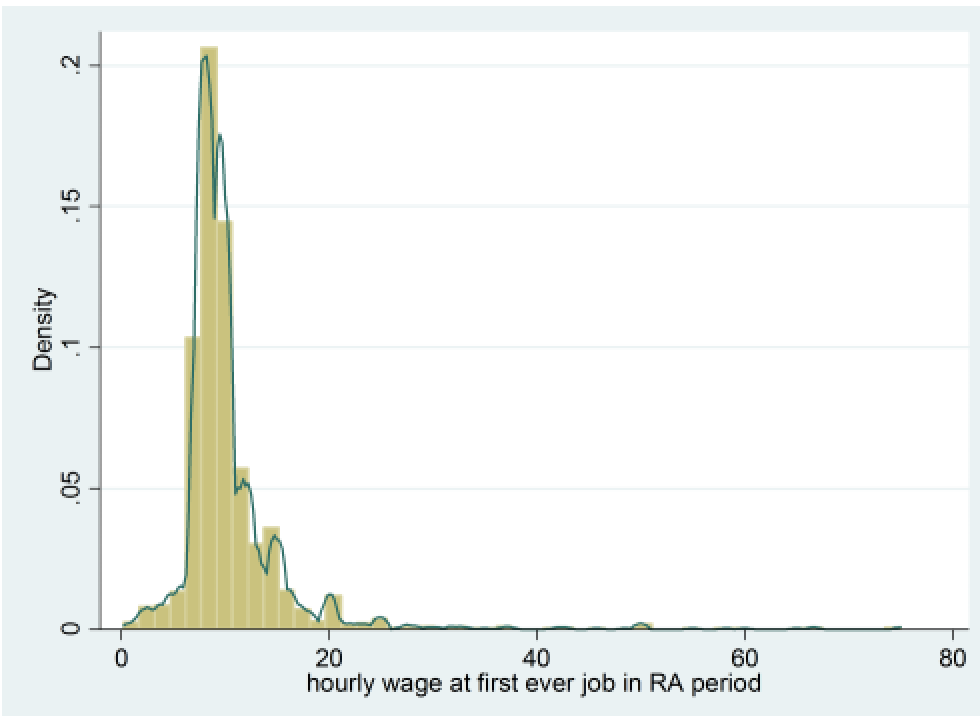
Because equation (10) is nested within equation (8), the hypothesis that the variance of the random slope parameter is equal to zero can be tested using a likelihood ratio test.[85] Put differently, this allows for testing of the presence of heterogeneous treatment effects. When this hypothesis is rejected, there is evidence that the variance of the random slope is nonzero, i.e. that the effect of the program varies significantly across sites. Conversely, if the hypothesis is not rejected, then there is no evidence that the effect of the program varies across sites.

85 Rabe-Hesketh and Skrondal (2012, volume 2).

Generalized Linear Model: Methods

Three of the central labor outcomes used in this analysis (wage at first job, wage at last job, and total income) exhibit a rightward skew, meaning that a few observations are far to the right of (larger than) the average value. This is visible in Figure A1, which portrays the distribution of observed values for the wage at first job following random assignment:

Figure A1: Skew in the Distribution of Wages at First Job



GLM regression with a log link ($E(Y_i|X_i) = \exp(X_i'\beta)$) has been recommended when the distribution of the outcome variable has a high degree of positive (rightward) skew, suggesting that OLS may produce biased and/or less precise estimates.[86] Alternate estimates using this approach (shown below) provide a sensitivity analysis for OLS results.

Sensitivity Analyses for Labor Outcomes

Each of the tables that follow presents results for the key labor market outcomes discussed in Chapter II. Table A1 contains results for estimating the effect of RExO on the probability of any employment in the first year following random assignment, Table A2 contains results for the effect on the probability of any employment in the second year, and so on. Except for survival

[86] Manning and Mullahy (2001).

analyses, each of the results tables in this section are structured as follows: model 1 compares the unweighted means of the outcome for the program and control groups. Model 2 compares the means of the outcome for the program and control groups, weighted to account for survey non-response; the point estimates in the tables in the main report were derived from models of this form. Model 3 extends model 2 by adding regression adjustment for demographic covariates; the p-values in the tables in the main report were derived from models of this form. Model 4 replicates model 3 using a hierarchical linear model (HLM) framework. This adds random effects at the site level and also allows for investigation of the degree to which outcomes and treatment effects vary across sites.

Table A1 also contains results estimated with regression adjustment for criminal justice as well as demographic covariates (model 5). These data are missing for two states, and as discussed below this introduces a potential for point estimates that do not generalize to the full sample; as a result models for the remaining outcomes omit the criminal justice covariates.

First year employment

Table A1 presents estimates of the effect of the RExO program on the probability that participants worked (at all) in the first year following random assignment.

Table A1:

RExO Effects on the Probability of Employment in the First Year Following Random Assignment

Employment in year 1	Model 1	Model 2	Model 3	Model 4	Model 5
Treatment effect	0.0338**	0.0347**	0.0362**	0.0378**	0.0487***
	(2.136)	(2.177)	(2.269)	(2.271)	(2.760)
Post-stratification weights	(no)	(yes)	(yes)	(yes)	(yes)
Demographic covariates	(no)	(no)	(yes)	(yes)	(yes)
Criminal justice covariates	(no)	(no)	(no)	(no)	(yes)
Hierarchical linear model	(no)	(no)	(no)	(yes)	(no)
Intraclass correlation coefficient (ICC)				0.108	
Observations	3,581	3,581	3,554	3,554	2,889

NOTES:
Z-statistics in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
All models present average marginal effects calculated after logit regression. HLM model estimated with random intercept at program level.

The first column reports the average marginal effect of the treatment indicator following a logit regression; this value is equivalent to the raw difference in the proportion of program and control group members who reported working in the first year.

The second model is identical to the first, except that post-stratification weights have been applied in order to reduce the possibility of bias resulting from survey non-response; model 2 theoretically has better generalizability to the study population as a whole than does model 1. The point estimate in Model 2 is equal to the value in the third column of Table III-1. The results of models 1 and 2 are qualitatively identical; the post-stratification weights do not significantly alter the findings.

Model 3 adds demographic covariates (discussed previously) to model 2. This increases the estimate of the size of the effect of RExO on employment in the first year, to about 3.6 percentage points. It also improves the precision of the estimate of the treatment effect, as reflected in the higher z-score. The p-values shown in the tables of the chapters of this report were generated from models of this type.

Model 4 is generated using the same variables and weights as model 3, but within an HLM framework. This allows for grantee-level differences in average outcome to be incorporated into the model, improving the precision of the estimate of the treatment effect. Model 4 estimates that RExO increased the probability of working at all in the first year following random assignment by about 3.8 percentage points, slightly above the model 3 estimate of 3.6 percentage points.

The HLM framework allows estimation of the Intraclass Correlation Coefficient or ICC, which is the proportion of residual variability in the outcome that is due to the group (site) level, as opposed to the individual level. For this outcome, the ICC is estimated to be around 11%. While not trivial, this amount is relatively small: most of the variation in the outcome occurs within program sites, rather than between them. An additional model (results not shown) extended model 4 by allowing the estimate of the treatment effect to vary randomly across sites. The p-value for a likelihood ratio test comparing this model to model 4 was 0.33, a result that suggests that the estimated treatment effect (+3.8%) did not vary significantly across sites.

Finally, model 5 adds criminal justice covariates to model 3. This has the effect of increasing both the size and statistical significance of the estimate of the treatment effect. However this also reduces the sample size by about 20%; as previously mentioned, this is because these covariates were not available for all study participants. At first glance, it is not clear whether the increase in the estimated treatment effect is due to the increased precision gained by the inclusion of the criminal justice covariates or due to the restriction of the sample to observations from states where full criminal justice data were available. A version of model 3 subset to offenders from these states (not shown), produces an estimate of the treatment effect that is very similar to the model 5 estimate (4.9%). This suggests that the increased estimate of the treatment effect is unique to the subsample of states for which there are criminal justice data available rather than due to increased precision. Because estimates relying on this subsample are therefore not readily generalizable to the entire sample criminal justice covariates do not appear in any of the remaining models.

Second year employment

The second outcome is similar to the first, except that it covers the second year following random assignment. Table A2 presents the results of the analysis of the effect of RExO on this outcome, and is structured identically to Table A1, except that model 5 (criminal justice covariates) is omitted:

Table A2:

RExO Effects on the Probability of Employment in the Second Year Following Random Assignment

Employment in year 2	**Model 1**	**Model 2**	**Model 3**	**Model 4**
Treatment effect	0.0260	0.0258	0.0304*	0.0312
	(1.604)	(1.584)	(1.867)	(1.603)
Post-stratification weights	(no)	(yes)	(yes)	(yes)
Demographic covariates	(no)	(no)	(yes)	(yes)
Criminal justice covariates	(no)	(no)	(no)	(no)
Hierarchical linear model	(no)	(no)	(no)	(yes)
Intraclass correlation coefficient (ICC)				0.016
Observations	3,581	3,581	3,554	3,554

NOTES:

Z-statistics in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

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

The effects of the RExO program on the probability of employment in the second year following RA appear broadly consistent with the effects on the one-year probability of employment (Table A1). 68.0% of (weighted) program group members worked at least once in the second year following random assignment, compared with 65.4% of control group members, a difference of about 2.6 percentage points. As was the case with first-year employment probability, the inclusion of demographic covariates improves the precision of the estimate of the treatment effect. In Model 4 the effect of RExO on employment in the second year is estimated to be about 3.1 percentage points. About 1.6% of the variance is attributable to the site level. The p-value of the likelihood ratio test for the random treatment effect model (model not shown) was 0.067, suggesting the possibility of significant heterogeneity in the treatment effect across sites.

Time to job acquisition (between randomization and interview)

Table A3 reports results from a series of survival analyses that model the time to job acquisition of program and control group members.

Table A3:

RExO Effects on the Time to Job Acquisition

Time to first job	Model 1	Model 2	Model 3	Model 4
Treatment effect	1.109***	1.108***	1.111***	1.099**
	(2.712)	(2.682)	(2.735)	(2.452)
Post-stratification weights	(no)	(yes)	(yes)	(yes)
Demographic covariates	(no)	(no)	(yes)	(yes)
Criminal justice covariates	(no)	(no)	(no)	(no)
Program-level fixed effects	(no)	(no)	(no)	(yes)
Observations	3,470	3,470	3,444	3,444

NOTES:

Z-statistics in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

Coefficients are hazard ratios from Cox proportional hazard regressions.

The structure of Table A3 is similar to that of Tables A1 and A2: Model 1 presents raw estimates with no weights and no covariate adjustment; model 2 adds post-stratification weights; and model 3 adds demographic covariates. The results from model 3 were reported in table II-1.

Survival analysis results differ from results elsewhere in this section in three ways. First, they use all available information: all "first jobs" are counted, including those that were acquired after the two-year period following random assignment.[87] Second, treatment effect estimates are hazard ratios. This means that they are not interpreted as the amount by which some outcome increases or decreases as a result of RExO participation; instead they represent the ratio of the probability of job acquisition by program and control group members in the next time period, conditional on employment having not yet been found. Values greater than one indicate that program group members are more likely to find employment. Finally, for Model 4, estimates are generated using a fixed-effects specification, rather than random effects. This is because at the

[87] Ideally, interviews were conducted at the close of the two year period following RA. However due to the difficulty involved in tracking down study participants after an extended period of time, some interviews were conducted well after the close of the two year window. For example, approximately 20% of interviews were conducted at least three years after the date of RA.

time of this writing, the software package used in this analysis (Stata 12.1) is not capable of estimating a Cox proportional hazard model that simultaneously incorporates *both* post-stratification weights and random effects (also known as shared frailty in the survival setting).

All of the models in Table A3 are consistent with the hypothesis that RExO improved the ability of participants to find employment. In general, the estimate of the hazard ratio is approximately 1.1. As was the case in Tables A2 and A3, the addition of demographic covariates (model 2 →model 3) appears to improve statistical precision.

Model 4 is estimated with fixed effects. This is conceptually similar to the random intercept structure presented in the previous tables: Both allow for a program-specific effect on the outcome. The difference is that in fixed effects modeling, the effect is explicitly estimated; this entails a small loss of statistical precision in the estimate of the treatment effect. Model 4 suggests that at any point in time following randomization, program group members *who had not already found employment* were about 9.9% more likely to find employment in the next time interval than were control group members who had not already found employment.

Total days of employment in the first two years following random assignment

Table A4 presents results for the total number of days worked during the two-year follow up period. In order to avoid confounding the effect of the program on the ability of participants to find employment (at all), these models are estimated conditional on having worked some amount during the period. This is reflected in the lower sample sizes (compared with previous models)—individuals who did not work at all are omitted. The results in the main body of this report show essentially no difference between the program and control group average values for this measure. The addition of HLM structure does not alter this finding. In Model 4 the estimate is that the effect of RExO was to increase the total time worked by an average of 2.2 days. However, this finding is extremely imprecise from a statistical standpoint. About one percent of the residual variation occurred at the site level, and the random treatment effect model (not shown) did not appear to significantly improve on model 4 (likelihood ratio test result: p=0.321).This suggests that the variance in the treatment effect across sites was not significantly different from zero.[88]

[88] For the remaining outcomes discussed in this appendix, HLM results were broadly similar: very little of the residual variation occurred at the grantee level, and in general provided no support for the hypothesis of heterogeneous treatment effects. For brevity, we will henceforth only note deviations from these general conclusions.

Table A4:

RExO Effects on Total Days of Employment in the First Two Years Following Random Assignment

Total days employed in years 1-2	Model 1	Model 2	Model 3	Model 4
Treatment effect	-1.139	-0.550	1.571	2.173
	(-0.116)	(-0.0556)	(0.160)	(0.204)
Post-stratification weights	(no)	(yes)	(yes)	(yes)
Demographic covariates	(no)	(no)	(yes)	(yes)
Criminal justice covariates	(no)	(no)	(no)	(no)
Hierarchical linear model	(no)	(no)	(no)	(yes)
Intraclass correlation coefficient (ICC)				0.01
Observations	2,410	2,410	2,392	2,392

NOTES:

T-statistics in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

All models present estimates from OLS regressions. HLM model estimated with random intercept at program level.

Days of employment are conditional on post-RA employment, which is itself affected by program status. As a result, values of this outcome across the program and control groups are no longer strictly comparable. The findings for this outcome should be considered suggestive rather than true impact estimates. Alternative models not subset in this way also show no significant effects.

Hourly wage at first job following random assignment

Table A5 presents results from models that assess the impact of RExO participation on average hourly wage at the first job obtained following random assignment; the first four models are structured as in previous tables. In all cases, the average wage among the program group was higher than the average wage among the control group (by between $0.54 and $0.70), but these findings are only weakly statistically significant. Findings are conditional on both having had a first job and on ability to recall the wages received at that first job, and this is reflected in the smaller sample sizes (compared with previous results).

The wage and income models below also include a fifth set of results derived using a Generalized Linear Model (GLM) with log link; this is done to address concerns about bias and imprecision resulting from skew in the outcome. It is not immediately obvious whether model 4 or model 5 is superior. Model 4 allows for heterogeneity in the baseline levels of the outcome variable across sites; Model 5 corrects for the non-normal distribution of the outcome variable.

The point estimates of the treatment effect in both models are very close - \$0.62 and \$0.70, respectively. Both findings are significant at the $\alpha=0.10$ level.

Table A5:
RExO Effects on Hourly Wage at First Job Following Random Assignment

Hourly wage, first job	Model 1	Model 2	Model 3	Model 4	Model 5
Treatment effect	0.541	0.564	0.624*	0.607	0.617*
	(1.309)	(1.545)	(1.705)	(1.640)	(1.664)
Post-stratification weights	(no)	(yes)	(yes)	(yes)	(yes)
Demographic covariates	(no)	(no)	(yes)	(yes)	(yes)
Criminal justice covariates	(no)	(no)	(no)	(no)	(no)
Hierarchical linear model	(no)	(no)	(no)	(yes)	(no)
Generalized Linear Model	(no)	(no)	(no)	(no)	(yes)
Intraclass correlation coefficient (ICC)				0.01	
Observations	1,725	1,725	1,712	1,712	1,712

NOTES:

T-statistics in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
All models present estimates from OLS regressions, except model 5, which is GLM with log link.
HLM model estimated with random intercept at program level.
Wages are received conditional on post-RA employment, which is itself affected by program status. As a result, values of this outcome across the program and control groups are no longer strictly comparable. The findings for this outcome should be considered suggestive rather than true impact estimates.

Hourly wage at current or most recent job

A second earnings-based outcome variable was the hourly wage at participants' current, or most recent, job. Consistent with the results in Tables II-1, results do not support the hypothesis that RExO increases average wages at the currently-held or most recently held job. Although all of the estimated effects are positive, none achieve statistical significance. As in the previous table, Model 5 presents results from a GLM regression with log link. Again, it is not clear whether model 4 or model 5 is superior, but neither indicates there is an effect of RExO on the hourly wage at participants' current (or most recent) job.[89]

[89] For consistency, we use the results from model 5 for multiple comparison adjustment.

Table A6:

RExO Effects on Hourly Wage at Current or Most Recent Job

Hourly wage, last job	**Model 1**	**Model 2**	**Model 3**	**Model 4**	**Model 5**
Treatment effect	0.251	0.222	0.321	0.335	0.326
	(0.183)	(0.170)	(0.243)	(0.215)	(0.290)
Post-stratification weights	(no)	(yes)	(yes)	(yes)	(yes)
Demographic covariates	(no)	(no)	(yes)	(yes)	(yes)
Criminal justice covariates	(no)	(no)	(no)	(no)	(no)
Hierarchical linear model	(no)	(no)	(no)	(yes)	(no)
Generalized Linear Model	(no)	(no)	(no)	(no)	(yes)
Intraclass correlation coefficient (ICC)				0.01	
Observations	2,657	2,657	2,638	2,638	2,638

NOTES:

T-statistics in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

All models present estimates from OLS regressions, except model 5, which is GLM with log link. HLM model estimated with random intercept at program level.

Wages are received conditional on post-RA employment, which is itself affected by program status. As a result, values of this outcome across the program and control groups are no longer strictly comparable. The findings for this outcome should be considered suggestive rather than true impact estimates.

Total personal income from all sources

Table A7 displays estimates of the effect of RExO on annual income (in the second year following random assignment.) The results suggest an effect that is both statistically and practically significant – on the order of $1,000 dollars, about a ten percent increase. The magnitude of these effects is proportionally larger than the estimated effect on wages, suggesting that one of the impacts of the RExO intervention may have been to increase the ability of participants to acquire non-wage income.

Table A7:

RExO Effects on Annual Income from All Sources

Annual income from all sources	Model 1	Model 2	Model 3	Model 4	Model 5
Treatment effect	905.7*	995.1**	1,108**	1,116**	1,104**
	(1.811)	(2.081)	(2.351)	(2.540)	(1.981)
Post-stratification weights	(no)	(yes)	(yes)	(yes)	(yes)
Demographic covariates	(no)	(no)	(yes)	(yes)	(yes)
Criminal justice covariates	(no)	(no)	(no)	(no)	(no)
Hierarchical linear model	(no)	(no)	(no)	(yes)	(no)
Generalized Linear Model	(no)	(no)	(no)	(no)	(yes)
Intraclass correlation coefficient (ICC)				0.01	
Observations	2,976	2,976	2,957	2,957	2,957

NOTES:

T-statistics in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

All models present estimates from OLS regressions, except model 5, which is GLM with log link. HLM model estimated with random intercept at program level.

This outcome is *not* subset on post-RA employment. As a result these results remain valid estimates of the impact of the program.

Sensitivity Analyses for Criminal Justice Outcomes

First year arrest

The tables that follow describe results for the key criminal justice outcomes discussed in Chapter III. Table A8 summarizes the effects of the program on the probability that participants were arrested in the first year following random assignment. The estimates of the treatment effect are all negative, which is consistent with the hypothesis that RExO reduces the probability of offender rearrest. However they are only marginally significant. Model 4 estimates that RExO reduces the probability of re-arrest within one year by about 2.3 percentage points, a finding that is significant at the α=0.10 level.

Table A8:

RExO Effects on the Probability of Arrest within One Year of Random Assignment

Arrest in year 1	Model 1	Model 2	Model 3	Model 4
Treatment effect	-0.0202	-0.0207	-0.0228*	-0.0233*
	(-1.475)	(-1.507)	(-1.682)	(-1.744)
Post-stratification weights	(no)	(yes)	(yes)	(yes)
Demographic covariates	(no)	(no)	(yes)	(yes)
Criminal justice covariates	(no)	(no)	(no)	(no)
Hierarchical linear model	(no)	(no)	(no)	(yes)
Intraclass correlation coefficient (ICC)				0.03
Observations	3,558	3,558	3,531	3,531

NOTES:

Z-statistics in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

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

Time to re-arrest (between randomization and interview)

As with the measure of time to employment, an alternative statistical technique (survival analysis) is appropriate for assessing how RExO affects the ability of participants to avoid re-arrest. As discussed earlier in this appendix, survival analysis avoids the loss of information associated with categorizing participants into only two groups (those who were/were not re-arrested within some discrete time interval). Results are reported in Table A9, below.

Table A9:

RExO Effects on Risk of Re-Arrest between Randomization and Follow-up Interview

Time to first re-arrest	Model 1	Model 2	Model 3	Model 4
Treatment effect	0.925	0.920	0.913	0.912
	(-1.289)	(-1.355)	(-1.488)	(-1.499)
Post-stratification weights	(no)	(yes)	(yes)	(yes)
Demographic covariates	(no)	(no)	(yes)	(yes)
Criminal justice covariates	(no)	(no)	(no)	(no)
Program-level fixed effects	(no)	(no)	(no)	(yes)
Observations	3,575	3,575	3,548	3,548

NOTES:

Z-statistics in parentheses.*** p<0.01, ** p<0.05, * p<0.1.
Coefficients are hazard ratios from Cox proportional hazard regressions.

As was the case with Table A3 (time to job acquisition), the results in Table A9 must be interpreted differently than results presented elsewhere in this section. Table A9 was constructed using available information: all "first arrests" are incorporated, including those that occurred after the end of the two-year period following random assignment. In Table A9, treatment effect estimates are hazard ratios—the ratio of the probability of arrest by program and control group members in the next time period, conditional on not having yet been arrested. Values less than one indicate that program group members are less likely to be re-arrested. In addition, Model 4 estimates use a fixed-effects specification, as opposed to random effects.

None of the estimates achieve statistical significance. As in previous examples, the first three models iterate through the addition of post-stratification weights and covariates. Model 4 includes fixed effects at the program level. In general the results of the survival analyses do not support the hypothesis that RExO increases the time to offender re-arrest.

Incarceration during the analysis period

Table A10 assesses the effect of RExO on the probability of incarceration during the two-year period following randomization. Effect size estimates are similar to those for the probability of rearrest (Tables A8 and A9) – a reduction on the order of about two percentage points. Table III-7 shows that that program group members were slightly less likely to be incarcerated during this time frame, compared with the control group; however this difference was not statistically significant. Adding demographic covariates and random effects seems to increase both the precision and magnitude of the estimate, but none of these alternative specifications reaches statistical significance.

Table A10:

RExO Effects on the Probability of Incarceration within Two Years of Random Assignment

Incarceration in years 1-2	Model 1	Model 2	Model 3	Model 4
Treatment effect	-0.0186	-0.0202	-0.0229	-0.0239
	(-1.091)	(-1.181)	(-1.365)	(-1.519)
Post-stratification weights	(no)	(yes)	(yes)	(yes)
Demographic covariates	(no)	(no)	(yes)	(yes)
Criminal justice covariates	(no)	(no)	(yes)	(no)
Hierarchical linear model	(no)	(no)	(no)	(yes)
Intraclass correlation coefficient (ICC)				0.03
Observations	3,565	3,565	3,538	3,538

NOTES:

Z-statistics in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

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

Total days of incarceration in the two years following random assignment

Finally, Table A11 provides estimates of the effect of RExO on total days of incarceration in the two year period following random assignment. As was the case with the estimation of the effect of RExO on total days of employment, the models in Table A11 are estimated conditional on some non-zero amount of incarceration having occurred. This is done to avoid confounding the effect of RExO on the probability of conviction with any effect on the total length of time incarcerated. The conditional nature of these models is reflected in the decreased sample sizes. None of the models support the hypothesis that RExO significantly reduces the duration of offender incarceration.

Table A11:

RExO Effects on Total Days of Incarceration in the First Two Years Following Random Assignment

Total days incarcerated in years 1-2	Model 1	Model 2	Model 3	Model 5
Treatment effect	4.483	4.313	2.377	3.034
	(0.330)	(0.315)	(0.176)	(0.2096)
Post-stratification weights	(no)	(yes)	(yes)	(yes)
Demographic covariates	(no)	(no)	(yes)	(yes)
Criminal justice covariates	(no)	(no)	(yes)	(no)
Hierarchical linear model	(no)	(no)	(no)	(yes)
Intraclass correlation coefficient (ICC)				0.03
Observations	1,505	1,505	1,498	1,498

NOTES:

T-statistics in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

All models present estimates from OLS regressions. HLM model estimated with random intercept at program level.

Days of incarceration are conditional on *any* post-RA incarceration, which is itself affected by program status. As a result, values of this outcome across the program and control groups are no longer strictly comparable. The findings for this outcome should be considered suggestive rather than true impact estimates. Alternative models not subset in this way also show no significant effects.

Multiple Comparisons

In the classic frequentist approach to hypothesis testing, a hypothesis of some effect (the "alternative" hypothesis) is contrasted with a hypothesis of no effect (the "null" hypothesis). The estimate of the effect size is combined with its estimated standard error to generate a probability value (p-value), which is typically interpreted as the probability of obtaining an estimate as least as large as that of the model due to random chance alone. If this p-value is small (typically smaller than 0.10 in quantitative program evaluation), standard practice is to conclude that there is sufficient evidence to reject the null hypothesis—a conservative way of stating that there is evidence that the program had a statistically significant effect.

The problem of multiple comparisons arises when researchers are interested in conducting many hypothesis tests using a single sample. This occurs frequently in program evaluation, including the present study, which seeks to simultaneously test whether the RExO program has a statistically significant effect on different measures of earnings, employment, and recidivism.

There are two alternatives ways of quantifying the multiple comparisons problem – the familywise error rate (FWER) and the false discovery rate (FDR). Given multiple tests, the FWER is the probability of committing one or more type I errors – concluding that the program had a statistically significant effect on an outcome when in fact it did not. Given two statistical tests with p-values of 0.10, the FWER is equal to 19.0%. Given ten such tests, the FWER is 65.1% $(1-(0.90)^{10})$.[90] The FWER-control philosophy of multiple comparisons holds that committing even a single type I error is a serious problem. FWER adjustments essentially seek to guarantee that the FWER does not exceed a baseline level, typically 0.05 or 0.10. The canonical control for the FWER is the Bonferroni correction, which works as follows:[91]

Given M concurrently estimated hypotheses, let $p_1, \ldots, p_i, \ldots p_M$ denote the p-values from those hypothesis tests. For any significance level α, declare the hypothesis associated with p_i significant if $p_i \leq \frac{\alpha}{M}$. For example, given an α of 0.10 and two hypothesis tests with $p_1 = 0.04$ and $p_2 = 0.06$, p_1 would be declared significant because $0.04 < \frac{0.10}{2}$, while $p_2$would not be declared significant because $0.06 > \frac{0.10}{2}$.

FWER adjustments such as the Bonferroni correction are not costless. They reduce the statistical power of tests, often by a significant amount, meaning that the probability of committing a type II error (concluding that there is no effect when in fact one exists) is greatly increased.

In contrast to the FWER, the FDR is the expected proportion of false positive discoveries—the percentage of all *significant* findings that are spurious. These are shown in Table A12 below.[92]

Table A12:

Conceptual Framework for Understanding the

Familywise Error Rate (FWER) and the False Discovery Rate (FDR)

	Statistical Test Results		
Truth (unobserved)	**Do not reject H_{0j}**	**Reject H_{0j}**	**Total**
H_{0j} is true (no impact)	A	B	N
H_{0j} is false (some impact)	C	D	(M-N)
Total	(M-Q)	Q	M

90 Duflo et al. (2007).

91 Bland and Altman (1995).

92 This table is adapted from Schochet (2008).

The rows of table A12 represent the (unknowable) true state of the world: The first row describes instances where the null hypotheses is true – there is no impact. The second row describes instances where the null hypothesis is in fact false – there is some impact. The columns of table A12 describe the results of statistical tests for each of these hypotheses. The first column describes instances where the null is not rejected – where no finding of a significant result is found. The second column describes the reverse – tests where the null hypothesis is rejected and evidence of an effect is found.

In the table, a total of M hypotheses (and associated tests) exist. Of these, Q null hypotheses are rejected, meaning evidence for a significant effect is found. Out of these Q, B is the number of instances where a significant effect is found when in fact no effect is present (instances of type I error), while D tests correctly find support for an effect that in fact exists. The FWER is simply the probability that B is greater than zero. By contrast, the FDR is the expected proportion of all positive findings that are in fact spurious,[93] i.e. $E[B/Q]$.

Similar to the FWER, a variety of procedures have been developed that allow researchers to "control for" the FDR—to ensure that it does not exceed some pre-specified tolerance value, such as 0.10. An additional similarity is that FDR control also reduces statistical power, although that loss can be reduced (relative to that of FWER control procedures), particularly when many tests generate statistically significant results. (Statistical power is the ability to detect an effect, given that it exists.)[94]

One method to control the FDR is the Benjamini and Hochberg procedure.[95] It works as follows:

Given a set of p-values from M statistical tests from tests conducted in a multiple comparisons setting $\{p_1, p_2, \ldots, p_i, p_M\}$ and an arbitrary significance level α, the Benjamini and Hochberg procedure is to:

(1) Rank order the M p-values.

(2) Define k as the largest i such that $p_i \leq \frac{i}{M}\alpha$.

(3) Reject null hypotheses for all tests i =1,…, k

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[93] The FDR is defined to be 0 when Q = 0.

[94] Schochet (2008).

[95] Benjamini and Hochberg (1995) .

Benjamini and Hochberg proved that this approach controls the false discovery rate, meaning that no more than α percent of the null hypothesis rejected in this manner will be true null hypotheses.

This appendix presents results from both the Bonferroni and Benjamini-Hochberg corrections for multiple comparisons. Multiple comparison adjustments are performed jointly across labor and criminal justice outcomes.

A third approach for the multiple comparisons problem is the composite domain method described by Schochet.[96] This method consolidates the outcome measures for a given construct into a single composite measure. This composite measure is then used as an outcome variable in a new analysis, with the results providing general information about the effect of the program on the construct in question.

An obvious question is how best to combine outcome measures that are scaled in different units (for example wages and total annual income). This is accomplished as follows:

Let $i = 1 \ldots N$ iterate individuals and $j = 1 \ldots M$ iterate different measures of a common construct (like employment). First, generate a normalized version of each outcome with mean 0 and standard deviation 1:

$$Y_{ij}^{norm} = \frac{(Y_{ij} - \bar{Y}_j)}{\sigma_{Y\,j}}$$

The composite measure is constructed as the weighted sum of the individual normalized outcomes:

$$C_i = \sum_{j=1}^{M} w_j Y_{ij}^{norm}$$

Where the w_j are weights denoting the relative importance of each of the Y_j. This analysis uses unit weights, giving equal importance to each outcome measure.

The evaluation team constructed two such composite outcomes—one each for the combined labor market measures and the combined criminal justice measures, respectively.

96 Schochet (2008).

Results of Multiple Comparisons Analysis

Tables A13 and A14 present results of Bonferroni and Benjamini-Hochberg adjustment.[97]

Table A13:

Bonferroni Adjustment of p-values, Employment & Recidivism Impact Analyses

Outcome	Un-adjusted p-value	α/M ($\alpha = 0.10$)	Significant?
1st year employment	0.023231	0.0909	No
2nd year employment	0.108925	0.0909	No
Days to 1st job (survival)	0.014356	0.0909	No
Total days employed	0.838208	0.0909	No
Wage (1st job)	0.082914	0.0909	No
Wage (current/last job)	0.843200	0.0909	No
Total income	0.046567	0.0909	No
1st year arrest	0.081207	0.0909	No
Days to re-arrest (survival)	0.133877	0.0909	No
Total days incarcerated	0.831577	0.0909	No
2-year incarceration	0.128743	0.0909	No

Table A14:

Benjamini-Hochberg Adjustment of p-values, Employment & Recidivism Impact Analyses

Outcome (ranked by p-value)	Unadjusted p-value	$\frac{i}{M}\alpha$ ($\alpha = 0.10$)	Significant?
Days to 1st job (survival)	0.014356	0.009091	No
1st year employment	0.023231	0.018182	No
Total income	0.046567	0.027273	No
1st year arrest	0.081207	0.036364	No
Wage (1st job)	0.082914	0.045455	No
2nd year employment	0.108925	0.054545	No
2-year incarceration	0.128743	0.063636	No
Days to re-arrest (survival)	0.133877	0.072727	No
Total days incarcerated	0.831577	0.081818	No
Total days employed	0.838208	0.090909	No
Wage (current/last job)	0.843200	0.100000	No

[97] In general, both sets of comparisons use p-values from models specified using HLM structure. There are two exceptions: P-values from models with fixed effects are used for survival analyses, and p-values from GLM models are used for wage and income outcomes.

Before adjusting for multiple comparisons, RExO appears to have a statistically significant effect on five of the eleven measures analyzed ($\alpha = 0.10$). Additionally, three of these are significant at the $\alpha = 0.05$ level. When the Bonferroni correction is applied, none of these findings of significance persist. This is also the case when the Benjamini-Hochberg procedure is applied, although in that case the tests for the first two outcomes (the indicator for first-year employment and the survival analysis for time to first job acquisition) verge on statistical significance even following adjustment.

As previously noted both here and by other authors, FWER and FDR-control techniques such as these enact severe penalties on statistical power,[98,99] implying a substantial increase in the probability of concluding that no effects exist when in fact one (or more) does. For this reason some authors have chosen to eschew these approaches entirely. This concern is particularly relevant in the context of evaluations of applied policy interventions such as RExO: in these environments sample size—and the improved statistical power that it affords—are often costly and/or difficult to increase.

The final approach for joint consideration of the statistical significance of the findings presented in this impact analysis is the composite domain approach described by Schochet. This involves construction of two such composite domain outcomes: One incorporating six measures corresponding to the general construct of labor market success , and one incorporating three measures corresponding to the general construct of criminal recidivism.[100] In a statistical model that matches the previously described structure (adjusted for non-response bias, controlling for demographic covariates, and incorporating HLM structure). The effect of the RExO program on the first composite measure is statistically significant ($\beta = 0.059, p = 0.005$) while the effect on second is not ($\beta = -0.040,\ p = 0.186$).

Because of the composite nature of the outcomes, these betas are not directly interpretable. However, the composite outcomes have standard deviation = 1 by design. Viewed in this context, both effect size estimates are small (relative to the overall variance of the outcomes), but mimic the general results described in the chapters. There appears to be a significant effect of RExO on labor market outcomes, but little to no effect on recidivism.

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[98] Schochet (2008).

[99] Bloom and Michalopoulos (2010) .

[100] The measures used for survival analyses are omitted from these composite domain measures because they are not structurally compatible with the model specification of the composite domain analysis (linear regression with random effects).

APPENDIX B: ADDITIONAL SUBGROUP TABLES

Services Received by Program and Control Groups

Table B – 1: Service Receipt by Age

	Age							
	Under 27				27 and Older			
Service Received	**Program**	**Control**	**Difference**	**P-value**	**Program**	**Control**	**Difference**	**P-value**
Work Readiness								
Job Club/Job Readiness Training(%)	73.8	52.3	21.6	0.000***	67.5	46.6	20.9	0.000***
Number of Days in Job Readiness Training†	59.6	52.8	6.8	0.072*	50.2	53.7	-3.4	0.486
Vocational Training								
Vocational training(%)	17.6	13.4	4.1	0.002***	18.6	12.2	6.4	0.035**
Number of weeks of vocational training†	13.4	21.2	-7.7	0.001***	14.9	15.7	-0.8	0.866
Received vocational certification/credentials(%)†	78.7	74.1	4.6	0.410	74.2	62.0	12.2	0.218
Job Search/Interviewing Assistance								
Independent job search(%)	48.0	35.1	12.9	0.000***	44.6	28.2	16.3	0.000***
Received job search assistance(%)	40.9	23.0	18.0	0.000***	36.2	20.9	15.3	0.000***
Referred to job opening by program(%)	40.5	29.3	11.2	0.000***	35.9	30.4	5.6	0.182
Received advice about job interviewing(%)	71.1	65.3	5.8	0.003***	66.7	62.7	4.0	0.318

	Age							
	Under 27				27 and Older			
Service Received	**Program**	**Control**	**Difference**	**P-value**	**Program**	**Control**	**Difference**	**P-value**
Received advice from program on answering employers' questions about criminal history(%)	72.8	63.6	9.3	0.000***	64.6	57.2	7.4	0.094*
Received advice about behavior at job from program(%)	68.3	59.4	8.9	0.000***	61.9	55.2	6.7	0.099*
Given people to contact about jobs in the community by program(%)	53.4	46.6	6.8	0.002***	47.1	40.7	6.3	0.150
Given help putting together resume by program(%)	74.9	68.3	6.6	0.001***	69.3	64.3	5.0	0.182
Given help filling out job applications by program(%)	66.9	58.3	8.6	0.000***	60.4	53.6	6.8	0.092*
Education Services								
Adult Basic Education/GED(%)	9.4	9.9	-0.5	0.729	17.7	15.4	2.3	0.428
Received GED, High school, or other degree/ diploma(%)	4.5	4.4	0.0	0.835	8.2	8.5	-0.2	0.910
Took college courses for credit(%)	13.1	13.4	-0.2	0.993	20.4	17.3	3.1	0.214
Mentoring								
Participated in formal mentoring at any agency(%)	24.8	10.4	14.4	0.000***	14.9	8.3	6.6	0.013**
Person (from program) to turn to for advice on family/personal issues(%)	61.2	51.1	10.1	0.000***	51.7	48.9	2.8	0.535
Do you have a mentor or guide (from program) (%)	54.5	42.0	12.5	0.000***	43.3	35.4	7.9	0.060*
Helpfulness of mentor in helping to avoid crime††	1.3	1.3	0.0	0.921	1.4	1.3	0.1	0.489
Pre-Release/Parole Referrals								
While incarcerated staff referred me to agencies/ organizations to find a job(%)	35.6	32.0	3.6	0.063*	30.5	24.5	6.0	0.079*
Parole or probation officer referred me to agency/ organization for help finding job(%)	40.1	35.0	5.1	0.015**	37.6	39.0	-1.4	0.649
Other Services								
Helpfulness of employment services†† ≠	1.5	1.9	-0.4	0.000***	1.7	1.7	0.0	0.944
Participated in other employment-related programs(%)≠	12.8	8.7	4.1	0.001***	10.2	11.4	-1.2	0.581

Service Received	Age: Under 27 Program	Control	Difference	P-value	Age: 27 and Older Program	Control	Difference	P-value
Number of weeks in employment-related program†	13.6	15.1	-1.5	0.561	14.9	10.0	4.9	0.203
Person (from program) who went out of their way to help me(%)≠	64.7	53.9	10.8	0.000***	53.4	50.2	3.2	0.496
Participated in sessions offering counseling or advice to former offenders(%)≠	51.8	42.0	9.8	0.000***	41.9	42.4	-0.5	0.848
Received help dealing with child support enforcement system(%)	10.1	7.4	2.6	0.023**	9.2	6.2	3.0	0.298

SOURCE: Two-year follow-up survey of participants

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

Probability values in this table are regression-adjusted for pre-random assignment characteristics.

† These items were only asked for those participants who received the given service; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.

†† These items indicate self-reported helpfulness of the service (on a scale of 1 to 5) and are reverse-coded such that lower scores indicate a more favorable rating. They also were only asked for those participants who received the given service.

For outcomes marked with an inequality sign (≠), the difference between subgroup effects in a fully interacted model were statistically significant (Lowenstein, 2014).

Table B – 2: Service Receipt by Cohort

	Cohort 1							
	Enrolled Prior to October 1				Enrolled After October 1			
Service Received	**Program**	**Control**	**Difference**	**P-value**	**Program**	**Control**	**Difference**	**P-value**
Work Readiness								
Job Club/Job Readiness Training(%)	72.8	49.7	23.0	0.000***	72.0	54.8	17.2	0.000***
Number of Days in Job Readiness Training†	57.5	49.2	8.3	0.035**	59.0	61.3	-2.4	0.872
Vocational Training								
Vocational training(%)	18.5	13.2	5.3	0.000***	16.0	13.4	2.6	0.255
Number of weeks of vocational training†	13.2	19.4	-6.2	0.003***	15.2	22.2	-7.0	0.081*
Received vocational certification/credentials(%)†	77.8	73.1	4.8	0.409	77.7	69.6	8.1	0.283
Job Search/Interviewing Assistance								
Independent job search(%)	48.0	32.4	15.7	0.000***	45.6	37.5	8.1	0.007***
Referred to job opening by program(%)	39.9	28.7	11.2	0.000***	38.6	31.2	7.4	0.018**
Received advice about job interviewing(%)	70.8	63.3	7.6	0.001***	68.7	68.4	0.3	0.711
Received advice from program on answering employers' questions about criminal history(%)	71.6	61.2	10.4	0.000***	70.2	65.5	4.8	0.106
Received advice about behavior at job from program(%)	67.5	56.9	10.6	0.000***	65.8	62.9	3.0	0.235
Given people to contact about jobs in the community by program(%)≠	52.8	43.4	9.3	0.000***	50.5	50.6	-0.1	0.836
Given help putting together resume by program(%)	73.8	65.9	7.9	0.000***	73.8	71.6	2.2	0.345
Given help filling out job applications by program(%)	65.1	55.5	9.6	0.000***	66.6	61.8	4.8	0.087*
Education Services								
Adult Basic Education/GED(%)	10.5	11.0	-0.5	0.629	12.4	10.7	1.7	0.389
Received GED, High school, or other degree/ diploma(%)	5.3	5.1	0.1	0.764	5.1	5.3	-0.2	0.926
Took college courses for credit(%)	14.9	14.0	0.9	0.390	13.9	14.4	-0.6	0.857

	Cohort 1							
	Enrolled Prior to October 1				Enrolled After October 1			
Service Received	**Program**	**Control**	**Difference**	**P-value**	**Program**	**Control**	**Difference**	**P-value**
Mentoring								
Participated in formal mentoring at any agency(%)≠	23.8	9.4	14.4	0.000***	20.4	11.4	9.0	0.000***
Person (from program) to turn to for advice on family/personal issues(%)	60.7	50.2	10.5	0.000***	55.7	51.7	3.9	0.193
Do you have a mentor or guide (from program) (%)	53.1	40.1	13.0	0.000***	50.0	42.3	7.7	0.020**
Helpfulness of mentor in helping to avoid crime††	1.3	1.3	0.0	0.958	1.4	1.3	0.1	0.687
Pre-Release/Parole Referrals								
While incarcerated staff referred me to agencies/ organizations to find a job(%)	35.3	30.1	5.2	0.010**	32.8	31.8	1.1	0.671
Parole or probation officer referred me to agency/organization for help finding job(%)	39.7	35.9	3.9	0.114	39.3	35.6	3.7	0.292
Other Services								
Helpfulness of employment services††	1.5	1.8	-0.3	0.000***	1.6	2.0	-0.4	0.004***
Participated in other employment-related programs(%)≠	12.7	7.4	5.3	0.000***	11.2	13.6	-2.5	0.284
Number of weeks in employment-related program†	13.4	13.7	-0.3	0.937	15.2	14.4	0.8	0.410
Person (from program) who went out of their way to help me(%)	63.1	53.8	9.3	0.000***	60.8	51.8	9.0	0.004***
Participated in sessions offering counseling or advice to former offenders(%)	49.7	42.1	7.6	0.002***	50.1	42.0	8.1	0.007***
Received help dealing with child support enforcement system(%)	10.7	7.9	2.8	0.045**	8.0	5.7	2.3	0.270

SOURCE: Two-year follow-up survey of participants

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

Probability values in this table are regression-adjusted for pre-random assignment characteristics.

† These items were only asked for those participants who received the given service; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.

†† These items indicate self-reported helpfulness of the service (on a scale of 1 to 5) and are reverse-coded such that lower scores indicate a more favorable rating. They also were only asked for those participants who received the given service.

For outcomes marked with an inequality sign (≠), the difference between subgroup effects in a fully interacted model were statistically significant (Lowenstein, 2014).

Table B – 3: Service Receipt by Gender

	Gender							
	Female				Male			
Service Received	**Program**	**Control**	**Difference**	**P-value**	**Program**	**Control**	**Difference**	**P-value**
Work Readiness								
Job Club/Job Readiness Training(%)	75.4	52.1	23.3	0.000***	71.8	51.2	20.6	0.000***
Number of Days in Job Readiness Training†	58.8	59.7	-0.9	0.970	57.8	50.8	7.0	0.090*
Vocational Training								
Vocational training(%)[≠]	12.3	14.3	-2.0	0.641	19.2	12.9	6.3	0.000***
Number of weeks of vocational training†	14.9	23.9	-9.0	0.086*	13.5	19.2	-5.6	0.005***
Received vocational certification/credentials(%)†	72.3	62.5	9.8	0.435	78.6	74.4	4.1	0.255
Job Search/Interviewing Assistance								
Independent job search(%)	46.6	36.4	10.2	0.007***	47.6	33.2	14.4	0.000***
Received job search assistance(%)	36.8	19.7	17.2	0.000***	40.8	23.2	17.5	0.000***
Referred to job opening by program(%)	37.6	28.0	9.7	0.019**	40.1	30.0	10.1	0.000***
Received advice about job interviewing(%)	69.4	61.5	7.9	0.046**	70.4	65.8	4.6	0.018**
Received advice from program on answering employers' questions about criminal history(%)	72.9	62.4	10.5	0.011**	70.8	62.6	8.2	0.000***
Received advice about behavior at job from program(%)	66.3	59.2	7.0	0.069*	67.3	58.5	8.8	0.000***
Given people to contact about jobs in the community by program(%)	51.0	44.6	6.4	0.178	52.6	45.7	6.9	0.001***
Given help putting together resume by program(%)	76.4	68.5	8.0	0.035**	73.2	67.4	5.7	0.003***
Given help filling out job applications by program(%)	63.4	55.4	8.0	0.035**	66.1	57.9	8.2	0.000***
Education Services								
Adult Basic Education/GED(%)	13.1	12.8	0.3	0.791	10.4	10.5	-0.1	0.882
Received GED, High school, or other degree/ diplom(%)a	4.3	6.0	-1.7	0.385	5.5	5.0	0.5	0.538
Took college courses for credit(%)	19.4	19.1	0.3	0.794	13.5	12.9	0.6	0.594
Mentoring								
Participated in formal mentoring at any agency(%)	22.9	10.9	12.0	0.000***	22.8	9.9	13.0	0.000***
Person (from program) to turn to for advice on family/personal issues(%)	62.5	53.9	8.6	0.044**	58.5	49.7	8.7	0.000***
Do you have a mentor or guide (from program) (%)	56.3	49.1	7.1	0.160	51.2	38.4	12.8	0.000***
Helpfulness of mentor in helping to avoid crime††	1.3	1.3	0.0	0.884	1.3	1.3	0.0	0.782

	Gender							
	Female				Male			
Service Received	**Program**	**Control**	**Difference**	**P-value**	**Program**	**Control**	**Difference**	**P-value**
Pre-Release/Parole Referrals								
While incarcerated staff referred me to agencies/ organizations to find a job(%)	38.0	31.3	6.7	0.060*	33.8	30.6	3.2	0.075*
Parole or probation officer referred me to agency/ organization for help finding job(%)	36.2	30.7	5.6	0.161	40.5	37.0	3.5	0.163
Other Services								
Helpfulness of employment services††	1.6	1.9	-0.3	0.060*	1.5	1.8	-0.3	0.000***
Participated in other employment-related programs(%)	12.4	8.1	4.2	0.102	12.2	9.5	2.8	0.023**
Number of weeks in employment-related program†	14.1	18.6	-4.5	0.241	13.7	12.9	0.9	0.651
Person (from program) who went out of their way to help me(%)	66.9	52.1	14.7	0.001***	61.5	53.3	8.2	0.000***
Participated in sessions offering counseling or advice to former offenders(%)≠	52.5	36.2	16.3	0.000***	49.0	43.4	5.6	0.006***
Received help dealing with child support enforcement system(%)	7.9	5.1	2.8	0.135	10.6	7.9	2.7	0.053*

SOURCE: Two-year follow-up survey of participants

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

Probability values in this table are regression-adjusted for pre-random assignment characteristics.

† These items were only asked for those participants who received the given service; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.

†† These items indicate self-reported helpfulness of the service (on a scale of 1 to 5) and are reverse-coded such that lower scores indicate a more favorable rating. They also were only asked for those participants who received the given service.

For outcomes marked with an inequality sign (≠), the difference between subgroup effects in a fully interacted model were statistically significant (Lowenstein, 2014).

Table B – 4: Service Receipt by Timing of Random Assignment

	Timing of Random Assignment (Relative to Release from Prison)							
	Early Assignment				Late Assignment			
Service Received	**Program**	**Control**	**Difference**	**P-value**	**Program**	**Control**	**Difference**	**P-value**
Work Readiness								
Job Club/Job Readiness Training(%)	72.8	51.0	21.8	0.000***	70.8	52.6	18.2	0.000***
Number of Days in Job Readiness Training†	57.1	51.4	5.7	0.124	61.9	53.4	8.5	0.406
Vocational Training								
Vocational training(%)	17.6	12.8	4.8	0.001***	18.3	14.0	4.4	0.098*
Number of weeks of vocational training†	13.5	20.6	-7.2	0.001***	13.5	19.5	-6.1	0.300
Received vocational certification/credentials(%)†	77.7	69.6	8.2	0.098*	76.5	79.5	-3.0	0.240
Job Search/Interviewing Assistance								
Independent job search(%)	48.3	34.0	14.3	0.000***	44.9	33.0	11.9	0.001***
Received job search assistance(%)	40.7	22.7	18.0	0.000***	38.0	21.4	16.5	0.000***
Referred to job opening by program(%)$^{\neq}$	40.1	27.3	12.8	0.000***	38.8	37.1	1.7	0.715
Received advice about job interviewing(%)	70.0	64.8	5.2	0.011**	70.3	65.8	4.5	0.283
Received advice from program on answering employers' questions about criminal history(%)	71.2	61.7	9.5	0.000***	70.0	64.4	5.6	0.183
Received advice about behavior at job from program(%)$^{\neq}$	67.1	56.6	10.5	0.000***	66.3	65.0	1.3	0.761
Given people to contact about jobs in the community by program(%)	53.3	45.4	7.9	0.000***	48.8	45.6	3.2	0.569
Given help putting together resume by program(%)$^{\neq}$	73.6	65.4	8.1	0.000***	73.7	75.5	-1.8	0.570
Given help filling out job applications by program(%)	65.3	55.5	9.7	0.000***	65.7	63.1	2.5	0.558
Education Services								
Adult Basic Education/GED(%)	10.8	11.2	-0.4	0.921	11.6	9.4	2.2	0.442
Received GED, High school, or other degree/diploma(%)	4.9	5.3	-0.3	0.736	6.5	5.0	1.5	0.340
Took college courses for credit(%)	15.0	14.3	0.7	0.662	13.4	14.3	-0.9	0.922
Mentoring								
Participated in formal mentoring at any agency(%)	23.4	10.3	13.1	0.000***	21.4	8.6	12.7	0.000***
Person (from program) to turn to for advice on family/personal issue(%)s	59.9	51.5	8.4	0.000***	56.5	47.9	8.7	0.042**
Do you have a mentor or guide (from program) (%)	52.7	41.0	11.7	0.000***	51.5	38.5	13.0	0.004***
Helpfulness of mentor in helping to avoid crime††	1.3	1.3	0.0	0.911	1.3	1.3	0.0	0.909

Service Received	Timing of Random Assignment (Relative to Release from Prison)							
	Early Assignment				Late Assignment			
	Program	Control	Difference	P-value	Program	Control	Difference	P-value
Pre-Release/Parole Referrals								
While incarcerated staff referred me to agencies/ organizations to find a job(%)	36.5	32.0	4.5	0.018**	26.3	27.2	-0.9	0.712
Parole or probation officer referred me to agency/ organization for help finding job(%)	39.8	35.4	4.3	0.062*	39.8	36.3	3.4	0.572
Other Services								
Helpfulness of employment services††	1.5	1.8	-0.3	0.000***	1.6	1.9	-0.3	0.088*
Participated in other employment-related programs(%)	11.8	9.1	2.7	0.028**	14.0	8.7	5.3	0.056*
Number of weeks in employment-related program†	12.9	13.6	-0.6	0.839	17.2	15.9	1.3	0.580
Person (from program) who went out of their way to help me(%)	63.3	53.2	10.1	0.000***	59.3	52.3	7.0	0.113
Participated in sessions offering counseling or advice to former offenders(%)≠	49.1	43.4	5.7	0.007***	52.9	36.5	16.4	0.000***
Received help dealing with child support enforcement system(%)	10.4	7.9	2.5	0.061*	8.9	4.7	4.2	0.098*

SOURCE: Two-year follow-up survey of participants

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

Probability values in this table are regression-adjusted for pre-random assignment characteristics.

† These items were only asked for those participants who received the given service; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.

†† These items indicate self-reported helpfulness of the service (on a scale of 1 to 5) and are reverse-coded such that lower scores indicate a more favorable rating. They also were only asked for those participants who received the given service.

For outcomes marked with an inequality sign (≠), the difference between subgroup effects in a fully interacted model were statistically significant (Lowenstein, 2014).

Table B – 5: Service Receipt by Number of Prior Convictions

	Number of Prior Convictions							
	3 or Fewer				4 or More			
Service Received	**Program**	**Control**	**Difference**	**P-value**	**Program**	**Control**	**Difference**	**P-value**
Work Readiness								
Job Club/Job Readiness Training(%)	74.5	51.8	22.7	0.000***	75.3	50.9	24.4	0.000***
Number of Days in Job Readiness Training†	58.7	52.2	6.6	0.236	60.5	56.1	4.3	0.358
Vocational Training								
Vocational training(%)	18.0	13.5	4.5	0.014**	19.9	13.6	6.3	0.001***
Number of weeks of vocational training†	13.1	21.9	-8.8	0.002***	13.0	17.4	-4.4	0.086*
Received vocational certification/credentials(%)†	79.1	75.1	3.9	0.686	78.4	72.5	5.9	0.200
Job Search/Interviewing Assistance								
Independent job search(%)≠	50.9	33.3	17.6	0.000***	47.1	35.8	11.3	0.000***
Received job search assistance(%)	41.0	23.2	17.8	0.000***	42.0	22.3	19.7	0.000***
Referred to job opening by program(%)	40.4	32.1	8.3	0.003***	40.1	26.6	13.5	0.000***
Received advice about job interviewing(%)	72.6	66.2	6.4	0.008***	69.9	64.8	5.1	0.069*
Received advice from program on answering employers' questions about criminal history(%)	72.6	63.4	9.2	0.000***	71.8	61.6	10.2	0.001***
Received advice about behavior at job from program(%)	67.5	60.7	6.9	0.003***	68.2	56.2	12.0	0.000***
Given people to contact about jobs in the community by program(%)	51.4	45.8	5.6	0.042**	53.6	43.4	10.2	0.000***
Given help putting together resume by program(%)	75.6	67.8	7.8	0.001***	74.9	69.6	5.2	0.039**
Given help filling out job applications by program(%)	66.8	55.7	11.2	0.000***	66.8	60.9	5.9	0.025**
Education Services								
Adult Basic Education/GED(%)	11.7	10.3	1.4	0.438	9.4	9.9	-0.5	0.954
Received GED, High school, or other degree/diploma(%)	6.1	5.9	0.2	0.805	4.0	3.7	0.3	0.767
Took college courses for credit(%)	15.9	15.9	0.0	0.871	13.8	12.6	1.2	0.646
Mentoring								
Participated in formal mentoring at any agency(%)	24.1	8.9	15.2	0.000***	23.4	10.9	12.4	0.000***
Person (from program) to turn to for advice on family/personal issues(%)	57.5	51.1	6.3	0.019**	62.2	47.4	14.8	0.000***
Do you have a mentor or guide (from program) (%)	51.9	38.5	13.4	0.000***	53.4	41.7	11.7	0.000***
Helpfulness of mentor in helping to avoid crime††	1.3	1.3	0.0	0.738	1.3	1.3	-0.0	0.485

Service Received	Number of Prior Convictions							
	3 or Fewer				4 or More			
	Program	Control	Difference	P-value	Program	Control	Difference	P-value
Pre-Release/Parole Referrals								
While incarcerated staff referred me to agencies/ organizations to find a job(%)	32.9	29.7	3.2	0.171	38.0	33.8	4.2	0.114
Parole or probation officer referred me to agency/ organization for help finding job(%)	40.4	36.8	3.6	0.379	40.1	35.2	4.8	0.065*
Other Services								
Helpfulness of employment services††	1.6	1.9	-0.3	0.001***	1.4	1.8	-0.4	0.000***
Participated in other employment-related programs(%)	13.1	10.2	3.0	0.054*	12.5	8.9	3.6	0.054*
Number of weeks in employment-related program†	14.1	15.4	-1.2	0.708	14.3	12.6	1.7	0.267
Person (from program) who went out of their way to help me(%)≠	62.7	48.7	14.0	0.000***	64.7	55.1	9.6	0.001***
Participated in sessions offering counseling or advice to former offenders(%)	51.2	41.3	9.9	0.000***	52.2	43.2	9.0	0.002***
Received help dealing with child support enforcement system(%)	7.6	6.7	0.9	0.498	12.1	6.8	5.2	0.003***

SOURCE: Two-year follow-up survey of participants

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

Probability values in this table are regression-adjusted for pre-random assignment characteristics.

† These items were only asked for those participants who received the given service; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.

†† These items indicate self-reported helpfulness of the service (on a scale of 1 to 5) and are reverse-coded such that lower scores indicate a more favorable rating. They also were only asked for those participants who received the given service.

For outcomes marked with an inequality sign (≠), the difference between subgroup effects in a fully interacted model were statistically significant (Lowenstein, 2014).

Table B – 6: Service Receipt by Education

Outcome	Program group	Control group	Difference (Impact)	P-value
	Education Subgroups			
No GED/HS Diploma				
Work Readiness				
Job Club/Job Readiness Training(%)	73.3	55.1	18.2	0.000***
Number of Days in Job Readiness Training†	61.5	56.3	5.2	0.467
Vocational Training				
Vocational training(%)	18.0	12.8	5.3	0.009***
Number of weeks of vocational training†	13.3	17.0	-3.7	0.124
Received vocational certification/credentials(%)†	78.6	67.8	10.8	0.127
Job Search/Interviewing Assistance				
Independent job search(%)≠	45.4	36.6	8.9	0.001***
Received job search assistance(%)	38.3	22.9	15.3	0.000***
Referred to job opening by program(%)	39.2	30.5	8.6	0.005***
Received advice about job interviewing(%)	69.9	64.8	5.1	0.080*
Received advice from program on answering employers' questions about criminal history(%)	69.4	61.6	7.8	0.009***
Received advice about behavior at job from program(%)	67.8	62.7	5.1	0.065*
Given people to contact about jobs in the community by program(%)	52.7	46.3	6.4	0.045**
Given help putting together resume by program(%)	73.7	69.7	4.0	0.143
Given help filling out job applications by program(%)	66.8	61.2	5.5	0.051*
Education Services				
Adult Basic Education/GED(%)	19.0	19.1	-0.1	0.709
Received GED, High school, or other degree/diploma(%)	6.5	6.5	0.0	0.814
Took college courses for credit(%)	10.6	9.7	1.0	0.545
Mentoring				
Participated in formal mentoring at any agency(%)	20.1	8.8	11.3	0.000***
Person (from program) to turn to for advice on family/personal issues(%)	57.7	52.3	5.4	0.070*
Do you have a mentor or guide (from program) (%)	52.7	41.5	11.2	0.000***
Helpfulness of mentor in helping to avoid crime††	1.3	1.3	0.0	0.876
Pre-Release/Parole Referrals				
While incarcerated staff referred me to agencies/ organizations to find a job(%)	32.7	32.2	0.6	0.940
Parole or probation officer referred me to agency/ organization for help finding job(%)	41.4	41.6	-0.2	0.955
Other Services				
Helpfulness of employment services††	1.5	1.7	-0.2	0.027**
Participated in other employment-related programs(%)	11.1	9.4	1.7	0.291
Number of weeks in employment-related program†	14.6	16.9	-2.2	0.465
Person (from program) who went out of their way to help me(%)	62.1	55.7	6.5	0.032**
Participated in sessions offering counseling or advice to former offenders(%)≠	47.3	45.0	2.2	0.603
Received help dealing with child support enforcement system(%)	10.6	9.3	1.3	0.536

Outcome	Program group	Control group	Difference (Impact)	P-value
Education Subgroups				
GED				
Work Readiness				
Job Club/Job Readiness Training(%)	70.6	48.1	22.5	0.000***
Number of Days in Job Readiness Training†	56.1	46.2	9.9	0.114
Vocational Training				
Vocational training(%)	17.9	14.4	3.5	0.174
Number of weeks of vocational training†	14.6	20.5	-5.9	0.123
Received vocational certification/credentials(%)†	72.2	67.7	4.4	0.897
Job Search/Interviewing Assistance				
Independent job search(%)≠	47.8	30.0	17.7	0.000***
Received job search assistance(%)	41.6	21.1	20.5	0.000***
Referred to job opening by program(%)	37.8	28.2	9.6	0.011**
Received advice about job interviewing(%)	68.1	65.8	2.4	0.448
Received advice from program on answering employers' questions about criminal history(%)	71.5	63.3	8.2	0.019**
Received advice about behavior at job from program(%)	65.3	55.2	10.1	0.006***
Given people to contact about jobs in the community by program(%)	51.8	43.1	8.7	0.023**
Given help putting together resume by program(%)	74.0	65.4	8.7	0.009***
Given help filling out job applications by program(%)	65.8	57.8	8.0	0.026**
Education Services				
Adult Basic Education/GED(%)	4.9	3.6	1.3	0.355
Received GED, High school, or other degree/diploma(%)	3.1	3.4	-0.3	0.783
Took college courses for credit(%)	20.1	17.2	2.9	0.338
Mentoring				
Participated in formal mentoring at any agency(%)	24.3	9.9	14.3	0.000***
Person (from program) to turn to for advice on family/personal issues(%)	57.5	47.8	9.7	0.007***
Do you have a mentor or guide (from program) (%)	50.5	38.4	12.1	0.001***
Helpfulness of mentor in helping to avoid crime††	1.3	1.3	0.0	0.836
Pre-Release/Parole Referrals				
While incarcerated staff referred me to agencies/organizations to find a job(%)	37.0	31.2	5.8	0.071*
Parole or probation officer referred me to agency/organization for help finding job(%)	42.3	36.9	5.4	0.168
Other Services				
Helpfulness of employment services††	1.5	2.0	-0.4	0.001***
Participated in other employment-related programs(%)	12.9	10.2	2.7	0.205
Number of weeks in employment-related program†	12.6	10.6	2.0	0.545
Person (from program) who went out of their way to help me(%)	60.9	48.9	12.1	0.001***
Participated in sessions offering counseling or advice to former offenders(%)≠	49.4	34.8	14.6	0.000***
Received help dealing with child support enforcement system(%)	9.5	4.4	5.1	0.022**
HS Diploma				

Education Subgroups

Outcome	Program group	Control group	Difference (Impact)	P-value
Work Readiness				
Job Club/Job Readiness Training(%)	73.2	48.8	24.5	0.000***
Number of Days in Job Readiness Training†	54.2	53.5	0.7	0.734
Vocational Training				
Vocational training(%)	17.2	12.8	4.4	0.049**
Number of weeks of vocational training†	13.5	24.2	-10.7	0.002***
Received vocational certification/credentials(%)†	81.8	81.5	0.3	0.805
Job Search/Interviewing Assistance				
Independent job search(%)≠	49.7	33.4	16.3	0.000***
Received job search assistance(%)	41.0	23.4	17.7	0.000***
Referred to job opening by program(%)	41.7	29.0	12.8	0.000***
Received advice about job interviewing(%)	72.6	64.3	8.4	0.011**
Received advice from program on answering employers' questions about criminal history(%)	73.5	63.0	10.5	0.002***
Received advice about behavior at job from program(%)	67.2	55.7	11.5	0.000***
Given people to contact about jobs in the community by program(%)	51.5	46.5	5.0	0.093*
Given help putting together resume by program(%)	73.8	66.5	7.3	0.014**
Given help filling out job applications by program(%)	63.6	51.8	11.8	0.000***
Education Services				
Adult Basic Education/GED(%)	5.0	6.2	-1.2	0.240
Received GED, High school, or other degree/diploma(%)	5.3	5.0	0.3	0.628
Took college courses for credit(%)	15.5	17.3	-1.8	0.479
Mentoring				
Participated in formal mentoring at any agency(%)	25.5	11.6	13.9	0.000***
Person (from program) to turn to for advice on family/personal issues(%)				
Do you have a mentor or guide (from program) (%)	53.2	41.9	11.3	0.001***
Helpfulness of mentor in helping to avoid crime††	1.4	1.3	0.0	0.756
Pre-Release/Parole Referrals				
While incarcerated staff referred me to agencies/ organizations to find a job(%)	35.0	28.0	7.0	0.020**
Parole or probation officer referred me to agency/ organization for help finding job(%)	34.2	27.0	7.2	0.025**
Other Services				
Helpfulness of employment services††	1.6	2.0	-0.3	0.007***
Participated in other employment-related programs(%)	13.3	8.1	5.2	0.007***
Number of weeks in employment-related program†	13.9	13.3	0.6	0.808
Person (from program) who went out of their way to help me(%)	64.3	53.5	10.8	0.000***
Participated in sessions offering counseling or advice to former offenders(%) ≠	53.9	44.1	9.9	0.003***
Received help dealing with child support enforcement system(%)	9.3	7.0	2.3	0.269

Education Subgroups

Outcome	Program group	Control group	Difference (Impact)	P-value

SOURCE: Two-year follow-up survey of participants

NOTES: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1.
Probability values in this table are regression-adjusted for pre-random assignment characteristics.
† These items were only asked for those participants who received the given service; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.
†† These items indicate self-reported helpfulness of the service (on a scale of 1 to 5) and are reverse-coded such that lower scores indicate a more favorable rating. They also were only asked for those participants who received the given service.
For outcomes marked with an inequality sign (≠), the difference between subgroup effects in a fully interacted model were statistically significant (Lowenstein, 2014).
Twelve percent of the sample reported their level of education as being "some college" or higher. This fraction was too small for meaningful subgroup analysis specific to this group; instead these individuals were consolidated with individuals who reported receipt of a HS diploma and analyzed as a single group.

Impacts on other Outcomes: Child Support

Table B – 7: Child Support Outcomes by Age

	Under 27				27 and Older			
Child Support Outcomes	**Program**	**Control**	**Difference**	**P-value**	**Program**	**Control**	**Difference**	**P-value**
Required by court to pay child support for children living away from home(%)	30.9	32.7	-1.8	0.380	26.6	20.1	6.6	0.180
Number of children required to pay child support for†	1.9	1.9	-0.0	0.767	1.5	1.7	-0.2	0.373
Approximate total amount provided, excluding child support required by court($)†≠	693	960	-267	0.045**	633	372	261	0.184
Number of children this support covered†	1.9	1.8	0.0	0.778	1.8	1.7	0.1	0.759
Concerns about owing child support affected willingness to accept job offers(%) †	11.7	12.5	-0.7	0.827	7.6	18.9	-11.3	0.118
Child Support Enforcement System								
Currently required to pay child support through the child support enforcement system(%)	85.1	86.9	-1.8	0.520	85.9	93.0	-7.1	0.370
Paid child support through the child support enforcement system during last month(%)†	61.5	62.7	-1.1	0.823	62.0	55.1	6.9	0.503
Amount paid through child support enforcement system during last month($)†	292	275	17	0.546	258	263	-6	0.879
Gave money directly to parent or guardian instead of going through child support system(%)†	39.5	43.5	-4.0	0.198	44.5	43.9	0.5	0.887
Assistance to Parent/Guardian of Child								
Gave money to parent or guardian during last six months(%)	95.2	89.5	5.7	0.080*	88.8	89.3	-0.5	0.906
Gave food to parent or guardian during last six months(%)	62.2	50.8	11.4	0.062*	72.1	46.2	25.9	0.118
Gave clothing to parent or guardian during last six months (%)	75.1	77.8	-2.7	0.500	82.7	82.5	0.2	0.944
Gave something else to parent or guardian during last six months(%)	39.9	35.6	4.3	0.502	44.2	27.9	16.4	0.318

Child Support Outcomes	Under 27				27 and Older			
	Program	Control	Difference	P-value	Program	Control	Difference	P-value

SOURCE: Two-year follow-up survey of participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

† These items were only asked of a subset of participants; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.

For outcomes marked with an inequality sign (≠), the difference between subgroup effects in a fully interacted model were statistically significant (Lowenstein, 2014).

Table B – 8: Child Support Outcomes by Cohort

Child Support Outcomes	Enrolled Prior to October 1				Enrolled after October 1			
	Program	Control	Difference	P-value	Program	Control	Difference	P-value
Required by court to pay child support for children living away from home(%)	32.1	31.3	0.8	0.694	25.7	30.4	-4.7	0.137
Number of children required to pay child support for†	1.8	1.9	-0.1	0.475	1.8	1.9	-0.0	0.791
Approximate total amount provided, excluding child support required by court($)†	682	980	-298	0.060*	692	751	-59	0.660
Number of children this support covered†	1.9	1.8	0.1	0.735	1.8	1.8	-0.0	0.964
Concerns about owing child support affected willingness to accept job offers (%)†	11.2	11.5	-0.3	0.982	11.2	17.0	-5.8	0.203
Child Support Enforcement System								
Currently required to pay child support through the child support enforcement system(%)	85.7	87.8	-2.2	0.471	83.7	86.3	-2.6	0.613
Paid child support through the child support enforcement system during last month(%)†	62.5	64.7	-2.2	0.581	58.6	54.8	3.8	0.477
Amount paid through child support enforcement system during last month($)†	$297.2	$286.5	$10.7	0.734	$250.6	$234.6	$15.9	0.821
Gave money directly to parent or guardian instead of going through child support system(%)†	41.6	43.3	-1.7	0.575	35.7	44.2	-8.4	0.182
Assistance to Parent/Guardian of Child								
Gave money to parent or guardian during last six months(%)	93.9	87.4	6.5	0.083*	95.3	94.7	0.5	0.939
Gave food to parent or guardian during last six months(%)	64.6	48.0	16.6	0.011**	60.5	56.5	4.0	0.777
Gave clothing to parent or guardian during last six months(%)	76.7	74.5	2.2	0.778	74.7	87.4	-12.7	0.138

Child Support Outcomes	Enrolled Prior to October 1				Enrolled after October 1			
	Program	Control	Difference	P-value	Program	Control	Difference	P-value
Gave something else to parent or guardian during last six months(%)	37.7	33.4	4.2	0.526	51.0	39.0	12.0	0.298

SOURCE: Two-year follow-up survey of participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

† These items were only asked of a subset of participants; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.

Table B – 9: Child Support Outcomes by Gender

Child Support Outcomes	Female				Male			
	Program	Control	Difference	P-value	Program	Control	Difference	P-value
Required by court to pay child support for children living away from home(%)	16.3	16.3	-0.1	0.971	34.4	35.6	-1.2	0.583
Number of children required to pay child support for†	2.0	1.8	0.2	0.386	1.8	1.9	-0.1	0.343
Approximate total amount provided, excluding child support required by court($)†	289	543	-254	0.080*	712	939	-226	0.077*
Number of children this support covered†	1.9	1.5	0.4	0.267	1.9	1.9	0.0	0.950
Concerns about owing child support affected willingness to accept job offers(%) †	13.3	11.4	2.0	0.693	10.9	13.3	-2.4	0.351
Child Support Enforcement System								
Currently required to pay child support through the child support enforcement system(%)	84.3	87.0	-2.6	0.723	85.2	87.4	-2.1	0.444
Paid child support through the child support enforcement system during last month(%)†	51.6	52.1	-0.4	0.960	63.0	63.1	-0.1	0.967
Amount paid through child support enforcement system during last month($)†	164	215	-51	0.391	301	282	18	0.545
Gave money directly to parent or guardian instead of going through child support system(%)†	29.1	29.6	-0.5	0.923	41.6	45.7	-4.1	0.208
Assistance to Parent/Guardian of Child								
Gave money to parent or guardian during last six months(%)	76.1	82.3	-6.2	0.737	96.5	90.1	6.4	0.033**
Gave food to parent or guardian during last six months(%)	76.2	45.8	30.4	0.103	62.3	50.8	11.5	0.056*
Gave clothing to parent or guardian during last six	76.0	92.0	-16.0	0.347	76.2	77.0	-0.8	0.747

Child Support Outcomes	Female				Male			
	Program	Control	Difference	P-value	Program	Control	Difference	P-value
months(%)								
Gave something else to parent or guardian during last six months(%)	41.7	38.9	2.8	0.799	40.7	34.6	6.0	0.326

SOURCE: Two-year follow-up survey of participants
NOTES: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1.
Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.
† These items were only asked of a subset of participants; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.

Table B – 10: Child Support Outcomes by Timing of Random Assignment

Child Support Outcomes	Early Assignment				Late Assignment			
	Program	Control	Difference	P-value	Program	Control	Difference	P-value
Required by court to pay child support for children living away from home(%)	31.8	31.8	0.0	0.987	24.7	31.2	-6.5	0.111
Number of children required to pay child support for†	1.8	1.9	-0.0	0.517	1.9	1.9	-0.0	0.876
Approximate total amount provided, excluding child support required by court($)†	691	876	-185	0.131	650	1075	-425	0.295
Number of children this support covered†	1.9	1.8	0.0	0.782	1.9	1.9	0.1	0.877
Concerns about owing child support affected willingness to accept job offers (%)†	11.3	13.0	-1.7	0.533	11.8	13.7	-1.9	0.785
Child Support Enforcement System								
Currently required to pay child support through the child support enforcement system(%)	86.4	86.6	-0.3	0.853	79.8	89.5	-9.7	0.183
Paid child support through the child support enforcement system during last month(%)†	62.1	62.2	-0.1	0.966	58.5	60.2	-1.7	0.914
Amount paid through child support enforcement system during last month($)†	277	277	0.4	0.969	343	269	74	0.335
Gave money directly to parent or guardian instead of going through child support system(%)†≠	39.1	46.2	-7.1	0.051*	45.5	36.0	9.5	0.277
Assistance to Parent/Guardian of Child								
Gave money to parent or guardian during last six months(%)	94.8	90.8	4.0	0.251	94.1	82.8	11.3	0.162
Gave food to parent or guardian during last six months(%)	64.0	50.7	13.3	0.038**	60.7	48.7	12.0	0.318

Child Support Outcomes	Early Assignment				Late Assignment			
	Program	Control	Difference	P-value	Program	Control	Difference	P-value
Gave clothing to parent or guardian during last six month(%)s	77.1	77.4	-0.3	0.885	72.6	81.8	-9.2	0.364
Gave something else to parent or guardian during last six months (%)	39.6	37.6	2.1	0.764	46.1	22.1	24.0	0.069*

SOURCE: Two-year follow-up survey of participants
NOTES: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1.
Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.
† These items were only asked of a subset of participants; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.
For outcomes marked with an inequality sign (≠), the difference between subgroup effects in a fully interacted model were statistically significant (Lowenstein, 2014).

Table B – 11: Child Support Outcomes by Number of Prior Convictions

Child Support Outcomes	3 or Fewer				4 or More			
	Program	Control	Difference	P-value	Program	Control	Difference	P-value
Required by court to pay child support for children living away from home(%)	26.9	28.5	-1.6	0.469	35.2	32.4	2.8	0.280
Number of children required to pay child support for†	1.8	2.0	-0.2	0.081*	1.8	1.8	0.0	0.930
Approximate total amount provided, excluding child	620	1125	-506	0.038**	777	717	60	0.796

Child Support Outcomes	3 or Fewer Program	Control	Difference	P-value	4 or More Program	Control	Difference	P-value
support required by court($)†≠								
Number of children this support covered†	1.9	1.9	-0.0	0.940	1.8	1.8	0.1	0.760
Concerns about owing child support affected willingness to accept job offers(%) †	11.1	13.2	-2.1	0.539	11.6	13.8	-2.2	0.616
Child Support Enforcement System								
Currently required to pay child support through the child support enforcement system(%)	86.4	87.3	-0.8	0.835	82.7	88.6	-5.9	0.120
Paid child support through the child support enforcement system during last month(%)†	62.3	64.2	-2.0	0.709	64.1	61.0	3.1	0.454
Amount paid through child support enforcement system during last month($)†	314	311	4	0.907	268	247	21	0.632
Gave money directly to parent or guardian instead of going through child support system(%)†	41.0	46.3	-5.3	0.250	41.8	38.7	3.2	0.523
Assistance to Parent/Guardian of Child								
Gave money to parent or guardian during last six months(%) ≠	90.3	92.9	-2.6	0.623	97.0	86.6	10.4	0.024**
Gave food to parent or guardian during last six months(%)	65.8	58.4	7.4	0.427	60.6	44.4	16.2	0.055*
Gave clothing to parent or guardian during last six months (%)≠	77.5	90.6	-13.2	0.055*	74.7	69.9	4.7	0.623
Gave something else to parent or guardian during last six months(%)	51.2	38.5	12.7	0.151	36.3	36.6	-0.3	0.890

SOURCE: Two-year follow-up survey of participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

† These items were only asked of a subset of participants; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.

For outcomes marked with an inequality sign (≠), the difference between subgroup effects in a fully interacted model were statistically significant (Lowenstein, 2014).

Table B – 12: Child Support Outcomes by Education

Outcomes	Program	Control	Difference	P-value
No GED/HS Diploma				
Child Support Outcomes				
Required by court to pay child support for children living away from home(%)	29.8	30.7	-0.9	0.776
Number of children required to pay child support for†	1.9	1.9	-0.0	0.789
Approximate total amount provided, excluding child support required by court($)†	717	763	-46	0.687
Number of children this support covered†	1.9	1.8	0.1	0.610
Concerns about owing child support affected willingness to accept job offers(%) †	14.0	11.0	3.0	0.425
Child Support Enforcement System				
Currently required to pay child support through the child support enforcement system(%)	86.4	88.2	-1.8	0.700
Paid child support through the child support enforcement system during last month(%)†	58.0	67.0	-8.9	0.135
Amount paid through child support enforcement system during last month($)†	255	242	13	0.717
Gave money directly to parent or guardian instead of going through child support system(%)†	38.3	42.6	-4.3	0.410
Assistance to Parent/Guardian of Child				
Gave money to parent or guardian during last six months(%)≠	96.1	82.9	13.3	0.019**
Gave food to parent or guardian during last six months(%)	59.4	47.5	11.9	0.198
Gave clothing to parent or guardian during last six months(%)	69.4	71.5	-2.1	0.746
Gave something else to parent or guardian during last six months(%)	37.0	29.8	7.2	0.398
GED				
Child Support Outcomes				
Required by court to pay child support for children living away from home(%)	31.2	32.4	-1.1	0.830
Number of children required to pay child support for†	1.8	1.8	0.0	0.923
Approximate total amount provided, excluding child support required by court($)†	660	660	0.2	0.915
Number of children this support covered†	1.9	1.7	0.1	0.536
Concerns about owing child support affected willingness to accept job offers (%)†	11.8	14.2	-2.4	0.627
Child Support Enforcement System				
Currently required to pay child support through the child support enforcement system(%)	85.7	83.0	2.7	0.580
Paid child support through the child support enforcement system during last month(%)†	58.7	54.1	4.6	0.506
Amount paid through child support enforcement system during last month($)†	293	312	-19	0.588
Gave money directly to parent or guardian instead of going through child support system(%)†	42.0	45.4	-3.5	0.531
Assistance to Parent/Guardian of Child				
Gave money to parent or guardian during last six months(%)≠	96.7	92.4	4.3	0.382

Outcomes	Program	Control	Difference	P-value
Gave food to parent or guardian during last six months(%)	66.9	57.0	9.9	0.327
Gave clothing to parent or guardian during last six months(%)	80.0	84.7	-4.7	0.476
Gave something else to parent or guardian during last six months(%)	44.0	44.1	-0.1	0.911
HS Diploma				
Child Support Outcomes				
Required by court to pay child support for children living away from home(%)	30.1	30.4	-0.3	0.780
Number of children required to pay child support for†	1.8	1.9	-0.1	0.218
Approximate total amount provided, excluding child support required by court($)†	662	1211	-549	0.066*
Number of children this support covered†	1.8	1.9	-0.1	0.434
Concerns about owing child support affected willingness to accept job offers (%)†	6.5	14.6	-8.1	0.048**
Child Support Enforcement System				
Currently required to pay child support through the child support enforcement system(%)	83.0	90.4	-7.4	0.097*
Paid child support through the child support enforcement system during last month(%)†	69.8	62.2	7.6	0.206
Amount paid through child support enforcement system during last month($)†	322	295	27	0.551
Gave money directly to parent or guardian instead of going through child support system(%)†	41.3	43.0	-1.8	0.629
Assistance to Parent/Guardian of Child				
Gave money to parent or guardian during last six months(%)$^{\neq}$	89.1	95.4	-6.4	0.300
Gave food to parent or guardian during last six months(%)	66.4	47.7	18.7	0.069*
Gave clothing to parent or guardian during last six months(%)	82.0	80.6	1.5	0.880
Gave something else to parent or guardian during last six months(%)	42.0	33.0	9.1	0.340

SOURCE: Two-year follow-up survey of participants
NOTES: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1.
Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

† These items were only asked of a subset of participants; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.

For outcomes marked with an inequality sign (≠), the difference between subgroup effects in a fully interacted model were statistically significant (Lowenstein, 2014).

Twelve percent of the sample reported their level of education as being “some college” or higher. This fraction was too small for meaningful subgroup analysis specific to this group; instead these individuals were consolidated with individuals who reported receipt of a HS diploma and analyzed as a single group.

Impacts on other Outcomes: Physical and Mental Health

Table B – 13: Physical and Mental Health Outcomes by Age

	Under 27				27 and Older			
Outcomes	**Program**	**Control**	**Difference**	**P-value**	**Program**	**Control**	**Difference**	**P-value**
Physical Health								
Needed to go to a doctor or hospital but lacked money or insurance(%)	40.5	40.4	0.2	0.955	32.6	36.5	-3.8	0.322
Needed to see a dentist but lacked money or insurance(%)	52.0	51.6	0.4	0.884	43.6	49.6	-6.0	0.103
Any visits to emergency room/urgent care(%)	49.7	53.3	-3.6	0.045**	40.8	46.7	-6.0	0.109
Number of visits to emergency room/urgent care†	3.1	3.5	-0.4	0.058*	2.5	3.1	-0.5	0.241
Number of those visits that were for emergencies and not routine care†	2.7	2.9	-0.2	0.223	2.2	2.5	-0.3	0.423
General state of health††	2.7	2.7	0.0	0.394	3.1	3.0	0.1	0.472
Physical health limited type of work or activities during last month(%)	26.1	30.9	-4.7	0.006***	11.4	14.7	-3.3	0.220
How much physical health interfered with normal work†,††	3.8	3.7	0.1	0.451	3.3	3.7	-0.4	0.151
Mental Health								
Emotional problems limited type of work or activities during last month(%)	21.0	22.1	-1.1	0.437	11.9	14.4	-2.5	0.341
How much emotional problems interfered with normal work both†,††	3.7	3.7	-0.1	0.459	3.2	3.2	0.0	0.833

SOURCE: Two-year follow-up survey of participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

† These items were only asked of a subset of participants; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.

†† These items report the means of responses to a Likert-scale question, where 5 indicates “a lot” or “excellent” and 1 indicates “not at all” or “poor.”

Table B – 14: Physical and Mental Health Outcomes by Cohort

Outcomes	Enrolled Prior to October 1				Enrolled After October 1			
	Program	Control	Difference	P-value	Program	Control	Difference	P-value
Physical Health								
Needed to go to a doctor or hospital but lacked money or insurance(%)	38.5	38.5	0.0	0.953	40.2	42.5	-2.4	0.496
Needed to see a dentist but lacked money or insurance(%)	49.5	50.1	-0.6	0.661	52.3	54.1	-1.8	0.636
Any visits to emergency room/urgent care (%)≠	45.9	52.4	-6.4	0.001***	52.8	51.3	1.5	0.595
Number of visits to emergency room/urgent care†	3.0	3.5	-0.5	0.035**	3.0	3.1	-0.2	0.536
Number of those visits that were for emergencies and not routine care†	2.6	2.9	-0.3	0.158	2.7	2.7	-0.1	0.707
General state of health††	2.8	2.8	0.0	0.385	2.7	2.7	0.1	0.393
Physical health limited type of work or activities during last month(%)	22.8	27.0	-4.2	0.016**	24.0	29.9	-5.9	0.054*
How much physical health interfered with normal work†,††	3.7	3.6	0.1	0.592	3.7	3.8	-0.1	0.642
Mental Health								
Emotional problems limited type of work or activities during last month(%)	17.9	20.6	-2.7	0.079*	22.3	20.7	1.6	0.563
How much emotional problems interfered with normal work both†,††	3.5	3.6	-0.1	0.533	3.7	3.7	-0.0	0.837

SOURCE: Two-year follow-up survey of participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

† These items were only asked of a subset of participants; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.

†† These items report the means of responses to a Likert-scale question, where 5 indicates "a lot" or "excellent" and 1 indicates "not at all" or "poor."

For outcomes marked with an inequality sign (≠), the difference between subgroup effects in a fully interacted model were statistically significant (Lowenstein, 2014).

Table B – 15: Physical and Mental Health Outcomes by Gender

Outcomes	Female Program	Female Control	Female Difference	Female P-value	Male Program	Male Control	Male Difference	Male P-value
Physical Health								
Needed to go to a doctor or hospital but lacked money or insuranc(%)e	39.8	38.9	0.9	0.786	38.8	39.8	-0.9	0.603
Needed to see a dentist but lacked money or insurance(%) ≠	56.6	50.6	6.0	0.126	48.7	51.4	-2.6	0.156
Any visits to emergency room/urgent care(%)	57.1	63.1	-6.0	0.112	45.5	49.3	-3.8	0.033**
Number of visits to emergency room/urgent care†	3.6	4.1	-0.4	0.273	2.8	3.2	-0.4	0.055*
Number of those visits that were for emergencies and not routine care†	3.1	3.6	-0.5	0.236	2.5	2.6	-0.2	0.340
General state of health†† ≠	2.5	2.6	-0.1	0.240	2.9	2.8	0.1	0.087*
Physical health limited type of work or activities during last month(%)	28.7	33.5	-4.7	0.192	21.6	26.3	-4.7	0.005***
How much physical health interfered with normal work†,††	3.7	3.8	-0.2	0.232	3.7	3.6	0.1	0.432
Mental Health								
Emotional problems limited type of work or activities during last month(%)	24.9	28.1	-3.2	0.413	17.6	18.8	-1.2	0.331
How much emotional problems interfered with normal work both†,††	3.7	3.9	-0.2	0.387	3.5	3.5	-0.0	0.977

SOURCE: Two-year follow-up survey of participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

† These items were only asked of a subset of participants; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.

†† These items report the means of responses to a Likert-scale question, where 5 indicates “a lot” or “excellent” and 1 indicates “not at all” or “poor.”

For outcomes marked with an inequality sign (≠), the difference between subgroup effects in a fully interacted model were statistically significant (Lowenstein, 2014).

Table B – 16: Physical and Mental Health Outcomes by Timing of Random Assignment

Outcomes	Early Assignment				Late Assignment			
	Program	Control	Difference	P-value	Program	Control	Difference	P-value
Physical Health								
Needed to go to a doctor or hospital but lacked money or insurance(%)	38.2	39.5	-1.3	0.467	42.3	39.1	3.2	0.387
Needed to see a dentist but lacked money or insurance(%)	49.4	50.8	-1.5	0.432	53.5	51.6	1.9	0.681
Any visits to emergency room/urgent care(%)	48.1	51.0	-2.9	0.114	47.0	55.2	-8.2	0.026**
Number of visits to emergency room/urgent care†	3.0	3.3	-0.3	0.095*	3.1	3.5	-0.4	0.346
Number of those visits that were for emergencies and not routine care†	2.6	2.8	-0.2	0.207	2.8	2.8	-0.0	0.974
General state of health††	2.8	2.8	0.1	0.229	2.8	2.8	-0.1	0.633
Physical health limited type of work or activities during last month(%)	22.5	27.7	-5.3	0.002***	24.3	24.7	-0.4	0.949
How much physical health interfered with normal work†,††	3.7	3.7	0.1	0.422	3.5	3.7	-0.2	0.398
Mental Health								
Emotional problems limited type of work or activities during last month(%)	19.0	19.6	-0.6	0.663	19.8	21.8	-2.0	0.415
How much emotional problems interfered with normal work both†,††	3.6	3.6	-0.0	0.940	3.6	3.7	-0.1	0.819

SOURCE: Two-year follow-up survey of participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

† These items were only asked of a subset of participants; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.

†† These items report the means of responses to a Likert-scale question, where 5 indicates "a lot" or "excellent" and 1 indicates "not at all" or "poor."

Table B – 17: Physical and Mental Health Outcomes by Number of Prior Convictions

Outcomes	3 or Fewer Program	Control	Difference	P-value	4 or More Program	Control	Difference	P-value
Physical Health								
Needed to go to a doctor or hospital but lacked money or insurance(%)	39.4	37.7	1.7	0.523	37.9	40.5	-2.6	0.313
Needed to see a dentist but lacked money or insurance(%)	52.1	51.3	0.8	0.848	47.7	51.4	-3.7	0.146
Any visits to emergency room/urgent care(%)	47.1	51.3	-4.1	0.096*	46.5	51.7	-5.2	0.038**
Number of visits to emergency room/urgent care†	2.9	3.2	-0.3	0.353	2.9	3.4	-0.6	0.041**
Number of those visits that were for emergencies and not routine care†	2.6	2.7	-0.1	0.698	2.6	2.9	-0.3	0.182
General state of health††	2.8	2.8	0.0	0.887	2.8	2.8	0.0	0.486
Physical health limited type of work or activities during last month(%)	20.1	23.6	-3.5	0.110	24.7	31.6	-6.9	0.003***
How much physical health interfered with normal work†,††	3.6	3.6	0.0	0.995	3.8	3.7	0.1	0.444
Mental Health								
Emotional problems limited type of work or activities during last month(%)	17.6	19.0	-1.4	0.549	20.5	21.4	-0.9	0.553
How much emotional problems interfered with normal work both†,††	3.6	3.7	-0.0	0.792	3.6	3.7	-0.0	0.772

SOURCE: Two-year follow-up survey of participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

† These items were only asked of a subset of participants; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.

†† These items report the means of responses to a Likert-scale question, where 5 indicates “a lot” or “excellent” and 1 indicates “not at all” or “poor.”

Table B – 18: Physical and Mental Health Outcome by Education

Outcomes	Program	Control	Difference	P-value
No GED/HS Diploma				
Physical Health				
Needed to go to a doctor or hospital but lacked money or insurance(%)	41.0	40.8	0.2	0.899
Needed to see a dentist but lacked money or insurance(%)	52.3	49.6	2.7	0.296
Any visits to emergency room/urgent care(%)	46.2	50.7	-4.5	0.088*
Number of visits to emergency room/urgent care†	2.9	3.3	-0.4	0.177
Number of those visits that were for emergencies and not routine care†	2.6	2.9	-0.3	0.271
General state of health††	2.9	2.8	0.1	0.165
Physical health limited type of work or activities during last month(%)	22.6	26.4	-3.8	0.130
How much physical health interfered with normal work†,††	3.6	3.5	0.1	0.347
Mental Health				
Emotional problems limited type of work or activities during last month(%)	18.7	20.8	-2.2	0.288
How much emotional problems interfered with normal work both†,††	3.5	3.6	-0.1	0.594
GED				
Physical Health				
Needed to go to a doctor or hospital but lacked money or insurance(%)	34.5	37.0	-2.5	0.384
Needed to see a dentist but lacked money or insurance(%)	47.3	51.9	-4.7	0.147
Any visits to emergency room/urgent care(%)	50.7	52.2	-1.5	0.605
Number of visits to emergency room/urgent care†	2.8	3.2	-0.5	0.100
Number of those visits that were for emergencies and not routine care†	2.5	2.9	-0.3	0.206
General state of health††	2.8	2.8	0.0	0.583
Physical health limited type of work or activities during last month(%)	23.6	28.4	-4.9	0.101
How much physical health interfered with normal work†,††	3.8	3.9	-0.0	0.853
Mental Health				
Emotional problems limited type of work or activities during last month(%)	21.7	20.6	1.1	0.625
How much emotional problems interfered with normal work both†,††	3.7	3.5	0.1	0.325
HS Diploma				
Physical Health				
Needed to go to a doctor or hospital but lacked money or insurance(%)	40.0	40.2	-0.3	0.930
Needed to see a dentist but lacked money or insurance(%)	50.1	52.8	-2.7	0.325
Any visits to emergency room/urgent care(%)	47.8	53.6	-5.9	0.038**

Outcomes	Program	Control	Difference	P-value
Number of visits to emergency room/urgent care†	3.3	3.6	-0.3	0.395
Number of those visits that were for emergencies and not routine care†	2.8	2.8	-0.0	0.886
General state of health††	2.7	2.7	-0.0	0.915
Physical health limited type of work or activities during last month(%)	23.6	29.2	-5.6	0.030**
How much physical health interfered with normal work†,††	3.8	3.8	-0.0	0.637
Mental Health				
Emotional problems limited type of work or activities during last month(%)	17.6	20.4	-2.8	0.181
How much emotional problems interfered with normal work both†,††	3.7	3.9	-0.2	0.268

SOURCE: Two-year follow-up survey of participants

NOTES: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1.
Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

† These items were only asked of a subset of participants; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.

†† These items report the means of responses to a Likert-scale question, where 5 indicates “a lot” or “excellent” and 1 indicates “not at all” or “poor.”

Twelve percent of the sample reported their level of education as being “some college” or higher. This fraction was too small for meaningful subgroup analysis specific to this group; instead these individuals were consolidated with individuals who reported receipt of a HS diploma and analyzed as a single group.

Impacts on other Outcomes: Substance Abuse

Table B-19: Substance Abuse Outcomes by Age

Substance Abuse Outcomes	Under 27 Program	Under 27 Control	Under 27 Difference	Under 27 P-value	27 and Older Program	27 and Older Control	27 and Older Difference	27 and Older P-value
Treatment At Any Point Since RA								
In treatment program for substance abuse at any point(%) ≠	31.5	31.2	0.3	0.697	24.5	32.0	-7.6	0.048**
Treatment was mandated/condition of parole(%)†	68.6	72.9	-4.3	0.198	76.9	80.1	-3.3	0.569
In self-help groups, such as Alcoholics Anonymous or Narcotics Anonymous(%)†	70.7	61.1	9.6	0.152	65.5	59.4	6.1	0.709
Treatment Within the Most Recent Month†								
In any treatment programs during last month(%)	33.6	29.5	4.1	0.162	29.6	19.8	9.7	0.081*
In detoxification during last month(%)	15.0	18.5	-3.5	0.421	15.6	7.3	8.3	0.405
In outpatient drug free program in last month(%)	52.8	42.0	10.8	0.083*	35.1	38.9	-3.8	0.709
In medicinal treatment (i.e., Methadone) program during last month(%)	10.2	10.8	-0.6	0.953	12.3	7.4	4.8	0.552
In residential program during last month(%)	32.3	37.6	-5.3	0.315	53.3	52.9	0.4	0.989
In other type of treatment during last month(%)≠	19.4	11.1	8.4	0.077*	25.1	0.0	25.1	n/a‡
Substance Use in Most Recent Month								
Used any illegal drugs or prescription drugs without prescription during last month(%)	9.1	9.8	-0.6	0.627	11.2	10.6	0.7	0.649
Illegal drug usage at least weekly(%)	5.7	5.9	-0.2	0.870	7.8	7.4	0.4	0.704
One or more days drinking 5 or more drinks in a row, within a couple hours during last month(%)	16.4	16.6	-0.3	0.799	21.2	22.2	-1.0	0.731

SOURCE: Two-year follow-up survey of participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

† These items were all only asked for those participants who reported being in treatment at any point since RA; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.

For outcomes marked with an inequality sign (≠), the difference between subgroup effects in a fully interacted model were statistically significant (Lowenstein, 2014).

‡ No p-values reported because there were not enough cases to perform this test. No members of the control group who were age 27 or younger reported participating in this type of program.

Table B – 20: Substance Abuse Outcomes by Cohort

Substance Abuse Outcomes	Enrolled Prior to October 1				Enrolled After October 1			
	Program	Control	Difference	P-value	Program	Control	Difference	P-value
Treatment At Any Point Since RA								
In treatment program for substance abuse at any point(%)	28.8	31.5	-2.6	0.246	33.2	31.1	2.2	0.405
Treatment was mandated/condition of parole(%)†	69.9	73.5	-3.6	0.357	70.1	76.3	-6.2	0.187
In self-help groups, such as Alcoholics Anonymous or Narcotics Anonymous(%)†	72.3	59.5	12.8	0.072*	64.6	64.0	0.6	0.958
Treatment Within the Most Recent Month†								
In any treatment programs during last month(%)	33.4	26.9	6.5	0.045**	31.9	29.6	2.3	0.522
In detoxification during last month(%)	18.5	16.7	1.9	0.753	7.2	17.5	-10.3	0.131
In outpatient drug free program in last month(%)≠	47.9	45.7	2.2	0.765	55.4	32.0	23.4	0.032**
In medicinal treatment (i.e., Methadone) program during last month(%)	12.7	10.9	1.8	0.539	5.6	9.1	-3.4	0.596
In residential program during last month(%)	37.8	42.0	-4.2	0.468	29.9	34.6	-4.7	0.647
In other type of treatment during last month(%)	22.0	9.2	12.8	0.017**	16.4	10.6	5.8	0.536
Substance Use in Most Recent Month								
Used any illegal drugs or prescription drugs without prescription during last mont(%)h≠	9.8	9.0	0.9	0.402	8.8	12.3	-3.5	0.102
Illegal drug usage at least weekl(%)y≠	6.4	5.1	1.3	0.148	5.5	9.0	-3.4	0.047**
One or more days drinking 5 or more drinks in a row, within a couple hours during last month(%)≠	17.9	16.0	1.9	0.263	15.9	21.6	-5.7	0.019**

SOURCE: Two-year follow-up survey of participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

† These items were all only asked for those participants who reported being in treatment at any point since RA; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.

For outcomes marked with an inequality sign (≠), the difference between subgroup effects in a fully interacted model were statistically significant (Lowenstein, 2014).

Table B – 21: Substance Abuse Outcomes by Gender

Substance Abuse Outcomes	Female				Male			
	Program	Control	Difference	P-value	Program	Control	Difference	P-value
Treatment At Any Point Since RA								
In treatment program for substance abuse at any point(%)	32.3	34.3	-2.0	0.601	29.7	30.5	-0.8	0.855
Treatment was mandated/condition of parole(%)† ≠	70.1	64.2	5.9	0.312	70.0	77.2	-7.2	0.025**
In self-help groups, such as Alcoholics Anonymous or Narcotics Anonymous(%)†	64.6	68.7	-4.1	0.668	71.4	59.2	12.2	0.070*
Treatment Within the Most Recent Month†								
In any treatment programs during last month(%)	33.8	22.4	11.4	0.051*	32.6	29.6	3.0	0.269
In detoxification during last month(%)	6.5	17.8	-11.3	0.122	17.6	16.7	0.9	0.828
In outpatient drug free program in last month(%)≠	60.7	26.6	34.1	0.018**	46.8	44.9	1.9	0.741
In medicinal treatment (i.e., Methadone) program during last month(%)	11.8	9.2	2.6	0.723	10.2	10.6	-0.4	0.897
In residential program during last month(%)	30.0	48.7	-18.7	0.143	37.1	37.7	-0.6	0.851
In other type of treatment during last month(%)	14.3	8.6	5.7	0.613	22.1	9.8	12.3	0.015**
Substance Use in Most Recent Month								
Used any illegal drugs or prescription drugs without prescription during last month(%)	10.2	9.0	1.2	0.512	9.4	10.3	-0.9	0.517
Illegal drug usage at least weekly(%)	6.6	4.7	1.9	0.237	6.1	6.6	-0.5	0.584
One or more days drinking 5 or more drinks in a row, within a couple hours during last month(%)	12.3	10.9	1.4	0.637	18.8	19.5	-0.7	0.614

SOURCE: Two-year follow-up survey of participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

† These items were all only asked for those participants who reported being in treatment at any point since RA; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.

For outcomes marked with an inequality sign (≠), the difference between subgroup effects in a fully interacted model were statistically significant (Lowenstein, 2014).

Table B – 22: Substance Abuse Outcomes by Timing of Random Assignment

Substance Abuse Outcomes	Early Assignment Program	Control	Difference	P-value	Late Assignment Program	Control	Difference	P-value
Treatment At Any Point Since RA								
In treatment program for substance abuse at any point(%)	30.5	31.2	-0.7	0.870	29.2	31.3	-2.2	0.641
Treatment was mandated/condition of parole(%)†	71.2	74.9	-3.7	0.276	66.5	74.2	-7.7	0.245
In self-help groups, such as Alcoholics Anonymous or Narcotics Anonymous(%)†≠	66.6	66.9	-0.3	0.874	81.9	50.8	31.1	0.011**
Treatment Within the Most Recent Month†								
In any treatment programs during last month(%)	33.1	26.6	6.5	0.041**	32.5	31.7	0.8	0.740
In detoxification during last month(%)	13.7	18.5	-4.8	0.316	22.7	14.5	8.2	0.410
In outpatient drug free program in last month(%)	49.7	42.0	7.7	0.259	54.8	32.9	21.8	0.103
In medicinal treatment (i.e., Methadone) program during last month(%)≠	12.5	9.1	3.4	0.301	2.8	15.2	-12.4	0.101
In residential program during last month(%)	37.3	42.1	-4.8	0.407	30.6	37.4	-6.9	0.596
In other type of treatment during last month	22.0	10.9	11.1	0.038**	15.0	7.2	7.8	0.339
Substance Use in Most Recent Month								
Used any illegal drugs or prescription drugs without prescription during last month(%)	10.0	10.8	-0.9	0.589	7.5	7.0	0.5	0.759
Illegal drug usage at least weekly(%)	6.6	6.9	-0.4	0.808	4.6	3.7	0.8	0.603
One or more days drinking 5 or more drinks in a row, within a couple hours during last month(%)	17.2	18.0	-0.7	0.600	17.7	16.5	1.2	0.741

SOURCE: Two-year follow-up survey of participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

† These items were all only asked for those participants who reported being in treatment at any point since RA; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.

For outcomes marked with an inequality sign (≠), the difference between subgroup effects in a fully interacted model were statistically significant (Lowenstein, 2014).

Table B – 23: Substance Abuse Outcomes by Number of Prior Convictions

Substance Abuse Outcomes	3 or Fewer Program	Control	Difference	P-value	4 or More Program	Control	Difference	P-value
Treatment At Any Point Since RA								
In treatment program for substance abuse at any point(%)≠	28.8	33.4	-4.6	0.065*	33.3	31.5	1.8	0.361
Treatment was mandated/condition of parole(%)†	75.2	77.1	-1.9	0.623	68.5	75.7	-7.2	0.095*
In self-help groups, such as Alcoholics Anonymous or Narcotics Anonymous(%)†	70.3	59.1	11.2	0.268	71.9	61.8	10.1	0.221
Treatment Within the Most Recent Month†								
In any treatment programs during last month(%)	28.6	25.0	3.5	0.303	37.7	33.8	3.9	0.345
In detoxification during last month(%)	15.5	13.9	1.6	0.949	15.5	20.8	-5.3	0.411
In outpatient drug free program in last month(%)	41.5	38.2	3.2	0.853	54.9	47.7	7.2	0.365
In medicinal treatment (i.e., Methadone) program during last month(%)	7.1	6.0	1.1	0.789	12.8	14.5	-1.7	0.924
In residential program during last month(%)	40.5	39.2	1.2	0.890	34.2	39.2	-5.0	0.447
In other type of treatment during last month(%)	25.3	8.0	17.3	0.026**	16.0	10.3	5.7	0.298
Substance Use in Most Recent Month								
Used any illegal drugs or prescription drugs without prescription during last month(%)	7.7	9.6	-1.8	0.242	11.3	12.0	-0.7	0.676
Illegal drug usage at least weekl(%)y	4.9	6.0	-1.2	0.342	7.3	7.3	0.0	0.973
One or more days drinking 5 or more drinks in a row, within a couple hours during last month(%)	16.7	17.2	-0.5	0.729	16.1	17.2	-1.1	0.571

SOURCE: Two-year follow-up survey of participants

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

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

† These items were all only asked for those participants who reported being in treatment at any point since RA; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.

For outcomes marked with an inequality sign (≠), the difference between subgroup effects in a fully interacted model were statistically significant (Lowenstein, 2014).

Table B – 24: Substance Abuse Outcomes by Education

Substance Abuse Outcomes	Program	Control	Difference	P-value
No GED/HS Diploma				
Treatment At Any Point Since RA				
In treatment program for substance abuse at any point(%)	28.2	32.3	-4.1	0.205
Treatment was mandated/condition of parole(%)†	70.2	76.3	-6.1	0.159
In self-help groups, such as Alcoholics Anonymous or Narcotics Anonymous(%)†	68.1	68.2	-0.2	0.874
Treatment Within the Most Recent Month†				
In any treatment programs during last month(%)	35.2	28.4	6.8	0.081*
In detoxification during last month(%)	17.6	22.5	-4.9	0.399
In outpatient drug free program in last month(%)	53.4	39.4	14.0	0.127
In medicinal treatment (i.e., Methadone) program during last month(%)	12.9	13.1	-0.2	0.929
In residential program during last month(%)	43.0	48.9	-5.9	0.464
In other type of treatment during last month(%)	16.0	2.3	13.7	0.037**
Substance Use in Most Recent Month				
Used any illegal drugs or prescription drugs without prescription during last month(%)	9.3	7.6	1.8	0.173
Illegal drug usage at least weekly(%)	6.0	5.1	0.9	0.446
One or more days drinking 5 or more drinks in a row, within a couple hours during last month(%)	18.4	20.9	-2.5	0.206
GED				
Treatment At Any Point Since RA				
In treatment program for substance abuse at any point(%)	32.3	31.8	0.5	0.889
Treatment was mandated/condition of parole(%)†	71.7	74.5	-2.8	0.673
In self-help groups, such as Alcoholics Anonymous or Narcotics Anonymous(%)†	69.5	52.5	17.1	0.189
Treatment Within the Most Recent Month†				
In any treatment programs during last month(%)	27.4	28.1	-0.6	0.938
In detoxification during last month(%)	11.6	12.4	-0.8	0.895
In outpatient drug free program in last month(%)	51.6	43.7	7.9	0.468
In medicinal treatment (i.e., Methadone) program during last month(%)	11.3	6.5	4.8	0.430
In residential program during last month(%)	25.1	38.2	-13.1	0.136
In other type of treatment during last month(%)	22.8	16.7	6.1	0.450
Substance Use in Most Recent Month				
Used any illegal drugs or prescription drugs without prescription during last month(%)	12.2	13.6	-1.3	0.580
Illegal drug usage at least weekly(%)	8.7	7.7	1.0	0.556
One or more days drinking 5 or more drinks in a row, within a couple hours during last month(%)	16.9	15.4	1.5	0.569
HS Diploma				
Treatment At Any Point Since RA				
In treatment program for substance abuse at any point(%)	30.8	29.7	1.1	0.692
Treatment was mandated/condition of parole(%)†	68.1	71.3	-3.3	0.519
In self-help groups, such as Alcoholics Anonymous or Narcotics Anonymous(%)†	72.8	57.2	15.6	0.124

Substance Abuse Outcomes	Program	Control	Difference	P-value
Treatment Within the Most Recent Month†				
In any treatment programs during last month(%)	35.1	26.4	8.7	0.082*
In detoxification during last month(%)	14.2	12.7	1.5	0.765
In outpatient drug free program in last month(%)	44.8	42.6	2.2	0.859
In medicinal treatment (i.e., Methadone) program during last month(%)	6.8	9.6	-2.8	0.731
In residential program during last month(%)	32.7	27.1	5.6	0.509
In other type of treatment during last month(%)	24.1	14.2	9.9	0.296
Substance Use in Most Recent Month				
Used any illegal drugs or prescription drugs without prescription during last month(%)	7.4	10.0	-2.6	0.144
Illegal drug usage at least weekly(%)	4.2	6.4	-2.2	0.114
One or more days drinking 5 or more drinks in a row, within a couple hours during last month(%)	16.1	15.3	0.9	0.742

SOURCE: Two-year follow-up survey of participants

NOTES: Statistical significance levels are indicated as follows: *** = $p < .01$; ** = $p < .05$; * = $p < .1$.
Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

† These items were all only asked for those participants who reported being in treatment at any point since RA; thus the comparisons are not experimental in nature and therefore may not provide an unbiased estimate of the effect of RExO.

Twelve percent of the sample reported their level of education as being “some college” or higher. This fraction was too small for meaningful subgroup analysis specific to this group; instead these individuals were consolidated with individuals who reported receipt of a HS diploma and analyzed as a single group.

Impacts on other Outcomes: Housing

Table B – 25: Housing Outcomes by Age

Current Housing Status	Program	Control	Difference	P-value
Under 27				
Lives in public housing(%)	5.5	5.8	-0.3	0.731
Lives in Section 8 housing(%)	2.8	2.7	0.1	0.813
Days at Current Residence	1107.5	1154.5	-47.1	0.431
Contributing to rent/cost(%)	62.1	61.5	0.6	0.632
Lives with partner(%)	27.2	24.6	2.6	0.089*
Lives with children(%)	21.2	22.4	-1.2	0.637
Lives with parents(%)	20.9	19.9	1.1	0.657
Lives with other family(%)	18.0	21.1	-3.1	0.055*
Lives with friends(%)	10.0	9.7	0.3	0.889
27 and Older				
Lives in public housing(%)	6.2	7.7	-1.5	0.420
Lives in Section 8 housing(%)	3.0	3.1	-0.1	0.920
Days at Current Residence	1055.4	1177.4	-122.0	0.410
Contributing to rent/cost(%)	55.2	52.2	3.1	0.404
Lives with partner(%)	27.8	22.8	5.0	0.120
Lives with children(%)	20.3	20.3	0.0	0.962
Lives with parents(%)	28.6	32.9	-4.3	0.220
Lives with other family(%)	32.6	32.3	0.4	0.931
Lives with friends(%)	9.6	6.4	3.3	0.122

SOURCE: Two-year follow-up survey of participants
NOTES: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1.

Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

Table B – 26: Housing Outcomes by Cohort

Current Housing Status	Program	Control	Difference	P-value
Enrolled Prior to October 1				
Lives in public housing(%)	5.8	6.7	-0.9	0.372
Lives in Section 8 housing(%)	2.9	2.5	0.4	0.516
Days at Current Residence	1141.5	1187.5	-46.0	0.462
Contributing to rent / cost(%)≠	62.3	59.0	3.4	0.074*
Lives with partner(%)	28.5	23.9	4.5	0.006***
Lives with children(%)	21.9	22.9	-1.0	0.828
Lives with parents(%)	23.7	23.6	0.0	0.886
Lives with other family(%)	20.9	23.5	-2.6	0.113
Lives with friends(%)	9.8	8.8	1.0	0.433
Enrolled after October 1				
Lives in public housing(%)	5.1	4.8	0.4	0.831
Lives in Section 8 housing(%)	2.6	3.4	-0.8	0.492
Days at Current Residence	985.6	1086.0	-100.4	0.422
Contributing to rent / cost(%)≠	56.7	61.8	-5.1	0.156
Lives with partner(%)	24.5	25.0	-0.6	0.794
Lives with children(%)	18.7	19.7	-1.0	0.648
Lives with parent(%)s	19.5	19.0	0.4	0.996
Lives with other family(%)	21.2	22.5	-1.2	0.670
Lives with friends(%)	10.2	9.9	0.3	0.858

SOURCE: Two-year follow-up survey of participants
NOTES: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1.
Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.
For outcomes marked with an inequality sign (≠), the difference between subgroup effects in a fully interacted model were statistically significant (Lowenstein, 2014).

Table B – 27: Housing Outcomes by Gender

Current Housing Status	Program	Control	Difference	P-value
Female				
Lives in public housing(%)	6.0	6.6	-0.6	0.635
Lives in Section 8 housing(%)	2.3	4.0	-1.7	0.203
Days at Current Residence≠	755.6	1211.3	-455.7	0.002***
Contributing to rent / cost(%)	63.9	61.1	2.8	0.428
Lives with partner(%)	24.1	19.6	4.5	0.159
Lives with children(%)	36.0	36.9	-0.9	0.953
Lives with parents(%)	17.3	18.0	-0.7	0.804
Lives with other family(%)	20.1	20.8	-0.7	0.695
Lives with friends(%)	11.4	9.7	1.7	0.569
Male				
Lives in public housing(%)	5.6	6.1	-0.5	0.600
Lives in Section 8 housing(%)	2.9	2.5	0.5	0.445
Days at Current Residence≠	1177.8	1142.3	35.5	0.972
Contributing to rent / cost(%)	60.1	59.4	0.6	0.600
Lives with partner(%)	28.2	25.4	2.8	0.068*
Lives with children(%)	17.2	18.1	-0.9	0.685
Lives with parents(%)	23.8	23.4	0.3	0.951
Lives with other family(%)	21.4	24.0	-2.6	0.120
Lives with friends(%)	9.6	9.0	0.6	0.546
Lives in public housing(%)	5.6	6.1	-0.5	0.600

SOURCE: Two-year follow-up survey of participants

NOTES: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1. Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

For outcomes marked with an inequality sign (≠), the difference between subgroup effects in a fully interacted model were statistically significant (Lowenstein, 2014).

Table B – 28: Housing Outcomes by Timing of Random Assignment

Current Housing Status	Program	Control	Difference	P-value
Early Assignment				
Lives in public housing(%)	5.5	5.9	-0.4	0.649
Lives in Section 8 housing(%)	2.7	2.9	-0.2	0.757
Days at Current Residence	1039.2	1172.8	-133.6	0.093*
Contributing to rent / cost(%)	60.3	59.7	0.6	0.632
Lives with partner(%)	27.0	24.7	2.3	0.141
Lives with children(%)	20.6	21.7	-1.2	0.648
Lives with parents(%)	21.7	22.2	-0.5	0.606
Lives with other family(%)	20.5	23.0	-2.5	0.122
Lives with friends(%)	9.8	8.6	1.2	0.339
Late Assignment				
Lives in public housing(%)	6.5	6.8	-0.4	0.755
Lives in Section 8 housing(%)	3.2	2.5	0.7	0.564
Days at Current Residence	1243.2	1103.2	140.0	0.838
Contributing to rent /cost(%)	64.0	61.8	2.2	0.518
Lives with partner(%)	28.5	23.9	4.6	0.127
Lives with children(%)	23.3	22.6	0.7	0.789
Lives with parents(%)	24.9	21.2	3.7	0.337
Lives with other family(%)	23.7	23.8	-0.1	0.933
Lives with friends(%)	11.3	11.3	0.0	0.961

SOURCE: Two-year follow-up survey of participants
NOTES: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1. Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

Table B – 29: Housing Outcome by Number of Prior Convictions

Current Housing Status	Program	Control	Difference	P-value
3 or Fewer				
Lives in public housing(%)	5.0	6.3	-1.3	0.249
Lives in Section 8 housing(%)	2.4	2.7	-0.3	0.722
Days at Current Residence	990.2	1137.4	-147.1	0.155
Contributing to rent / cost(%)	62.1	61.8	0.3	0.855
Lives with partner(%)	27.2	24.1	3.1	0.157
Lives with children(%)	22.7	22.9	-0.1	0.954
Lives with parents(%)	23.4	24.5	-1.1	0.542
Lives with other family(%)≠	23.9	24.8	-0.9	0.686
Lives with friends(%)	10.0	8.2	1.8	0.243
4 or More				
Lives in public housing(%)	5.5	5.6	-0.1	0.941
Lives in Section 8 housing(%)	2.9	2.7	0.2	0.740
Days at Current Residence	1140.1	1037.1	103.0	0.777
Contributing to rent / cost(%)	60.5	57.8	2.7	0.222
Lives with partner(%)	27.6	23.2	4.4	0.035**
Lives with children(%)	17.8	20.0	-2.1	0.452
Lives with parents(%)	21.4	19.6	1.8	0.486
Lives with other family(%)≠	15.4	20.7	-5.3	0.012**
Lives with friends(%)	11.0	9.7	1.3	0.430

SOURCE: Two-year follow-up survey of participants
NOTES: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1.
Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

For outcomes marked with an inequality sign (≠), the difference between subgroup effects in a fully interacted model were statistically significant (Lowenstein, 2014).

Table B – 30: Housing Outcomes by Education

Current Housing Status	Program	Control	Difference	P-value
No GED/HS Diploma				
Lives in public housing(%)	7.0	7.3	-0.3	0.733
Lives in Section 8 housing(%)	3.4	2.7	0.7	0.417
Days at Current Residence≠	1271.5	1340.6	-69.1	0.500
Contributing to rent / cost(%)	60.8	58.6	2.2	0.329
Lives with partner(%)	27.2	25.1	2.1	0.298
Lives with children(%)	22.5	23.9	-1.4	0.661
Lives with parents(%)	25.7	25.8	-0.1	0.801
Lives with other family(%)	23.6	25.2	-1.6	0.433
Lives with friend(%)s	9.5	6.9	2.5	0.124
GED				
Lives in public housing(%)	4.5	5.4	-1.0	0.554
Lives in Section 8 housing(%)	2.2	2.7	-0.5	0.779
Days at Current Residence≠	708.6	1098.5	-389.9	0.003***
Contributing to rent / cost(%)	55.7	54.1	1.6	0.552
Lives with partner(%)	28.5	24.0	4.5	0.113
Lives with children(%)	21.8	21.7	0.0	0.794
Lives with parents(%)	18.1	21.2	-3.0	0.200
Lives with other family(%)	21.7	22.0	-0.3	0.998
Lives with friends(%)	10.3	12.6	-2.4	0.343
HS Diploma				
Lives in public housing(%)	4.6	5.1	-0.5	0.690
Lives in Section 8 housing(%)	2.5	2.9	-0.4	0.627
Days at Current Residence≠	1171.1	975.7	195.3	0.211
Contributing to rent / cost(%)	65.1	66.0	-0.9	0.843
Lives with partner(%)	26.5	23.4	3.2	0.211
Lives with children(%)	18.2	19.8	-1.6	0.546
Lives with parents(%)	21.7	18.7	3.0	0.259
Lives with other family(%)	16.5	21.6	-5.1	0.034**
Lives with friends(%)	10.3	9.0	1.2	0.549

SOURCE: Two-year follow-up survey of participants

NOTES: Statistical significance levels are indicated as follows: *** = p < .01; ** = p < .05; * = p < .1. Results in this table are regression-adjusted for pre-random assignment characteristics. We describe this process in detail in the Technical Appendix.

For outcomes marked with an inequality sign (≠), the difference between subgroup effects in a fully interacted model were statistically significant (Lowenstein, 2014). Twelve percent of the sample reported their level of education as being “some college” or higher. This fraction was too small for meaningful subgroup analysis specific to this group; instead these individuals were consolidated with individuals who reported receipt of a HS diploma and analyzed as a single group.

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APPENDIX C: REFERENCES

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