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Evaluation of Seven Second Chance Act Adult Demonstration Programs: Impact Findings at 30 Months

Final 30-Month Impact Report
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SPR SOCIAL POLICY RESEARCH
A S S O C I A T E S

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1. Allegheny County (Pennsylvania) Department of Human Services
2. Kentucky Department of Corrections
3. Marion County (Oregon) Sheriff's Office
4. Oklahoma Department of Corrections
5. San Francisco (California) Department of Public Health
6. San Mateo County (California) Division of Health and Recovery Services
7. South Dakota Department of Corrections

Each agency complied with the requirements for the study, and their staff graciously answered our many questions. We also acknowledge help from state and local agencies who provided arrest, conviction, and jail and prison incarceration data for the study sample.

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All these individuals, and others, were instrumental in bringing this report to conclusion. Remaining deficiencies are the authors' responsibility alone.

Table of Contents

Abstract

This report describes the impacts of re-entry programs developed by seven grantees that were awarded funds under the Second Chance Act (SCA) Adult Demonstration Program to reduce recidivism by addressing the challenges faced by adults returning to their communities after incarceration. In estimating impacts, the evaluation used a randomized controlled trial, whereby 966 individuals eligible for SCA were randomly assigned to either a program group whose members could participate in individualized SCA services, or a control group whose members could receive all re-entry services otherwise available but not individualized SCA services. Each study participant was measured on a range of outcomes at 18 months after random assignment and again approximately one year later. An earlier report described impacts measured at 18 months. This report extends those results by describing the longer-term impacts and analyzing program costs.

SCA grant funds were important for expanding the grantees' capacity for re-entry services. All grantees used their SCA funds to provide services after individuals were released from incarceration, and some also used their funds to enhance pre-release services. Case management was a common service element; case managers were either parole officers who had reduced caseloads or staff members from social services agencies or community-based organizations. Other services included employment assistance, substance abuse treatment and cognitive behavioral therapy. The earlier report showed that the program group was significantly more likely than the control group to have a re-entry plan and a case manager whom they trusted. They were also more likely to receive job search assistance, cognitive behavioral therapy and other needed services. At the end of 18 months, those in the program group nonetheless reported having just as many unmet service needs as those in the control group. The net service cost — the marginal cost of serving an individual SCA enrollee — was approximately $2,800.

In the 30 months following random assignment, those in the program group were no less likely to be re-arrested, reconvicted or re-incarcerated; their time to re-arrest or re-incarceration was no shorter; and they did not have fewer total days incarcerated (including time in both prisons and jails). They had a slightly greater total number of re-arrests and reconvictions, possibly because enhanced case management might have increased the likelihood of catching new offenses.

Those in the program group had better longer-term employment and earnings. In the second year after random assignment, the SCA program group reported consistently higher employment rates and, towards the end of the observation period, earned 83 percent more than the control group.

There are several possible reasons why being assigned to the program group improved employment and earnings but did not reduce recidivism. First, the service differential between the program and control groups was largest for employment-related assistance, and (although statistically significant) more modest for other services. The modest service differential suggests that control group members were often able to access similar services elsewhere and highlights the fact that this study measures the impacts of the grantees' services relative to the alternative services available and not relative to no services. Furthermore, SCA funds did not seem adequate to meet the many complex needs of those returning from incarceration. Finally, there were limitations to the grantees' service models; most emphasized case management, and prior research has suggested that casework alone is not very successful as a re-entry approach.

The grantees in this study were among the first to receive SCA funding. Grant requirements were substantially tightened for grantees that received funding in subsequent rounds of competition. Further research is needed to determine the impacts of these enhanced requirements.

List of Acronyms

ABE = Adult basic education

ASI = Addiction Severity Index

BIF = Baseline Information Form

BJA = Bureau of Justice Assistance, U.S. Department of Justice

CAIS = Correctional Assessment and Intervention System

DOC = Department of corrections

DOJ = U.S. Department of Justice

FY = Fiscal year

GED = General Educational Development

HHS = U.S. Department of Health and Human Services

HLM = Hierarchical linear modeling

ICC = Intraclass correlation coefficient

ID = Identification number

IRB = Institutional Review Board

ITT = Intent to treat

LS/CMI = Level of Service/Case Management Inventory

LSI-R = Level of Service Inventory-Revised

MDD = Minimum detectable difference

MIS = Management information system

NCIC = National Crime Information Center

NDNH = National Directory of New Hires

NIJ = National Institute of Justice, U.S. Department of Justice

OCSE = Office of Child Support Enforcement, U.S. Department of Health and Human Services

OLS = Ordinary least squares

PMT = Performance Management Tool

PO = Probation or parole officer

RA = Random assignment

RCT = Randomized controlled trial

RNR = Risk-need-responsivity

SCA = Second Chance Act of 2007

ili: SPR

SPR = Social Policy Research Associates

UI = Unemployment Insurance

URICA = University of Rhode Island Change Assessment Scale

Executive Summary

The Second Chance Act (SCA), signed into law in 2008 with widespread bipartisan support, authorizes grants to government agencies and nonprofit organizations to reduce the recidivism of individuals being released from prisons and jails. Thus far, the U.S. Department of Justice Bureau of Justice Assistance (BJA) has awarded hundreds of grants under various categories of competition to state, local and tribal governments to develop or enhance re-entry programs serving adults. This report describes the impacts of programs developed by seven agencies that were awarded grants through the first round of funding under the SCA Adult Demonstration Program. The Adult Demonstration Program represents only one of a number of separate grant programs authorized through SCA. Because these seven grantees were purposively selected and were drawn from only one grant program, this study's findings cannot be generalized to other grantees that received Adult Demonstration funds or to SCA as a whole.

About the Evaluation

The evaluation was conducted by Social Policy Research Associates (SPR) and its partners MDRC and NORC at the University of Chicago. It uses a random assignment (RA) design and administrative and survey data to study the impacts of these seven grantees' programs. For the impact study, 966 individuals eligible for SCA were assigned to either:

- A *program group* whose members could receive individualized SCA services, or

- A *control group* whose members could receive all services otherwise available but could not receive individualized SCA services.

RA commenced in the last week of 2011 and continued through March 2013. Of the 966 study participants, 63 percent were randomly assigned to the program group, and 37 percent were assigned to the control group.

Data on study participants are from a number of sources.

- *Baseline Information Forms (BIFs)*. Just before RA, all study participants completed a one-page BIF; this form asked about background and criminal history.

- *Data extracted from each grantee's management information system (MIS).* The grantees provided the study team with data showing which pre- and post-release services program group members received as part of their participation in SCA.

- *Administrative data from state and local criminal justice agencies*. State and local criminal justice agencies provided data on arrests, convictions, and prison and jail incarcerations for the 10 years prior to each individual's RA date and the 30 months following RA.

- *Administrative data from the National Directory of New Hires (NDNH)*. NDNH, built up from states' quarterly Unemployment Insurance (UI) program wage and claimant files, federal employment files and the directory of new hires, provides information on study participants' employment and earnings.

- *A follow-up survey*. The research team administered a follow-up survey to cover the 18 months following RA. All study participants were in the survey sampling frame, and an interview was completed with 82.3 percent of them (82.2 percent of the program group and 82.6 percent of the control group).

The study team also learned about the grantees' programs by obtaining copies of the quarterly financial status reports and quarterly program reports that grantees submitted as part of their grant requirements, and through site visits conducted to each program to learn about SCA implementation.

Using the survey and administrative data on study participants, the study team estimated the impacts of being assigned to the SCA program for the full sample as well as for five subgroups defined by gender, age (under 30 years versus 30 years and older), risk of recidivism (lower versus higher risk), length of time from RA to release from incarceration (RA was more than 30 days prior to release versus within 30 days of release or after release), and type of grantee (a criminal justice agency versus a social service or health agency).

In estimating impacts, the study used an intent-to-treat framework by comparing the outcomes of those assigned to the SCA program group to the outcomes of those assigned to the control group. Some program group members might not have received all the SCA services intended for them and, conversely, control group members could have received very similar services from sources other than SCA. This study measures the impacts of assignment to the program group relative to these alternative services and not relative to no services. Impacts measured 18 months after RA, described in a separate report, cover program effects on services received, recidivism, employment and earnings, and self-reported health status and substance abuse, among other outcomes. The present report describes program impacts on recidivism and employment outcomes measured approximately 30 months after RA, based on administrative data.

At least two of the seven grantees used a portion of SCA funds for general system improvements. The control group members could have benefited from these improvements, just as any other individual returning from incarceration. Therefore, this study assesses the impacts of the personalized services that program group members received as part of SCA and not of these system improvements.

About the Grantees and Their Programs

According to the SCA grant solicitation, grantees were expected to serve adults with a moderate to high risk of recidivism, develop re-entry plans for them based on validated risk and needs assessments, and provide supervision and comprehensive services that should include, as needed, educational, literacy, vocational and job placement services; substance abuse treatment; housing assistance; and mental and physical health care.

The grantees were a diverse group. Three of the seven grantees were state departments of corrections (DOCs), one was a sheriff's office, and three were local government social services or health agencies. Some recruited SCA participants exclusively from prisons, others exclusively from jails, and others from both prisons and jails. Some grantees served only females, some served only males, and others served both females and males.

SCA funds enhanced re-entry services. The grantees reported that their SCA grants helped them fill gaps in their existing re-entry services and expand service capacity. Partly through their grants, the grantees improved their partnerships with other community agencies and strengthened the connection between pre- and post-release services.

The emphasis on pre-release services was greater in some sites than others. Three grantees delivered fairly extensive pre-release services as part of their SCA programs and therefore required participants to have an extended period of incarceration remaining before SCA enrollment. Others relied on existing programming in institutions for pre-release services and focused on using their SCA funds for transition planning and post-release services; they generally enrolled participants in SCA nearer to release and, sometimes, after release. Because of these differences, the adequacy of the continuum of care from incarceration through release was better developed in some sites than others. Overall, approximately 55 percent of participants were enrolled in SCA three or more months prior to release, 28 percent within three months of release, and 17 percent after release. According to the grantees' MIS data, 36 percent of those randomly assigned to the program group received both pre-release and post-release SCA services, 40 percent received only post-release services, and just over 20 percent received only pre-release services.

Case management was a key service. Case management was a key feature of all the grantees' programs except one. Across grantees, the goal of case management was to help prevent recidivism by providing individualized support and coordinating access to services based on identified needs and risk factors. These case managers were either probation or parole officers (POs) who commonly had reduced caseloads and extra training provided through the grant, or came from social services agencies and had more traditional case management backgrounds (e.g., social workers, counselors). In the latter case, SCA participants might also have been required to report to a PO after release, but this individual was not the SCA case manager.

Grantees provided other services, either directly or through referral. The grantees offered education and training, employment assistance, substance abuse treatment, mental health services, cognitive behavioral therapy, prosocial services, housing assistance and supportive services. The grantees provided some of these services directly. Other services were provided by partners, either on a fee-for-service basis or through unfunded referrals. All grantees used all three methods of providing services, but they differed in which services they provided directly versus through funded or unfunded referral.

Grantees that were criminal justice agencies faced very different challenges and opportunities than did those that were social services agencies. Grantees that were DOCs generally had POs carry out SCA's post-release case management functions. These individuals had the challenge of balancing supervisory and rehabilitative functions and were sometimes slower to gain the trust of SCA participants. However, they were better able to coordinate with jail and prison staff, and having a PO as a case manager was beneficial for ensuring that participants kept their case management appointments. Social services agencies, by contrast, had wider networks of social service agencies with whom they could partner but weaker connections with prisons and jails. Further, participant retention could be a challenge for them, in that participants who were randomly assigned to the program group sometimes did not take advantage of the case management offered.

SCA funds led to important system changes, but fidelity to the re-entry model envisioned by BJA was incomplete. The contrast between services under SCA and what came before was readily apparent — more attention was given to re-entry planning, additional funds were available for services, partnerships were strengthened, and, especially after release from incarceration, participants could access case management that was expected to be much more robust than what was normally available. Further, for DOCs, the SCA grant continued a shift away from strictly an enforcement mindset toward a rehabilitative philosophy. However, full implementation of the service model envisioned by BJA also fell short in some important ways. In particular, the continuum of care from incarceration to release was not well developed in some sites. Furthermore, none of the grantees had adequate funding to directly deliver all services that participants needed. Therefore, they all relied heavily on unfunded referrals to provide many services. Where unfunded referrals were used, coordination with the grantee's program was typically weak and case managers could not always track whether participants received the services to which they were being referred. Finally, participant retention could be a problem, especially for grantees that were social services agencies. For these reasons, it would be hard to argue that every SCA participant received all the services needed.

Summary of Impact Findings

The 18-month impact report demonstrated that those randomly assigned to the program group were significantly more likely to receive a wide range of re-entry services. They were more likely to report that they received help with re-entry and to have individual case plans. They were also more likely to receive cognitive behavioral therapy, help with looking for a job, substance abuse treatment, housing assistance and mentoring. At the end of 18 months, those in the program group nonetheless reported having just as many unmet service needs as those in the control group; more than half of both groups said that they wanted additional housing assistance, job placement assistance, job training, health services, and educational services.

The earlier report also examined impacts on recidivism and other outcomes measured 18 months after RA. At that point, those in the program group did not have less involvement with the criminal justice system, regardless of whether recidivism was measured using survey or administrative data. There were also no impacts on employment and earnings, health-related outcomes, or self-reported substance abuse.

The present report extends these findings by looking at impacts on recidivism and employment outcomes measured for an additional year, and it describes the results of a cost analysis.

As of 30 months after RA, those in the program group did not show improved desistance. Those in the program group were no less likely than those in the control group to be re-arrested, reconvicted, or re-incarcerated; their time to re-arrest or re-incarceration was no shorter; and they did not have fewer total days incarcerated (including time in both prisons and jails). Program group members were somewhat more likely to have more arrests and convictions. This greater frequency may have come about because enhanced case management for those in the program group could have increased the likelihood of catching new offenses when they occurred.

Toward the end of the follow-up period, the program group had better employment outcomes. In the seventh and eighth follow-up quarters (approximately 22 to 27 months after RA), members of the program group earned, on average, approximately $1,800 more than members of the control group, which represents more than a 70 percent improvement over the control group's earnings. This difference comes about because those in the program group were more likely to be employed and, among those employed, they earned significantly more.

At best, there are modest differences across subgroups. We compared the estimated impacts across different subgroups — males versus females, those younger versus those older, those at lower versus higher risk of recidivism, those enrolled well before release versus those enrolled near or after release, and those served by corrections agencies versus social services agencies. There appear to be only modest differences in program impacts across these groups and no consistent patterns.

The study's major impact findings are robust to alternative model specifications. We estimated impacts as a simple difference in means between the program and control groups and using more complex statistical methods. The findings summarized above hold up in these alternative model specifications.

Summary of the Cost Analysis

Grantees received grant awards ranging from $1.5 million to $3.25 million. The average unit cost — that is, the average cost of providing individual services — was highest for inpatient substance abuse treatment, at more than $7,000. Case management also had a high unit cost, with an average of $2,600, though this was highly variable across the grantees. Net service costs — that is, the difference in costs for serving an SCA participant compared with a person in the control group — was approximately $2,800.

Why Were There Impacts on Employment but not Recidivism?

SCA represented a substantial infusion of funds for these seven grantees, and this study has demonstrated that this led to a significant increase in service receipt for the program group and improved their employment and earnings. Why were there impacts on employment outcomes but not on recidivism? A number of general reasons can be suggested (although not every reason applies to each grantee).

1. ***The service differential was largest for employment-related assistance and more modest for other services.*** The percentage of the program group receiving employment-related assistance was nearly 20 points higher than the percentage of the control group. Service differentials for other services were generally statistically significant, but more modest in size. This suggests that control group members were often able to access similar services elsewhere. Even if the services were effective, the gap in service receipt between the groups might not be large enough across the board to translate into differences in recidivism.

2. ***Given available SCA funding, there were limitations to what the grantees could do.*** Those returning from incarceration face challenges to re-entry that are many and complex. The grantees' services could not help participants fully overcome these challenges. Due to their resource constraints, all of the grantees relied heavily on informal referrals to provide many services. For services that were not SCA funded, program group members did not have priority access over anyone else who sought services. As a consequence, SCA participants reported many unmet service needs 18 months after RA.

3. ***There were inherent limitations to the programs that grantees developed.*** Although the grantees used evidence on what works in developing their programs, there were limitations to their service models.

a. *Case management, even with reduced caseloads, has not been demonstrated to be effective*. All but one of the grantees emphasized case management as part of their SCA programs. For several grantees, this case management was provided by traditional POs who were given reduced caseloads. However, in their review of correctional rehabilitation approaches, Cullen and Gendreau (2000) cited evidence that "casework" has not been demonstrated to be very successful as a re-entry approach. Others have concluded that giving POs reduced caseloads does not by itself appear to reduce recidivism, and the increased supervision can increase revocation rates (Petersilia 1999; Jalbert et al. 2011). A study of re-entry programs that emphasized case management provided by community-based organizations also found no impacts on recidivism (Wiegand et al. 2015).

b. *It was hard to ensure that participants got the services they needed.* Many services were provided through unfunded referrals, which had some clear advantages: this strategy conserved limited project resources and enabled grantees to draw on a wide network of community agencies experienced at addressing the many complex needs of those returning from incarceration. However, grantees did not always ensure that participants sought out the services to which they were referred, and the quality of services provided by loosely connected partners can be uncertain. For some grantees, participant retention seemed to be a problem.

4. ***Developing strong programs based on the risk-need-responsivity (RNR) framework is difficult****.* Programs that address criminogenic needs have been shown to be effective in reducing recidivism (e.g., Latessa and Lowenkamp 2006). However, Bonta and Andrews (2007) argued that taking the RNR framework out of a tightly controlled setting and trying to widely use its principles in the real world tends to make the model much less effective. Furthermore, in their systematic review of the literature, Weisburd et al. (2017) noted that, while we generally know what works in reducing recidivism, the specific guidance that practitioners need to convert principles into practice is often lacking. In short, implementing evidence-based practices and taking them to scale is not easy.

Conclusions

SCA grant funds helped grantees enhance their existing programs and strengthen their partnerships. Unquestionably, the funds were needed and they expanded capacity, which enabled a greater level of support to more individuals than would have been possible without the grant. Absence of evidence that these funds reduced recidivism to some degree highlights a well-known limitation of impact studies: if there are alternative sources of funds for services, then each source is important in expanding a community's capacity but no one source is singularly impactful when compared with all the others (Heckman et al. 2000). This study does not show that program group services were not effective, but rather that they were about as effective as the services received by the control group.

Nevertheless, improvements to the service models that the grantees developed might have led to better outcomes. Resources to fill all service needs remained well short of need and, partly for this reason but also for others, not all participants appeared to receive a continuum of care or adequate dosage of services, and grantees often had difficulty ensuring that participants received all the services they were assigned, particularly if the services were delivered by partners. Further, case management as a focal approach to re-entry has not yet been found to be effective, and it has challenges regardless of whether POs serve as the case managers or social services agencies do.

Some important improvements are already underway. Even before these impact findings were made available, the Department of Justice (DOJ) learned from the experiences of the grantees in this study — as well as others that received early funding — and endeavored to improve program models for grantees that received subsequent waves of funding under the Adult Demonstration program (now called Smart Reentry). For example:

- To ensure adherence to evidence-based practices and the provision of meaningful re-entry services, grantees are required to complete a planning process before being approved for implementation funds. During this time, they are to work with a technical assistance provider to improve their program models.

- Grantees must engage with participants prior to release.

- Grantees are required to establish a memorandum of understanding with providers to ensure that there is a mechanism for follow-up when referrals are made.

- Grantees must ensure adequate dosage of cognitive-based interventions.

- Grantees are expected to engage a research partner to help develop actionable research.

With these modifications to grant requirements, this next generation of Smart Reentry holds promise for yielding more meaningful impacts.

It also seems clear that we need much more research to enhance our understanding about ways of designing effective re-entry programs. As Rhine et al. (2006) have pointed out, successful implementation of evidence-based practices in concrete settings — critical if re-entry programs are to be effective in context — is challenging. Although there seems to be broad consensus about the general principles of effective strategies, a more fine-grained understanding of what works best for whom in what context still seems far off. Only improved data systems and an aggressive research agenda can truly advance our understanding of how best to put evidence-based principles into effective practice.

I. Introduction

This report presents the longer-term impacts from a random assignment (RA) study of seven Second Chance Act (SCA) Adult Demonstration programs. It extends the short-term findings presented in an earlier report, which described impacts on services received, recidivism, employment and earnings, family stability and other outcomes measured 18 months after participants enrolled in the study (D'Amico et al. 2017). The present report describes impacts on recidivism, employment and earnings measured for up to an additional year, and it describes how grantees used their grant funds.

This chapter provides background to establish the context for the study, presents the evaluation design, and describes study participants. The next chapter describes the grantees' programs and summarizes the short-term (18-month) impact findings, which were detailed in earlier reports. Subsequent chapters describe the costs that grantees expended in running their programs and present the longer-term impact results.

Background

At the end of 2015, approximately 6.74 million individuals were under some form of supervision by the U.S. adult correctional system, which represents about 1 in 37 adults in the U.S. (Kaeble and Glaze 2016). The total figure includes more than 1.5 million adults held in state or federal prisons, about 728,000 confined in local jails, and more than 4.65 million under community supervision.[1] Although the total figure represents a substantial decline since a peak of 7.34 million in 2007, about three-and-a-half times as many adults were under supervision in 2015 as in 1980, when national estimates first became available (Glaze 2010). Furthermore, flows are substantial. In 2015, there were more than 600,000 admittances to prisons (Carson and Anderson 2016), 10.9 million admittances to jails (Minton and Zeng 2016), and approximately 2.4 million admittances to community supervision (Kaeble and Bonczar 2016). These figures suggest that the burden on the nation's correctional system is extraordinary.

Adding to the challenge, those released from incarceration face substantial obstacles to successful re-entry. More than 40 percent of prison and jail inmates lack a high school degree or its equivalent (Harlow 2003), and many report problems with substance abuse and mental health or physical impairments (Bronson and Berzofsky 2017; Hammett et al. 2001; Mumola and Karberg 2006; Petersilia 2003). Upon release, they have difficulty finding jobs for these reasons and because of the stigma that comes with their status as former offenders (Holzer et al. 2004; Pager 2003; Raphael 2014). Moreover, the formerly incarcerated tend to be released

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[1]   The sum of the components exceeds the total because some individuals had multiple correctional statuses (see Kaeble and Glaze 2016).

into a relatively small number of urban neighborhoods that are characterized by high rates of poverty and other social problems (La Vigne and Kachnowski 2003; Travis et al. 2001).

Not surprisingly given these challenges, about two-thirds of those released from state prisons are rearrested, and nearly half are returned to prison within three years of release, either for violations of parole conditions or new crimes (Durose et al. 2014; Pew Center on the States 2011). This cycle of imprisonment and re-entry has tremendous personal consequences for the men and women who churn in and out of the criminal justice system as well as costs that extend to many spheres of public policy and community life. High rates of recidivism impose a financial drain on federal and state governments, impair public safety, strain community resources and impose hardship on the families of those who are imprisoned. Reducing recidivism is therefore critical, both to reduce corrections costs and to address the interrelated problems faced by low-income families and vulnerable communities.

In recognition of the gravity of the situation and the urgency of the need, SCA was signed into law on April 9, 2008, with widespread bipartisan support. Since then, more than $475 million has been authorized under various categories of competition for grants and technical assistance to state, local, and tribal government agencies and community organizations to help those returning from incarceration.[2]

One category of grant awards consists of Adult Reentry Demonstrations, from which the grantees included in this study are drawn. The U.S. Department of Justice (DOJ) Bureau of Justice Assistance (BJA) has made annual awards in this category since fiscal year (FY) 2009, with more than 150 grants thus far awarded to state and local governments and federally recognized Indian tribes for planning and implementing strategies to address the challenges faced by adults returning to their communities after incarceration.[3] According to the grant solicitation, grantees are expected to use validated and dynamic risk and needs assessments to deliver evidence-based services. Far removed from a time when it seemed that "nothing works," there is now considerable evidence that well-designed re-entry programs can make a difference.[4] Grantees are expected to draw on this evidence in designing their programs.

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[2] These figures come from the website of the Office of Justice Programs, Bureau of Justice Assistance, accessed at https://www.bja.gov/ProgramDetails.aspx?Program_ID=90 on October 23, 2017.

[3] These figures come from a fact sheet prepared by the Conference of State Governments Justice Center, accessed at https://csgjusticecenter.org/wp-content/uploads/2014/08/SCA_Fact_Sheet.pdf on March 9, 2018.

[4] See the reviews by Seiter and Kadela (2003), Drake et al. (2009), MacKenzie (2008), and Cullen and Gendreau (2000), among others. While lauding the literature for its insights, Petersilia (2004) has noted the paucity of impact studies using rigorous methods.

About the Evaluation

This evaluation uses an RA design and administrative and survey data to study seven SCA Adult Demonstration grantees that were selected by BJA to participate in the research.

Design and Implementation of the Study

DOJ's National Institute of Justice (NIJ) awarded a grant to Social Policy Research Associates (SPR) and its partners, MDRC and NORC, to evaluate seven grantees awarded FY 2009 SCA Adult Demonstration funding. These grantees were awarded their funds in late summer 2009 and began enrolling participants several months after that. Some of the grantees are state departments of corrections (DOCs); others are local government agencies, including a sheriff's office and public health and social service agencies.

The evaluation was designed to:

1. Study the implementation of the seven programs to learn about their service designs and the challenges they encountered.

2. Estimate the impacts of program services on participants' recidivism, employment and other outcomes, as well as calculate program costs.

For the implementation study, the research team reviewed documents and conducted site visits to each of the programs. During the site visits, the team interviewed program administrators and line staff and conducted focus groups with program participants. Results from the implementation study are described in detail in a separate report (D'Amico et al. 2013) and summarized in the next chapter.

In estimating impacts, 966 individuals determined eligible for SCA were randomly assigned to either:

- A *program group* whose members could receive individualized SCA services, or

- A *control group* whose members could receive all services normally available but could not receive individualized SCA services.

> **Grantees Selected by BJA for the Study**
>
> **State Agencies**
>
> 1. Kentucky Department of Corrections [Kentucky]
> 2. Oklahoma Department of Corrections [Oklahoma]
> 3. South Dakota Department of Corrections [South Dakota]
>
> **Local Agencies**
>
> 4. Allegheny County (Pennsylvania) Department of Human Services [Allegheny County]
> 5. Marion County (Oregon) Sheriff's Office [Marion County]
> 6. San Francisco (California) Department of Public Health [San Francisco]
> 7. San Mateo County (California) Division of Health and Recovery Services [San Mateo County]
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> *Note:* The shorthand names of the grantees used in this report are shown in brackets.

RA for the impact study commenced in the last week of 2011 (or approximately two years after the grantees began operating their SCA programs) and continued through March 2013. The

exact date when RA started varied by grantee and was contingent on each grantee's readiness to participate in the study; one grantee started on December 23, 2011, and the last grantee started on May 8, 2012. RA ceased when each grantee reached its enrollment target or by the end of March 2013, whichever occurred sooner. All the grantees conducted RA for at least eight months, and during this time 966 individuals were randomly assigned. The timeline for the grantees and the study is displayed schematically in Exhibit I-1.

Exhibit I-1: Timeline for SCA and Study Implementation for the Study's Grantees
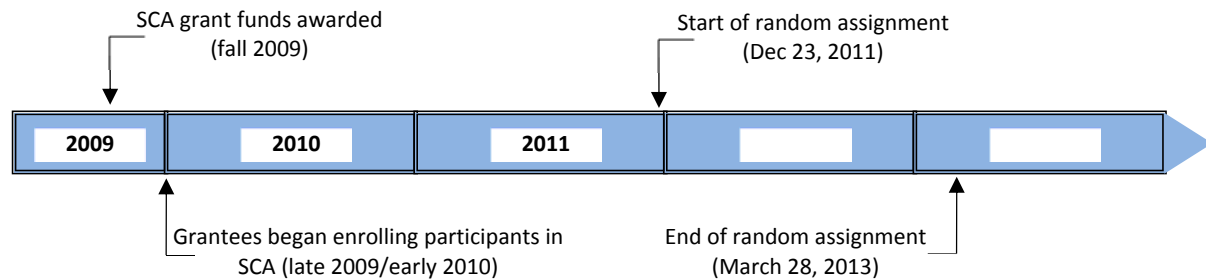
In addition to varying the date when RA started and stopped, the study team adapted RA procedures for each grantee in other ways so that the study would assess SCA as it was intended to operate in each site. For example, given their different service designs and grant amounts, each grantee was assigned a different enrollment target. Further, the rate of RA to the program group varied. A condition of obtaining approval for the research design from the study's Institutional Review Board (IRB) was that no grantee could have unfilled SCA program slots. Given the expected sizes of their eligible populations in relation to their service capacities, most grantees could assign approximately 60 percent of those eligible to the program group and 40 percent to the control group. However, for two grantees, the study team randomly assigned approximately 75 percent of the grantees' applicants to the program group. Numbers that each grantee enrolled in the program and control groups are shown in Exhibit I-2. Appendix A provides more information about the mechanics of RA.

The study team also let each grantee determine when in the transition from incarceration to release RA was to occur. Some grantees enrolled individuals in SCA six months or more before an individual's expected release from incarceration and provided individualized pre-release services during that time. By contrast, other grantees generally relied on the institutions' pre-existing pre-release services and began individualized SCA services only as the release date neared or after release. Regardless, RA always occurred just before an individual's intensive and personalized involvement with SCA was expected to occur. Given the grantees' varying program designs and enrollment strategies, some study participants were randomly assigned well before they were released, others near the date of release, and still others after release.

Finally, the study allowed each grantee to establish its own criteria for determining who was eligible for SCA and what services would be provided.

Exhibit I-2: Number of Study Participants, by Grantee and Group

	Total	Program Group	Control Group
Allegheny County	133	105	28
Kentucky	187	113	74
Marion County	119	85	34
Oklahoma	134	74	60
San Francisco	77	45	32
San Mateo County	114	64	50
South Dakota	202	120	82
TOTAL	966	606	360

Source: RA system.

Data Collection for the Impact and Cost Study

The evaluation team collected data for the implementation study through multiday visits to each grantee. For the impact and cost study, the team collected data from these additional sources:

1. *Baseline Information Forms (BIFs)*. All study participants completed a one-page BIF just before RA, which asked about background and criminal history (e.g., gender, age, race and ethnicity, level of education, employment history, type of crime for which the most recent incarceration occurred, length of sentence). Additionally, the program applicant was asked to provide identifying information, such as a social security number and prison or jail identification (ID) number.

2. *Financial reports.* As a condition of their grants, grantees were to submit quarterly financial reports to BJA. We have copies of these reports and used them to calculate program expenditures. We supplemented this source with additional data that we collected from the grantees on the costs of providing various services.[5]

3. *Data extracted from each grantee's management information system (MIS).* We asked each grantee to provide us with data extracted from its MIS on the services that SCA program participants received. The data elements we requested represent a subset of those that grantees needed for the Performance Management Tool (PMT) to meet the quarterly aggregate reporting requirements of their grants as stipulated by BJA. These elements included each participant's date of SCA enrollment and date of last service, as well as indicators for which pre- and post-release services SCA participants received, including substance abuse treatment, mental health services, and employment services.

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[5] Additional details about cost data are described in Chapter III.

These data were only available for SCA participants and not those assigned to the control group.

4. *Administrative data from state and local criminal justice agencies*. We forwarded the identifying information available from the BIFs to state and local criminal justice agencies for matching with their records. These agencies included DOCs, departments of justice, offices of the court, sheriffs' offices and others. Depending on each agency's data system, matching was conducted using criminal justice IDs, social security numbers, names and birthdates, or combinations of these. We requested participant data from each agency twice: once for data covering a period beginning at least 10 years prior to the start of RA through 18 months after RA, and again covering the period through 30 months after RA. Using these data, we created three sets of measures of arrests, convictions and incarcerations (both prison and jail), with each set benchmarked to the RA date. One set of measures covered the 10 years prior to each individual's RA date and was used to describe the sample's criminal history and create subgroups used in the analysis. Another set covered the period from the RA date through 18 months after RA; this set constitutes key outcomes used in the 18-month impact report. A final set covered the period through 30 months after RA, and this is the focus of the present report.

5. *Administrative data from the National Directory of New Hires (NDNH)*. NDNH is maintained by the Office of Child Support Enforcement (OCSE) of the U.S. Department of Health and Human Services (HHS). It is built up from states' quarterly Unemployment Insurance (UI) program wage and claimant files, federal employment files, and Directory of New Hires, and it includes information on covered workers' dates of hire, quarterly employment and earnings, and UI claimant benefit amounts. The database is maintained to assist states in enforcing child-support obligations for noncustodial parents, but can be used for research purposes under strictly defined circumstances. Through an agreement between HHS and DOJ, the study team gained access to NDNH data for study participants in order to calculate employment and earnings for a period following RA.

6. *A follow-up survey*. The research team administered a follow-up survey to study participants covering the 18 months following RA. All study participants were included in the sampling frame, and 82.3 percent of them completed surveys (82.2 percent of the program group and 82.6 percent of the control group). The survey covered pre-RA characteristics (e.g., demographics, criminal history); services received since RA, whether from the SCA program or other sources; and outcomes. Outcomes include recidivism (arrests, convictions and incarcerations), employment (whether worked since RA, whether currently employed, and wages and salary), health status, housing status, family status, substance abuse, fulfillment of child-support obligations, and other topics. Appendix B of the 18-month impact report (D'Amico et al. 2017) provides more information about survey administration and analysis.

Having multiple data sources allows us to take advantage of the best characteristics of each. For example, administrative data provide an objective source for measuring key outcomes and are not subject to recall error or respondent reporting bias. On the other hand, survey data cover a much broader set of outcomes and provide greater depth about each topic. Administrative and survey data were used together in the 18-month impact report, which was released previously; because this report used both administrative and survey data, it provided an opportunity to corroborate key findings using independent sources of evidence. Survey data were not collected beyond 18 months, so the analysis of longer-term impacts described in the present report only uses administrative data.

Estimating Program Impacts

This report presents the estimated impacts of the grantees' programs on recidivism, employment and earnings. We measure impacts on recidivism for the period covering 30 months after each individual's RA date and on employment and earnings for approximately 22 to 27 months following RA.

General Approach

The study uses an intent-to-treat (ITT) framework in that we compare the outcomes of those randomly assigned to the SCA program group to the outcomes of those assigned to the control group. RA is considered the "gold standard" for estimating program impacts because it is the best way of ensuring that there are no pre-existing differences between the program group and those to whom they are being compared. Through RA, we can assume that program group members, on average, are like those in the control group on observable and unobservable characteristics — for example, they are not more motivated than those in the control group, and the two groups will have similar criminal histories and criminogenic needs. Because of the pre-RA equivalence between the groups, estimation methods can be relatively simple: we can attribute the mean difference in the outcomes between the groups to the effects of being assigned to the program group.[6]

We built off this simple approach in several ways. First, we weighted the sample to account for the fact that the probability of assignment to the program group was not constant across the grantees. The weight we used is the inverse of the probability that an individual was assigned to his or her observed study group. When using survey data, we also weighted to account for potential nonresponse bias.

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[6] By contrast, alternative approaches under the broad category of quasi-experimental methods often use statistical adjustments to define a comparison group to which the outcomes of the program group can be compared. The disadvantage to this is that one cannot confidently rule out the possibility that any observed difference in outcomes between groups is due to unobserved pre-existing differences rather than an effect of the intervention.

ili: SPR

Because RA effectively neutralizes the impact of pre-existing characteristics, we calculated impacts as the simple difference in means between the program and control groups. However, we calculated whether these differences are statistically significant by using ordinary least squares (OLS) regression models (for outcomes that are continuous variables) or logit models (for outcomes that are dichotomous) that take into account individual characteristics before RA. Observed mean differences in outcomes provide an unbiased estimate of the treatment effect, but regression adjustment improves statistical precision by reducing the variance of the estimates. We used this approach predominantly. However, some outcomes (e.g., date of first re-arrest following RA) are based on elapsed time to an event. For outcomes of this type, we used survival analysis, which is more appropriate for analyzing duration data.

We also conducted additional analyses that are refinements to the general approach to test the sensitivity of our results to alternative model specifications. For example, we estimated hierarchical models that took into account the fact that study participants were nested within grantees. These models yielded very similar conclusions to those from the simpler models just described; to avoid needless complexity, the simpler models are presented in this report. However, results from the additional models are described in Appendix B.

Subgroup Analysis

We estimated impacts for the full sample, but also separately for subgroups that were deemed *a priori* to be of substantive or policy interest.[7] The subgroup analysis was designed to "unpack the black box," by identifying whether impacts varied depending on the types of participants served or program design features. These subgroups were of three types: one type was based on pre-existing characteristics of participants, a second was defined based on a key program design feature, and a third was based on grantee characteristics.

Subgroups based on participant characteristics. Prior research has shown that the risk of recidivism and the impacts of re-entry services may be different for different types of individuals (Gendreau et al. 1996; Jonson and Cullen 2015). Based on this research, we have identified the following key subgroups, each defined based on study participants' pre-RA characteristics:

- *Gender*. Adult men have significantly different criminal behaviors than do adult women and are at higher risk of recidivism. Moreover, women have very different criminogenic needs than men and face different transition challenges, suggesting the need for re-entry services that are gender specific (Berman 2005; Bloom et al. 2003). A program's

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[7] To reduce the chance of drawing attention to spurious evidence of subgroup effects, best practices suggest that subgroups should be identified *a priori* based on theory or prior research evidence; the post-hoc "mining" of data should be curtailed or entirely avoided (see, for example, Wang et al. 2007 and Cook et al. 2004).

ability to respond to these needs may mean that the grantees were more or less effective for women than men.

- *Age*. Although explanations for the relationship abound, it has been well established that crime rates peak in early adulthood and decline steeply thereafter (Hirschi and Gottfredson 1983). Further, interventions aimed at increasing desistance can be more effective for those who are older (Uggen 2000). To test whether being in the program group had different impacts for study participants of different ages, we defined two subgroups: those younger than 30 years and those 30 years or older.

- *Risk of Recidivism*. Gender and age are two well established predictors of recidivism, but there are others, including criminal history and dynamic factors that are indicators of criminogenic need. Some researchers have found that interventions can be more effective for higher-risk individuals and that, in fact, programs targeted to those at lower risk can increase failure rates in some instances (Latessa and Lowenkamp 2006; Lipsey and Cullen 2007).[8] The study's SCA grantees determined risk by using validated assessment instruments. We did not have access to those scores, but instead used a procedure developed by Kemple and Snipes (2001) to divide the sample into lower-risk and higher-risk individuals (see Appendix C for details). All those eligible for SCA were supposed to be at medium or high risk of recidivism, so this classification represents a relative ranking within a truncated range.[9]

Subgroup based on program design. Research shows that recidivism is highest shortly after release from incarceration (Durose et al. 2014), suggesting that interventions can be most effective if they are applied during the transition from incarceration to release rather than after release (Petersilia 2003). The SCA grant solicitation recognizes this by defining successful re-entry as something that requires "delivery of a variety of evidenced-based program services in both a pre- and post-release setting designed to ensure that the transition from prison or jail to the community is safe and successful" (U.S. Department of Justice 2009, p. 2). Accordingly, we defined a subgroup that captured the potential importance of the timing of SCA enrollment.

- *Timing of SCA Entry.* To measure the possibility that program impacts are greater when individuals are enrolled well before release rather than after, we defined two groups: those randomly assigned at least 31 days before release and those randomly assigned no more than 30 days prior to release or after release.[10]

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[8] However, see Wilson and Zozula (2011) for an example of an evaluation that found contrary evidence.

[9] As a condition of their grants, the study grantees were to target those at medium or high risk of recidivism. Thus, the risk scale we created does not capture the full variation of risk level across the population of adults scheduled for release from incarceration; rather, it merely provides a relative ranking of individuals in this sample.

[10] Generally, the date of RA is coincident or very shortly before the date that individualized SCA services began for study participants assigned to the program group. Note that individuals who were randomly assigned after

Subgroup based on grantee type. Some grantees were associated with the criminal justice system (i.e., a DOC or sheriff's office), while others were health or social service agencies. For the first group, the key point of contact for participants accessing post-release SCA services was generally a probation or parole officer (PO), whereas health and social services agencies assigned a case manager apart from the PO. This difference can have important implications for the way re-entry services were delivered (D'Amico and Geckeler 2014). Further, the two types of agencies had different challenges in designing their programs, as will be discussed in the next chapter. For these reasons, the type of grantee may matter for determining program effectiveness.

- *Grantee Type.* This subgroup divides grantees into those that were state DOCs or local sheriff's offices versus those that were local social service or health agencies.

Confirmatory and Exploratory Analyses

We measured impacts on a range of outcomes relating to services received, recidivism, employment and others. These impacts were measured for the full sample and for the various subgroups described above. With so many comparisons, at conventional thresholds for determining statistical significance we are likely to find some impacts simply by chance (that is, even if true impacts are zero). This is known in the literature as the multiple testing problem.

Statistical adjustments that have been proposed for dealing with multiple testing typically reduce the threshold for determining statistical significance. These approaches decrease the likelihood of false positives (that is, of claiming that there is an impact when in fact the difference between groups occurred purely by chance). However, as a consequence, these methods reduce statistical power and increase the likelihood of false negatives (that is, of failing to conclude that a difference between groups is real even when it is).

To avoid this loss of statistical power, we adopted an approach recommended by a panel of experts (Schochet 2008) that treats a main analysis as confirmatory and other analyses as exploratory. Our confirmatory analysis considers re-incarceration for the full sample anytime within 30 months after RA as the main outcome of interest, and considers other analyses as exploratory. Further, we focused on patterns of effects rather than isolated impacts.

Sample Sizes and Statistical Power

Statistical power refers to the ability of a significance test to confidently detect an effect when in fact an effect exists. Among the factors that determine statistical power, two of the most important are the study's sample size and the size of the effect one is trying to detect. In a study with 966 study participants split unevenly between program and control groups, we can

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release might have received pre-release services; however, those services were available to both program and control group members.

confidently detect a difference between the groups on a binary variable if the difference is at least nine percentage points.[11] Thus, this study was powered to detect effects that are approximately that large. Some analyses for this study were conducted on subsets of participants, and there was a very modest amount of missing data on some items.[12] These analyses have weaker statistical power.

About Study Participants

To be eligible to participate in the SCA Adult Demonstration Program, individuals had to:

- Be 18 years of age or older;

- Be convicted as an adult;

- Have been imprisoned in a state, local, or tribal prison or jail; and

- Be classified as being at medium or high risk of recidivism.

Within this pool, grantees were expected to identify the specific subset that their programs intended to target, which could include, among others:

- A specific demographic group (e.g., based on age or gender), and

- Those returning to a specific community or neighborhood.

Once determined eligible for SCA participation according to whatever criteria the grantee had established, the individual was provided an orientation to SCA program services and to the RA study. Those who agreed to participate in both the program and the study were randomly assigned using an online system maintained by the study team, thus becoming study participants. Those who did not provide written consent to participate in the study were not randomly assigned and could not receive individualized SCA services (see Appendix A).

As was noted, 966 individuals were randomly assigned, with 606 (62.7 percent) assigned to the program group and 360 (37.3 percent) assigned to the control group.[13] Exhibit I-3 shows key characteristics of program and control group members measured at the time of RA. By virtue of

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[11] This calculation assumes a 95 percent confidence level for a two-tailed test, an 80 percent level of power, that the outcome variable has an observed value of approximately 50 percent (a worst-case scenario), and that the test of the difference is run unweighted without controlling for covariates. A change to any of these parameters can change the minimum detectable difference (MDD); for example, MDDs will be considerably better for variables that are more skewed.

[12] Results for an outcome are not reported if more than five percent of the sample had missing data on the item.

[13] In actuality, 973 individuals were randomly assigned. However, after RA one grantee lost the signed consent forms for seven individuals. These individuals were dropped from the study.

RA, we would expect those in the program and control groups to have, on average, very similar characteristics, and the exhibit shows that indeed they do. In each group:

- Approximately 80 percent were male.

- Approximately half were white and one-third were African American.

- Approximately half were 30 years old or less, and approximately one-fourth were older than 40.

- Approximately one-fourth had not obtained a high school diploma or General Educational Development (GED) degree, and just over 40 percent had achieved a GED. Very small percentages attended college.

- Nearly all had been employed at some time in their lives prior to RA. Approximately half were employed at the time of incarceration that preceded RA, usually full time, and the remaining half were not employed.

- Just over 10 percent had a disability (self-reported and defined as a condition limiting one's physical activity or kind of work).

- Nearly all spoke English as their primary language.

Importantly, there are almost no statistically significant differences between the program and control groups on the characteristics shown here. The one exception is the modest difference in the percentage who worked sometime prior to RA.

Exhibit I-4 reports the criminal history of study participants in the period before RA. Program and control group members were arrested and incarcerated a similar number of times in the period prior to RA, and their offense categories are comparable. The length of their most recent prior sentence was comparable, with just over one-half of both groups serving more than a two-year sentence. One difference is that 88 percent of program group members were randomly assigned while still incarcerated, while the figure for the control group is 83 percent.[14]

Taken as a whole, these findings suggest that, despite some minor differences that occurred by chance, RA succeeded in defining two equivalent groups.

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[14] Whether an individual was incarcerated at the time of RA was determined by comparing spells of incarceration measured from administrative data to the date of RA. As discussed later in this chapter, we do not have prison and jail data for jurisdictions nationwide. Therefore, these estimates likely undercount the actual percentage incarcerated at the time of RA. Survey data show that the percentage who self-reported that they were incarcerated on the date of RA is approximately 88 percent for both the program and control groups.

Exhibit I-3: Background Characteristics of Program and Control Group Members

	Program	Control	Difference
Demographic Characteristics			
Gender			
Female	21.8	19.9	1.9
Male	78.2	80.1	-1.9
Race and Ethnicity			
White	52.3	49.0	3.3
African American	31.2	33.8	-2.7
American Indian/Alaska Native	13.2	15.6	-2.4
Hispanic	10.2	9.2	1.1
Hawaiian Native/Pacific Islander	1.8	2.4	-0.6
Asian	0.9	1.7	-0.8
Age			
18 to 21	8.4	9.3	-0.8
22 to 25	17.8	20.6	-2.8
26 to 30	23.7	23.3	0.4
31 to 35	15.7	12.9	2.8
36 to 40	8.2	11.2	-2.9
41 to 50	18.6	17.6	1.0
51 or more	7.5	5.2	2.3
Highest Degree Attained			
Less than high school degree or GED	25.0	23.2	1.8
GED	44.9	43.4	1.4
High school diploma	24.4	27.1	-2.8
Some college	5.7	6.2	-0.5
Employment Characteristics			
Worked sometime in the past	93.0	88.8	4.2**
Employment status at time of most recent incarceration prior to RA			
Was employed full time	32.7	33.3	-0.6
Was employed part time	14.4	15.4	-1.0
Was not employed	52.9	51.3	1.6
Other Characteristics			
Has a disability	13.6	11.6	2.0
English as a primary language	98.7	98.8	-0.1
Sample Size	**606**	**360**	

Note: Numbers in the first two columns represent the percentage of study participants with the characteristics in question; the third column represents the difference between the two (program group value minus control group value). Estimates were weighted to equalize the odds of selection into the groups and, where appropriate, to account for potential survey nonresponse bias. The sample sizes shown are for items taken from the BIF.

Sources: BIF, except for English as a primary language, which is taken from the survey.

*/**/*** Statistically significant at the .1/.05/.01 level.

Exhibit I-4: Criminal History of Program and Control Group Members

	Program	Control	Difference
Number of separate times arrested in the 10 years prior to RA[a]			
1 to 2	14.8	15.2	-0.4
3 to 5	21.4	23.3	-1.9
6 to 10	26.3	25.4	0.9
11 or more	37.5	36.1	1.4
Most serious arrest offense in the 10 years prior to RA[a]			
Violent	52.0	52.0	-0.0
Property	35.4	33.5	1.9
Drug	10.6	12.7	-2.1
Public order	2.0	1.8	0.2
Number of separate times incarcerated in prison or jail any time prior to RA[b]			
1	11.1	13.3	-2.2
2 to 4	38.3	34.2	4.2
5 or more	50.5	52.5	-2.0
Type of crime for which most recently incarcerated prior to RA[b#]			
Violent	19.8	19.5	0.2
Property	34.5	29.9	4.6
Drug	43.9	49.5	-5.5
Public order	26.9	26.9	-0.0
Length of current or most recent sentence prior to RA[b]			
Less than 90 days	3.6	4.8	-1.2
At least 90 days but less than 6 months	6.6	7.3	-0.7
At least 6 months but less than 1 year	14.1	13.2	0.8
1 year to 2 years	21.0	20.2	0.8
More than 2 years	54.7	54.5	0.3
Total days incarcerated in prison or jail in 10 years prior to RA[a]			
Up to 1 year	28.4	25.8	2.6
1 to 3 years	36.0	33.7	2.3
3 to 5 years	16.9	19.7	-2.8
More than 5 years	18.7	20.8	-2.1
Incarcerated on the date of RA[c]	87.8	83.1	4.6**

Note: Numbers in the first two columns represent the percentage of study participants with the characteristics in question; the third column represents the difference between the two (program group value minus control group value). Estimates were weighted to equalize the odds of selection into the groups. Types of crime were coded according to Durose et al. (2014).

Sources: [a]=Administrative data; [b]=BIF; [c]=Both administrative data and the study's RA system.

[#] The sum across the categories exceeds 100 percent because multiple types could have been recorded.

*/**/*** Statistically significant at the .1/.05/.01 level.

Limitations of the Study

Although the research design ensures a rigorous and unbiased estimate of intent-to-treat, the interpretation of impact findings is subject to certain limitations and cautions.

- *Control group members were allowed to access re-entry services.* Those randomly assigned to the program group could receive individualized SCA services, while those assigned to the control group had access to whatever re-entry services were normally available but could not receive individualized SCA services. The study thus represents a comparison of the effectiveness of access to the individualized SCA services relative to other services, and not in comparison to no services whatever. In service-rich environments, control group members could have accessed significant services — even ones very comparable to SCA — from other sources.

- *Some SCA funds were spent on control group members.* Two of the grantees participating in this study used a portion of their SCA funds for general system improvements that could have benefited all those returning from incarceration, including the control group, to some degree. For example, they used a portion of their funds to modify pre-release classes or workshops that all those who were incarcerated could access on equal footing, whether or not they were SCA eligible. Because these changes were general system improvements, it was not practical to deny control group members access to them.[15] The study thus captures the effect of the personalized services that SCA provided, but not the general system improvements.

- *The grantees' programs varied in important ways.* Because of small sample sizes in each grantee site, it is not practical to estimate grantee-specific impacts. Accordingly, for our main analyses, we pool observations across the seven grantees and use subgroup analysis to subdivide the grantees in ways believed to capture important programmatic differences across them. However, differences within these group remain and their effects are not well captured by the impact analysis.

- *We cannot generalize findings beyond the study sample.* The seven grantees included in this study were purposively selected by BJA from a larger group of 15 grantees that received FY 2009 funding, because BJA believed that these seven were best able to participate in a rigorous evaluation. Because the grantees were purposively selected, we cannot generalize findings to the larger pool of FY 2009 grantees. Moreover, BJA made hundreds of SCA awards in subsequent fiscal years and under different categories of competition; we cannot generalize this study's results to those other grantees. In other words, this is a study of the impacts of these seven grantees' programs, not of SCA more generally.

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[15] Only modest amounts were spent on general system improvements and, in most sites, no funds were spent in this way at all.

- *Outcomes are measured imperfectly.* As is inevitable with studies of this nature, outcomes are not always measured with perfect accuracy. For example:

  - Recidivism data provided by state and local agencies were collected only from the jurisdictions in which individuals were most likely to be involved — arrest, conviction and prison incarceration data were collected from the states in which these SCA programs operated, and jail incarceration data from the counties to which SCA program participants were released. This means that the administrative data used in the study miss involvement with the criminal justice system that occurred outside these jurisdictions.[16]

  - Administrative data do not fully measure criminal activity. Administrative data only capture events available in state and local agencies' data systems; some criminal activity may have occurred without being recorded (for example, if the event did not come to the attention of the criminal justice system), and some records may have been expunged.

  - Matching with criminal justice databases could be subject to coding errors. Given this, we could have failed to turn up evidence of recidivism that did occur.[17]

  - NDNH does not capture employment and earnings from self-employment and selected other sources.

Fortunately, outcomes are measured in the same way for both the program and control groups, which minimizes the role of reporting bias in the estimation of impacts. Furthermore, for the 18-month impact findings, results were based on both administrative and survey data, which provided an opportunity to test the robustness of conclusions.

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[16] Because we do not have data from other jurisdictions, this study likely underreports recidivism to some degree. We attempted to access recidivism data through the National Crime Information Center (NCIC) maintained by the FBI to overcome this limitation. This would have provided national coverage and a more uniform source of recidivism data. The FBI denied the data request, so this approach was not possible. However, even though recidivism is measured from separate state and local sources, it is measured consistently for program and control group members in each jurisdiction, which minimizes potential measurement bias on estimates of program impacts.

[17] We assumed that everyone in the sample should have been arrested, convicted and incarcerated in the 10 years prior to the RA date. If an individual had no evidence of arrest, conviction or incarceration in this period based on the administrative data we were provided, we assumed that the agency could not successfully match this individual to its records. In these cases, the corresponding measures of recidivism in the post-RA period were set to missing. This is a conservative assumption that prevents us from falsely assuming recidivism did not occur when in fact there was a problem with the match. Fortunately, the incidence of missing data of this nature was very small.

Focus of This Report

The present report describes the grantees' costs of providing program services and presents impacts measured 30 months (for recidivism) or 27 months (for employment and earnings) after RA. Because the survey was administered just once, at a point approximately 18 months after each participant was randomly assigned, survey data cannot be used for measuring these longer-term outcomes. Instead, this report describes impacts on recidivism measured from data supplied by state and local criminal justice agencies, and employment and earnings measured from NDNH.

The report includes the following sections:

- Chapter II summarizes results from the earlier, 18-month impact report (D'Amico et al. 2017) and an implementation report (D'Amico et al. 2013). The 18-month impact results relied on both administrative and survey data and, therefore, provided independent sources of evidence and covered a wider range of outcomes. The implementation report drew on data collected from site visits to learn about the grantees' programs. Both reports provide important context for the longer-term outcomes that are the focus of the present report.

- Chapter III presents an analysis of the costs that grantees expended in providing SCA services and estimates net per-participant costs.

- Chapter IV presents the longer-term impacts of the grantees' programs on recidivism. It measures arrests, convictions, and jail and prison incarcerations based on administrative data.

- Chapter V presents the longer-term impacts of the programs on employment and earnings using NDNH data.

- Chapter VI presents a summary and conclusions.

Appendices present technical material, including details on the estimation techniques and sensitivity analyses.

II. Prior Study Findings

Two study reports have already been produced. One report (D'Amico et al. 2013) described results from the implementation study. The second report (D'Amico et al. 2017) drew upon the grantees' MIS data to describe the services that those in the SCA program received and used survey and administrative data to present impact findings measured 18 months after RA. In the current chapter we summarize these findings.

Findings from the Implementation Study

According to the SCA grant solicitation, grantees were expected to develop individualized re-entry plans based on validated risk and needs assessments and to provide supervision and coordination of "all necessary services," including educational, literacy, vocational and job placement services; substance abuse treatment; housing assistance; and mental and physical health care (U.S. Department of Justice 2009, pp. 2-3). Each grantee received from $1.5 million to more than $3.2 million in SCA Adult Demonstration funding to accomplish these goals, and each was required to provide a match of 100 percent of the SCA award using state or local government funds, grantee or partner contributions, or other public or foundation funds.[18]

The grantees were a diverse group. Three of the seven study grantees were state DOCs, one was a sheriff's office, and three were local government social services or health agencies (see Exhibit II-1). Some recruited SCA participants exclusively from prisons, others exclusively from jails, and others from both prisons and jails. Some grantees served only females, some served only males, and others served both females and males.

SCA funds helped expand re-entry services. Grantees reported that their SCA grants helped them fill gaps in existing re-entry services and expand service capacity. Partly through their grants, the grantees improved their partnerships with other community agencies and strengthened connections between pre-release and post-release services.

The emphasis on pre-release services was greater in some sites than others. Three grantees delivered fairly extensive pre-release services as part of their SCA programs and, therefore, required participants to have an extended period of incarceration remaining before SCA enrollment. Others relied on existing programming in institutions for pre-release services and focused on using SCA funds for transition planning and post-release services; they generally enrolled participants in SCA nearer to release and sometimes after release. Because of this difference, the adequacy of the continuum of care from incarceration through release was better developed in some sites than others. Overall, approximately 55 percent of participants

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[18]   Further information about grant amounts and spending is presented in Chapter III of this report.

were enrolled in SCA three or more months prior to release, 28 percent within three months of release and 17 percent after release.

Exhibit II-1: Grantees by Level of Government and Type of Agency

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┌──────────────────────────────┐          ┌──────────────────────────────┐
│  Local Government Agencies   │          │  State Government Agencies   │
└──────────────────────────────┘          └──────────────────────────────┘
         │                                          │
         │   ┌────────────────────────────┐        │   ┌────────────────────────────┐
         ├───│      Criminal Justice      │        └───│      Criminal Justice      │
         │   │ • Marion County Sheriff's  │            │ • Kentucky DOC             │
         │   │   Office                   │            │ • Oklahoma DOC             │
         │   └────────────────────────────┘            │ • South Dakota DOC         │
         │                                             └────────────────────────────┘
         │   ┌──────────────────────────────────────────────────────────┐
         └───│              Health and Human Services                   │
             │ • Allegheny County Department of Human Services          │
             │ • San Francisco Department of Public Health              │
             │ • San Mateo County Division of Health and Recovery       │
             │   Services                                               │
             └──────────────────────────────────────────────────────────┘
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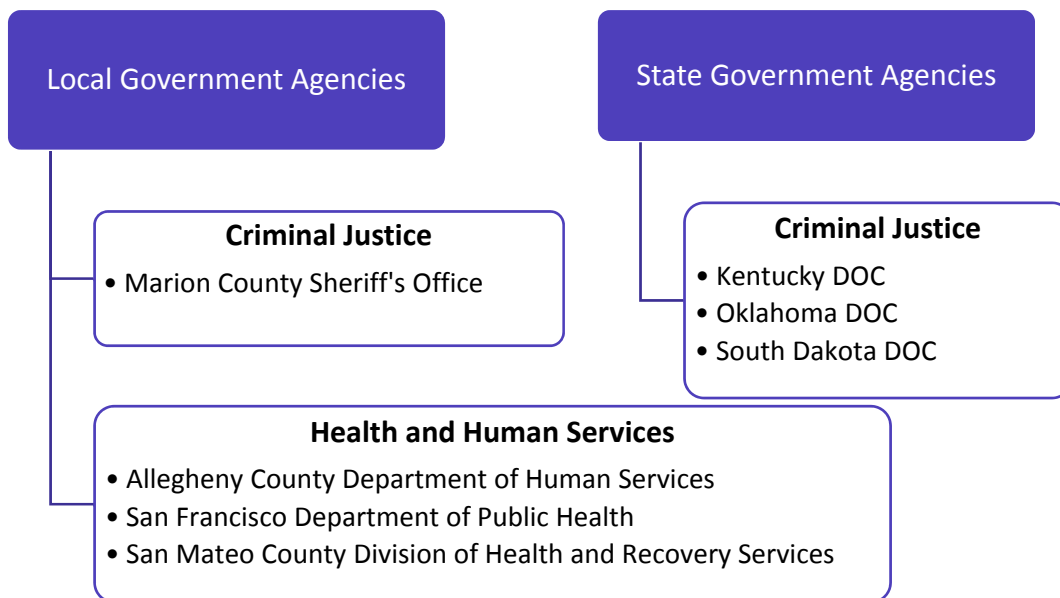
Source: Results from the implementation study.

Assessments were used to establish program eligibility and to customize services and update service plans over time. Assessments were administered periodically and sometimes different instruments were used at different times. For example, one grantee used a proxy indicator (based on age, age at first arrest, and number of arrests) to establish program eligibility, but, once the individual was enrolled in SCA, administered LSI-R to develop a re-entry plan. Another used LSI-R at the outset, but also used Starting Point: My Personal Assessment for ongoing case planning. Still another used CAIS to establish eligibility, but, for service planning, supplemented it with ASI and URICA, among other instruments.

Examples of Assessment Instruments Used by Grantees

- Addiction Severity Index (ASI)
- Correctional Assessment and Intervention System (CAIS)
- Level of Service Inventory-Revised (LSI-R)
- Level of Service/Case Management Inventory (LS/CMI)
- University of Rhode Island Change Assessment Scale (URICA)

Case management was a key service. Case management was a key feature of all the grantees' programs except one. Across grantees, the goal of case management was to help prevent recidivism by providing individualized support and coordinating access to services based on identified needs and risk factors. These case managers were either POs who commonly had

reduced caseloads and extra training provided through the grant, or came from social services agencies and had more traditional case management backgrounds (e.g., social workers, counselors). In the latter case, SCA participants might also have been required to report to a PO after release, but this individual was not the SCA case manager.

One grantee implemented a very different program model. It had program participants attend a structured set of classes that took place full time, Monday through Friday, during the 12 weeks after release. Classes covered cognitive behavioral therapy, employment assistance, substance abuse treatment, and life skills, among other topics, and were provided either by the lead agency or through partners. This 12-week period was followed by a period of "aftercare," consisting of 1-2 hour sessions for an additional 12 weeks.

Grantees provided a range of services, either directly or through referral. Although case management was typically the focal service, all the grantees also offered education and training, employment assistance, substance abuse treatment, mental health services, cognitive behavioral therapy, prosocial services, housing assistance and supportive services (see Exhibit II-2). The grantees provided some of these services directly. Other services were provided by partners, either on a fee-for-service basis (formal partnerships) or through unfunded referrals (informal partnerships). There were advantages and disadvantages to each method (see Exhibit II-3). All grantees used all three methods of providing services but they differed in which services they provided directly versus through funded or unfunded referral.

Partnerships were important, and they grew over the grant period. Partnerships were crucial for service delivery, as the grantees lacked the resources or expertise to deliver the full range of services themselves. These partnerships served to improve the comprehensiveness of services. The strengthening of partnerships was one key achievement of the grants.

Grantees that were criminal justice agencies faced very different challenges and opportunities than did those that were social services agencies. Grantees that were DOCs generally had POs carry out SCA's post-release case management functions. These individuals had the intrinsic challenge of balancing supervisory and rehabilitative functions and were sometimes slower to gain the trust of SCA participants. However, they were better able to coordinate with jail and prison staff, and having a PO as a case manager was beneficial for ensuring that participants kept their case management appointments. Social services agencies, by contrast, had wider networks of social service agencies with whom they could partner but weaker connections with prisons and jails. Further, participant retention could be a challenge for them, in that participants who were randomly assigned to the program group sometimes did not take advantage of the case management offered.

Exhibit II-2: Types of Program Services

Category	Service Description
Education Services	GED preparation and testing, adult basic education (ABE), and community college education
Employment Assistance	Job search and placement assistance, employment opportunities, soft-skills training, and resume and interviewing skills development
Substance Abuse Treatment	Inpatient or outpatient treatment administered by licensed specialists
Mental Health Services	Mental health screenings and referrals to mental health services
Cognitive Behavioral Therapy	Psychotherapeutic approach that addresses dysfunctional emotions, maladaptive behaviors/cognitive processes and contents through goal-oriented, explicit systematic procedures
Prosocial Services	Stress and anger management services, peer support, leisure activities, family and parenting classes, and mentoring
Housing Assistance and Other Supportive Services	Subsidized housing, housing placement services, and vouchers for food, transportation, and other needs

Source: Results from the implementation study.

Exhibit II-3: Three Approaches for Delivering Program Services

	Service Approach		
	Direct Service	**Formal Partnership**	**Informal Partnership**
Nature of Agreement	Directly provided by the organization operating the SCA program	Grantee makes formal arrangement with provider for services	No specific terms or agreement; SCA program staff provide referrals
Priority for Participants	The service is exclusively for SCA participants	SCA participants given priority over others	SCA participants are like all others seeking services
Advantages	Specifically tailored to SCA participants; grantee controls access and engagement	Provides SCA participants with priority access; services coordinated by the program	Most flexible, least costly, and allows SCA staff members to use any service provider available in the community
Limitations	Grantee lacks resources and expertise to deliver all services directly	Typically costs the program money	SCA participants have the same access to services as others; little formal follow-up on participant involvement in services

Source: Results from the implementation study.

SCA funds led to important system changes, but fidelity to the re-entry model envisioned by BJA was incomplete. The contrast between services under SCA and what came before was readily apparent — more attention was given to re-entry planning, additional funds were available for services, partnerships were strengthened, and, especially after release from incarceration, participants could access case management services that were expected to be much more robust than what was normally available. Further, for DOCs, the SCA grant continued a shift away from strictly an enforcement mindset toward a rehabilitative philosophy.

However, full implementation of the service model envisioned by BJA also fell short in some important ways. In particular, the continuum of care from incarceration to release was less well developed in some sites than others. Furthermore, none of the grantees had adequate funding to directly deliver all services that participants needed. Therefore, they all relied heavily on unfunded referrals to provide many services. Where unfunded referrals were used, coordination with the grantee's program was typically weak and case managers could not always track whether participants received the services to which they were being referred. Given the reliance on unfunded referrals for many post-release services and problems in some sites with participant retention, it would be hard to argue that every SCA participant received "all necessary services," a topic that will be further explored later in this chapter.

Summary of Results from Grantees' MIS Data

Prior research suggests that dosage and continuity of care are important elements of effective re-entry programs (e.g., Domurad and Carey 2010). Using the grantees' MIS data, the 18-month impact report described the services that grantees provided to program participants. These are services that the grantees knew about and entered into their data systems and pertain only to those randomly assigned to the SCA program group.

Just over one-third of those assigned to the SCA program group received both pre-release and post-release SCA services following their enrollment in the program. According to grantees' data, 36 percent of those in the SCA program group received both pre-release and post-release services as part of SCA, 40 percent received only post-release services, and 24 percent received only pre-release services (see Exhibit II-4). Participants who were enrolled more than 30 days prior to release were more likely to receive both pre-release and post-release services as part of program participation. Note that those not receiving pre-release or post-release services as part of SCA could have received these services from other sources.

Exhibit II-4: Percentage of the Program Group Receiving Pre-Release Services, Post-Release Services, or Both
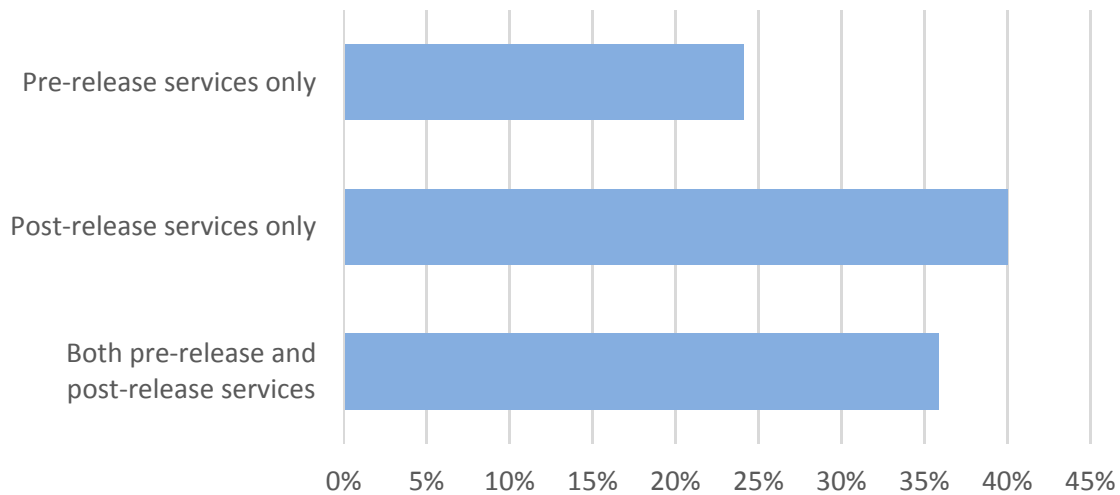
Source: MIS data provided by the grantees.

Employment assistance, cognitive behavioral therapy, and substance abuse treatment were the most common services provided through SCA, both before and after release. Nearly one-half of the SCA program group received employment assistance and cognitive behavioral therapy as part of SCA while they were still incarcerated, and more than one-third received substance abuse treatment. These three services were also the most common ones provided through SCA after release (see Exhibit II-5).

The length and intensity of participation in SCA varied. Approximately 25 percent of those assigned to the program group participated in SCA for more than one year, and another 37 percent participated for more than six months. A little less than 40 percent participated for up to six months (see Exhibit II-6).

Summary of 18-Month Impact Findings

The 18-month report used survey data and administrative data to examine short-term impacts on services received, recidivism, employment and earnings, and other outcomes.

Being in the program group increased access to services, but many needs remained unmet. The logic underlying the program model is that the grantees will provide those returning from incarceration with more comprehensive and coordinated re-entry services than they would have received in the absence of SCA, and that these services will, in turn, improve desistance and lead to other desirable outcomes. An important step in the evaluation of the grantees,

**Exhibit II-5: Incidence of Pre-Release and Post-Release Services
for Those in the Program Group**
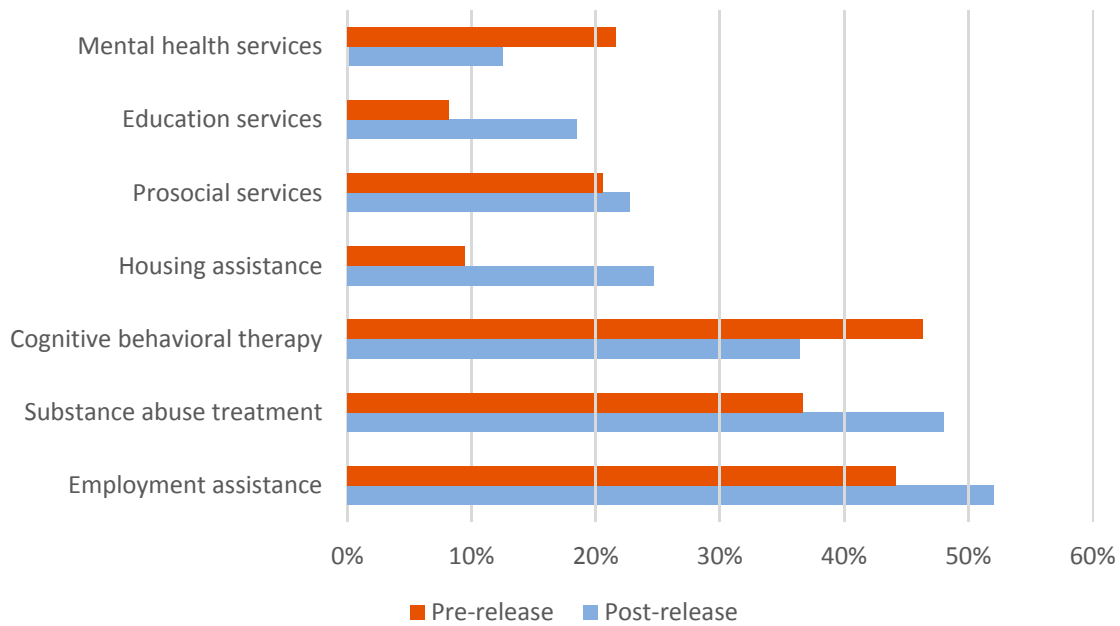
Source: MIS data provided by the grantees.

Exhibit II-6: Duration of Participation in SCA for Those in the Program Group
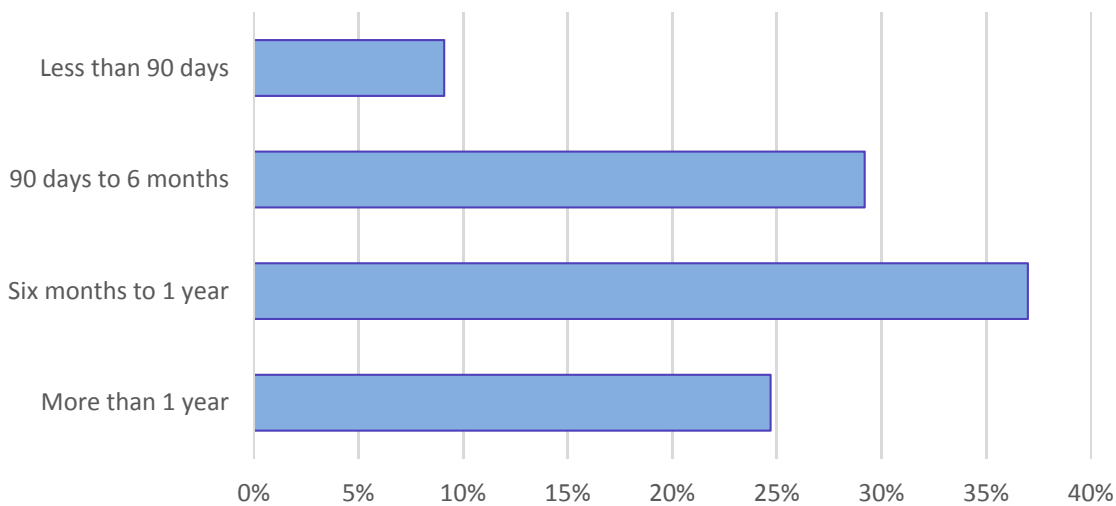
Source: MIS data provided by the grantees.

therefore, was to assess whether, in fact, those assigned to the SCA program group received more services than those assigned to the control group. Using survey data for both the program and control groups, we found important differences on services received as well as important similarities.

The program group was significantly more likely than the control group to receive an array of re-entry services (see Exhibit II-7). For example, they were more likely to report getting case management assistance; this included self-reports of getting help with re-entry, having a re-entry plan, and having a person they felt went out of their way to help. Those in the program group were also significantly more likely to receive help finding a job, cognitive behavioral therapy, housing assistance, outpatient substance abuse treatment, and prosocial services.

Exhibit II-7: Impacts on Services 18 Months after RA

	Program	Control	Difference
Case management	85.7	69.4	16.2***
Got help with re-entry	77.5	59.0	18.5***
Had a case plan	56.8	35.2	21.6***
Had a person who went out of the way to help	62.8	42.0	20.8***
Employment assistance	68.1	48.3	19.7***
Got help with job-finding skills	60.8	39.6	21.2***
Got help finding a job	30.3	19.9	10.4***
Got vocational training	12.3	12.3	-0.0
Cognitive behavioral therapy	61.4	41.5	19.8***
Housing assistance	20.8	6.3	14.5***
Substance abuse treatment			
Inpatient	37.7	33.5	4.2
Outpatient	49.4	38.1	11.3***
Prosocial activities	36.5	26.6	9.9***
Participated in a mentoring program	21.5	14.3	7.2**
Participated in sponsored social activities	26.1	20.1	5.9*
Other mental health services	31.9	28.1	3.8
Education services (took ABE or GED classes)	18.7	19.1	-0.3
Sample Size	**495**	**294**	

Note: Numbers in the first two columns represent the percentage receiving the service since the date of RA; the third column represents the difference between the first two columns. The incidence of case management, employment assistance, or prosocial services is a function of the percentage who reported getting any of the components shown. Sample sizes represent those who responded to the survey. For more detail about the computation of services received, see D'Amico et al. (2017).

Source: 18-month survey.

*/**/*** Statistically significant at the .1/.05/.01 level.

Being in the program group had no impact on the likelihood of receiving other mental health services or education and vocational training.[19]

However, despite the fact that the grantees' programs had a significant impact on services received, the program group reported having many unmet service needs 18 months after RA (see Exhibit II-8). In fact, their needs for additional services were no less than the control group's needs. For example, approximately two-thirds of both groups reported wanting additional housing assistance and job placement assistance, and more than half wanted additional health services, educational services and job training. More than one-third of both groups wanted family reunification services, substance abuse treatment and mental health services.

Exhibit II-8: Additional Services Study Participants Would Have Liked

	Program	Control	Difference
Housing support	68.5	69.2	-0.7
Job placement	60.2	61.5	-1.3
Health services	57.6	55.5	2.2
Educational services	54.2	56.1	-1.9
Job training	52.3	54.0	-1.7
Advice on getting a job	52.0	49.9	2.2
Family reunification	38.8	38.0	0.8
Substance abuse treatment	38.7	41.6	-2.9
Mental health services	38.3	38.3	0.1
Child-support issues	27.9	29.0	-1.1
Sample Size	**450**	**260**	

Note: Numbers in the first two columns represent the percentage reporting that they desired additional services in the category; the third column represents the difference between the first two columns. These questions were asked only of those not continuously incarcerated since RA (n=711). Because release after RA may be determined by participation in SCA, differences between the groups are only suggestive of true estimates of impacts.

Source: 18-month survey.

*/**/*** Statistically significant at the .1/.05/.01 level.

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[19] As described in Chapter I, all estimates in this and other tables were weighted to account for different rates of RA across grantees and to adjust for survey non-response.

This resource was prepared by the author(s) using Federal funds provided by the U.S. Department of Justice. Opinions or points of view expressed are those of the author(s) and do not necessarily reflect the official position or policies of the U.S. Department of Justice.

Being in the program group did not improve desistance 18 months after RA. The study measured recidivism as involvement with the criminal justice system in the 18 months after RA that led to re-arrest, reconviction or re-incarceration. Whether recidivism was measured using survey or administrative data, program participants were not less likely than those in the control group to be re-arrested, reconvicted or re-incarcerated, and they did not have fewer total days incarcerated (see Exhibit II-9). Further, their time to re-arrest or re-incarceration was no shorter. There is some evidence that those in the program group were somewhat more likely to be convicted of a new crime or have probation or parole revoked; this higher incidence may have occurred because enhanced case management for those in the program group could have increased the likelihood of catching new offenses and violations of terms of parole or probation when they occurred.

There were no short-term program impacts on employment-related outcomes. Whether effects were measured using NDNH administrative data or survey data, assignment to the program group did not improve the probability of being employed in the 18 months after RA. During the final six months, those in the SCA program group earned an average of approximately $3,200 compared with approximately $3,000 for the control group, but the difference is not statistically significant (see Exhibit II-10).

Being in the program group may have improved income adequacy; there were no effects on a range of other outcomes. Those in the program group were more likely than those in the control group to report that they had enough income to support themselves during the month prior to the survey. There were no effects on the adequacy of housing or health status, the self-reported incidence of illegal drug use, or the ability to meet child-support obligations (see Exhibit II-10).

Table II-9: Impacts on Recidivism at 18 Months

	Program	Control	Difference
A. Outcomes Measured from Administrative Data			
Arrests			
Arrested (%)	44.7	41.6	3.1
Average number of arrests	1.3	1.0	0.3**
Arrests by arrest type (%)[a]			
Violent crime	6.6	9.1	-2.4
Property crime	21.4	13.3	8.1**
Drug crime	18.5	16.7	1.8
Public order crime	34.0	32.6	1.3
Convictions			
Convicted of a crime (%)	31.3	24.8	6.4*
Average number of convictions	0.4	0.3	0.1***
Incarcerations (prison or jail)			
Was re-incarcerated in prison or jail (%)	48.4	43.8	4.6
Experienced a new jail incarceration (%)	40.6	37.6	3.0
Experienced a new prison incarceration (%)	22.3	20.0	2.3
Total days incarcerated[b]	246.5	245.2	1.3
Days incarcerated after initial release[c]	71.5	69.0	2.5
Sample size	**606**	**360**	
B. Outcomes Measured from the Survey			
Arrests			
Arrested (%)	34.0	28.7	5.3
Average number of arrests	0.8	0.8	0.0
New Charges and Convictions			
Formally charged with a new crime (%)	21.3	17.8	3.5
Convicted of a new crime (%)	15.2	12.6	2.6
Average number of new convictions	0.2	0.1	0.1
Parole/Probation Violations			
Charged with a violation (%)	32.0	30.5	1.5
Probation/parole revoked (%)	19.5	14.9	4.7*
Incarcerations (prison or jail)			
Was re-incarcerated in prison or jail (%)	39.9	36.6	3.2
Average number of re-incarcerations	0.9	0.8	0.1
Currently incarcerated (%)	32.2	34.7	-2.5
Sample Size	**494**	**294**	

Note: Numbers in the first two columns represent outcomes measured in the 18 months following the date of RA for the program and control groups; the third column represents the difference between the first two columns.

Sources: Administrative data from state and local agencies and 18-month survey data.

[a] The sum across categories exceeds the percentage ever arrested because individuals could have been arrested more than once and with different arrest charges in the 18-month follow-up period.

[b] For those incarcerated at the time of RA, total days includes days incarcerated following RA but before release.

[c] This excludes time incarcerated from the date of RA to initial release. Because time to initial release could be determined by the program, differences between the groups are only suggestive of true estimates of impacts.

*/**/*** Statistically significant at the .1/.05/.01 level.

Exhibit II-10: Impacts on Employment and Selected Other Outcomes Measured at 18 Months

	Program	Control	Difference
A. Outcomes Measured from NDNH			
Employed anytime in the fifth or sixth quarters after RA (%)	43.3	40.4	2.9
Earnings in the fifth and sixth quarters after RA ($)[a]	3,207	2,975	232
Sample size	**602**	**355**	
B. Outcomes Measured from the Survey			
Employment Outcomes			
Ever employed since RA (%)	72.8	72.5	0.4
Employed on the survey date (%)	33.0	34.9	-1.9
Of those employed on the survey date[b]			
Employed full time (%)	68.6	64.6	4.0
Employed part time or in temporary or seasonal jobs, or off-the-books (%)	31.4	35.4	-4.0
Hourly rate of pay ($)	12.39	11.43	0.96
Other Outcomes			
Living in own house, apartment, or room[c]	25.0	23.8	1.1
Health is good, very good, or excellent[d]	78.6	77.9	0.7
Used illegal drugs last month[d]	12.7	13.9	-1.2
Paid required child support[e]	53.7	54.9	-1.1
Had enough income to support self last month[d]	68.6	60.2	8.4**
Sample Size	**494**	**294**	

Note: Numbers in the first two columns represent the percentage with the outcome, measured in the 18 months following the date of RA; the third column represents the difference between the first two columns. Sample sizes for analysis using survey data are less than those shown for subsetted outcomes. One individual in the SCA program group was excluded from the calculation of NDNH earnings because this individual's earnings were an extreme outlier.

Sources: 18-month survey and NDNH.

[a] Those not employed in the quarter are treated as having zero earnings.

[b] These are conditional outcomes, with the results restricted to those who were employed at the time of the survey. Therefore, the RA design does not ensure equivalence in baseline characteristics between the program and control groups, and differences in outcomes between the groups are only suggestive of true estimates of impacts.

[c] Own house, apartment or room does not include those living in transitional housing or treatment facilities.

[d] This is a conditional outcome, with the results restricted to those who were not incarcerated at the time of the survey. Therefore, RA does not ensure equivalence in baseline characteristics between the groups, and differences are only suggestive of true estimates of impacts.

[e] This is a conditional outcome, with the results restricted to those who were not incarcerated at the time of the survey and who had an order to pay child support. Therefore, RA does not ensure equivalence in baseline characteristics between the groups, and differences are only suggestive of true estimates of impacts.

*/**/*** Statistically significant at the .1/.05/.01 level.

III. Program Costs

This chapter provides context for the impact findings by describing the costs that grantees expended in operating their programs, covering all costs since the grantees received their grants (and not just during the period of RA). It also describes the net costs of serving the program group, which are the per-person costs of providing services to those randomly assigned to the program group above the costs spent on those in the control group.[20]

Findings in Brief

- Grantees received SCA grants in amounts from $1.5 million to $3.25 million.

- Total costs per new SCA enrollee varied greatly across the grantees, ranging from a low of less than $1,000 per person to a high of more than $20,000.

- The cost per person who received a given service was highest for inpatient substance abuse treatment, with an average unit cost of more than $7,000. Case management also had a high unit cost, with an average of $2,600. However, the unit cost for case management was highly variable across the grantees.

- The net service cost — that is, the difference between the cost for serving an SCA participant and the cost for serving a person in the control group — was highest for case management, at more than $1,300 per SCA enrollee. The total net service cost was approximately $2,800 per SCA enrollee

Data and Methods

The cost analysis draws from four major data sources.

- *The grantees' financial reports*. As a condition of their grants, grantees were to submit quarterly financial reports to BJA, showing current quarter and cumulative expenditures drawn from the SCA grant and from matching funds. We have copies of these reports from the first quarter of 2012 to the end of each grantee's period of performance.

- *The grantees' PMT data.* In addition to financial reports, grantees were required to submit quarterly data on program activities through the PMT. We used these data to

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[20] We began this chapter with an analysis of the programs' cost-effectiveness in mind. However, since there were no short-term impacts on recidivism, we describe the total and marginal costs of providing services. The latter is essentially the numerator of a cost-effectiveness calculation.

calculate each grantee's program enrollees from 2012 through the end of the grantee's period of performance.

- *Participant survey data.* Study participants were surveyed by telephone 18 months after RA. These data were used to calculate the incidence of services accessed by program and control group members. The survey asked about case management, employment assistance, education and training services, mental health counseling, cognitive behavioral therapy, substance abuse treatment, prosocial activities and housing assistance.

- *Cost data collected by the evaluation team from grantees.* We asked grantees to provide data on the costs for providing each type of SCA service as well as the number of persons to whom they provided the service during the 2012 calendar year. Because not all grantees funded each type of service through their SCA programs, not every grantee provided cost data for every service type.

We used data from the quarterly financial reports to describe the total grant awards, the proportion of funds for the SCA program drawn from the match requirement versus federal funds, and trends in expenditures. We combined these data with enrollment data drawn from the PMT to calculate expenditures per enrollee for each grantee. These calculations cover all funds spent during the grant period, not just during the period of RA.

We next calculated net per-person program costs strictly for SCA participants who were randomly assigned. These calculations begin by using the data that grantees provided to the evaluation team on the costs of providing each service type and on the number of persons they served with each service. This allowed us to calculate a unit cost — that is, the cost per person who accessed the service. Where possible, we calculated a unit cost for those in the program group and a separate unit cost for those in the control group; otherwise, we assumed that the unit cost was the same for both groups.[21]

These unit costs were then multiplied by the rates of participation in services, calculated for each grantee and separately for the program and control groups, to yield a service cost per program group and per control group enrollee. The difference in service costs per enrollee for the program and control groups yields the net service cost for a single SCA participant. For services with a different unit cost for program and control group enrollees, the net service cost is a function of the difference in the unit price and the difference in the service utilization rate

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[21] Some services, such as case management provided by the DOCs' enhanced POs, substituted for supervision provided by regular POs. Enhanced POs had smaller caseloads, so the unit cost of supervision was different for program and control group members. In these cases, we calculated separate unit costs for the program and control groups. Unit costs for other services (e.g., substance abuse treatment or cognitive behavioral therapy) are assumed to be generally the same for program group and control group members who received the service.

between the groups; for services with a single unit cost, the net service cost is solely a function of differences in utilization rates. Importantly, the net service cost represents the difference in the cost of serving the average member of the program group compared with the cost for the average member of the control group, regardless of which organization provided the service and with what sources of funding.[22]

Total Grant Awards and Expenditures

BJA first awarded SCA Adult Demonstration funding to the study's grantees in FY 2009 (see Exhibit I-1, presented in Chapter I, for the grant timeline). It also awarded additional funding in subsequent years, contingent upon the grantees' participation in the impact study. Exhibit III-1 identifies the seven grantees' total grant amounts, which ranged from $1.5 million to $3.25 million in total federal funds.

However, funds for the grantees' SCA programs were greater than these amounts because the BJA FY 2009 grant solicitation specified a 100 percent matching requirement. Sources for the match could include other government funds, grantee or partner contributions, or other public

Exhibit III-1: Federal SCA Adult Demonstration Grant Awards

	Total	Initial Federal Award	Subsequent Federal Awards
Allegheny County	$2,653,339	$608,339	$2,045,000
Kentucky	$3,250,000	$750,000	$2,500,000
Marion County	$1,502,768	$302,768	$1,200,000
Oklahoma	$3,250,000	$750,000	$2,500,000
San Francisco	$2,600,000	$600,000	$2,000,000
San Mateo County	$2,937,674	$677,674	$2,260,000
South Dakota	$3,249,749	$749,749	$2,500,000

Note: Amounts include only federal funding, not the match requirement.
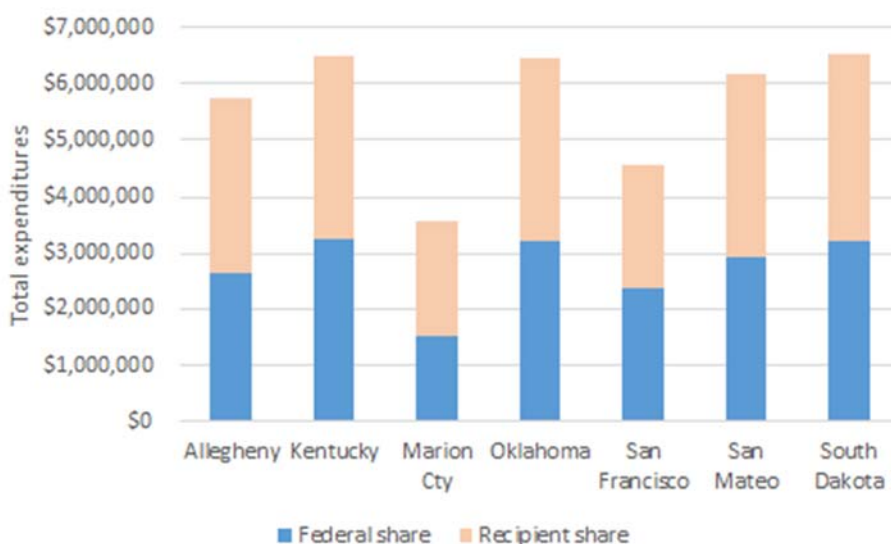
Source: Quarterly financial reports submitted by grantees.

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[22] The calculation of net service costs focuses strictly on services whose service costs per enrollee are expected to differ between treatment and control group members; service costs not expected to differ between the groups are ignored. For example, grantees that are social service agencies assigned a case manager to each SCA participant; members of both the program and control groups generally had supervision requirements from a PO who was not the case manager but, because SCA was not expected to affect this supervision, PO costs for participants served by social services agencies were not included in the calculations. The calculation of net service costs also excludes costs that were strictly administrative, including grantees' costs of managing their grants, coordinating with partners, and fulfilling grant reporting requirements.

or foundation funds. The grant amounts were intended to supplement rather than supplant non-federal funds.

Using federal and non-federal expenditure data reported by grantees on their quarterly financial reports, Exhibit III-2 shows total expenditures as well as the federal and non-federal portions. All grantees contributed at least the required 100 percent match, but some — most notably Marion County — contributed more.

Exhibit III-2: Federal and Recipient Share of SCA Expenditures by Grantee

Note: Amounts include total expenditures of federal and recipient contributions.
*Source:* Quarterly financial reports submitted by grantees.

Exhibit III-3 shows cumulative expenditures over time. Most grantees had a steep and steady rate of spending from one quarter to the next, although there were periodic plateaus in a few cases. All grantees spent out their grant awards, but they did so over different periods of time. Marion County was the first to spend out its funds, and did so in the first quarter of 2014; Oklahoma spent out its funds more than two years later.

We also calculated costs per person served, calculated as costs from the beginning of 2012 through the end of the grant period divided by *new* participants enrolled during that time.[23]

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[23] Our timeframe began with 2012 because we do not have data on persons enrolled prior to that time. The number of new participants since 2012 was calculated from the PMT as the quarterly sum of the number of new pre-release participants plus the number of new post-release participants, minus the number of new post-release participants who also received pre-release services. Some money expended from 2012 to the end of

Exhibit III-3: Cumulative Federal and Recipient Expenditures, by Grantee and Quarter
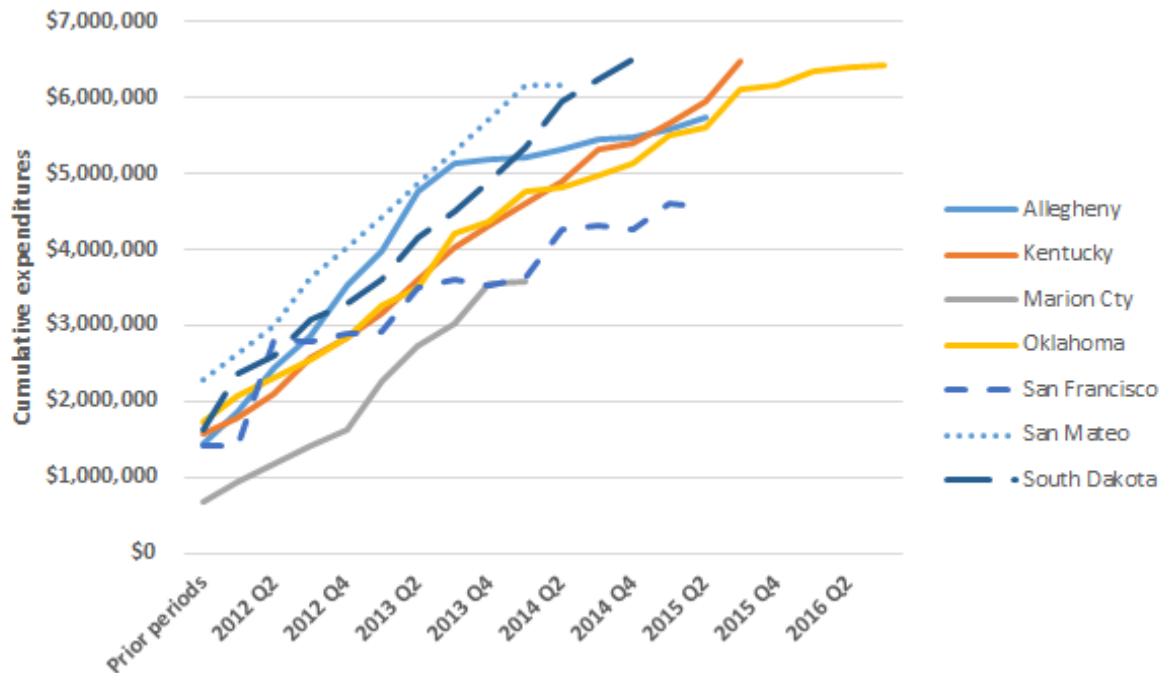
Note: Amounts include cumulative expenditures of federal and recipient contributions. Figures tabulated for the prior periods include each grantee's expenditures from its grant award in fall 2009 through the end of 2011. The last point plotted for each grantee represents the quarter in which it spent out its federal funds.

Source: Quarterly financial reports submitted by grantees.

These results are shown in Exhibit III-4, which plots the number of new participants (on the vertical axis) and costs per new participant (on the horizontal axis), for each grantee. The wide dispersal on the vertical axis makes clear that grantees differed in whom they considered SCA participants for purposes of grant reporting. For example, some used a portion of their grant funds to improve pre-release workshops that all those returning from incarceration could access and counted all of these individuals as SCA participants in the reports it submitted.[24] Others only enrolled individuals if they were expected to receive personalized services as part of SCA. For purposes of this chapter, we calculated costs per participant using whatever criteria each grantee established in submitting data for the PMT.

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the grant period was doubtless spent on "carry-in" participants who were enrolled in prior periods. For this reason, total persons served with money spent from the beginning of 2012 is greater than the number of new participants, and per person expenditures therefore represent an upper-bound estimate.

[24] Individuals who participated in these group services and no other SCA services were not randomly assigned and were not included in the impact study. RA occurred at each grantee site only when an individual was expected to begin personalized SCA services, which usually commenced with a meeting with a case manager.

The scatterplot of costs per new participant by the number of new participants suggests that grantees were of two types. Four grantees (those in the lower right portion of the plot) enrolled fewer than 350 new participants during this time and spent from $13,000 to $22,000 per new participant. The remaining three grantees enrolled at least 850 participants (up to 5,100, in one case), and their per-participant expenditures were therefore much lower (no more than a few thousand dollars per person).

Exhibit III-4: Approximate Costs per Participant and Number of Participants, by Grantee (2012 to End of Grant)

Note: Grantees were promised anonymity in results displayed, so grantees are identified as plot points in this graph, and not by name. A dot represents each grantee, plotted according to the number of new participants it enrolled from the beginning of 2012 through the end of the grant period (vertical axis) and its costs per new participant (horizontal axis). Costs per new participant are shown next to the dots and were calculated by dividing expenditures from 2012 through the end of the grant period by the number of new participants enrolled during this time (according to the PMT). Costs per new participant are upper-bound estimates because some money spent after the beginning of 2012 was used to serve participants who enrolled in prior periods.

Sources: Quarterly financial reports and PMT data submitted by grantees.

Per-Unit and Net Program Costs

The prior section showed total grant costs, not confined to costs spent during the period of RA. This section, by contrast, focuses on unit and net costs for serving SCA participants who were randomly assigned — that is, those expected to be provided individualized SCA services during the period of RA. Exhibit III-5 shows unit costs — the cost per person who received the service

— based on data that the grantees provided us for each service.[25] Note that not all grantees provided data for each service type, either because they did not fund that service through their SCA grant[26] or because they were otherwise unable to provide the necessary cost detail.

For each service type, the exhibit shows average unit costs (that is, unit costs averaged across grantees able to provide data), the costs for the grantees with the lowest and highest unit costs, and the number of grantees that provided unit cost information. Inpatient substance abuse treatment had the highest average unit cost, at more than $7,000. The average case management unit cost was also high, but was quite variable across the grantees, from a low of $767 to a high of more than $8,000.[27]

Exhibit IV-5: Unit Costs for SCA Program Services, by Service Type

Service Category	Average Unit Cost	Minimum Unit Cost	Maximum Unit Cost	Number of Grantees with Data
Case management	$2,594	$767	$8,304	7
Cognitive behavioral therapy	$410	$309	$508	4
Mental health treatment	$1,606	---	---	1
Employment assistance	$1,489	$363	$2,435	5
Education services	$197	$165	$228	2
Substance abuse treatment				
Inpatient	$7,149	$7,097	$7,201	2
Outpatient	$2,729	$152	$8,282	4
Prosocial activities	$990	$584	$1,396	2
Housing assistance	$1,811	$30	$4,888	5

Note: Unit costs represent the cost per SCA participant who received the service. The average unit cost for case management was calculated after excluding one grantee whose case managers also provided substance abuse treatment.

Source: Cost data provided by grantees.

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[25] In general, unit costs were assumed to be the same for program and control group members, except in the few instances (mostly restricted to case management services provided by DOCs) where grantees provided separate unit costs for the two groups. The numbers shown in the exhibit are unit costs for SCA participants.

[26] As summarized in Chapter II of this report, grantees provided many services through unfunded referrals. The grantees would have no way of estimating unit costs in these cases, since they did not fund the activities.

[27] Case managers for the grantee with the highest case management unit cost were licensed substance abuse counselors. Therefore, some of the costs it considered case management costs could have been spent delivering a wider array of services than case management alone.

We next calculated service costs per enrollee by multiplying the unit costs by the service participation rates as estimated from the survey. These participation rates, calculated separately for the program and control groups, were shown in the first two columns of Exhibit II-7 presented in the prior chapter, and represent the study participants' self-reports of whether they received a given service anytime in the 18 months after RA, regardless of who provided the service and whether or not the service was funded by SCA. Results showed that the program group received significantly more services than the control group did for services of nearly every type. This differential comes about because the grantees funded some services out of their SCA grants, which improved access, and because the SCA case managers facilitated access to services that existed in the community, even services that SCA did not directly fund.

These participation rates and the unit costs shown in the table above were multiplied together to calculate average service costs per enrollee, shown in Exhibit III-6; we used the actual unit costs for a service for grantees that provided a unit cost, and the average unit cost otherwise.[28] This calculation yielded *gross service costs* per study participant. Whereas the unit costs presented earlier represent the costs per person who received the service, gross service costs per study participant represent the costs per person regardless of whether the person received the service.[29]

The first two columns of Exhibit III-6 show the service costs per program group member and per control group member, calculated as described above. Inpatient substance abuse treatment was the service with the highest average cost per study participant. This is heavily driven by a high unit cost (an average of more than $7,000, as shown earlier), which outweighs this service's modest utilization rate (38 percent for the program group, according to Exhibit II-7 in the prior chapter). Case management also had a high average service cost; this service had both a high unit cost and a high rate of service utilization. The least costly services on a per person basis were those with low unit costs or low utilization rates, or both, including cognitive behavioral therapy and education services.

The final column of the exhibit shows the difference across the program and control groups in the average service cost per person. This represents our estimate of the *added* amount that was expended on each service to serve an average SCA participant, over and above what would

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[28]   Where available, we used separate unit costs for the control group rather than the ones shown in Exhibit III-5.

[29]   Two major categories of costs are omitted from these computations. First, we exclude service costs unlikely to be affected by SCA. For example, among grantees that were social services agencies, both program and control group members generally had to report to a PO as a condition of release, and case management through SCA was not provided by the PO. Therefore, we assume that SCA had no effect on these costs in these sites and are roughly equivalent for the program and control groups. Also excluded are the grantees' administrative costs of managing their SCA programs.

have been expended in the absence of SCA. The net cost summed across the services was more than $2,800 per SCA enrollee.

Exhibit III-6: Average Service Cost per Program and Control Group Member and Net Service Cost, by Service Type

	Service Cost per Program Group Member	Service Cost per Control Group Member	Net Service Cost
Case management	$2,476	$1,118	$1,358
Cognitive behavioral therapy	$242	$169	$74
Other mental health services	$512	$451	$61
Employment assistance	$1,000	$717	$283
Education services	$37	$38	-$1
Substance abuse treatment			
Inpatient	$2,694	$2,393	$283
Outpatient	$1,354	$1,011	$344
Prosocial activities	$345	$179	$166
Housing assistance	$359	$120	$240
TOTAL	$9,020	$6,194	$2,826

Note: The service cost per group member represents the unit cost of a given service multiplied by the service utilization rates shown in Exhibit II-7; these are costs per study participant regardless of whether the person received the service. Unit costs used in the calculation are the actual unit costs for the service for grantees that could provide this cost detail, and average unit costs (shown in Exhibit III-5) otherwise. A separate unit cost was used for the control group when it was available. The net service cost is calculated as the difference between the service cost for the program and control groups, and represents the average additional cost for serving one SCA participant.

Sources: 18-month survey and unit costs provided by grantees.

Summary

This chapter showed that grant amounts varied markedly across the grantees, from $1.5 million to $3.25 million. Grantees made different decisions about whom to count as SCA participants. Some used an expansive definition and enrolled many participants in SCA, which yielded a per person cost of no more than a few thousand dollars; others used a narrower definition of SCA participation, yielding a per person cost that was several times larger.

The average unit cost — the average cost of providing individual services — ranged from a high of $7,000 per service recipient for inpatient substance abuse transition to a low of less than $500 for education assistance and cognitive behavioral therapy. The net service cost — the

marginal cost of serving an individual SCA enrollee — was calculated to be approximately $2,800.

IV.   Longer-Term Impacts on Recidivism

The earlier impact report, summarized in Chapter II, found that the grantees' programs did not reduce recidivism measured 18 months after RA (D'Amico et al. 2017). However, most study participants were still incarcerated at the time of RA, and some were not released for six or more months after enrolling in the study. Thus, 18 months represents a relatively short post-release observation period.

This chapter uses administrative data collected for an additional year to extend the earlier findings. We measure recidivism as involvement with the criminal justice system in the 30 months after RA that led to re-arrest, reconviction or re-incarceration. The impact of the grantees' programs on re-incarceration measured for the full sample is the confirmatory analysis for this report; other analyses are considered exploratory.

Findings in Brief

- **Assignment to the program group did not reduce the probability of re-arrest, reconviction or re-incarceration.** In the 30 months after RA, those in the program group were no less likely than those in the control group to be re-arrested, reconvicted or re-incarcerated, their time to first re-arrest or re-incarceration was no shorter, and their total days incarcerated (in either prison or jail) were no fewer. Within the 30 months after RA:

    – Nearly 60 percent of those in both groups were re-arrested, and approximately 45 percent were reconvicted.

    – Approximately 60 percent were re-incarcerated; most of these were jail incarcerations.

    – Excluding the time between the RA date and initial release, study participants spent approximately 140 days incarcerated.

- **The program group showed a larger number of re-arrests and reconvictions.** The effect is small, but statistically significant.

- **There were, at best, modest subgroup differences**. Generally, being assigned to the program group did not reduce recidivism for any of the subgroups we examined. The one subgroup difference is that being in the program group may have increased involvement with the criminal justice system for those who were younger, but not for those who were older.

Impacts Overall

We have data on recidivism measured 30 months after RA for all study participants. We first discuss whether assignment to the program group had an impact on the timing of initial release from incarceration and, consequently, the duration of risk for recidivism within the 30-month observation period. We then examine impacts on recidivism for the full sample as measured for the 30 months from the date of RA.

Impacts on Time at Risk

The 18-month impact report noted that approximately 85 percent of study participants in both the program and control groups were incarcerated at the time of RA. That report also found that assignment to the program group had no impact on the timing of initial release, and that the average time to release was approximately six months for both groups.

Building off these findings, Exhibit IV-1 shows the cumulative percentage of participants by time at risk for the program and control groups. Reading off the graph, the value at the intercept shows that approximately 25 percent of each group had virtually the entire 30 months at risk; these individuals were either not incarcerated at RA or were incarcerated but were released very shortly afterwards. Approximately 60 percent of each group had at least 24 months at risk.

Exhibit IV-1: Months at Risk of Recidivism for Program and Control Group Members
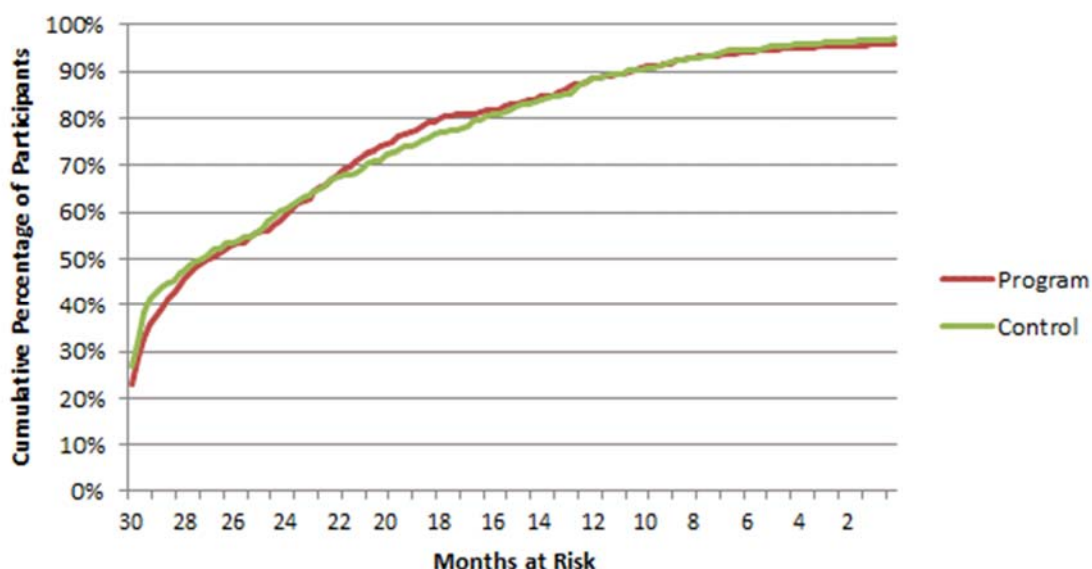
Note: The plotted lines represent the cumulative percentage of participants by the number of months that outcomes were observed following release from incarceration, which represents the time at risk of recidivism.

Source: Administrative data from state and local agencies.

The lines for the two groups are largely parallel, and the cumulative percentages do not differ significantly at any point along the distribution. Average time at risk is just under two years for both the program and control groups.

A condition for the IRB's approval for the study was that, for those randomly assigned while incarcerated, the study should not alter the duration or conditions of confinement. The study's RA procedures were established to ensure this requirement, and thus these results confirm our expectations.

Impacts on Recidivism Since Random Assignment

In a study using RA (or, indeed, any impact analysis), an event that occurs after services begin is viewed as endogenous, meaning that it could be affected by the treatment. Selecting on an endogenous variable can give rise to selectivity bias. For this reason, this chapter reports impacts on recidivism in the 30 months after the date of RA and not from when initial release from incarceration occurred.[30] Exhibit IV-2 presents these results, which are summarized below.

- Nearly 60 percent of both the program and control groups were re-arrested sometime in the 30 months since RA, with no significant difference between the groups.

- There was an average of approximately two arrests per person, with the average slightly higher for the program group than for the control group. Nearly half of both groups were arrested with a public order offense. Violent crimes were relatively uncommon.

- Approximately 45 percent were convicted of a new crime,[31] and the rate of reconviction did not differ significantly between the groups. Those in the program group had a slightly higher average number of convictions.

- Rates of re-incarceration do not significantly differ between the groups — approximately 60 percent of both groups were re-incarcerated in either a prison or jail.

- Including the time from the date of RA to initial release for those incarcerated at the time of RA, those in the study spent approximately 340 total days in either prison or jail during the follow-up period. Excluding the days incarcerated before initial release reduces the figure to about 140 days. There was no difference between the groups in the total days incarcerated.

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[30] The above section suggests that initial release from incarceration was not affected by assignment to the program group, so the likelihood that bias would arise by measuring recidivism from initial release was remote. Nonetheless, the study took the more conservative approach and measured recidivism from RA. However, as an exploratory analysis, we calculated impacts on recidivism measured from the date of initial release; these results are reported in Appendix D.

[31] Given that most individuals were incarcerated at the time of RA, we assume that convictions that occurred after RA were generally for new crimes. However, this assumption may not hold in all cases.

Exhibit IV-2: Impacts on Recidivism at 30 Months for the Full Sample

	Program	Control	Difference
Re-arrests			
Re-arrested (%)	59.3	58.1	1.2
Average number of re-arrests	2.2	1.8	0.4**
Re-arrests by offense type (%)[a]			
Violent crime	12.8	14.7	-2.0
Property crime	28.1	25.6	2.5
Drug crime	29.5	27.2	2.3
Public order crime	47.2	48.0	-0.8
Reconvictions			
Convicted of a new crime (%)	46.0	43.1	2.9
Average number of reconvictions	0.8	0.6	0.2***
Incarcerations (prison or jail)			
Was re-incarcerated in prison or jail (%)	60.0	59.4	0.6
Experienced a new jail incarceration (%)	51.7	50.3	1.4
Experienced a new prison incarceration (%)	29.9	30.6	-0.7
Total days incarcerated[b]	344.5	341.7	2.8
Days incarcerated after initial release[c]	142.4	138.6	3.9
Sample Size	**606**	**360**	

Note: Numbers in the first two columns represent outcomes measured for the 30 months following the date of RA for the program and control groups; the third column represents the difference between the first two columns.

Source: Administrative data from state and local agencies.

[a] The sum across categories exceeds the percentage ever arrested because individuals could have been arrested more than once and with different arrest charges in the 30-month follow-up period.

[b] For those incarcerated at the time of RA, total days includes days incarcerated following RA but before release.

[c] Days incarcerated after initial release excludes time incarcerated from the date of RA to initial release.

*/**/*** Statistically significant at the .1/.05/.01 level.

Evidence that the grantees' programs may have increased the number of arrests and convictions is at first glance surprising. However, the difference between the groups is small. Further, prior research shows that increased supervision — of the kind that might be associated with the increased case management of the program group — can increase the likelihood of catching new offenses and violations of the terms of parole or probation when they occur (Jalbert et al. 2011; Taxman 2002).

We also calculated the time elapsed from the date of RA to the first re-arrest or re-incarceration. The cumulative frequency distributions of the first occurrence plotted by time elapsed are shown in Exhibit IV-3 for both the program and control groups. The trajectories run almost completely in parallel and the gap between the groups is very small. There are no significant differences between the groups in the cumulative percentage with an occurrence at any time during the 30 months.

Overall, although the extent of recidivism is obviously greater with the longer follow-up period, the general conclusions about the effects of assignment to the program group are very consistent with those reported in the 18-month impact report.

Impacts for Subgroups

We calculated impacts on recidivism for each of the five subgroups described in Chapter I. Exhibits IV-4 and IV-5 show these results. The numbers tabulated in the exhibits are the *impacts* of being assigned to the SCA program group — that is, the *difference* in outcomes between those assigned to the program and control groups *within* each subgroup. Asterisks denote whether this difference is statistically significant, and the † symbol denotes whether the difference in the impact of being assigned to the program group is significantly different between the subgroup pair (for example, whether being assigned to the program group has a different effect for females than for males).

Exhibit IV-4 presents impacts for the first three subgroups — those categorized by gender, age and relative risk of recidivism. Assignment to the program group did not significantly improve desistance for any of these subgroups, and the effect of the program was generally consistent for males and females and for those at lower and higher relative risks of recidivism. However, there is some evidence that among younger participants — but not those who were older — being assigned to the program group increased re-incarceration rates (both prison and jail) and led to more re-arrests.

Exhibit IV-5 shows the results for the two remaining subgroups. The program's impact is not significantly different between those randomly assigned well before release from incarceration and those randomly assigned nearer release or after release. Grantees that are associated with the state or local criminal justice system appear to be about as effective as local social services agencies.

For exploratory purposes, we also estimated site-specific impacts on recidivism — that is, separate impacts on re-arrest, reconviction, and re-incarceration for each of the seven grantees. Because of the much-reduced statistical power, we used a threshold of .10 for identifying significant effects. Using this test, no grantee's program significantly reduced

Exhibit IV-3: Risk Curves for Re-arrest and Re-incarceration in the 30 Months after RA for Program and Control Group Members

Arrest Failure Curves

Incarceration Failure Curves
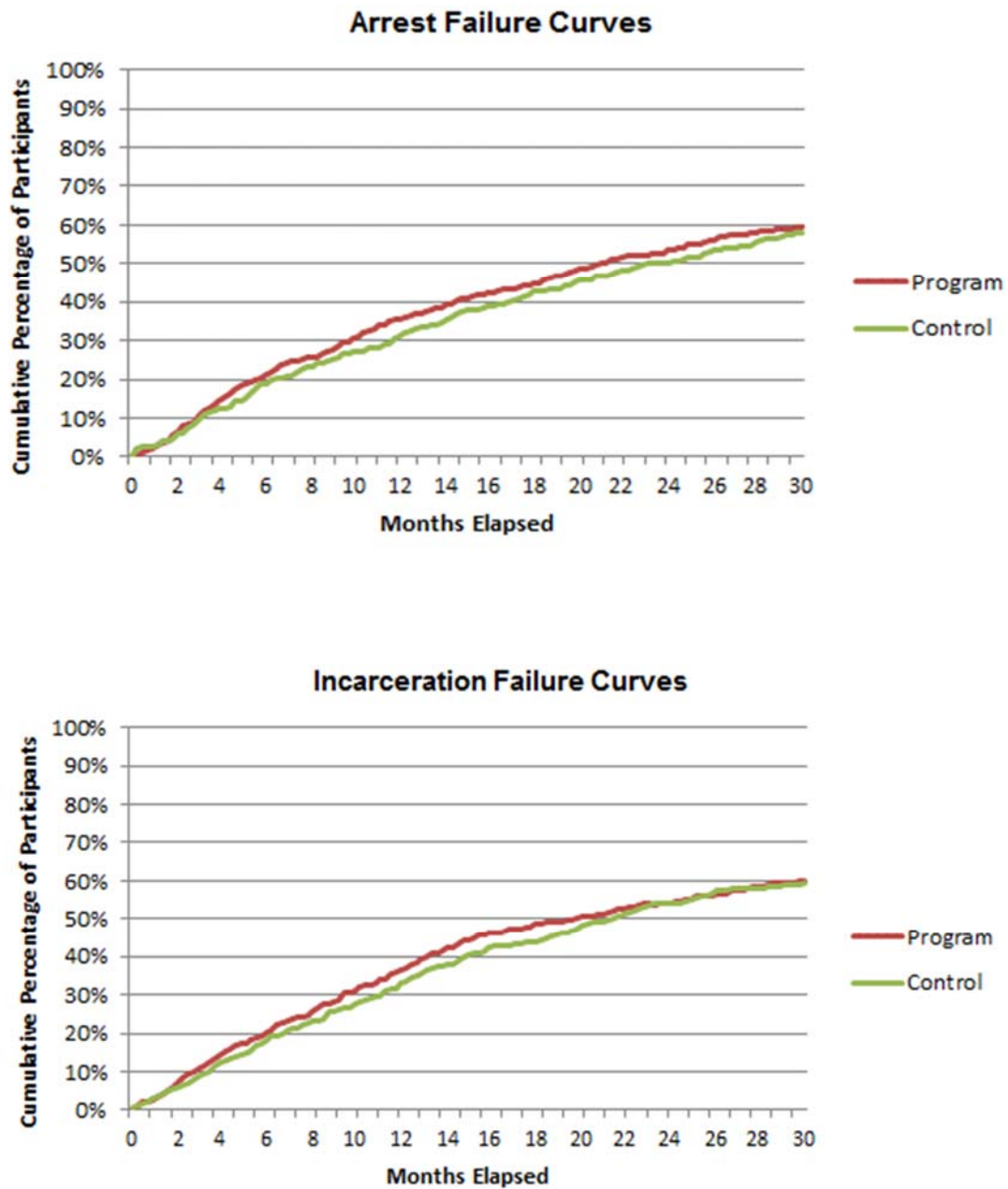
Note: The plotted lines represent the cumulative percentage of participants by the first occurrence of the event following the RA date.

Source: Administrative data from state and local agencies.

*/**/*** Statistically significant at the .1/.05/.01 level.

Exhibit IV-4: Impacts on Recidivism at 30 Months, by Gender, Age and Risk Subgroups

	Gender		Age		Level of Risk	
	Female	Male	Younger than 30	30 or Older	Lower	Higher
Arrests						
Arrested (%)	4.5	0.5	5.9	3.3	4.0	0.2
Average number of arrests	0.6*	0.3	0.8***†	0.1	0.5**	0.4
Arrests by offense type (%)						
Violent crime	-4.8	1.1	0.7	4.0	4.0	2.8
Property crime	12.1***†	0.1	6.6 -	1.4	6.3	1.1
Drug crime	7.0	1.1	7.7	2.5	4.5	2.1
Public order crime	0.3 -	0.8	4.9 -	6.2 _	0.6	0.8
Convictions						
Convicted of a new crime (%)	5.8 -	2.3	9.1 -	2.0	6.5	2.6
Average number of convictions	0.1	0.2**	0.3***	0.1 _	0.2**	0.2
Incarcerations (prison or jail)	-		-			
Was re-incarcerated in prison or jail (%)	4.3	0.1	12.1**†	8.5	0.9	4.8
Experienced a new jail incarceration (%)	6.3*	0.2	13.4**†	8.6	2.1	2.9
Experienced a new prison incarceration (%)	5.3	2.0	8.8†	7.9	0.7	1.6
Total days incarcerated[a]	2.2	7.9	0.1 -	15.9 _	11.5	2.9
Days incarcerated after initial release [b]	25.5 -	2.4	31.3 -	18.6	14.3	6.4
Sample Size	**203**	**763**	**440**	**526**	**466**	**464**

Note: Numbers in the exhibit represent the impact estimates. A positive number denotes that the incidence or mean value is higher for the program group than for the control group; a negative number denotes that the incidence or mean value is higher for the control group. Subgroups are defined in Chapter I.

Source: Administrative data from state and local criminal justice agencies.

[a] For those incarcerated at the time of RA, total days incarcerated includes days incarcerated following RA but before release.

[b] Days incarcerated after initial release excludes the time incarcerated from the date of RA to initial release.

*/**/*** The difference between the program and control groups within the subgroup is statistically significant at the .1/.05/.01 level.

† The difference in the impact of the program between subgroups in the subgroup pair is statistically significant at the .05 level (the symbol is placed by the impact estimate of the first group of the subgroup pair).

Exhibit IV-5: Impacts on Recidivism at 30 Months, by Timing of Entry and Grantee Type

	Timing of Entry		Grantee Type	
	Well Before Release	Nearer to Release	Criminal Justice Agency	Social Services Agency
Arrests				
Arrested (%)	3.1	1.6	4.0	-4.7
Average number of arrests	0.4	0.5*	0.4	0.3*
Arrests by offense type (%)				
Violent crime	-1.6	-1.9	-0.9	-4.2
Property crime	3.3	2.7	1.6	4.2
Drug crime	5.2	-0.5	2.7	1.5
Public order crime	1.3	0.5	2.2	-7.0
Convictions				
Convicted of a new crime (%)	5.3	1.4	2.5	3.7
Average number of convictions	0.2**	0.2**	0.2**	0.1*
Incarcerations (prison or jail)				
Was re-incarcerated in prison or jail (%)	5.3	-5.4	0.0	1.8
Experienced a new jail incarceration (%)	7.2	-6.3	2.7	-1.2
Experienced a new prison incarceration (%)	2.4	-4.0	-1.4	0.9
Total days incarcerated[a]	-12.4	-20.1	0.6	7.2
Days incarcerated after initial release [b]	19.9	-19.9	4.7	-2.3
Sample Size	**594**	**372**	**642**	**324**

Note: Numbers in the exhibit represent the impact estimates — that is, the difference within each subgroup between the incidence or mean value for the program group versus the control group. A positive number denotes that the incidence or mean value is higher for the program group than for the control group; a negative number denotes that the incidence or mean value is higher for the control group. Subgroups are defined in Chapter I.

Source: Administrative data from state and local criminal justice agencies.

[a] For those incarcerated at the time of RA, total days incarcerated includes days incarcerated following RA but before release.

[b] Days incarcerated after initial release excludes the time incarcerated from the date of RA to initial release.

*/**/*** The difference between the program and control groups within the subgroup is statistically significant at the .1/.05/.01 level.

† The difference in the impact of the program between subgroups in the subgroup pair is statistically significant at the .05 level (the symbol is placed by the impact estimate of the first group of the subgroup pair).

recidivism on any outcome. Further, for each grantee, point estimates (that is, the coefficients denoting the sizes of the impacts) were positive on some outcomes but negative on others. Thus, none of the seven programs seemed to perform consistently better than the others. These results should be interpreted with caution, however, because the study was not powered to detect effects at the grantee level.

Summary

As of 30 months after RA, assignment to the program group did not improve desistance and may have slightly increased the number of re-arrests and reconvictions. This picture does not change appreciably for the various subgroups considered, except that being in the program group increased subsequent involvement with the criminal justice system among younger study participants but not among those who were older. These findings are very consistent with the findings reported in the 18-month impact report.

V. Longer-Term Impacts on Employment and Earnings

Although the nature of the relationship between employment and recidivism is not clear cut (Apel and Horney 2017; Tripoldi et al. 2009; Uggen 2000), there are strong theoretical and practical reasons for believing that helping the formerly incarcerated obtain employment can improve desistance (Agnew 2016; Duran et al. 2013; Uggen and Staff 2001). Accordingly, all the SCA grantees provided employment assistance as part of their programs, either directly or through formal or informal partnerships (see D'Amico et al. 2013 for details about program services). Using administrative data, the 18-month impact report found that the program group showed higher employment rates and earnings than the control group, but the differences were not statistically significant. This chapter extends that work by examining whether there are impacts on employment and earnings when outcomes are measured over a longer period.

Findings in Brief

- **Being in the program group increased employment.** Those in the program group were more likely to be employed in the seventh and eighth quarters after RA (covering approximately 22 to 27 months after RA). The difference is statistically significant in the seventh quarter.

- **Being in the program group increased earnings.** In each of those same quarters, those in the program group earned, on average, $780 to $1,000 more than those in the control group. This represents a boost in earnings of between 64 percent and 83 percent. The differences are statistically significant in both quarters.

- **There were modest differences across subgroups**. Assignment to the program group had larger effects on employment and earnings for those who were randomly assigned well before release from incarceration.

Data and Methods

The 18-month impact analysis measured employment and earnings from both the participant survey and NDNH administrative data, providing independent sources for measuring these important outcomes. However, the participant survey was only administered a single time, 18 months after each participant's RA date, so this chapter updates the earlier findings by using only NDNH data.

Due to restrictions on data access and time lags in data availability, we do not have NDNH data covering the several years following RA for any part of the sample. To maximize what data are available and build on findings already reported in the 18-month impact report, we concentrate in this chapter on employment and earnings during the seventh and eighth quarters after the quarter in which RA occurred (outcomes for earlier quarters were presented in the earlier report). Quarter 7 and Quarter 8 after RA correspond to a period covering approximately 22 to 27 months after each participant's RA date.[32] We have NDNH data covering these two quarters for those randomly assigned during the first 12 of the 15 months in which RA occurred (see Chapter I for the timing of RA). This subset covers 755 participants, or approximately 80 percent of the full sample. Using NDNH, we can measure whether a person was employed at any time during each of these two calendar quarters and how much they earned each quarter.[33]

Impacts Overall

The top panel of Exhibit V-1 shows the percentage of the program and control groups employed at any time in the seventh and eighth quarters following the RA quarter. The program group shows a higher employment rate than the control group in each of these quarters, and the difference is statistically significant in Quarter 7.

The bottom panel reports the average earnings in each of these quarters (those not employed in a quarter are treated as having zero earnings). Again, the SCA program group has a consistent advantage over the control group, with the former out-earning the latter by $780 to more than $1,000 per quarter. The gap is statistically significant and widens over time. An earnings advantage of this magnitude represents a boost of between 64 percent and 83 percent of the control group's earnings.

Exhibit V-2 shows the earnings differences but excludes those with zero earnings. Among those with earnings in a given quarter, the program group earned on average between $6,000 and $7,100 each quarter, while the control group earned between $4,200 and $4,600. By Quarter 8, the gap among earners was more than $2,800.

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[32] NDNH reports data in calendar quarters (that is, whether a person was employed in a calendar quarter and how much they earned that quarter). The study team treated the quarter in which RA occurred as Quarter 0, and measured employment and earnings in the subsequent quarters. This chapter reports outcomes measured for the seventh through eighth quarters after the RA quarter. For someone randomly assigned very early in Quarter 0, this period covers 22 to 27 months after the person's RA date; for someone randomly assigned very late in Quarter 0, it covers 19 to 24 months after the person's RA date.

[33] RA should ensure baseline equivalence between the program group and the control group for the subset randomly assigned in the first 12 of the 15 months of RA. We demonstrated baseline equivalence for the full sample (see Chapter I); to examine equivalence for this subsample, we compared the program and control groups on demographic characteristics, employment history and criminal history (prior to RA) for just this cohort. That analysis is presented in Appendix E and confirms baseline equivalence.

Exhibit V-1: Employment and Earnings in Quarter 7 and Quarter 8 after RA, by Study Group

Employment Rates
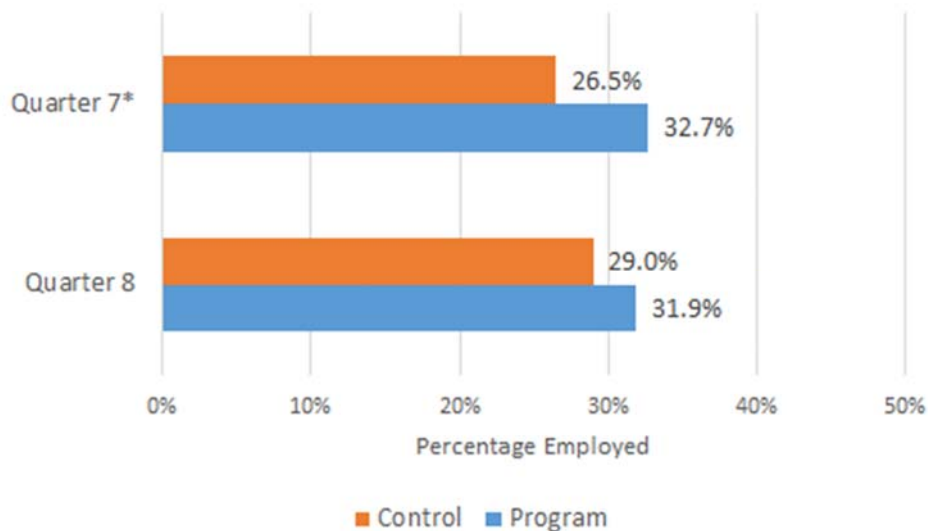
Average Earnings (Including Zero Earners)
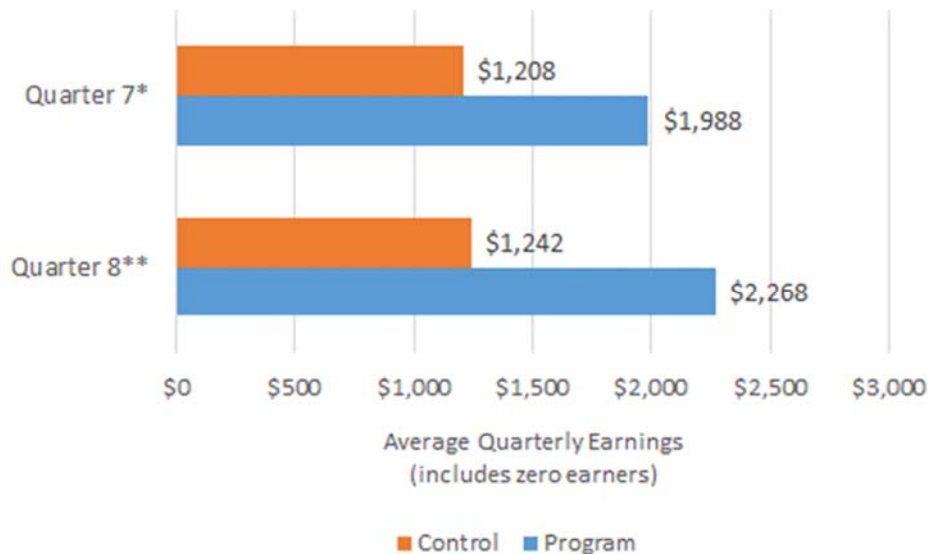
Note: Numbers represent the percentage of SCA program and control groups who were employed (in the top panel) and their average quarterly earnings (in the bottom panel) measured for the seventh and eighth calendar quarters following the quarter in which RA occurred. Those not employed in a given quarter were treated as having zero earnings. N = 755.

Source: Administrative data from NDNH.

*/**/*** Statistically significant at the .1/.05/.01 level.

Exhibit V-2: Average Earnings (Excluding Zero Earners) in Quarter 7 and Quarter 8 after RA, by Study Group
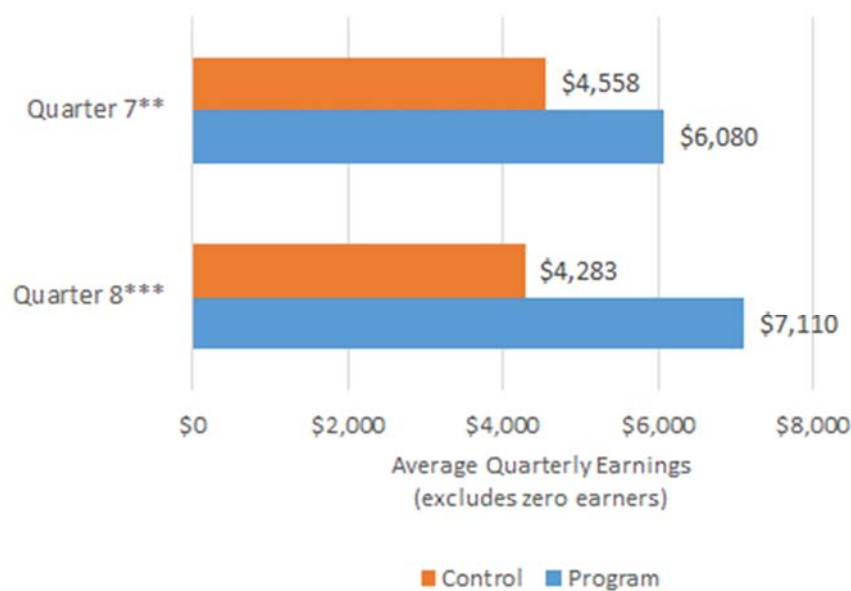
Note: Numbers represent average quarterly earnings measured for the seventh and eighth calendar quarters following the quarter in which RA occurred. Those without earnings in a given quarter were omitted from the calculation. Because of this sample subsetting, the RA design does not ensure equivalence on baseline characteristics and the differences in outcomes between the groups are only suggestive of true program impacts. N = 230 in Quarter 8.

Source: Administrative data from NDNH

*/**/*** Statistically significant at the .1/.05/.01 level.

In combination, these findings suggest that the earning gaps that we observed in Exhibit V-1 (bottom panel) come about both because the program group was more likely to be employed (top panel of Exhibit V-1) and because, among those employed, those in the program group earned considerably more (Exhibit V-2).

Impacts for Subgroups

Exhibit V-3 presents impacts for four of the five subgroups described in Chapter I.[34] The numbers shown in the exhibit are the *impacts* of being assigned to the SCA program group, not mean outcomes — that is, the number shown is the *difference* between outcomes for those assigned to the program and control groups *within* each subgroup. Asterisks denote that

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[34]   Due to limitations on data access, it was not possible to estimate impacts for the risk subgroups.

Exhibit V-3: Impacts on Employment and Earnings in Quarter 7 and Quarter 8, by Gender and Age

	Gender		Age	
	Female	**Male**	**Younger than 30**	**30 or Older**
Employment (%)				
Employed in Quarter 7	3.7	7.0*	5.0	7.7
Employed in Quarter 8	4.3	2.5	1.1	5.1
Average Earnings ($)				
Earnings in Quarter 7	325	944*	948	587
Earnings in Quarter 8	579	1,187*	1,310	711
Sample Size	**175**	**580**	**384**	**371**

by Timing of Entry and Grantee Type

	Timing of Entry		Grantee Type	
	Well Before Release	**Nearer to Release**	**Criminal Justice Agency**	**Social Service Agency**
Employment (%)				
Employed in Quarter 7	8.0*	2.4	5.3	7.6
Employed in Quarter 8	4.1	0.0	1.0	5.6
Average Earnings ($)				
Earnings in Quarter 7	1,007**	266	488	1,190
Earnings in Quarter 8	1,518***†	-226	426	1,860*
Sample Size	**566**	**189**	**437**	**318**

Note: Numbers in both exhibits represent the impact estimates — that is, the difference within each group between the incidence or mean value for the program group versus the control group. A positive number denotes that the incidence or mean value is higher for the program group than for the control group; a negative number denotes that the incidence or mean value is higher for the control group. For earnings outcomes, those not employed in a given quarter are treated as having zero earnings. Subgroups are defined in Chapter I.

Source: Administrative data from NDNH.

*/**/*** Statistically significant at the .1/.05/.01 level.

† The difference in the impact of the program between the subgroups in the subgroup pair is statistically significant at the .05 level (the symbol is placed by the impact estimate of the of the first group of the subgroup pair).

the difference is statistically significant, and the † symbol denotes the difference in the impact of being in the program group is significantly different for the subgroup pair (for example, being assigned to the program group has a different effect for females than it does for males).

With only one exception, being in the program group is associated with a greater probability of employment and higher earnings in both Quarters 7 and Quarter 8 for each of the eight subgroups — that is, almost all values in the exhibits are positive; however, due to small sample sizes, these impact estimates do not always attain statistical significance. There is some evidence that SCA had a larger positive impact on earnings in Quarter 8 for those randomly assigned well before their expected release from incarceration as opposed to those nearer to release.

Summary

This chapter examined the impacts of being assigned to the program group in the seventh and eighth quarters after the quarter in which each study participant was randomly assigned, covering approximately 22 to 27 months after the RA date. Over this time for the full sample, those in the program group were more likely to be employed and had higher average earnings, and the differences were generally statistically significant. Using administrative data, the 18-month impact report found that the program group had higher employment rates and employment and earnings than the control group, but the differences were not statistically significant. The updated findings therefore suggest that employment and earnings differentials in favor of the program group have widened over time.

Social control, rational choice, social learning, differential association, and strain theories all suggest that being employed can reduce the risk of recidivism (e.g., Agnew 2016), and employment is one of the mitigators of risk identified in the risk-need-responsivity (RNR) framework (Bonta and Andrews 2007). Given this strong theoretical foundation, it is curious that being in the program group had positive effects on employment and earnings but that this did not in turn reduce recidivism.

One explanation might be that the effects on employment and earnings were not large enough to translate into reductions in recidivism. Another explanation might be that, because of the stigma that comes from their prior criminal involvement, weak employment histories, lower levels of education, and other characteristics that made them hard to employ, the jobs that the formerly incarcerated obtained might generally have been for undesirable work. If so, it could suggest that work itself may not be as important in improving desistance as the types of jobs held. This conclusion would be consistent with Apel and Horney's (2017) finding that work that provides meaning has a strong effect on desistance, but not jobs generally, even those that pay well.

These reasons might also explain why programs that aim to reduce recidivism by providing employment assistance have shown, at best, mixed results. For example, in their meta-analyses, Drake et al. (2009) concluded that employment programs have small but statistically significant impacts on reducing recidivism, but MacKenzie (2008) and Visher and colleagues (2005) concluded that there are no such effects. Recent large-scale randomized controlled trials (RCTs) have similarly produced mixed results. For example, a study of the Center for Employment Opportunity, a transitional jobs program in New York City, found small effects on recidivism (Redcross et al. 2012), but a larger study of a transitional jobs program in four sites found no such effects (Jacobs 2012). A recent rigorous study of DOL's Re-integration of Ex-Offenders program, which provided job readiness training and job placement assistance, similarly found modest effects on improving employment but no effects on reducing recidivism (Wiegand et al. 2015).

It may be, then, that the effects that the programs in our study had on employment and earnings were not large enough to translate into reductions in recidivism, or that the types of jobs that those who found employment obtained were not of sufficient quality to deter criminal activity. It is also worth noting that significant effects on employment and earnings emerged late in the observation period; therefore, it is possible that the employment effects in our study were concentrated among a subset of participants already inclined towards desistance, suggesting that the direction of causality between employment and recidivism among the formerly incarcerated is not that clear (National Research Council 2008).

Ideally, we would investigate the relationship between employment and recidivism in more detail. Unfortunately, the terms of use dictated by HHS (the custodian of NDNH data) prohibit the linking of study participants' NDNH employment and earnings data with anything other than an indicator for the participants' group assignments (program or control), pre-RA characteristics (e.g., age, gender, race), and indicators for the subgroups. These restrictions make an analysis of the relationship between employment and recidivism infeasible with these data.

VI. Summary and Conclusions

This study estimated the longer-term impacts of seven programs that were awarded grants through an early round of funding authorized under the SCA Adult Demonstration Program. Impacts were estimated using an RA design. Individuals who had been screened and determined eligible for SCA were randomly assigned to either be allowed to receive individualized SCA program services (the program group) or to receive re-entry services normally available but not individualized SCA services (the control group). The differences in outcomes between the two groups were then compared. The study also examined the costs that grantees expended in operating their programs.

Summary of Impact Findings

The 18-month impact report (D'Amico et al. 2017) demonstrated that those randomly assigned to the program group were significantly more likely to receive a wide range of re-entry services. They were more likely to report that they received help with re-entry and to have individual case plans. They were also more likely to receive cognitive behavioral therapy, help with looking for a job, substance abuse treatment, housing assistance and mentoring. However, they also reported that they had many unmet service needs. In fact, 18 months after RA, more than half of both the program and control groups said that they wanted additional housing assistance, job placement assistance, job training, health services, and educational services.

The earlier report also examined impacts on recidivism and other outcomes measured 18 months after RA. At that point, those in the program group did not have less involvement with the criminal justice system regardless of whether recidivism was measured using survey or administrative data. There were also no notable impacts on employment and earnings, health-related outcomes or self-reported substance abuse.

The present report extends these findings by looking at outcomes for an additional year.

As of 30 months after RA, those in the program group did not show improved desistance. Those in the program group were no less likely than those in the control group to be re-arrested, reconvicted or re-incarcerated; their time to re-arrest or re-incarceration was no shorter; and they did not have fewer total days incarcerated (including time in both prisons and jails). Program group members were somewhat more likely to have more arrests and convictions; this greater frequency may have come about because enhanced case management for those in the program group could have increased the likelihood of catching new offenses when they occurred.

Towards the end of the follow-up period, the program group had higher employment rates and earnings. In the seventh and eighth follow-up quarters (approximately 22 to 27 months

after RA), the program group earned on average $1,800 more than the control group, which represents more than a 70 percent improvement over the control group's earnings. This difference comes about because those in the program group were more likely to be employed and, among those employed, earn significantly more.

There were no consistent differences across subgroups. We compared the estimated impacts across different subgroups — males versus females, those younger versus those older, those at lower versus higher risk of recidivism, those enrolled well before release versus those enrolled near or after release, and those served by corrections agencies versus social services agencies. There appear to be only modest differences in program impacts across these groups; that is, assignment to the program group worked about the same for each subgroup in the pair.

The study's major impact findings are robust to alternative model specifications. We estimated impacts as a simple difference in means between the program and control groups and using more complex statistical methods. The findings summarized above hold up in these alternative model specifications.

Summary of the Cost Analysis

Grant amounts varied markedly across the grantees, from $1.5 million to $3.25 million. The average unit cost — the average cost of providing individual services — was highest for inpatient substance abuse treatment, with an average unit cost of more than $7,000. Case management also had a high unit cost, with an average of $2,600. However, the unit cost for case management was highly variable across grantees. The average net service cost — that is, the difference in cost for serving an SCA participant compared with a person in the control group — was approximately $2,800.

Why Were There Impacts on Employment but not Recidivism?

SCA represented a substantial infusion of funds for these seven grantees, and this study has demonstrated that this led to a significant increase in service receipt for the program group and improved their employment and earnings. Why were there impacts on employment outcomes but not on recidivism? A number of general reasons can be suggested (although not every reason applies to every grantee).

1. ***The service differential was largest for employment-related assistance and more modest for other services.*** The percentage of the program group receiving employment-related assistance was nearly 20 points higher than the percentage of the control group. Service differentials for other services were generally statistically significant, but more modest in size. This suggests that control group members were often able to access similar services elsewhere. Even if the services were effective, the gap in service receipt

between the groups might not be large enough across the board to translate into differences in recidivism.

2. ***Given available SCA funding, there were limitations to what the grantees could do.*** Those returning from incarceration face challenges to re-entry that are many and complex. The grantees' services could not help participants fully overcome these challenges. Due to their resource constraints, all of the grantees relied heavily on informal referrals to provide many services. For services that were not SCA funded, program group members did not have priority access over anyone else who sought services. As a consequence, SCA participants reported many unmet service needs 18 months after RA.

3. ***There were inherent limitations to the programs that grantees developed.*** Although the grantees used evidence on what works in developing their programs, there were limitations to their service models.

    a. *Case management, even with reduced caseloads, has not been demonstrated to be effective*. All but one of the grantees emphasized case management as part of their SCA programs. For several grantees, this case management was provided by traditional POs who were given reduced caseloads. However, in their review of correctional rehabilitation approaches, Cullen and Gendreau (2000) cited evidence that "casework" has not been demonstrated to be very successful as a re-entry approach. Others have concluded that giving POs reduced caseloads does not by itself appear to reduce recidivism, and the increased supervision can increase revocation rates (Petersilia 1999; Jalbert et al. 2011). A study of re-entry programs that emphasized case management provided by community-based organizations also found no impacts on recidivism (Wiegand et al. 2015).

    b. *It was hard to ensure that participants got the services they needed.* Many services were provided through unfunded referrals, which had some clear advantages: this strategy conserved limited project resources and enabled grantees to draw on a wide network of community agencies experienced at addressing the many complex needs of those returning from incarceration. However, grantees did not always ensure that participants sought out the services to which they were referred, and the quality of services provided by loosely connected partners can be uncertain. For some grantees, participant retention seemed to be a problem.

4. ***Developing strong programs based on the RNR framework is difficult.*** Programs that address criminogenic needs have been shown to be effective in reducing recidivism (e.g., Latessa and Lowenkamp 2006). However, Bonta and Andrews (2007) argued that taking the RNR framework out of a tightly controlled setting and trying to widely use its principles in the real world tends to make the model much less effective. Furthermore, in their systematic review of the literature, Weisburd et al. (2017) noted that, while we generally know what works in reducing recidivism, the specific guidance that

practitioners need to convert principles into practice is often lacking. In short, implementing evidence-based practices and taking them to scale is not easy.

Conclusions

SCA grant funds helped grantees enhance their existing programs and strengthen their partnerships. Unquestionably, the funds were needed and they expanded capacity, which enabled a greater level of support to more individuals than would have been possible without the grant. Absence of evidence that these funds reduced recidivism to some degree highlights a well-known limitation of impact studies: If there are alternative sources of funds for services, then each source is important in expanding a community's capacity, but no one source is singularly impactful when compared to all the others (Heckman et al. 2000).

Nevertheless, improvements to the service models that the grantees developed might have led to better outcomes. Resources to fill all service needs remained well short of need, a finding echoed in parallel evaluations of other SCA programs (Lindquist et al. 2015). Partly for this reason but also for others, not all participants appeared to receive a continuum of care or adequate dosage of services, and grantees often had difficulty ensuring that participants received all the services they were assigned, particularly if the services were delivered by partners. Further, case management as a focal approach to re-entry has not yet been found to be effective, and it has challenges regardless of whether POs serve as case managers (Astbury 2008, Bonta et al. 2008) or social services agencies do.

Although resources are unfortunately likely to remain short, some important improvements are already underway. Even before these impact findings were made available, DOJ learned from the experiences of the grantees in this study — and from others that received early funding — and endeavored to improve program models for grantees that received subsequent waves of funding under the Adult Demonstration program (now called Smart Reentry). For example:

- To ensure adherence to evidence-based practices and the provision of meaningful re-entry services, grantees are required to complete a planning process before being approved for implementation funds. During this time, they are to work with a technical assistance provider to improve their program models.

- Grantees must engage with participants prior to release.

- Grantees are required to establish a memorandum of understanding with providers to ensure that there is a mechanism for follow-up when referrals are made.

- Grantees must ensure adequate dosage of cognitive-based interventions.

- Grantees are expected to engage a research partner to help develop actionable research to improve program practices.

With these modifications to grant requirements, this next generation of Smart Reentry holds promise for yielding more meaningful impacts.

Still, much work remains. As Rhine et al. (2006) have pointed out, successful implementation of evidence-based practices in concrete settings — critical if re-entry programs are to be effective in context — is challenging. Although there seems to be broad consensus about the general principles of effective strategies, having a more fine-grained understanding of what works best for whom in what context still seems far off. For example, although a number of meta-analyses or reviews have examined the role of employment assistance as a re-entry strategy (e.g. Drake et al. 2009; MacKenzie 2008), a more nuanced look forces us to recognize that, in fact, many types of employment assistance strategies have been tried — vocational training, transitional jobs, job readiness training, job placement assistance, and others — and their effects can by varied (Drury 2013). Who delivers the services can also have important implications for success. For example, as was discussed in this report, whether community re-entry services are provided by criminal justice agencies or social service agencies can have important implications, as each agency type has advantages and faces challenges with regard to building trust, minimizing participant attrition, accessing community service networks, and effectively connecting pre-release with post-release services. Other than the present study, there have been few other attempts to examine this important distinction (see Ndrecka 2014 for an exception). Moreover, program services are usually multi-faceted and are rarely delivered in isolation; in this context, confidently identifying the effect of individual program components can be extremely challenging (Jonson and Cullen 2015).

To effectively answer the many pressing research questions that remain, we need better data and better data systems. As evidence of this at the most basic level, many of the grantees in this study had difficulty providing us with MIS data on program participants, because their data systems for tracking participant services were rudimentary. As a consequence, they sometimes could not confidently tell us which pre- and post-release services program participants had received, sometimes even if their own agency had provided the service but most especially if a partner agency did. This gap not only hampers independent research, but makes it difficult for re-entry programs themselves to track participant progress, assess adherence to re-entry plans, evaluate dosage, and engage in continuous improvement. On top of that, we have the well-known problem of establishing consensus on definitional issues, even something as fundamental as how recidivism is to be measured (e.g., Harris et al. 2009).

In short, although we have come very far from the days of despair when it was felt that "nothing works," only improved data systems and an aggressive research agenda can truly advance our understanding of how best to put evidence-based principles into effective practice.

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Appendix A: Implementation of Random Assignment

This appendix describes the way RA was implemented. It first discusses changes to pre-existing eligibility rules that the grantees made to accommodate RA, and then discusses the mechanics of RA.

Changes to Eligibility to Accommodate Random Assignment

As discussed in Chapter I, each grantee had its own criteria for determining eligibility for SCA and its own service model. The evaluation endeavored to accommodate these existing differences so that it would be evaluating the programs as the grantees meant them to operate. However, the grantees did make some changes as the evaluation was introduced, mostly to increase the pool of eligible individuals recruited for the study. These changes were modest and included the following changes in three sites.

- *Allegheny County*. This grantee's original plan was to recruit individuals into its SCA program who were in jail and had at least six months remaining on their sentences. The grantee changed this to five months remaining when the evaluation was introduced and, to increase its pool of eligible individuals, conducted outreach to those incarcerated in alternative housing sites as well as jails.

- *Kentucky*. As the study was getting underway, the state tightened its criteria for granting discretionary release. Although this change was not influenced by the study, as a consequence of it the grantee was falling short of its enrollment targets for the study. Consequently, the grantee began recruiting from jails as well as prisons.

- *South Dakota*. At the outset, persons who met South Dakota's eligibility criteria for SCA were required to participate in SCA — that is, they were required to meet with a re-entry staff member who coordinated pre-release services, and they were assigned to an "enhanced PO" upon release. According to conditions established by the study's IRB, participation in the study needed to be voluntary. Therefore, South Dakota made participation in SCA voluntary.

There were no notable changes to eligibility or outreach caused by the study in Marion County, Oklahoma, San Francisco or San Mateo County.

The Random Assignment Process

Each grantee randomly assigned persons determined eligible for individualized SCA services. The process laid out by the study team required that, before RA, grantees were to provide a study orientation to applicants and obtain informed consent, and only then could they conduct RA.

Providing an Orientation and Obtaining Consent

To adhere to the IRB's requirements for conducting RA, every potential study participant needed to understand the research study and give their consent to participate. The research team helped the grantees to provide this study orientation by developing materials for them to use. These materials, which were reviewed and approved by the IRB, included the following:

- *A video.* Grantees were provided with a short video on a DVD, which they could play at study orientation sessions. The video described the purposes of the study, the RA process, and what data would be collected for the study on each person who was randomly assigned.

- *Scripts.* Scripts for explaining the study and a list of answers to frequently asked questions were provided.

- *Notification materials.* Some grantees notified individuals about the results of RA by written correspondence. The research team provided grantees with draft letters for them to use for this purpose if they desired.

- *Informed consent forms.* After receiving an orientation to the study, every person being considered for RA needed to give written consent to participate in the study before RA could occur. The consent form was developed by the study team and approved by the study's IRB. It covered, among other things, the purposes of the study, what information would be collected on study participants, how participants' data would be kept secure, and the benefits and risks of participation. Importantly, the form made clear that participation was voluntary, that the decision to participate would not affect conditions of incarceration or the likelihood of receiving parole or probation, and that individuals could drop out of the study at any time without penalty. Those who declined to sign the consent form were not enrolled in the study and could not receive individualized SCA services. (Grantees told us that no more than a few people declined to give consent, and no one dropped out after RA.)

In addition to providing the materials described above, the study team also provided each grantee with a customized procedures manual and delivered an in-person training on how to use the above materials and carry out the study's procedures.

The Mechanics of Random Assignment

Once the study orientation was provided and written consent was given, each SCA applicant completed the BIF. Next, RA occurred.

To ensure rigor in conducting RA, the study team developed an online system that the grantees were required to use. Each grantee staff member conducting RA was given a personal username and password to log into a secure virtual private network to access the online RA system. Once logged in, the staff person would enter a few pieces of information about the

person to be randomly assigned, such as name and date of birth. Once these fields were entered, the applicant would be randomly assigned instantaneously, and the staff member would be instantaneously notified of the applicant's group assignment.

During the period of RA, the grantee sent the signed consent forms and BIFs to the study team in approximately monthly batches using a traceable delivery service. The study team checked the forms to be sure that a signed consent form and BIF were provided for each person randomly assigned. Those who were randomly assigned but lacked a consent form were removed from the study — a total of seven individuals.

During the period when RA occurred, an individual from the study team was designated as the primary site liaison for each grantee and was available to provide help. The site liaison scheduled regular telephone calls with the grant manager at each site; the calls were weekly when RA first began and less frequent after a time. The purpose of the calls was to provide support, answer questions and troubleshoot problems that arose. Additionally, the study team monitored sample build-up through weekly reports generated from the RA system and checked periodically that the program and control groups were balanced on the BIF's baseline characteristics (as would be expected if RA was being carried out properly). Finally, during the site visits conducted as part of the implementation study, the liaison provided additional support on RA to grantee staff and observed at least one study orientation session to be sure that procedures were being followed correctly.

Appendix B: Methods of Analysis

This technical appendix describes the statistical methods used to estimate program impacts in the seven grantee sites. We first describe the methods generally, including the simple differences in mean outcomes presented throughout the main body of the report as the estimate of impacts and alternative methods used to test the robustness of the report's major conclusions to different model specifications. After describing the methods, the appendix concludes by presenting impacts on recidivism using these alternative approaches.

Statistical Models Used

The evaluation implemented a randomized controlled trial (RCT), whereby those screened and eligible for SCA within each of the seven grantee sites were randomly assigned to either the program group or control group. The RA, by design, enabled unbiased estimates of the impact of being assigned to the program group by generating program and control groups that should not systematically differ in any way except in their exposure to the program and things affected by it. RA eliminates any selection biases that might occur in studies using observational data (where the program and comparison groups may systematically differ in both observed and unobserved ways), which can bias impact estimates.

To verify that the program and control groups were comparable, means for both groups were contrasted on observable background characteristics measured at baseline (refer to Chapter I). These characteristics included participant age, racial and ethnic background, disability status, employment history, criminal record and educational attainment. Generally, the program group was not statistically different from the control group on these background characteristics — with similar equivalence expected for unobserved characteristics.

Using an ITT approach, impacts of the program were assessed by comparing the outcomes of those assigned to the SCA program group to outcomes for those assigned to the control group. In keeping with ITT, control group members could have accessed re-entry services from other sources, but could not receive individualized SCA services; conversely, not all those randomly assigned to the program group necessarily received all the SCA services that they needed. Thus, impacts are properly interpreted as the effect of being allowed access to program-group services relative to having access to whatever re-entry services were normally available from other sources. The experiences of the control group provide estimates of what would have happened to the program group had the program not been available to them.

Difference in Means

Given the RCT design and the resulting baseline equivalence, the difference in means on outcomes provides an unbiased estimate of the treatment effect. These mean differences were predominantly used as the impact estimates presented throughout this report.

Regression Analysis

Regression analysis was used to assess levels of statistical significance and as a sensitivity test in estimating impacts. This method adds covariates in a model estimating the treatment effect. Including covariates is beneficial to the extent the covariates are correlated with the outcome. If they are, regression adjustment increases the overall variation explained and reduces unexplained error, which can improve the precision of the estimate of the treatment effect (for continuous variables) and increases the power of statistical tests (Hernandez et al. 2004; Kahan et al. 2014).

Two types of regression models were used for this study: ordinary least squares (OLS) for outcomes that are continuous, and logistic regressions for outcomes that are dichotomous. While OLS regressions are appropriate for outcomes that are measured on a continuous scale, logistic regressions are needed for assessing binary outcomes, as the OLS analysis of them violates assumptions regarding the distribution of errors.

The regression models we used included a vector of individual characteristics measured at baseline, as represented in Equation 1.

$$\mathbf{Y}_n = \beta_0 + \beta_1 \text{Group Assignment}_n + \Sigma \beta_p \mathbf{X}_{pn} + \varepsilon_n \tag{1}$$

In this equation, Group Assignment is coded 1 for those assigned to the program group and 0 otherwise; β_1 provides an estimate of the treatment effect of SCA on outcome $\mathbf{Y}$; $\mathbf{X}_p$ represents each of the covariates p, with β_p providing the corresponding coefficients for these covariates; the error term (ε) represents the difference between the observed and predicted outcome for person n. Because regression adjustment improves statistical power, the simple differences in means reported in the main body of the report were assessed for statistical significance after using regression adjustments.

Following guidance in the literature for deciding which covariates to include (e.g., European Medicines Agency 2015), we focused on factors felt to be moderate or strong predictors of recidivism, the main outcome of interest in this study. Based on literature identifying static predictors of recidivism (see, for example, Gendreau et al. 1996), the variables we included were gender, age and indicators of criminal history, among others. Exhibit A-1 shows the variables we included and their summary statistics. Note that not all baseline characteristics reported in Chapter I were included in these regression models. Some of these characteristics were not known to be strong predictors of recidivism, were collinear with variables already

included or had modest amounts of missing data. The inclusion of these variables would not increase the explanation of variance and could introduce bias in the estimation of the treatment effect (to the extent that sample cases needed to be dropped due to missing data).

Exhibit B-1: Descriptive Statistics for Background Characteristics Used in Regression Models

Variable	N	Mean	Standard Deviation
Male (1=yes, 0=no)	966	79.0	40.8
Age (in years)	966	33.3	10.4
Hispanic (1=yes, 0=no)	966	9.7	29.7
African American (1=yes, 0=no)	966	31.6	46.5
Other non-white, non-Hispanic (1=yes, 0=no)	966	12.6	33.2
Has at least a high school diploma or equivalent (1=yes, 0=no)	956	76.1	42.6
Ever worked prior to RA (1=yes, 0=no)	966	91.8	27.4
Incarcerated at time of RA (1=yes, 0=no)	966	80.5	39.6
Total years incarcerated in 10 years prior to RA	966	2.8	2.5
Number of arrests in 10 years prior to RA	939	10.7	10.1

Note: In addition to the variables shown, a treatment dummy variable was also included, representing whether the individual was randomly assigned to the SCA program group. Of the 966 study participants, 929 (96 percent) had non-missing data on all these variables; these individuals were included in the models. Dichotomous variables in the table above were multiplied by 100 for ease of interpretation. Estimates are unweighted.

Sources: BIF, except "incarcerated at the time of RA," which was measured using administrative data and the BIF.

Hierarchical Linear Modeling

Hierarchical linear modeling (HLM) is useful in analyzing data when sample members are drawn within discrete units. HLM takes this hierarchical structure into account, correcting for the correlation of errors within the clusters and eliminating potential bias (typically downward) in the estimation of standard errors (Chaplin 2003). In this evaluation, HLM is used to account for the nested structure of participants within grantee sites.

The intraclass correlation coefficient (ICC) examines how much of the total variance in the outcome measure can be attributed to group identification and is calculated by dividing the group-level variance over the total variance (see Equation 2). A multilevel model is generally only required when the ICC is non-trivial (Lee 2000).

$$ICC = \frac{var(U_{0j})}{var(\varepsilon_{nj}) + var(U_{0j})}$$
(2)

The multilevel model used in this study is represented through the following multilevel equation:

$$\mathbf{Y}_{nj} = \beta_{0j} + \beta_1 \text{Group Assignment}_{nj} + \Sigma \beta_p \mathbf{X}_{pn} + \varepsilon_{nj} \tag{3}$$

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

Equation 3 is identical to Equation 1 but with the addition of a level-2 equation, which allows the intercept to vary by site *j* around an overall mean intercept.

For the purposes of a sensitivity analysis, the impacts of program group assignment using HLM are presented in this appendix, but site-specific effects were not estimated.

Survival Analysis

Differences between group means on key recidivism outcomes included in Chapter II served as indicators of the program's impact on recidivism. While this approach provides simple and easy-to-understand metrics, nuances in the information on times to an event, such as re-arrest, reconviction or re-incarceration, may be lost. For example, one individual (Individual A) might have been re-incarcerated one month after RA, and a second individual (Individual B) might have been re-incarcerated 29 months after RA. At the time of the 30-month follow-up period, Individuals A and B are both identified as having been re-incarcerated, even though there is a difference between them in their time to re-incarceration. To supplement the key recidivism outcome measures reported in Chapter II, survival analysis was conducted to examine the impact of assignment to the program group on the time until recidivism.

One approach to conducting survival analysis is using the Cox proportional hazards model (McNeil and Binder 2007). While RA of individuals to the program group should account for confounding variables, the hazard model we used includes covariates to account for baseline characteristics to improve estimation precision — similar to adding covariates in the regression analysis. The hazard model estimates a hazard ratio, which is the probability of an event occurring at a specific time, given that the event has not already occurred. The survival analysis assessed the impact of assignment to the program group on the time to first re-arrest, reconviction and re-incarceration during the 30 months following RA.

A hazard ratio of 1 indicates that those in the program and control groups have a comparable probability of recidivism; a hazard ratio less than 1 indicates that those in the program group who have not yet recidivated have a lower probability of recidivism in the next period compared with the control group; and a hazard ratio greater than 1 indicates that those in the program group who have not yet recidivated have a higher probability of recidivism compared with the control group. Using a hazard ratio of 0.75 as an example, a more precise interpretation is that an individual from the program group who has not already recidivated by a specified time has 0.75 times the chance of recidivism by the next specified time compared with an individual from the control group. The hazard ratio can be converted to a probability

(shown in Equation 4), which provides a more intuitive interpretation of the results (Spruance et al. 2004).

$$\text{Hazard Ratio (HR)} = \text{odds} = P / (1 - P) \qquad (4)$$

$$P = \text{HR} / (1 + \text{HR})$$

Therefore, a hazard ratio of 0.75 means that an individual in the program group who has not already recidivated has a 43 percent chance of recidivating before an individual in the control group.

The results of the survival analysis served as a robustness test for the impacts of assignment to the program group on re-arrest, reconviction and re-incarceration reported in Chapter II.

Results of the Sensitivity Analysis

The exhibits in Chapters II and III reported the treatment effect as the difference in mean outcomes for the program and control groups. Whether the differences between the groups were statistically significant was assessed using regression analysis, as described earlier in this appendix. As a sensitivity test, this section of the appendix estimates the treatment effect in the alternative ways discussed above.

Exhibit B-2 compares three different ways of estimating the impacts of assignment to the SCA program group on the recidivism outcomes discussed in Chapter II:

- The simple difference in means (these estimates are identical to the ones reported in Chapter II);

- Regression analysis with inclusion of the control variables listed in Exhibit B-1 and a treatment dummy variable; and

- HLM with inclusion of the same set of variables.

The table reports estimated treatment effects. Consistent with Chapter II, all these analyses used weight-adjusted data. For outcome variables that are dichotomous, logit models were used. Because coefficients from logit models are not readily interpretable, we converted the change in log odds associated with assignment to the program group to a change in predicted probabilities for the program and control groups evaluated at the mean value of all covariates. Because logit analysis models the outcome as a nonlinear function of the independent variables, the treatment effect is not a constant value for all values of the covariates. For this reason, the logit-adjusted estimate of a treatment effect can be expected to differ from the unadjusted difference in means and will often be higher (Austin et al. 2010). Therefore, impact

Exhibit B-2: Alternative Estimates of Impacts on Recidivism

Outcome Variable	Difference in Means	OLS/Logit w/Covariates	HLM with Covariates
Re-arrests			
Re-arrested	1.23	1.82	2.32
Average number of re-arrests	0.37*	0.36**	0.36**
Re-arrests by offense type[a]			
Violent crime	-1.96	0.17	0.04
Property crime	2.47	1.31	1.31
Drug crime	2.32	2.61	2.91
Public order crime	-0.77	-0.13	0.40
Reconvictions			
Convicted of a new crime (%)	2.92	3.02	3.37
Average number of reconvictions	0.18***	0.17***	0.17***
Incarcerations (prison or jail)			
Was re-incarcerated in prison or jail (%)	0.61	1.48	1.67
Experienced a new jail incarceration (%)	1.36	2.31	2.66
Experienced a new prison incarceration (%)	-0.66	-0.58	-0.66
Total days incarcerated[b]	2.81	8.37	6.08
Days incarcerated after initial release[c]	1.95	5.32	4.54
Sample Size	**966**	**929**	**929**

Note: Numbers represent the estimated treatment effect, with p-values shown in parentheses. The first column shows the simple difference in means between the program and control groups, with p-values calculated based on a t-test for the difference in means. The second column estimates the treatment effect after controlling for covariates; OLS was used for outcomes measured on a continuous scale and logit analysis was used for dichotomous outcomes. For logit models, the logit coefficient of the treatment effect was converted to the change in predicted probabilities associated with assignment to the program group when evaluated at the mean value of the covariates. The final column includes variants of the multivariate models that account for the nested nature of the data; they were estimated with a random intercept at the program level. Covariates used are the ones shown in Exhibit B-1.

Source: Administrative data from state and local agencies.

[a] The sum across categories exceeds the percentage ever arrested because individuals could have been arrested more than once and with different arrest charges in the 30-month follow-up period.

[b] For those incarcerated at the time of RA, total days includes days incarcerated following RA but before release.

[c] Days incarcerated after initial release excludes days incarcerated from the date of RA to initial release.

*/**/*** Statistically significant at the .1/.05/.01 level.

estimates shown in Exhibit B-2 should not be expected to be the same across the columns for dichotomous outcomes.

Results in Exhibit B-2 reveal no substantive departures from conclusions drawn in the main body of this report from any of these sensitivity tests. Specifically, assignment to the program group is not shown to significantly reduce recidivism for any of the recidivism outcomes using any of the estimation methods. Moreover, according to all three estimation methods, there is evidence that assignment to SCA significantly increases the number of re-arrests and reconvictions. Thus, neither the inclusion of covariates nor inclusion of a multilevel framework notably alters the conclusions already presented. The ICCs were assessed to determine the need for utilizing a multilevel model. Generally, the ICCs were non-trivial but modest.

Exhibit B-3 reports results from the hazard models on time to first instance of re-arrest, re-conviction and re-incarceration. The first column shows the results from a hazard model without covariates, and the second adds the covariates previously discussed. There were no statistically significant differences between the program and control groups on any of these measures using either model. These results are consistent with those presented in Chapter II (Exhibits II-1 and II-2), which showed no significant differences between the groups in the incidence of recidivism events and no differences in risk curves, providing further evidence of the robustness of the study's conclusions.

Exhibit B-3: Results from Hazard Models of Time to Re-arrest, Reconviction and Re-incarceration

Outcome Variable	Hazard Model no Covariates	Hazard Model w/Covariates
Time to re-arrest	1.073	1.094
Time to reconviction	1.123	1.130
Time to re-incarceration (jail or prison)	1.047	1.090
Time to jail re-incarceration	1.055	1.096
Time to prison re-incarceration	0.990	0.987

Note: Numbers are the hazard ratios associated with being assigned to the program group. Hazard ratios greater than 1 indicate that those in the program group have a higher probability of having the event occur in the next period given that the event has not yet occurred. For the final column, covariates used are the ones shown in Exhibit B-1.

Source: Administrative data from state and local agencies.

*/**/*** Statistically significant at the .1/.05/.01 level.

Appendix C: Defining Risk Subgroups

Some research has suggested that re-entry services are most effective for those at higher risk of recidivism and, in fact, in some cases can increase recidivism if targeted to low-risk individuals (Bonta and Andrews 2007; Cullen and Gendreau 2000; Latessa and Lowenkamp 2006; Lipsey and Cullen 2007). For this reason, we estimated impacts for subgroups defined based on the relative risk of recidivism.

The first step was to define the subgroups. Ideally, we would have drawn on the validated assessment instruments used by the grantees in determining access to SCA and developing service plans. However, these scores were not available to us.[35] As an alternative, we used a regression-based approach described by Kemple and Snipes (2001). This approach takes advantage of the fact that, because of RA, the control group constitutes a pool for whom the determinants of recidivism in the absence of SCA can be identified. There were five steps:

1. *Identify the key outcome of interest*. In our study, recidivism was measured in several different ways (e.g., arrests, convictions and incarcerations; severity of charge; number of instances) using both administrative and survey data. For purposes of defining the risk subgroups, we used as the key outcome whether the individual was ever re-incarcerated in the 18 months after RA, measured using administrative data. We chose this variable because it corresponds to the outcome measure used for the study's confirmatory analysis (see Chapter I).

2. *Identify determinants of re-incarceration.* In the absence of having data on dynamic risk factors, we used static risk factors associated with the "second generation" of risk assessments (Andrews et al. 2006). Explanatory variables we used included:

    a. Demographic characteristics, specifically age and gender, and

    b. Criminal history (measured prior to RA), including total number of times incarcerated (one, two to four, five or more times), whether incarcerated at the time of RA, and total days incarcerated in the 10 years prior to RA (divided by 365, to convert to fractional parts of years).

3. *Model the relationship between the outcome and the predictors*. We used logit analysis to estimate the relationship between the predictor variables and the probability of re-incarceration. As noted, this relationship was modeled based on the control group

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[35] Even if those scores had been available, a problem with using them is that the seven grantees used different assessment instruments. Although many instruments in general use have been shown to be comparable as predictors of recidivism (e.g., Gendreau et al. 1996; James 2015; Kroner and Mills 2001), different instruments may not yield the same measure of risk for a given individual (Baird 2009; Baird et al. 2013). Therefore, when used in a pooled sample, scores from different assessment instruments used by different grantees might not yield comparable evidence of risk.

sample only, because participation in SCA could temper the underlying risk of recidivism. Coefficients from the estimation are shown in Exhibit C-1.

4. *Apply the coefficient weights to create a risk score for each individual*. Coefficients from the model were used to estimate a risk score for each person in both the program and control groups.

5. *Divide the sample into risk subgroups*. The sample was divided into two roughly equal groups, a lower-risk group and a higher-risk group.

Zweig et al. (2010) noted that the above procedure tends to overpredict the probability of re-incarceration for the control group. To correct this problem, they recommend dividing the control group sample into two equal halves and estimating the logit model with one-half of the sample and defining the control group risk subgroup using the second half. However, in the current study, small sample sizes made this refinement infeasible.

Note that to be eligible for SCA, individuals needed to be determined to be at medium or high risk of recidivism, based on whatever assessment instruments the grantee used and given its target population (e.g., females versus males, those incarcerated in prison versus jail). Therefore, the risk groups we defined represent those at *relative* risk of recidivism within the study sample.

Exhibit C-1: Coefficients from a Logit Model Predicting the Probability of Re-incarceration within 18 Months after RA

	Coefficient	Standard Error
Intercept	.6158	.6640
Male	.6135	.2948**
Age	-.0452	.0136***
Incarcerated 2-4 times	.5810	.4148
Incarcerated 5 or more times	1.2009	.3938***
Incarcerated at RA	-1.1024	.3276***
Time incarcerated in the prior 10 years	.0357	.0482

Sources: Administrative data and the BIF.

*/**/*** Statistically significant at the .1/.05/.01 level.

Appendix D: Impacts on Recidivism Since Initial Release

For reasons described in Chapter IV, we measured recidivism for the 30 months since each study participant's date of RA. However, most study participants were still incarcerated on the date of RA, so the 30-month follow-up period includes some time prior to initial release. This time reduces the total time at risk for recidivism from 30 months to something much less than that for some study participants. Exhibit IV-1 in the main body of the report showed the distribution of time at risk. That exhibit also showed that assignment to the program group did not have an impact on the timing of initial release; therefore, the *impact* of being in the program group on recidivism is unlikely to depend on whether recidivism is measured from the date of RA or the date of initial release. However, *rates* of recidivism could be different. As a sensitivity test, we measured rates of recidivism and program impacts for the period following initial release, and we describe those analyses in this appendix.

A complication is that the observation period following initial release is highly variable. We have a minimum 30-month observation period following RA for all study participants. However, this 30 months includes less or more time following initial release, depending on the study participant's incarceration status at the time of RA and (for those incarcerated) the participant's initial release date.

To address this variability, we restrict the analysis in this appendix to the approximately 75 percent of the sample that we know had at least 18 months at risk, and we measured recidivism during this 18-month period.[36] Exhibit D-1 shows, by group assignment, the percentage re-arrested, reconvicted, or re-incarcerated by the 18th month after initial release for this sample. There are no significant differences between the groups on any of these measures of recidivism.[37] Somewhat more than 50 percent of both groups were re-arrested and re-incarcerated (in either prison or jail) sometime in the 18 months following initial release, and between 35 percent and 40 percent were reconvicted.

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[36] Time at risk could not be calculated for approximately seven percent of the sample, because they had missing data on the date of initial release.

[37] These are conditional outcomes because they are measured only for those who had at least 18 months at risk of recidivism. Therefore, the RA design does not ensure equivalence in baseline characteristics between the program and control groups, and differences in outcomes between the groups are only suggestive of true estimates of impacts.

**Exhibit D-1: Recidivism Rates and Impacts on Recidivism
in the 18 Months after Initial Release**

	Program	Control	Difference
Was re-arrested (%)	57.1	56.1	1.0
Was convicted of a new crime (%)	40.6	34.7	5.9
Was re-incarcerated in prison or jail (%)	57.6	53.9	3.8
Experienced a new jail incarceration (%)	50.2	47.0	3.2
Experienced a new prison incarceration (%)	26.8	24.5	2.3
Sample Size	**452**	**254**	

Note: The analysis is restricted to study participants whose outcomes could be observed for at least 18 months following their initial release from incarceration. Numbers in the first two columns show rates of recidivism measured for the 18 months following initial release; the third column represents the difference between the first two columns.

Source: Administrative data from state and local agencies.

*/**/*** Statistically significant at the .1/.05/.01 level.

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Appendix E: Further Test of Baseline Equivalence

Exhibit E-1: Background Characteristics of Program and Control Group Members Randomly Assigned in the First 12 Months of RA

	Program	Control	Difference
Demographic Characteristics[a]			
Gender			
Female	24.4	21.8	2.6
Male	75.6	78.2	-2.6
Race and Ethnicity			
White	51.6	49.4	2.2
African American	30.0	34.6	-4.5
American Indian/Alaska Native	14.4	13.1	1.3
Hispanic	11.7	10.7	1.0
Hawaiian Native/Pacific Islander	2.1	3.1	-1.0
Asian	1.1	2.2	-1.0
Age			
18 to 21	9.3	10.6	-1.3
22 to 25	18.2	19.9	-1.6
26 to 30	23.5	23.2	0.3
31 to 35	16.0	13.8	2.2
36 to 40	7.3	10.9	-3.7*
41 to 50	17.9	16.3	1.6
51 or more	7.8	5.3	2.5
Highest Degree Attained			
Less than high school degree or GED	25.3	23.8	1.5
GED	44.3	42.1	2.2
High school diploma	24.5	27.8	-3.3
Some college	5.9	6.3	-0.4
Employment Characteristics			
Worked sometime in the past	92.5	87.7	4.8**
Employment status at time of most recent incarceration prior to RA			
Was employed full time	29.5	30.3	-0.9
Was employed part time	15.3	16.5	-1.2
Was not employed	55.3	53.2	2.1
Other Characteristics			
Has a disability[a]	12.7	11.1	1.6
English as a second language[b]	99.0	98.5	0.5
Sample Size	**489**	**275**	

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Exhibit E-2: Criminal History of Program and Control Group Members Randomly Assigned in the First 12 Months of RA

	Program	Control	Difference
Number of separate times arrested in the 10 years prior to RA[c]			
1 to 2	17.3	19.0	-1.7
3 to 5	23.0	24.4	-1.4
6 to 10	27.0	25.0	1.9
11 or more	32.7	31.6	1.1
Most serious arrest offense in the 10 years prior to RA[c]			
Violent	52.6	53.8	-1.2
Property	35.0	33.5	1.6
Drug	10.1	11.3	-1.4
Public order	2.3	1.4	1.0
Number of separate times incarcerated in prison or jail any time prior to RA[a]			
1 time	11.8	13.2	-1.3
2 to 4	38.0	33.5	4.5
5 or more	50.2	53.3	-3.1
Type of crime for which most recently incarcerated prior to RA[a#]			
Violent	20.0	18.8	1.2
Property	35.9	31.0	4.9
Drug	42.7	50.8	-8.2**
Public order	29.0	26.6	2.4
Length of current or most recent sentence prior to RA[a]			
Less than 90 days	4.1	6.2	-2.1
At least 90 days but less than 6 months	8.3	9.1	-0.8
At least 6 months but less than 1 year	16.9	16.1	0.8
1 year to 2 years	23.6	21.0	2.6
More than 2 years	47.0	47.6	-0.6
Total days incarcerated in prison or jail in 10 years prior to RA[c]			
Up to 1 year	31.9	29.9	2.1
1 to 3 years	36.3	32.2	4.1
3 to 5 years	14.8	18.5	-3.7
More than 5 years	17.0	19.4	-2.5
Incarcerated on the date of RA[d]	84.5	80.3	4.3
Sample Size	**489**	**275**	

Note: The sample for both tables is restricted to the 764 study participants (489 in the program group and 275 in the control group) who were randomly assigned from the start of RA through December 31, 2012. This period covers the first four of the five calendar quarters of RA and includes 79.1 percent of all study participants. Numbers in the first two columns represent the percentage of participants in each group with the characteristic; the third column represents the difference between the two (program group value minus control group value). Estimates were weighted to equalize the odds of selection into the groups and, where appropriate, to account for potential survey response bias.

Sources: [a]=BIF; [b]=18-month participant survey; [c]=Administrative data; [d]=Both administrative data and the study's RA system.

[#] The sum across the categories exceeds 100 percent because multiple types could have been recorded.

[*/**/***] Statistically significant at the .1/.05/.01 level.