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An Evaluation of Seven Second Chance Act Adult Demonstration Programs: Impact Findings at 18 Months

18-Month Impact Report
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SPR SOCIAL POLICY RESEARCH ASSOCIATES

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2. Kentucky Department of Corrections
3. Marion County (OR) Sheriff's Office
4. Oklahoma Department of Corrections
5. San Francisco (CA) Department of Public Health
6. San Mateo County (CA) Division of Health and Recovery Services
7. South Dakota Department of Corrections

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Table of Contents

Abstract

This report describes the impacts of seven programs that were awarded grants 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 enroll in SCA, or a control group whose members could not enroll in SCA but could receive all other services generally available. Using survey and administrative data, each study participant was measured on a range of outcomes 18 months after random assignment.

Using their SCA funds, the grantees improved their partnerships with community agencies and strengthened the connection between pre-release and post-release services. All used their SCA funds to provide services after individuals were released from incarceration, and most also enhanced pre-release services. Services included education and training, employment assistance, substance abuse treatment, mental health services, cognitive behavioral therapy, housing assistance, and supportive services. Grantees provided some of these services using their SCA funds and others through unfunded referrals to community partners. 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. Because case management was the focal point of most grantees' efforts, the impact study primarily represents the influence of this service. Nonetheless, given the diversity of approaches taken by the grantees, this study does not provide a test of a single program model.

Impact findings show that those assigned to the program group were significantly more likely than those assigned to the control group to have received help with re-entry and were more likely to have had an individual case plan. They were also more likely to have received cognitive behavioral therapy, help with looking for a job, substance abuse treatment, housing assistance, and mentoring. However, many control-group members also received these services, and, at the end of 18 months, SCA participants were just as likely as those in the control group to report that additional services would have been helpful.

Being assigned to the program group did not reduce involvement with the criminal justice system in the 18 months after random assignment. Whether recidivism was measured using survey or administrative data, 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). Those

in the program group were somewhat more likely to have had probation or parole revoked and to have new convictions.

Being assigned to the program group also did not significantly improve employment outcomes and had no effect on other outcomes, including the adequacy of housing, health status, or the ability to meet child-support obligations.

One reason why impacts were not greater is that, although SCA significantly increased access to a wide range of services, the difference in service receipt between the program group and the control group was modest. Furthermore, SCA funds did not seem adequate to meet the many and complex needs of those returning from incarceration. Finally, most grantees emphasized case management as the key service strategy, 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 whether these enhanced requirements led to programs that were effective in reducing recidivism.

List of Acronyms

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

IRB = Institutional Review Board

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

PO = Probation or parole officer

RA = Random assignment

RCT = Randomized controlled trial

SCA = Second Chance Act of 2007

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, more than 600 grants have been awarded for programs serving adults under various categories of competition.[1] This report describes the 18-month impacts of seven programs 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 programs 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

This evaluation uses a random assignment (RA) design and administrative and survey data to study the impacts of these seven SCA programs. For the impact study, 966 individuals eligible for SCA were assigned to either:

- A *program group* whose members could enroll in SCA, or
- A *control group* whose members could receive all services otherwise available but could not enroll in SCA.

RA for the impact study 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 to the control group. Data on study participants are from a number of sources.

- *Baseline Information Form (BIF)*. Just before RA, all study participants completed a one-page BIF; this form asked about the individual's background and criminal history.
- *Data extracted from grantees' management information systems (MISs).* The grantees provided the study team with data showing which pre-release 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 18 months following RA.

[1] Cited from the Office of Justice Programs, Bureau of Justice Assistance website, accessed at https://www.bja.gov/ProgramDetails.aspx?Program_ID=90#horizontalTab2 on July 8, 2016.

- *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 interviews were completed with 82.3 percent of them (82.2 percent of the program group and 82.6 percent of the control group).

The research team also conducted site visits to the grantees to learn about program implementation, and a separate report describes those findings.

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: those defined by gender, age (under age 30 versus ages 30 or more), risk of recidivism (lower versus higher risk), length of time from random assignment to release from incarceration (RA was more than 30 days prior to release from custody 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 uses 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.

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, the grantees were expected to serve individuals 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. With their initial awards in fiscal year (FY) 2009 and continuation funding, the grantees each received from $1.5 million to more than $3.2 million in SCA Adult Demonstration funding. They were required to provide a match of 100 percent of their SCA award using state or local government funds, grantee or partner contributions, or other public or foundation funds.

The implementation study revealed several key points about the grantees and their programs.

SCA funds helped expand 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-release and post-release services.

The grantees were a diverse group and targeted different populations. 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.

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 at the time of SCA enrollment. Others relied heavily 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. 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.

Case management was a key service. Case management was a central 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 (i.e., 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 to SCA participants directly and through referrals. The grantees made a range of services available: education and training, employment assistance, substance abuse treatment, mental health services, cognitive behavioral therapy, pro-social services, housing assistance, and supportive services. The grantees provided some of these services directly. Other services were provided through a network of partners, sometimes on a fee-for-service basis but often through unfunded referrals. Where unfunded referrals were used, coordination with the SCA program was typically weak and case managers could not readily track whether participants received the services to which they were being referred.

Given the centrality of case management to the grantees' service models, the impact study primarily represents the influence of this service. However, given the diversity of approaches taken by the grantees, SCA as it operated in these sites does not represent a single program model.

Types of Services Received by SCA Participants

Grantees provided MIS data for those assigned to the program group. These data capture the services provided to SCA participants that the grantees knew about and entered into their data systems.

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 the grantees' MIS 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 service, and 24 percent received only pre-release services. Note that those not receiving pre-release or post-release services as part of SCA could have received those services from other sources.

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.

The length of participation in SCA varied greatly. Approximately 26 percent of those assigned to the program group participated in SCA for more than one year, and another 35 percent participated for more than six months. A little less than 40 percent participated for up to six months.

Impacts on Services

The logic underlying the SCA grant program is that SCA funds are used to provide more comprehensive and coordinated re-entry services than would be available 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, therefore, was to assess whether, in fact, those assigned to the SCA program group received more services than those assigned to the control group. Service receipt was measured through a follow-up survey administered to both the program and control groups and covered the 18 months after RA.

The program group was significantly more likely to receive help with re-entry. Those assigned to the program group were significantly more likely to report getting help with re-entry, and they were more likely to have an individual case plan. They were also more likely to report that they had someone who went out of the way to help them and to whom they could turn for advice.

SCA significantly increased a wide range of other re-entry services. Those assigned to the program group were significantly more likely to receive cognitive behavioral therapy, help with finding a job, substance abuse treatment, housing assistance, and mentoring.

SCA could not provide all the services that participants desired. Despite the fact that SCA had a significant impact on services received, the program group reported having many unmet service needs 18 months after RA. 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 wanted family reunification services, substance abuse treatment, and mental health services.

Impacts on Recidivism

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. These outcomes were measured using both administrative and survey data.

As of 18 months after random assignment, increased access to services for SCA participants did not lead to increased desistance. Whether recidivism was measured using survey or administrative data, those in the program group were not 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). 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.

Impacts on Other Outcomes

There were no program impacts on employment-related outcomes. In the seven grantee sites participating in this study, assignment to the program group did not improve the probability of being employed in the follow-up period. In the last six months of follow-up, those in the

program group earned an average of approximately $3,200 and those in the control group approximately $3,000, but the difference between the groups is not statistically significant.

SCA may have improved income adequacy. Study participants were asked about their income for the last month of the 18-month follow-up period. 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 that month.

There were no effects on a range of other outcomes. Using survey data, the study measured the adequacy of housing, health status, the self-reported incidence of illegal drug use and excessive alcohol consumption, and the ability to meet child-support obligations. Assignment to the program group had no effect on any of these outcomes.

Other Analyses

There are, at best, modest differences across subgroups. As an exploratory analysis, the study estimated program impacts separately across the different subgroups mentioned previously. Although there were some modest differences, impacts of assignment to the program group were about the same for all the subgroups.

The study's major findings are robust to alternative model specifications and data sources. Program impacts were calculated as a simple difference in means between the program and control groups and using more complex statistical models. For recidivism, the key outcome of interest, program impacts were also estimated using both administrative data and survey data, which provide independent estimates of desistance. The findings summarized above are robust to these alternative model specifications and data sources.

Conclusions

SCA represented a substantial infusion of funds for these seven grantees, and this study has demonstrated that this led to a statistically significant increase in service receipt for the program group. Why did these additional services not improve desistance? A number of general reasons can be suggested (although not every reason applies to each grantee).

1. **Control-group members accessed many of the same services that program-group members did, both before and after release**. Although SCA significantly increased access to a wide range of services, the difference in service receipt between the program group and the control group was modest—at most, the program group was 25 percentage points more likely to receive a given service than the control group. For example, 61 percent of the program group reported getting help with job-finding skills, but 40 percent of the control group also reported receiving this service. Even if the services were effective, the gap in service receipt between the groups might not be

large enough to translate into differences in recidivism or other outcomes. There are several reasons why so many control group members were able to access services.

a. *Control-group members had access to services available in prisons and jails after RA but while still incarcerated*. Most institutions had courses and workshops available to their inmates without regard to SCA eligibility. Depending on the institution, these services included substance abuse treatment, adult literacy instruction, employment assistance, cognitive behavioral therapy, and others. RA generally occurred while individuals were incarcerated, so the control group, just as the program group, had access to these services. Although SCA case managers who worked with SCA participants prior to release might have made special efforts to encourage program-group members to take advantage of these services, the services were generally available to those in the control group without restriction.

b. *A substantial proportion of control group members got help with re-entry from a PO or case manager.* According to the participant survey, 59 percent of the control group reported that they got help with re-entry (compared to 78 percent of the program group). Whether this help was provided by a traditional PO or someone else, this individual could have provided many of the same services that SCA case managers did: assessing service needs, offering advice, and providing referrals. From qualitative findings, we know that SCA case managers and POs were more involved than traditional POs were in brokering services, but the difference was one of degree.

c. *Grantees and their partners had other sources of funding, which were, in many cases, quite substantial.* All the grantees were required to leverage funds from multiple sources, which could include state and local funds and grants from philanthropic organizations or other sources. Similarly, the grantees' partners were existing organizations with their own funding sources and pre-existing outreach mechanisms. SCA funds, while much appreciated and valued by all the grantees and the partners the grantees funded, were often not the largest share of the organizations' budgets. These other sources of funds were not specifically earmarked for SCA participants and could have been used to serve control group members and others in need.

2. **Given available 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 could not help participants fully overcome these challenges.

a. *Funds were not adequate to directly fund all participants' needs.* Due to resource constraints, all the grantees relied on unfunded partners and informal referrals to provide some services. For services that were not SCA funded, program-group members did not have priority access over anyone else who sought services.

b. *At the end of 18 months, SCA participants had many unmet needs*. Perhaps because of the funding constraints, those in the program group reported many unmet service needs 18 months after RA, including the need for housing assistance, job placement

assistance, job training, health services, and educational services. In fact, at the end of the 18-month follow-up period, program-group members were just as likely to express the need for additional services as those in the control group.

3. **There were inherent limitations to the projects that grantees developed.** Although the grantees used evidence on what works in developing their programs, there were limitations to their program 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 POs who were given reduced caseloads; for others, it was provided by staff members from a social services agency or community-based organization. However, in their review of correctional rehabilitation approaches, Cullen and Gendreau (2000) cite 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).

    b. *It is hard to ensure that participants get the services they need through unfunded referrals.* As noted, many services were provided through unfunded referrals. Using unfunded referrals to provide services 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, its limitation was that there was often no way for the grantee to ensure that participants would seek out the services to which they were referred. Moreover, the quality of services provided by loosely connected partners can be uncertain.

    c. *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) argue 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. In their systematic review of the literature, Weisburd et al. (2017) note 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.

Despite these limitations, the SCA grants were meaningful; they helped the grantees enhance their existing programs and capacity and strengthen partnerships. 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 against all the others (Heckman et al. 2000).

At the same time, modifications to the service models that the grantees developed might help improve outcomes. Even before these impact findings were made available, the Department of Justice learned from the experiences of the grantees in this study and others that received early funding through the Adult Demonstration Program. Based on what it learned from the grantees' implementation experiences, it tightened requirements for grantees that received subsequent waves of grant 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 now 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 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 engage with participants prior to release.
- Grantees must ensure adequate dosage of cognitive-based interventions.

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

Next Steps for the Evaluation

The findings described in this report cover 18 months after RA. This represents a relatively short observation period. Many SCA participants in this study were enrolled in SCA while they were incarcerated and were not released from custody for six or more months after RA. Therefore, the *post-release* period covered by this study is much shorter than 18 months for many individuals. As a consequence, there was limited time during the post-release observation period for program- and control-group members to differentiate themselves. It is possible that these programs will turn out to be effective with a longer post-RA observation period.

The next step for the evaluation is to estimate impacts measured 30 months after RA, which may shed additional light on the programs' effectiveness. A report based on these data is forthcoming.

I. Introduction

The annual number of inmates being released from incarceration increased dramatically in recent decades, and rates of recidivism for the formerly incarcerated have been disturbingly high. In light of these facts, significant resources have been devoted to improving the success of re-entry for persons making the transition from incarceration to the community. This report describes the impacts of re-entry services provided by seven grantees that received an early round of funding through the Second Chance Act (SCA) Adult Demonstration Program. Impacts are estimated on services received, recidivism, employment and earnings, family stability, and other outcomes measured for the 18 months after individuals were determined eligible to participate in SCA. This chapter establishes the context for the study, presents the evaluation design, discusses limitations, and describes study participants. Subsequent chapters present the study's findings.

Background

At the end of 2014, approximately 6.85 million individuals were under some form of supervision by the U.S. adult correctional system, representing about 1 in 36 adults in the U.S. (Kaeble et al. 2016). The total figure includes more than 1.5 million adults held in state or federal prisons, approximately 745,000 confined in local jails, and more than 4.7 million under community supervision.[2] Although the total figure represents a substantial decline since the peak in 2007, about three and a half times as many adults were under some form of supervision as in 1980, when national estimates first became available (Glaze 2010). Moreover, flows are substantial—each year, there are more than 450,000 entries to parole and several million entries to probation (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. Substantial numbers lack a high school degree or equivalent (Harlow 2003) and many have problems with substance abuse and mental health or physical impairments (Petersilia 2003, James and Glaze 2006, Hammett et al. 2001, Mumola and Karberg 2006). Upon release, they have difficulty finding jobs for these reasons, and because of the stigma that comes with their status as having been incarcerated (Pager 2003, Holzer et al. 2004, Raphael 2014). Moreover, the formerly incarcerated tend to be released into a relatively small number

[2] The sum of the components exceeds the total because some individuals had multiple correctional statuses (see Kaeble et al. 2016).

of urban neighborhoods that are characterized by high rates of poverty and other social problems (Travis et al. 2001, La Vigne and Kachnowski 2003).

Not surprisingly given these challenges, about two-thirds of those released from state prisons are re-arrested 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 and 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 as a means of reducing corrections costs and as a strategy for addressing the interrelated problems of 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 awarded through over 600 grants to government agencies and nonprofit organizations under various categories of competitions.[3]

One category of grant awards consists of Adult Demonstration Programs, 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.[4] Grantees are expected to use validated and dynamic risk and needs assessments for purposes of delivering 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.[5] Grantees are expected to draw on this evidence in designing their programs.

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[3] These figures are cited from the Office of Justice Programs, Bureau of Justice Assistance website (accessed at https://www.bja.gov/ProgramDetails.aspx?Program_ID=90 on October 19, 2016) and include grants awarded as part of the Adult Demonstration Program as well as other SCA grant competitions, such as the Reentry Program for Adults with Co-occurring Disorders, the Adult Mentoring Program, and the Reentry Courts Program, among others.

[4] The number of awards in the SCA Adult Demonstration Program is as of January 2016, as cited at https://csgjusticecenter.org/wp-content/uploads/2014/08/SCA_Fact_Sheet.pdf, accessed on October 19, 2016.

[5] See the reviews by Seiter and Kadela (2003), Drake et al. (2009), and Cullen and Gendreau (2000). While lauding prior work for its insights, Petersilia (2004) notes the paucity of impact studies using rigorous methods.

About the Evaluation

This evaluation uses a random assignment (RA) design and administrative and survey data to study SCA Adult Demonstration grantees that were selected by BJA to participate in the study.

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 at the University of Chicago (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 services agencies.

As part of the evaluation of these programs, the research team was to:

1. Conduct an implementation study of the seven programs to learn grantees' strategies for developing their programs and the challenges they encountered in providing re-entry services.
2. Estimate the impacts of the grantees' programs on participants' recidivism, employment, and other outcomes, and calculate program costs.

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

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 (PA) Department of Human Services [Allegheny County]
5. Marion County (OR) Sheriff's Office [Marion County]
6. San Francisco (CA) Department of Public Health [San Francisco]
7. San Mateo County (CA) Division of Health and Recovery Services [San Mateo County]

Note: The shorthand names by which grantees are referred to in this report are shown in brackets.

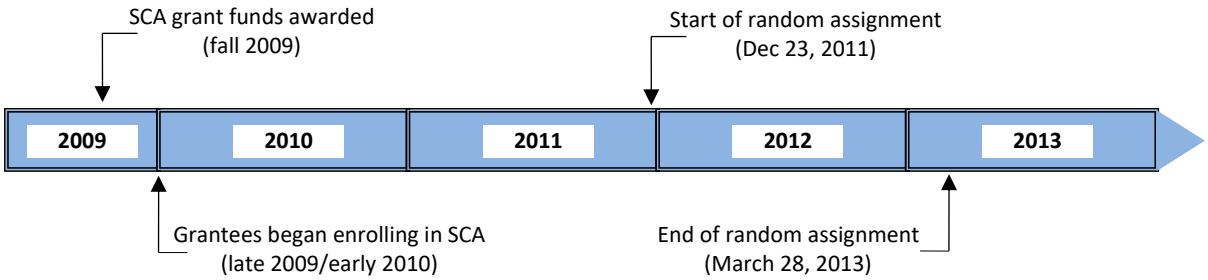
In estimating impacts, those determined eligible for SCA were randomly assigned to either:

- A *program group* whose members could enroll in SCA, or

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

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 timeline for the grantees and the study is displayed in Exhibit I-1. The exact date when RA started varied by grantee, and was contingent on each grantee's readiness to participate in the study. RA ceased when each grantee reached its enrollment target or by the end of March 2013, whichever occurred sooner.[6] All the grantees conducted RA for at least eight months; during study intake, 966 individuals were randomly assigned.

Exhibit I-1: Timeline for SCA and Study Implementation for the Study's Grantees



In addition to varying the dates when RA started and stopped for each grantee, the study team adapted RA procedures for each grantee in other ways so that the study would be assessing SCA as it was intended to operate in each site. For example, given their different funding levels and service designs, each grantee was assigned a different enrollment target. Further, the rate of random assignment 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 applicant pools in relation to their funding levels, most grantees were able to assign approximately 60 percent of those eligible for SCA to the program group and 40 percent to the control group. However, for two grantees, the study team randomly assigned approximately 75 percent of applicants to the program group. Exhibit I-2 shows the numbers that each grantee enrolled in the program and control groups. Appendix A provides more information about the mechanics of random assignment.

[6] The first grantee to start RA had its first applicant randomly assigned on December 23, 2011. The last grantee started on May 8, 2012. Random assignment ceased for some grantees in December 2012, and for the others by March 28, 2013.

The study team also let each grantee determine when in the transition from incarceration to release that RA occurred. Given their standard program designs, some grantees enrolled individuals in SCA six or more months before expected release from incarceration and provided pre-release services during that time. By contrast, other grantees generally relied on the institutions' pre-existing pre-release services and began what were uniquely 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, this means that 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, subject to the requirements of their grants (eligibility and service strategies are discussed in the next chapter).

Exhibit I-2: Number of SCA Participants in the Study, 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: Random assignment system.

Data Collection for the Impact Evaluation

The study team collected data for the implementation study through multi-day visits to each grantee site. For the impact study, data were collected from five additional sources:

1. *Baseline Information Forms*. All study participants completed a one-page Baseline Information Form (BIF) just before RA. The form asked about the individual's 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 numbers (IDs).

2. *Data extracted from the grantees' management information systems.* We asked each grantee to provide us with data extracted from its management information system (MIS) on the services that SCA program participants received. The data elements we requested represented a subset of those that grantees needed to report in the Performance Management Tool to meet the quarterly reporting requirements of their grants as stipulated by BJA. These elements included each participant's date of SCA enrollment and date of last service and indicators for which pre-release and post-release services that SCA participants received, including substance abuse treatment, mental health services, and employment services, among others. These data are only available for SCA participants, and not those assigned to the control group.

3. *Administrative data from state and local criminal justice agencies*. We forwarded participants' identifying information collected on the BIFs to state and local criminal justice agencies for matching with agency records. These agencies included departments of corrections, departments of justice, offices of the courts, sheriff's offices, and others. Depending on each agency's data system, the 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 up through September 2014, and again covering the period through September 2015. Using these data, we created measures of arrests, convictions, and incarcerations (both prison and jail) benchmarked to the RA date. One set of measures covered the ten-year period 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 data through 18 months after RA; this set constitutes key outcomes used in this analysis. (Data for the period through September 2015 were still being collected at the time this report was written. These data are used to create 30-month outcome variables; 30-month impacts will be described in a subsequent report.)

4. *Administrative data from the National Directory of New Hires*. The National Directory of New Hires (NDNH) is maintained by the Office of Child Support Enforcement (OCSE) of the U.S. Department of Health and Human Services (HHS). NDNH is built up from states' quarterly Unemployment Insurance (UI) program wage and claimant files, federal employment files, and the Directory of New Hires, and 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 to calculate study participants' employment and earnings for the period following RA.

5. *A follow-up survey*. The research team administered a follow-up survey to study participants to cover the 18 months following RA. All study participants were included in the survey sampling frame and interviews were completed with 82.3 percent of them (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. The latter includes recidivism (arrests, convictions, and incarcerations), employment (whether worked since RA, whether currently employed, wages), health status, housing status, family status, substance abuse, payment of child-support obligations, and other topics. Appendix B presents more detail on survey administration.

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 used together provide the opportunity to corroborate key findings using independent sources of evidence.

Estimating Program Impacts

This report presents the estimated impacts of the grantees' programs measured for the 18 months after each individual's date of random assignment.

General Approach

The study uses an intent-to-treat 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 are, on average, 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 SCA.[7]

We built off this simple approach in several ways. First, we have weighted the sample to account for the fact that the probability of assignment to the program group is not constant across the grantee sites. The weight 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

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

analyses to account for potential nonresponse bias (details of the survey weighting are described in Appendix B).

Because RA effectively neutralizes the impact of pre-existing characteristics, we calculate impacts as the simple difference in means between the program and control groups. However, we calculate whether these differences are statistically significant by using ordinary least squares regression models (for outcomes that are continuous variables) or logit models (for outcomes that are dichotomous), which take into account individuals' observed baseline characteristics, such as gender, age, and criminal history. 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.

This approach is used predominantly. However, some outcomes (e.g., date of first re-arrest following random assignment) 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 this general approach to test the sensitivity of the results to alternative model specifications. For example, we estimated hierarchical linear models that take into account the fact that study participants are nested within grantees. These models yielded very similar conclusions to the ones from the simpler models just described; to avoid needless complexity, the simpler models are predominantly used in this report. Statistical methods and results from the additional models are described in Appendix C.

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. The subgroup analysis is 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 subsets of the formerly incarcerated (e.g., Lipsey and Cullen 2007). Based on this research, we have identified the following key subgroups, each defined by 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

ability to respond to these needs may mean that these programs 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 the programs were more effective for study participants of different ages, we defined two subgroups: those less than age 30 and those ages 30 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 (see Chapter II). We do not have access to those scores, but instead follow the example of Kemple and Snipes (2001) in using simulations estimated on the control group 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]

Subgroups based on program design. Research also 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 before 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 define a subgroup that captures the potential importance of the timing of SCA enrollment.

- *Timing of SCA Entry.* To measure the possibility that the programs' impacts are greater when individuals are enrolled well before release rather than later, we define two

[8] However, see Wilson and Zozula (2011) as 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 who are scheduled for release from incarceration, but merely provides a relative ranking (based on static risk factors) of individuals in this sample.

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]

Subgroups based on grantee type. Some grantees were associated with the criminal justice system (i.e., DOCs or a sheriff's office), while others were health or social services agencies. For the first group, the key point of contact for participants accessing SCA services after release was generally a probation or parole officer (PO), whereas health and social services agencies assigned a case manager apart from the PO. This difference had important implications for the way re-entry services were delivered (D'Amico and Geckeler 2014) and represented a fundamental difference in program designs across the seven grantees in this study. Further, the two categories of agencies had different types of pre-existing partnerships, which had implications for their ability to leverage resources for different kinds of re-entry services (these differences are discussed in the next chapter). Accordingly, we define the subgroup below.

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

Confirmatory and Exploratory Analyses

We measure impacts on a range of outcomes relating to services received, recidivism, employment, family stability, and others. These impacts are 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 adopt 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 18 months after random assignment as the main outcome of interest, and considers

[10] Generally, the date of RA is coincident with or very shortly before the date of SCA enrollment for those study participants assigned to the program group. Note that even when RA occurred after release, it does not mean that the individual did not receive pre-release services; however, these services would have been provided apart from enrollment in SCA.

other analyses as exploratory. Further, we focus 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 the program and control groups, we can confidently detect a difference between the program and control groups on a binary variable if the difference is at least nine percentage points.[11] Thus, this study is powered to detect effects that are approximately that large.

Some analyses for this study are conducted on smaller sample sizes and therefore will have weaker statistical power. For example, results estimated from survey data are based on the 82 percent of study participants who responded to the survey. Additionally, some analyses are conducted on subsets of participants, and there is a very modest amount of missing data on some items.[12]

Limitations of the Study

The impact estimates presented later in this report represent differences in outcomes experienced by those randomly assigned to the SCA program group in comparison to those assigned to the control group. Although the research design ensures a rigorous and unbiased estimate of intent to treat, the interpretation of 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 were able to access the full range of SCA services, while those assigned to the control group could access other re-entry services but not SCA services. The study thus represents a comparison of the effectiveness of SCA in these sites relative to services otherwise available, and not in comparison to no services whatsoever. In service-rich environments, control-group members could have accessed significant services, even ones very comparable to SCA, from other sources.

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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 (the 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 calculated from administrative data are not reported if more than five percent of the group that should have data is missing on that item.

- *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 improve prison programming, such as by modifying pre-release classes or workshops that all those who were incarcerated could access on an 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.[13] The study thus captures the effect of the personalized services that SCA provided, but not the general system improvements.
- *The individual SCA program models 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. However, as will be described in more detail in Chapter II, grantees used their SCA funds to implement somewhat different program models.
- *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 the 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 SCA awards in subsequent fiscal years and under different categories of competition; we cannot generalize this study's results to those other grantees.
- *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 have involvement—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 study participants were expected to be released. This means that the administrative data used in the study miss involvement with the criminal justice system that occurred outside these jurisdictions.[14]

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

[14] Because we do not have data from other jurisdictions, this study likely under-reports recidivism to some degree. We attempted to access recidivism data through the National Crime Information Center (NCIC) maintained by the Federal Bureau of Investigation (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 the effect of potential measurement bias on the estimates of program impacts.

- Administrative data do not fully measure all criminal activity that occurred. Administrative data only capture events available in the state and local agencies' data systems; some events 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 incomplete if the identification information the study participant provided on the BIF were faulty or incomplete. As a result, criminal justice data may be missing for some individuals.[15]
- NDNH does not capture employment and earnings from self-employment and selected other sources.
- Survey data are self-reported and are therefore subject to recall or other respondent bias.

Fortunately, key outcomes are measured using both survey and administrative data, providing an opportunity to test the robustness of conclusions. Furthermore, outcomes are measured in the same way for both the program and control groups, which minimizes the role of reporting bias on the estimation of impacts.

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)
- Those returning to a specific community or neighborhood.

Once determined eligible for SCA participation according to whatever criteria each grantee established, the individual was provided an orientation to SCA program services and to the

[15] We assumed that everyone in the sample should have been arrested, convicted, and incarcerated in the ten 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 is very small.

random assignment study. Those who agreed to participate in both the program and the study were randomly assigned using an online random assignment 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 enroll in SCA (see Appendix A).

As was discussed, 966 individuals are included in the study, with 606 (62.7 percent) assigned to the program group and 360 (37.3 percent) assigned to the control group.[16] Exhibit I-3 shows key characteristics of program- and control-group members measured at the time of RA. By virtue of random assignment, we would expect those in the program and control groups to have very similar characteristics on average, and the exhibit shows that indeed they do. Results show that:

- Approximately 80 percent of study participants in each group are male.
- Approximately half are white and one-third are African-American.
- Approximately half of study participants in each group are 30 years old or less, and approximately one-fourth are older than 40.
- Approximately one-fourth had not obtained a high school diploma or general educational development degree (GED), and just under half 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 percent 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.

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

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

Roadmap to the Report

The remaining chapters describe the SCA programs that are being studied and present the impact estimates.

- Chapter II presents a summary of findings from the implementation study. It covers the amounts of SCA Adult Demonstration funding the grantees received, their grant management and partnerships, their targeting and enrollment practices, and the services they provided with their SCA funds. It also discusses how SCA changed the typical services that were available.
- Chapter III presents results tabulated from the grantees' MIS data, showing the types of services that SCA program-group members received and their duration of participation.
- Chapter IV presents impacts on the services that study participants received. It covers differences between the program and control groups on receipt of case management services, cognitive change therapy, employment and education services, housing assistance, substance abuse treatment, and other services.
- Chapter V covers impacts on recidivism. It measures arrests, convictions, and jail and prison incarcerations, with alternative measures created using survey data collected from study participants and administrative data collected from state and local criminal justice agencies.
- Chapter VI presents impacts on employment and earnings, using both survey data and NDNH data.
- Chapter VII presents impacts on a range of other outcomes, including physical and mental health, self-reported substance abuse, housing stability, and ability to meet child-support obligations.
- Chapter VIII presents a summary and conclusions.

The chapters presenting impacts (i.e., Chapters IV through VII) are structured in a similar way: we first present impacts for the full sample, and then for the various subgroups identified earlier in this chapter. Appendices present technical material, including details on random assignment, the survey administration and weighting, and estimation techniques.

Exhibit 1-3: Background Characteristics of Program and Control Groups

	Program	Control	Difference
Demographic Characteristics			
Gender			
Female	21.8	19.9	1.9
Male	78.2	80.1	-1.9
Race and Ethnicity[a]			
White	52.3	49.0	3.3
Black	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-Related			
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 is primary language[b]	98.7	98.8	-0.1

Notes: 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 response bias.

Source: Baseline Information Form, except where noted.

[a] The sum across the categories exceeds 100 percent, because some individuals indicated being of more than one race or being of Hispanic ethnicity and at least one race.

[b] Based on survey data

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

Exhibit 1-4: Criminal History of Program and Control Groups

	Program	Control	Difference
Number of separate times arrested in the 10 years prior to RA[a]			
1 or 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	37.4	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 time	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 12 months	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**

Notes: 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).

Source: [a]=Administrative data; [b]=Baseline Information Form; [c]=Both administrative data and the study's random assignment system.

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

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

II. About the SCA Programs

In 2013, SPR prepared an implementation study report that described the SCA programs developed by the seven impact-study grantees (D'Amico et al. 2013).[17] This chapter summarizes the findings of that report. It describes the overall organization of these seven grantees and the funding that supported them, their administrative structures and partnerships, their service models, the ways the grantees screened and enrolled participants, and the types of services that the programs provided. Finally, the chapter describes services that control-group members may have had available and how these services compared to those available to the program group.

Grant Funding and Use of Funds

BJA first awarded SCA Adult Demonstration funding to the study's grantees in FY 2009. It also awarded supplemental funding in FY 2010 and FY 2012, contingent upon the grantees' participation in the impact study. Exhibit II-1 identifies the seven grantees' grant amounts. However, funds for SCA programming were in actuality much greater than the amounts shown in the exhibit, because, following statutory requirements, the BJA FY 2009 grant solicitation specified a 100-percent matching requirement. At least 50 percent of the match needed to be made up of cash, and the rest of in-kind contributions. Sources for the match could include state or local government funds, grantee or partner contributions, or other public or foundation funds.

Exhibit II-1:
Grantees and their SCA Adult Demonstration Grant Awards

Grantee	Total	FY 2009	FY 2010	FY 2012
Allegheny County	$2,653,339	$608,339	$825,000	$1,220,000
Kentucky	$3,250,000	$750,000	$1,000,000	$1,500,000
Marion County	$1,502,768	$302,768	$400,000	$800,000
Oklahoma	$3,250,000	$750,000	$1,000,000	$1,500,000
San Francisco	$2,600,000	$600,000	$800,000	$1,200,000
San Mateo County	$2,937,674	$677,674	$900,000	$1,360,000
South Dakota	$3,249,749	$749,749	$1,000,000	$1,500,000

Source: Data provided by the Bureau of Justice Assistance.

[17] That report also included implementation study findings on three other grantees funded in FY 2009, but which did not receive continuation funding and which are not part of the impact study.

BJA expected grantees to use their SCA grants to fill gaps in their community's re-entry services. Following this directive, grantees blended their SCA grant funds with their matched amounts for three general purposes:

- *To provide service coordination for SCA participants.* Much of the grantees' funding was used to provide individualized case management and service planning for SCA participants.
- *To provide other services specifically or primarily for SCA participants.* Grantees used a portion of their funds to provide post-release services and, in some cases, pre-release services that were restricted to SCA participants or for which SCA participants had priority access.
- *To make general system improvements.* Two of the grantees (Kentucky and Marion County) used their SCA funds to make changes to, or provide partial support for, pre-release or post-release services that were generally available to anyone returning from incarceration. For example, one grantee used a portion of its funds to modify pre-release classes that anyone in the institutions could access. Because these changes were general system improvements, these services could have been accessed by those in the control group and their effects are not captured by the impact study.

Exhibit II-2 shows, for each grantee, the types of activities that SCA grant funds supported. The right-hand column of the exhibit highlights SCA program activities whose effects are being assessed through the impact study; these include all SCA-funded activities to which SCA participants had exclusive or priority access, but exclude the general system improvements in the two sites mentioned above.

The exhibit also shows that, broadly, case management with service coordination was the focal service for six of the seven grantees. As will be discussed, there were important differences among the six; for example, they differed in which type of organization provided the case management and which other services were funded with SCA grant funds. Nonetheless, brokering services through case management was the centerpiece in these six sites.

The seventh grantee (Marion County) operated quite differently. Although it provided case management, central to its program model was 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. Case managers were available to meet individually with participants as needed, but, because classes were structured and occurred virtually full time throughout the week, case management was more peripheral.

Exhibit II-2: Use of Funds and Focus of the Impact Study

Grantee	What SCA Grant Funded (in whole or part)	Captured by Impact Study
Allegheny County	• *Re-entry staff*, who provided SCA participants with assessments and service coordination, both pre-release and post-release, and developed participants' re-entry plans, and *family support specialists*, who worked to prepare families for the release of the inmate	√
	• *Pre-release and post-release classes*, including job readiness training, family support, cognitive behavioral therapy, and job placement assistance (SCA participants had priority for these classes, but others could participate on a space-available basis)	√
Kentucky	• *Reentry Parole Officers (RPOs)*, who provided parole supervision to SCA participants after release; RPOs had smaller caseloads than regular POs, but reporting requirements were the same	√
	• *Bus vouchers* that were provided to RPOs for disbursement to SCA participants as needed	√
	• *Prison re-entry coordinators*, who coordinated home placements	
	• *General system improvements*, such as upgrading instruments used for assessments and enhancing pre-releases classes, such as cognitive behavioral therapy and parenting	
Marion County	• *SOAR, a 12-week, full time, post-release course* with modules addressing topics of cognition, substance abuse, family support, job preparedness, and life skills; after the course, there are 12 weeks of "aftercare" classes, in 1-2 hour sessions	√
	• *Quest for Change House*, a living facility for SOAR participants who needed housing	√
	• *Expanded operating hours for a resource center* open to those released from incarceration (use is not restricted to SCA participants)	
	• *Enhanced "reach-in" classes*, available to incarcerated individuals nearing release	
Oklahoma	• *Program specialists*, who coordinated pre-release services for SCA participants	√
	• *Transition coordinators*, who created transition plans	√
	• *Community specialists*, who provided service coordination after release to those released without supervision requirements; for SCA participants released with supervision requirements, service coordination was provided by regular POs, who also were funded through SCA, with the expectation that case management would be more proactive than normal	√
	• *Pre-release and post-release classes*, including program slots for job readiness training, vocational training, cognitive behavioral therapy, and substance abuse treatment; SCA participants have priority access	√
	• *Vouchers* for housing, transportation, and other needs after release	√

Exhibit II-2 (concluded)

Grantee	What SCA Grant Funded (in whole or part)	Captured by Impact Study
San Francisco	• *Case managers* from a community-based organization, who first met with SCA participants prior to release to begin transition planning and were then available to meet regularly after release to coordinate services; case managers were trained in Motivational Interviewing	√
	• *Homeless Prenatal Program*, which helped participants navigate the child welfare system	√
	• *IRIS Center*, an outpatient mental health and substance abuse treatment program	√
	• *SF Clean City*, which offered work experience (street cleaning) with work readiness training to a small number of SCA participants	√
San Mateo	• *Case managers* from a community-based organization, who first met with SCA participants prior to release to begin transition planning and were then available to meet regularly after release to coordinate services	√
	• *Mentorship program* available to SCA participants	√
	• *Post-release classes*, including program slots reserved for SCA participants for job readiness training, transitional employment, and substance abuse treatment, and transitional housing	√
	• *Transportation vouchers* for SCA participants	√
South Dakota	• *Post-release case management*, provided by "enhanced" POs (intensive pre-release case management is also provided strictly to SCA participants, but is funded by the match requirement)	√
	• *Cognitive behavioral therapy*, both pre-release and post-release	√
	• *Additional post-release services for participants with special needs* (e.g., chemical dependency or substance abuse treatment, transitional housing, health and mental health care)	√
	• *Supportive services* for program participants, including transportation assistance	√

Managing the Programs and Partners

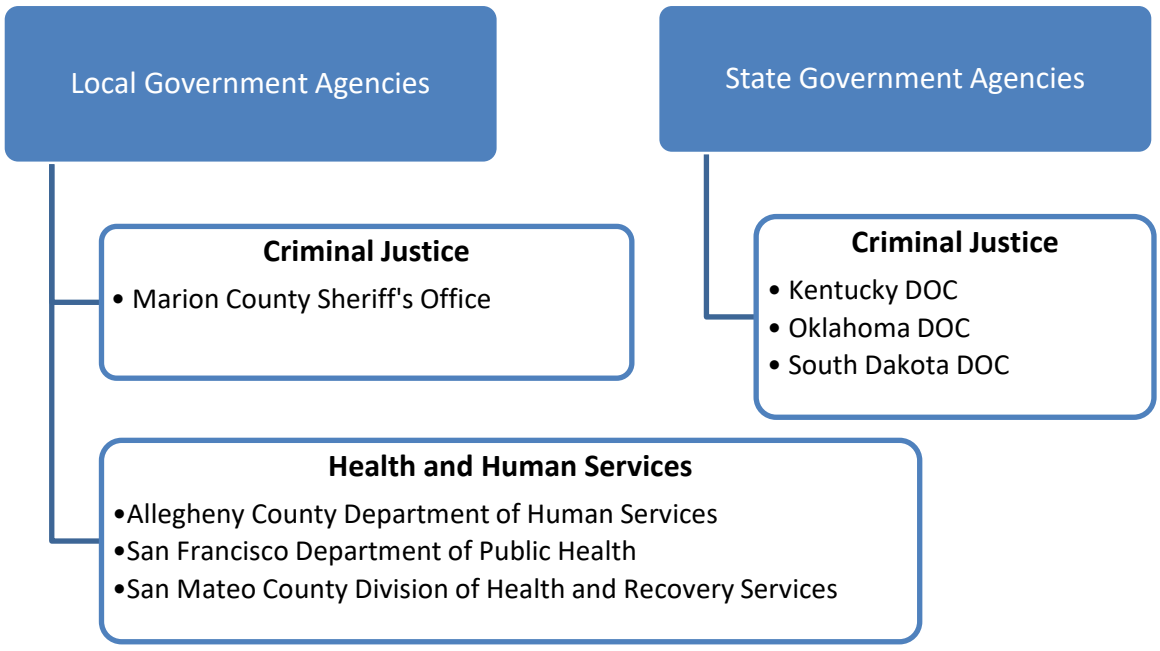
The SCA grantees were different types of organizations and relied heavily on partnerships for delivering services. They worked with their partners using different service models.

Types of SCA Grantees

As is illustrated in Exhibit II-3, the grantees represented both different levels of government and different types of government agencies.

- *Levels of government.* Four grantees were county or municipal agencies, while three were state agencies.
- *Type of organization.* Four grantees were criminal justice agencies, including three state departments of corrections and a sheriff's office, while three grantees were agencies responsible for administering health and human services programs.

Exhibit II-3:
SCA Grantees by Governmental Level and Type of Organization



These agencies were the formal grant recipients, responsible for fiscal management and reporting to BJA on the use of program funds and outcomes for participants. Most of them also designated persons within their organizations to manage their SCA programs, with responsibility for overseeing and coordinating implementation and operation. However, two

grantees (San Francisco and San Mateo County) subcontracted out much of the project management and operational responsibilities to nonprofit organizations.

Service Models and Partnerships

The grantees were not capable of providing all program services themselves, so they depended upon linkages with numerous partner organizations. With their partners, the programs delivered services in three ways.

- In the *direct service* approach, the agency that primarily operated the SCA program provided the service.
- With *formal partnerships*, the SCA program operator arranged with other providers (typically 2 to 5 such partners per grantee) to deliver services. The grantees paid for these services on a fee-for-service basis, in a lump sum to increase that provider's capacity, or through some other formal agreement with a provider to ensure that SCA participants had priority of service.
- Using *informal partnerships*, SCA staff members made unfunded referrals to various community organizations (upwards of 10 such agencies per grantee) to deliver services.

These three approaches are profiled in Exhibit II-4. Each grantee used all three approaches, but they varied in terms of which services they provided directly versus through informal or formal partnerships.

Given limitations of available funding and the grantee's own expertise, formal and informal partnerships were used extensively by each grantee. These partnerships were built upon previous re-entry efforts within the states or local areas, but existing partnerships were strengthened and new partnerships were developed as part of the grant. Partnerships were important in two distinct ways.

- *Partnerships strengthened grantees' capacity for service delivery.* Grantees created formal agreements with new providers to deliver services and expanded their knowledge of agencies to which they could make unfunded referrals. Some of these partnerships were with community-based organizations, while others were with public agencies, including those responsible for (among other things) public assistance, alcohol and drug treatment, mental health services, and education. Overall, these partnerships were important for building the capacities of grantees to serve participants during the grant and into the future. These partnerships also helped agencies and organizations share ideas and approaches to service delivery, expanding the perspectives and knowledge of grantee and partner staff.
- *Partnerships allowed for increased coordination between pre-release and post-release services.* An increase in the continuity of services was generally viewed by program staff as one of the more important successes of their grant activities. Through partnerships, case managers who generally worked with participants after release were able to begin

meeting participants who were still incarcerated so that they could cement relationships, gain participants' trust and begin planning for post-release services. It also allowed those who worked within jails or prisons to help share their knowledge about participants with those who could continue care after release. Among grantees that were correctional agencies, this strategy involved strengthening the relationships between jail or prison staff and POs. For grantees that were health and human services agencies, this coordination often led to new or stronger partnerships with correctional system agencies.

Regardless of the context, maintaining quality partnerships required strong communication among management staff for clarifying policies and procedures, and among service delivery staff for communicating participants' progress and needs.

Exhibit II-4:
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 has formal arrangement to provide services	No specific terms or agreement; SCA program staff provide referrals
Treatment of SCA 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

Screening and Enrollment

Grantees varied in their recruitment and enrollment procedures, including who was eligible to enroll in SCA and how eligibility was determined.

Subgroups Targeted for Project Participation

Chapter I described the SCA Adult Demonstration Program's eligibility requirements, but noted that grantees were expected to add their own targeting criteria.[18] All the grantees used location-based targeting and took risk of recidivism into account.

- ***Location-based targeting.*** Eligibility was restricted to individuals being released to specific cities, metropolitan areas, or counties.
- ***Risk of recidivism.*** Each of the grantees targeted individuals assessed as being at significant risk for recidivism. However, they used different assessment instruments and drew the threshold for eligibility at different places.

Beyond these general similarities, grantees used evidence of service gaps in their communities to define additional targeting criteria that differed among them.

- ***Gender*.** Allegheny County, Kentucky and South Dakota allowed participation by both men and women, while Marion County and Oklahoma served men exclusively, San Francisco served women exclusively, and San Mateo County gave priority to women.
- ***Age.*** Six of the grantees included adults of any age. South Dakota capped the age of enrollment at 30.
- ***Type of incarceration facility.*** Allegheny County and San Mateo County targeted individuals scheduled for release from county jails. Oklahoma and Marion County targeted individuals scheduled for release from state prisons. Kentucky and San Francisco started targeting individuals released from prison but expanded their enrollment to those released from jails. South Dakota targeted individuals from a range of state prisons, county jails, and tribal detention facilities.
- ***Expected time to release.*** Grantees had different ideas about when it was best to screen, recruit and enroll participants. Allegheny County, Oklahoma and South Dakota had more extensive pre-release services as part of their SCA programs, and typically enrolled individuals more than three months prior to release. The remaining grantees tended to rely on institutions' existing programming for pre-release services and used their SCA funds to concentrate on the post-release period; therefore, they enrolled participants in SCA within a few months prior to release or, sometimes, after release. The range of time between RA (a proxy for the SCA recruitment, screening and enrollment process) and release is shown in Exhibit II-5.[19]

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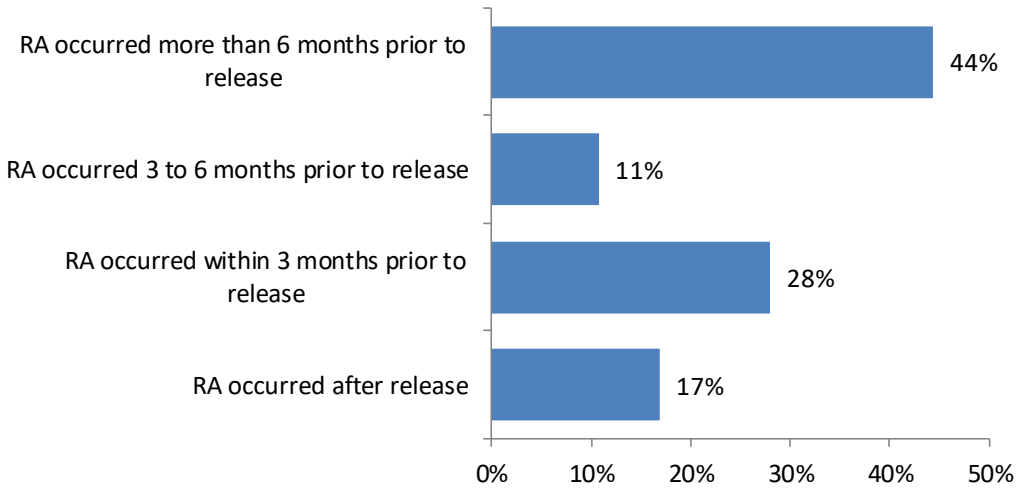
[18] The study did not specify eligibility criteria. In order to be enrolled in the study, an individual had to be eligible to participate in SCA based on whatever criteria each grantee used and give consent to be in the study.

[19] Generally, random assignment occurred just after eligibility for SCA was established and just before intensive and personalized SCA-funded services were expected to begin. The control group is used for the calculation in the exhibit because the date of release is an endogenous variable for the program group, and can be affected by SCA.

Exhibit II-5:
Time Elapsed between Random Assignment and Release from Incarceration



Notes: Numbers represent the time elapsed between the date of random assignment and the date of release from incarceration for members of the control group. Four percent of those in the control group were never released in the 18 months following RA; these individuals are included in the category of those randomly assigned more than six months prior to release.

Source: Administrative data and the study's random assignment system.

Assessment

Once targeting criteria were established, grantees identified and enrolled eligible individuals. One key step in this process was relying on an assessment instrument to identify individuals who met the risk threshold the program had established. Different types of instruments were used for this purpose, varying in their complexity. They were administered either by SCA staff members or correctional system staff members. The simplest such instrument was a proxy indicator constructed from age at first arrest, number of arrests, and age. Other instruments included the Level of Service Inventory-Revised (LSI-R), which scores potential participants on 54 risk items; the Level of Service/Case Management Inventory (LS/CMI), which includes a case management module; and the Correctional Assessment and Intervention System (CAIS).

Once risk levels were initially determined, SCA staff members went about identifying eligible individuals. Many grantees began by generating lists of potentially eligible participants from the full roster of inmates on a weekly, semi-weekly, or monthly basis. Additionally, grantees that enrolled some participants after release distributed flyers about SCA or relied on word-of-mouth or referrals from partners, especially probation and parole, to generate interest. Then, staff members conducted information sessions, made final eligibility determinations, and

obtained consent to participate.[20] Some grantees conducted eligibility screening and orientation sessions on an ongoing basis, whereas others held orientation sessions only when program slots opened up. Despite some initial concerns expressed by grantees about their ability to meet impact-study recruitment targets, only one had difficulty meeting its enrollment target and its initial target was revised down. The other six grantees met their enrollment targets.

Assessments were used not only to establish program eligibility but also to customize services and update the service plan over time. Assessments were administered periodically for the latter purpose 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 then, once the individual was enrolled in SCA, administered the 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 the Addiction Severity Index (ASI) and the University of Rhode Island Change Assessment Scale (URICA), among other instruments.

Examples of Assessment Instruments Used by Grantees

- Addiction Severity Index
- Correctional Assessment and Intervention System
- Level of Service Inventory-Revised
- Level of Service/Case Management Inventory
- University of Rhode Island Change Assessment Scale

Case Management Services

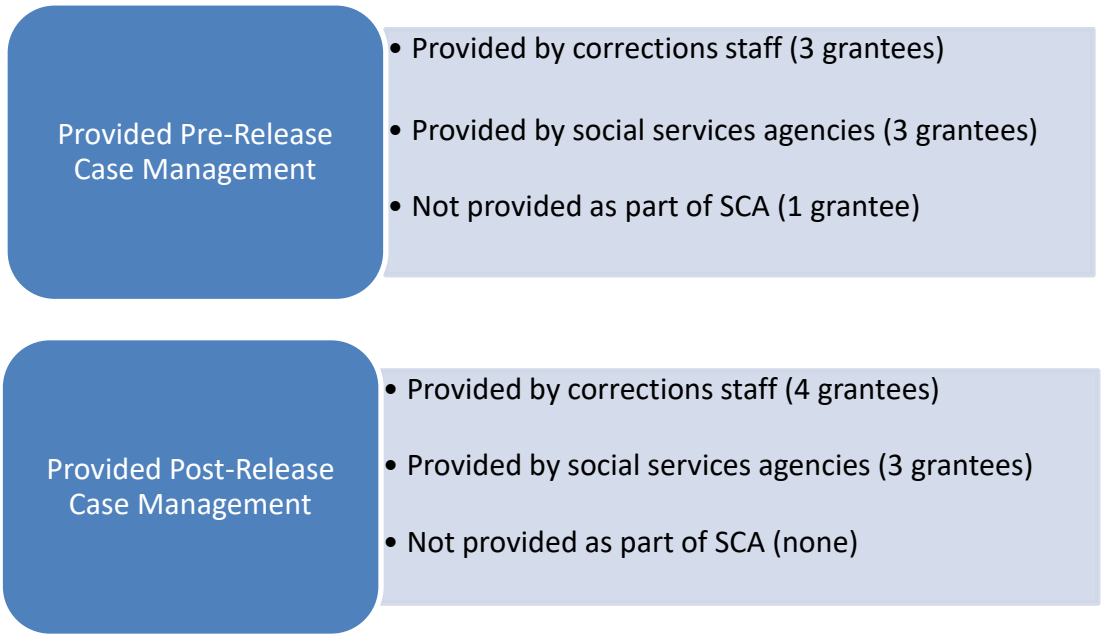
As noted, case management was the cornerstone of the SCA program model for six of the seven grantees. The goal of case management was to help prevent recidivism by providing individualized support and coordinating services based on identified needs and risk factors and, in some cases, by promoting compliance with parole or probation terms. Case managers functioned as mentors, enforcers, and brokers of SCA program services, and ensured that participants' risk assessments were used to guide service plans. While many services accessed by SCA participants were not reserved exclusively for them, case managers endeavored to make service access more likely, more efficient, and in keeping with a holistic view of participants' situations and needs.

As shown in Exhibit II-6, case management differed across the grantees in two key ways. One distinction was when SCA case management began: all seven grantees assigned participants to

[20] The study required each grantee to offer eligible individuals the option of declining study participation before random assignment occurred. Prior to the study, SCA program participation had sometimes been mandatory. The introduction of the impact study made SCA participation optional.

a case manager who worked with each participant after release, but six grantees also provided some form of case management prior to release with SCA funding. The pre-release aspect of the work generally involved having the case managers work with participants on transition planning, although in a few cases it went beyond that by including the coordination of pre-release services.[21]

Exhibit II-6:
Number of Grantees Providing Pre- and Post-Release SCA Case Management from Various Sources



Source: Site visits conducted as part of the evaluation.

A second way in which case management services varied across grantees was the position or role of the individual who served as the case manager. Correctional system grantees were more likely to provide pre-release case management through existing jail or prison staff, and post-release case management through "enhanced POs"—that is, POs that might have had special training and reduced caseloads. In contrast, grantees that were health and human services agencies designated separate individuals as SCA case managers. These case managers came from more traditional case management backgrounds (i.e., social workers, psychologists) within the social services system, and generally also had prior experience working with the formerly incarcerated. They developed transition plans, provided post-release case management, and

[21] Program participants in the one SCA program without pre-release case management worked with prison or jail staff around service planning, but these efforts were not coordinated with the SCA case manager or supported through the grant. Thus, they were no different from what any incarcerated individual would have received.

sometimes coordinated with correctional system staff members around the delivery of pre-release services or worked with participants while still incarcerated. Participants served by these grantees were very likely required to report to a PO after release, but this individual was separate from the SCA case manager.[22]

The two approaches to staffing the SCA case management function had both advantages and disadvantages. On the one hand, blending the roles of formal supervision and case management under an enhanced PO could help head off potential conflicts between POs and separate case managers, as occurred in at least one program where these two roles were split across two individuals. Also, having a PO as a case manager was beneficial for promoting participant retention in SCA because project participants knew they might face revocation if they did not show up for post-release appointments and services. On the other hand, participants often had negative perceptions of POs due to their own past associations with the criminal justice system, and this bias could adversely affect their ability to fully benefit from SCA (and from case management specifically).

Another challenge was that correctional system staff, either enhanced POs or other correctional system staff working with the program, sometimes needed to embrace a new approach to their work, one focused more on rehabilitation and less on enforcement. This latter point was a significant growing pain for some DOCs, but led to an important cultural shift, or change in mindset—a point that was echoed across respondents in the implementation study. This shift involved having correctional system staff members downplay the prevailing view that their role was about monitoring and enforcing and instead embrace a rehabilitative philosophy designed to support participants throughout the re-entry process using evidence-based approaches. To support them in this transition, the grantees typically gave these enhanced POs special training on the use of risk assessment tools and skills, or techniques, such as motivational interviewing. They also had smaller caseloads, typically around half to a third of their typical load, meaning they could meet both longer and more frequently with participants than could traditional POs.

Other Services

In addition to case management, each grantee coordinated and delivered other services that participants needed.

[22] According to the participant survey, approximately 97 percent of study participants served by health or social services agencies were on probation or parole after release from incarceration.

Types of Services

BJA established seven categories of services that grantees were expected to provide and on which they were to report participant involvement. These seven categories are outlined in Exhibit II-7. Although all grantees offered access to each of these services, they did not provide identical packages of services to SCA participants and did not deliver the services in the same way. As with case management, the type of grantee had some influence on how services were delivered. While criminal justice agency grantees had strong relationships within the criminal justice system, they often had to forge more new relationships with agencies to ensure the availability of the full range of services that participants might need after release. Health and human services agencies, on the other hand, often had to work hard to develop partnerships with correctional agencies, but had more community partners in place. They also provided many services themselves.

Exhibit II-7:
Seven Categories of SCA Program Services

Category	Service Description
Education and Training	GED preparation and testing, vocational training, 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	Intensive, outpatient, 12-step or change-model substance abuse 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 a number of goal-oriented, explicit systematic procedures
Pro-Social 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, vouchers for food, transportation, and other needs

Another important element of variability is that not all participants within a program received the same mix of services. SCA program staff placed a strong emphasis on "needs-based" service planning; therefore, participants accessed only some of the services a grantee offered, based upon that individual's service plan.[23]

Delivering Services to Participants

Grantees provided some services (in addition to case management) themselves and used partners to deliver other services. Overall, grantees made widespread use of informal partnerships and made limited and more targeted use of the direct service and formal partnership approaches—both of which required the expenditure of SCA funds. Exhibit II-8 shows how grantees used these latter two approaches for pre-release and post-release services. In this exhibit, a solid dot signifies a service that the lead agency provided directly while a hollow dot signifies a service provided through a formal partnership. Absence of a dot means that the service was delivered primarily through informal partnerships or existing institutions.

As the exhibit shows, with the exceptions of Allegheny County and Oklahoma, grantees relied more often on existing systems to provide services for participants still in jail or prison. SCA program staff members gave two main reasons for relying so heavily on existing systems for pre-release services: services of various types were generally already available as part of existing jail or prison programs, and it could be difficult to integrate unique SCA service components into the jail or prison environment.

For the provision of post-release services, by contrast, grantees commonly used the formal partnership models to provide the range of services, with some use of direct delivery for housing, cognitive behavioral therapies and pro-social services. Even when services were provided directly or through formal partnerships, limited SCA grant funding may have led the grantee to make extensive use of informal partnerships to increase capacity.

Services for the Control Group

Critical to the impact study is that SCA services were distinct from whatever services the control group may have received. Therefore, in addition to describing SCA programs and the services grantees provided, the implementation study learned about the services that were available to control-group members and the extent to which these differed from SCA program services.

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[23] The grantee that provided the 12-week structured course is to some degree an exception, in that all participants generally were expected to attend the same classes during the 12-week structured program.

Exhibit II-8: Pre- and Post-Release SCA Services Delivered by Direct Service or Formal Partnership

	Education and Training	Employment Assistance	Substance Abuse Treatment	Mental Health Services	Cognitive Behavioral Therapy	Pro-Social Services	Housing
Pre-Release							
Allegheny	○	○	○		○	○	(Not applicable)
Kentucky	*(Did not coordinate any pre-release services in SCA)*						
Marion County	*(Did not coordinate any pre-release services in SCA)*						
Oklahoma	○	●/○	○	○	●	●	
San Francisco					●		
San Mateo						○	
South Dakota					●		
Post-Release							
Allegheny	○	○	○			○	
Kentucky		○					○
Marion County		○	○		○	●/○	○
Oklahoma	○		○	○			●/○
San Francisco		○	○	○		○	
San Mateo	○	○	○			○	●
South Dakota			○		●/○	○	●

Notes: ● denotes services that the grantee provided directly; ○ denotes services provided through formal partnership. Other services were provided through informal partnerships or existing institutions.

Pre-Release Services for the Control Group

As was shown in Exhibit II-5, 83 percent of participants were randomly assigned while still incarcerated, and 55 percent spent more than three months in jail or prison before being released. Program-group members were able to use this time to access SCA-provided services in addition to what was provided within the jails or prisons. For the most part, this pre-release period primarily involved having SCA case managers work with participants to develop transition plans, which included navigating and taking advantage of existing prison or jail services and preparing for the period after release. While control-group members often had access to a correctional system staff person who was responsible for helping inmates navigate in-facility services, the time and attention these staff members had to assist control-group members was generally less than what was available to assist program group members.

In some sites, and most especially Allegheny County and Oklahoma (see Exhibit II-8), SCA funds were also used to develop in-institution workshops or other courses to which the SCA program group was given priority access. For example, in Allegheny County, services in the jail could include drug and alcohol treatment, cognitive behavioral therapy, education, mentoring, family support, and employment planning. Court-ordered inmates and SCA program participants were given priority access. Others (including control-group members) could participate only on a space-available basis. With the exception of Allegheny County and Oklahoma, though, most in-institution programming (apart from case management) was equally available to program and control-group members, so the service differential prior to release was not that great.

Post-Release Services for the Control Group

Although the incidence of release to supervision varies markedly by state (Pew Charitable Trusts 2014), most participants in this study were assigned to a PO after release from prison or jail. As described above, grantees assigned program-group members either to an enhanced PO (in lieu of a traditional PO) or to a social services case manager (to complement the work of a traditional PO). This means that most study participants—program and control-group members—should have received some form of "case management" after release, whether it was from a PO, a social service agency, or both.

However, case management provided to control-group members by traditional POs was expected to be less extensive and of a different quality than what SCA case managers (including enhanced POs) typically provided.

- *SCA caseloads were smaller*. Grantee and correctional system staff indicated that traditional POs had caseloads that were approximately two to three times greater than SCA case managers. Staff indicated that SCA case managers were available to meet with participants weekly or biweekly (and more often, as needed), for about 30-60 minutes. Traditional POs reportedly met with control-group members for 15-30 minutes once or twice a month. This difference was further magnified in some areas, like California, where AB 109 and AB 117, known as "Public Safety Realignment," placed additional burdens on probation as prison populations were released to local supervision, lessening the time probation officers could spend with individuals. One staff member indicated that probation sessions in the San Francisco area might sometimes last as little as five minutes.
- *SCA case managers may have approached case management from a different perspective than traditional POs.* SCA case managers received specialized training around evidence-based practices and approaches for addressing participants' criminogenic needs. While SCA case managers often focused on needs assessments, case planning, and service delivery, SCA program staff indicated that traditional POs were more focused on monitoring, ensuring that those they supervised were meeting the terms of their release, finding work, staying clean, and not committing new crimes.

- *SCA case managers had access to a greater array of programs and services.* SCA case managers could use providers committed to serving SCA participants. While many of these SCA service delivery partners also provided services to a broader pool, SCA funding or service delivery agreements ensured there were positions available for program-group participants, while control-group members had to compete with others for spaces. Furthermore, because of their SCA case manager, program-group members had support and guidance in learning about and accessing these services.

Overall, because SCA grantees provided both case management and other services, the service contrast between program and control-group members should have been fairly extensive. However, there were circumstances that caused the service contrast to be diminished in some ways. In Marion County and Allegheny County, the grantees, either through SCA funding or some additional funding, created drop-in centers that provided traditional POs with an easy one-stop referral source for their supervisees, thus increasing the likelihood that control-group members might have received at least some of the services also available to program-group members. Kentucky also used some SCA funding to support classes and workshops to which traditional POs may have referred control-group members. In San Francisco and San Mateo County, the nonprofit organizations that primarily operated the SCA programs also provided numerous other services that were available to the public. Since control-group members would have interacted with these agencies as part of the study enrollment process, they would have likely known about and had access to many of these services.

Summary

The FY 09 grant announcement for the Adult Demonstration Program issued by BJA (U.S. Department of Justice 2009) drew attention to the need for a continuity of services from incarceration to release and stipulated that funded projects:

1. Should use validated and dynamic assessment tools to determine risks and needs
2. Should provide "offenders … with all necessary services, including: (1) educational, literacy, vocational, and job placement services … (2) substance abuse treatment … and (3) coordinated supervision and comprehensive services … including housing and mental and physical health care to facilitate reentry … and which, to the extent applicable, are provided by community-based organizations entities" (pp. 2-3).

Site visits to the grantees occurred shortly after RA commenced, which was more than two years into the grantees' implementation of their grant program. The evaluation found that, at that time, grantees had relatively well established programs, with features as outlined below.

- ***Assessment guided service planning***. All the grantees used validated assessment instruments to identify criminogenic needs.

- ***Coordinated supervision was a central feature***. Each grantee's service delivery model relied on case managers, who generally planned and coordinated a wide range of services for participants.
- ***SCA grew partnerships***. Partnerships were crucial for service delivery, as the grantees lacked the capacity to deliver the full range of services themselves. These partnerships increased the linkages between pre-release and post-release services and improved coordination and service delivery between correctional system agencies and other governmental and social service system agencies.
- ***Services became more comprehensive***. On the whole, the grant created an increased continuity of services, both across partners and especially pre-release and post-release environments. Staff benefited from training, learning to use various assessments and case planning tools, and had access to a greater array of services for participants than they had before the grant.
- ***A new approach to re-entry services emerged***. Based on what we were told during the site visits, grantees that were DOCs developed new partnerships, training, and service delivery approaches, and moved away from a policing and enforcement mindset toward a rehabilitative philosophy that accepted evidence-based practices. This cultural shift was, in some cases, substantial.

In these ways, the service contrast between program- and control-group members, especially post-release services, was readily apparent.

Despite these similarities, the grantees' programs varied markedly in a number of ways.

- ***Case management providers varied***. The type of grantee influenced the type of individuals who served as SCA case managers: correctional system agencies used correctional system staff, while service-based agencies used case managers with social service backgrounds.
- ***Grantees used their SCA grant funds differently***. The grantees generally used a good portion of their grant funds to provide case management services, but, beyond that commonality, each used its funds to provide different types of services both before and after release.
- ***Different types of grantees emphasized different types of partnerships.*** Developing all the partnerships that were needed took considerable work. Correctional system grantees had an easier time implementing changes within the correctional system, but often had to grow additional partnerships with community organizations, especially to provide post-release services. By contrast, human services and health-based grantees had to build partnerships with correctional system agencies to allow for the delivery of pre-release and transitional services, but they typically had a stronger network of partnerships for the delivery of post-release services.
- ***The adequacy of the continuum of care from pre-release to post-release was better developed in some sites than others***. Some grantees devoted substantial SCA resources to pre-release services, while others began their SCA programs nearer to release. In the

latter instances, the grantees relied on the existing programming of the institutions to provide pre-release services and used the period prior to release to begin SCA service planning for the post-release period.

Notwithstanding these differences, the implementation study found that the grantees developed programs that, by and large, met the criteria outlined by BJA in the grant solicitation. However, full implementation of the service model envisioned by BJA also fell short in some ways. In particular, grantees lacked the funds to ensure that participants accessed all the services they needed. Therefore, they all relied heavily on unfunded referrals to provide many services. Where unfunded referrals were used, coordination with the SCA 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, it would be hard to argue that every SCA participant received "all necessary services," a topic that will be explored further in a subsequent chapter.

III. Participants' Experiences in SCA

Based on MIS data that the grantees provided, this chapter describes the SCA services that program-group members received and how long they participated in SCA. These data are only available for study participants who were randomly assigned to the SCA program group, because control group members were not eligible to receive SCA services. Furthermore, this chapter only describes SCA services that the grantees knew about and entered into their data systems.[24]

Findings in Brief

- Overall, 36 percent of those in the program group received both pre-release and post-release SCA services, according to the grantees' MIS data. As would be expected, those enrolled in SCA well before release were much more likely to receive both pre-release and post-release services.
- Nearly one half of the 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.
- More than one half of the program group received SCA employment assistance after release, and nearly one half received substance abuse treatment. Another common post-release service was cognitive behavioral therapy.
- For some grantees, participation in SCA was expected to last for up to six months, while for others it was expected to last for a year or more. Overall, approximately 25 percent of participants participated in SCA for more than one year and another 37 percent participated for more than six months.

Services Received

Exhibit III-1 shows the percent of program-group members who received only SCA pre-release services, only post-release services, or both pre-release and post-release services.[25] We noted in the previous chapter that some participants were enrolled in SCA well before release, and

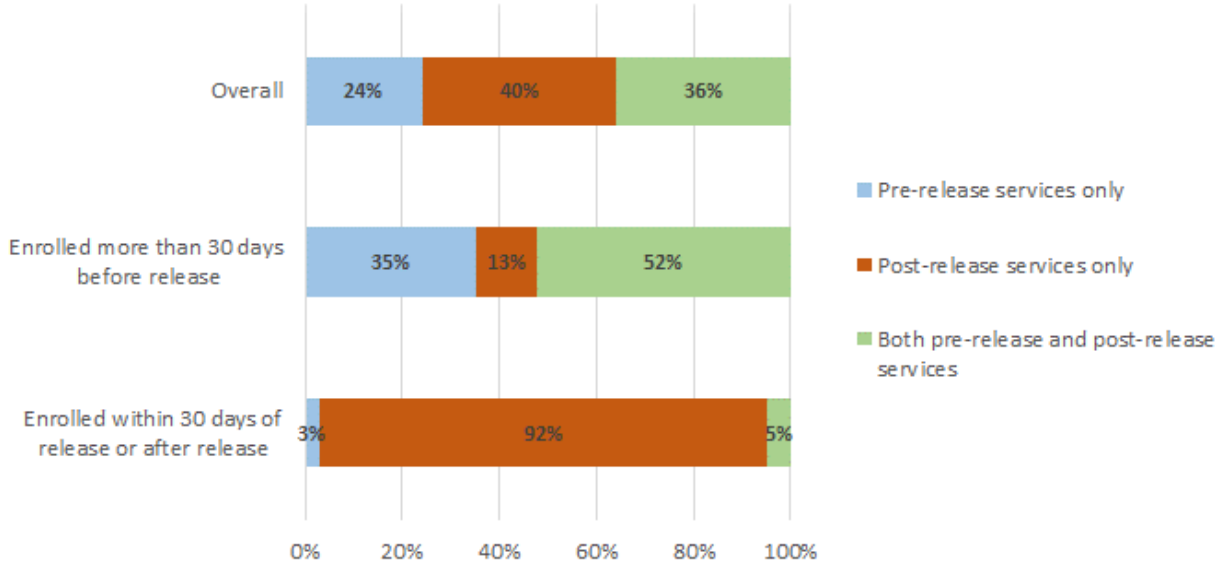
[24] The next chapter overcomes these limitations by using the survey of study participants—both program- and control-group members—to describe all the re-entry services received, regardless of source.

[25] As was noted elsewhere, individuals classified as not having received an SCA pre-release service could have received pre-release services from the institutions' own programs, and not as a consequence of SCA enrollment.

others nearer to or after release. To reflect this, the exhibit tabulates service usage by that distinction.

Overall, 36 percent of participants received both pre-release and post-release services as part of SCA, and 40 percent received post-release services only. However, as would be expected, participants who were randomly assigned to the SCA program group more than 30 days before release were much more likely to receive both pre-release and post-release services (52 percent), though an appreciable number (35 percent) received pre-release services only. By contrast, most participants (92 percent) randomly assigned nearer to release or after release received post-release services only.

Exhibit III-1: Percent of Participants Receiving Pre-release Services, Post-release Services, or Both, Overall and by Timing of Program Entry



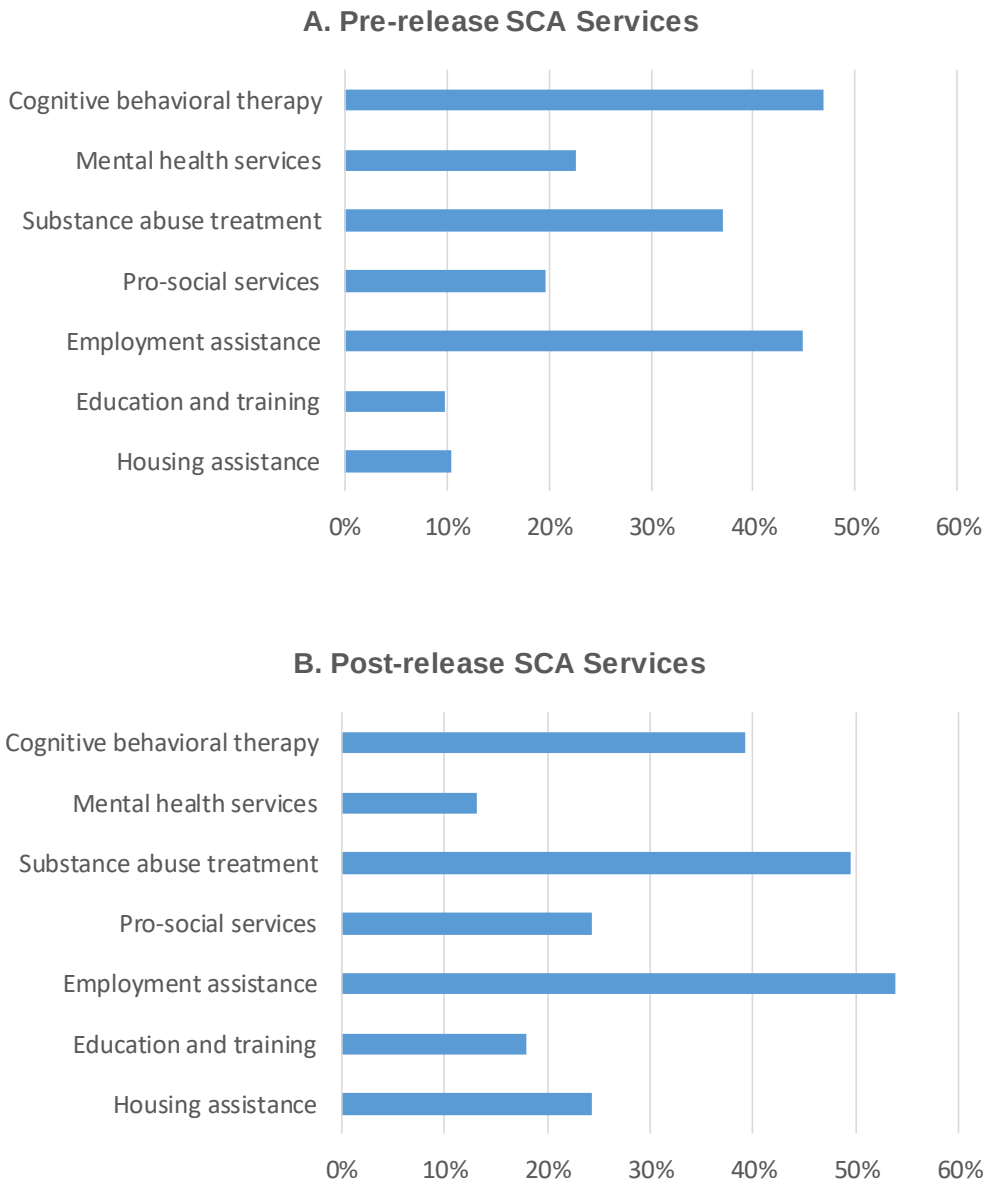
Note: Participants are tabulated according to whether their date of random assignment to the SCA program group was up to or more than 30 days from their date of release from incarceration. In this exhibit, the date of random assignment serves as a proxy for when program group members began receiving SCA services.

Source: MIS data provided by the grantees

Exhibit III-2 shows, overall, the percent of SCA participants who received various types of pre-release and post-release services. Panel A shows this for SCA pre-release services and indicates that, according to the grantees' MIS data, nearly half of the SCA participants received pre-release employment assistance and cognitive behavioral therapy, and more than one-third received substance abuse treatment. Panel B shows the incidence of post-release services. After release, the same three services—employment assistance, substance abuse treatment,

and cognitive behavioral therapy—were the most common, with employment assistance the most common of the three.

Exhibit III-2: Incidence of Pre-Release and Post-Releases Services among SCA Participants



Notes: Numbers in the exhibit represent the percent of the program group who received the service and reflect only SCA services that the grantees provided directly or otherwise knew about and captured in their data systems. Percentages are calculated after excluding the 11 percent of the program group who had missing data on these variables or might not have received any SCA services at all.

Source: MIS data provided by the grantees

The Duration of Participation

As shown in Exhibit III-3, the grantees had different guidelines regarding how long participants were expected to receive SCA program services. As revealed by the implementation study, Allegheny County, Oklahoma, and South Dakota planned to enroll participants with at least four months remaining before release, while enrollment was expected to occur nearer to release in the remaining sites. Participation was to continue up to one year after release for participants in Allegheny County, Oklahoma, and San Mateo County; in the remaining sites, post-release participation was generally expected to be shorter.

Exhibit III-3:
Expected Duration of SCA Participation

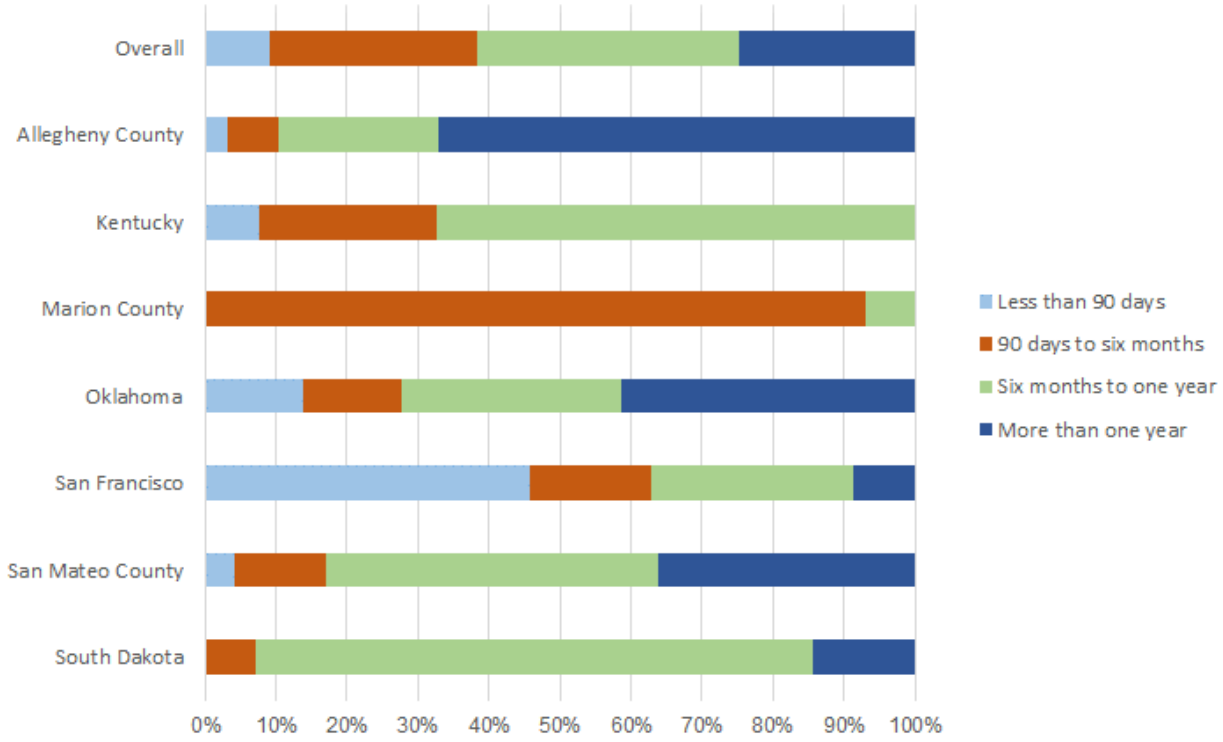
Allegheny County	• Enrollment occurs at least 5 months prior to release • Participation continues for 12 months after release
Kentucky	• Enrollment occurs within one month of release • Participation continues for 6 months after release
Marion County	• Enrollment occurs just prior to release • Participation continues for the duration of SOAR and aftercare classes, which occur during the 6 months after release
Oklahoma	• Enrollment occurs 4-6 months prior to release • Participation continues after release for a total of 18 months
San Francisco	• Enrollment occurs just prior to release • Participation continues until completion of parole or participant no longer seeks services
San Mateo County	• Enrollment occurs at least 2 months prior to release • Participation continues for 1 year after release
South Dakota	• Enrollment occurs up to 9 months prior to release • Participation continues until completion of parole

Notes: This exhibit describes the expected duration of participation were a participant to enroll in and complete SCA services according to the grantee's planned service model. Actual spells of participation could be longer or shorter given an individual participants' needs, and some participants can have much shorter spells if they fail to report for scheduled services. Kentucky and Marion County also used some portion of their SCA funds to enhance pre-release classes available in institutions, but SCA participants had no preferred access to these services; therefore, this time is not included in this exhibit.

Source: Data collected from the evaluation's implementation study (D'Amico et al. 2013).

In keeping with these guidelines, the average duration of participation—measured as time from SCA enrollment to the date of the last SCA service—varied across the grantees, as shown in Exhibit III-4.[26] Allegheny County had two-thirds of its participants receive services for more than one year, and, in Oklahoma and San Mateo County, more than one-third did. Spells were shortest in Marion County, but the calculation of duration in this site excludes pre-release services partly funded by SCA that participants might have received. Across all the grantees, the average duration of participation was approximately 8.5 months; approximately 25 percent of the sample participated for more than one year, and more than 60 percent participated for at least six months.

Exhibit III-4: Duration of Participation in SCA, Overall and by Grantee



Notes: The duration of participation is calculated as the time elapsed from the SCA enrollment date to the date of the last SCA service. Calculations exclude the 29.5 percent of participants missing their enrollment date and/or their date of last service. Date of last service was much more likely to be missing than the enrollment date and can be missing because of participant attrition or because the participant was still receiving services at the time the MIS data were extracted for the research team.

Source: MIS data provided by the grantees

[26] The calculations are restricted to those with both an enrollment date and a date of last service. We collected MIS data covering at least one year after RA for every participant. However, those with exceptionally long spells of participation will disproportionately be missing their date of last service; therefore, the figures could somewhat understate the true length of participation were complete data available for everyone.

IV. Impacts on Services

The logic underlying the grantees' program models is that SCA funding 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 improve desistance and lead to other desirable outcomes. A first step in the evaluation of the grantees, therefore, is to assess whether those allowed access to SCA received more services than those assigned to the control group who could have received other re-entry services but not SCA; if there are no differences in services between the groups, there is no reason to expect differences in desistance or other outcomes.

To examine service receipt, we analyze data collected from the follow-up survey for both the SCA program group and the control group. As noted in Chapter I, the survey was administered 18 months after random assignment and asked about a range of services.

Findings in Brief

- In comparison to those in the control group, those in the program group were more likely to:
  - Have someone they could turn to for advice and who went out of the way to help
  - Take classes to change how they think, feel, or act
  - Get assistance with finding a job
  - Receive substance-abuse treatment
  - Get housing assistance

  On these items, the program group was between 10 and 25 percentage points more likely to receive the service than the control group.
- Eighteen months after RA, those in the program group were just as likely as those in the control group to report having unmet needs, with more than half of those in both groups reporting the need for additional housing assistance, job placement assistance, job training, health services, and educational services.

Impacts on Services Received

As discussed in Chapter II, a central component of SCA in most study sites was to provide a social services case manager or enhanced PO who could help broker services. In keeping with this, Exhibit IV-1 shows that program-group members were much more likely to have received help with re-entry and have an individual case plan. They were much more likely to say that

they had a case manager or PO who was able to answer questions about available services, whom they were comfortable talking to, who went out of the way to help, and whom they could turn to for advice about personal issues. SCA participants were much more likely than control-group members to report that their case managers or POs were somewhat or very helpful for avoiding crime. All these differences are statistically significant and sizable in magnitude.[27]

Exhibit IV-1: Impacts of SCA on Case Management Services

	Program	Control	Difference
Got help with re-entry	77.5	59.0	18.5***
From whom did you get this help[a]			
Case manager	25.3	24.8	0.5
Probation/parole	36.7	46.8	-10.1**
Both	38.0	28.5	9.5**
Had an individual case plan	56.8	35.2	21.6***
Case manager or PO was able to answer questions about services	70.3	48.5	21.8***
Was comfortable talking to a case manager or PO	68.7	44.2	24.5***
Had a person who went out of the way to help	62.8	42.0	20.8***
Had a person to turn to for advice about personal or family issues	59.8	42.8	17.0***
Case manager or PO somewhat or very helpful for avoiding crime	64.7	43.5	21.2***
Sample size	**495**	**294**	

Notes: Numbers in the first two columns represent the percent receiving the service anytime since the date of random assignment; the third column represents the difference between the first two columns.

Source: 18-month survey

[a] This is a conditional outcome, with the results restricted to those who reported getting help with re-entry. Therefore, the random assignment 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.

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

[27] Estimates reported in this chapter and the ones to follow were weighted to equalize the odds of selection into the study groups and to adjust for survey response rates. Tests of significance were calculated using multivariate models that control for pre-RA characteristics, which improves the precision of the estimates. See Chapter I for more information about statistical methods. Except for the conditional outcomes noted, results in this chapter are being reported for the 789 study participants who completed the survey, excluding the very small number with missing data on given items.

Given that the program group reported receiving greater help with re-entry, they should be more likely to receive the services they needed. Exhibit IV-2 shows that, in fact, the program group received more of a greater variety of services than those in the control group. For example, they were more likely to attend classes to change how they thought or acted, which, based on the literature, can be critical for reducing recidivism (Lowenkamp et al. 2009).

Exhibit IV-2: Impacts of SCA on Other Services

	Program	Control	Difference
Cognitive Change & Mental Health Services			
Attended classes to change how I think, act, or feel	61.4	41.5	19.8***
Received other services to change how I feel, such as mental health services	31.9	28.1	3.8
Employment-Related Services			
Got help with job-finding skills (how to look for a job, prepare a resume, interview)	60.8	39.6	21.2***
Received advice on answering employers' questions about criminal history	55.9	31.3	24.6***
Case manager or PO somewhat or very helpful in finding or keeping a job	48.8	30.6	18.2***
Got help finding a job	30.3	19.9	10.4***
Got vocational training	12.3	12.3	-0.0
Education Services			
Took ABE or GED classes	18.7	19.1	-0.3
Took college courses for credit	12.9	11.5	1.3
Earned a diploma or degree	8.9	9.1	-0.2
Other Services			
Received inpatient or outpatient substance abuse treatment	66.3	55.1	11.1***
Help getting Food Stamps	27.5	22.7	4.8
Participated in sponsored social activities	26.1	20.2	5.9*
Participated in formal mentoring program	21.5	14.3	7.2**
Received housing assistance	20.8	6.3	14.5***
Help getting public assistance	13.5	10.4	3.1
Help with child-support system[a]	7.3	5.1	2.2
Sample Size	**495**	**294**	

Notes: Numbers in the first two columns represent the percent receiving the service since the date of random assignment; the third column represents the difference between the first two columns. The sample size shown does not apply to subsetted variables.

Source: 18-month survey

[a] This is a conditional outcome, with the results restricted to those who have children. Therefore, the random assignment 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.

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

The program group was also more likely to receive a range of employment-related services. For example, they were substantially more likely to get advice on how to look for a job, such as help with preparing a resume and coaching on how to interview for a job. They also were more likely to get advice on how to answer potential employers' questions about their criminal history, which might help address a significant barrier that the formerly incarcerated face when they apply for jobs (Pager 2003). Similarly, they felt that their case managers or POs were somewhat or very helpful in finding a job. However, program-group members were not more likely to receive vocational training or educational services (including adult basic education or general educational development classes, or college courses). Furthermore, they were not more likely to earn a diploma or certificate. In fact, the incidence of vocational training and post-secondary education was low for both groups.

Finally, the survey asked about a range of other services to help address other barriers that impair the successful re-entry of those returning from incarceration (Petersilia 2003). Those assigned to the program group were significantly more likely to receive either inpatient or outpatient substance abuse treatment and receive housing assistance, and were also more likely to participate in mentoring programs and sponsored social activities. However, there were no statistically significant differences between the groups in getting help with navigating the child-support system or getting food stamp benefits or public assistance.

Unmet Needs

The above tables show that substantial proportions of SCA participants received an array of services designed to help them with re-entry. But what needs remained unmet? To address this question, the survey asked study participants what additional services they would have liked to have received. Despite the fact that the incidence of service receipt was quite high in some cases (see the preceding tables in this chapter), Exhibit IV-3 shows that 18-months after RA both those in the program group and those in the control group had substantial unmet needs. For example, more than half of both groups reported wanting additional housing assistance, job placement assistance, health services, educational services, and job training. More than one-third wanted family reunification services, substance abuse treatment, and mental health services. Although SCA participants were much more likely than those in the control group to have received many of these services, they were no less likely to want more help.

Exhibit IV-3: Additional Services Would Have Liked[a]

	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**	

Notes: Numbers in the first two columns represent the percent reporting that they desired additional services in the category; the third column represents the difference between the first two columns.

Source: 18-month survey

[a] These questions were asked only of those not continuously incarcerated since random assignment (n=711). Because release after random assignment may be determined by participation in SCA, differences in outcomes between the groups are only suggestive of true estimates of impacts.

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

Impacts for Subgroups

Grantees were encouraged by the grant solicitation to use validated assessment tools at intake to determine the risks and needs of participants and provide them with appropriate services. If they followed this guidance, subsets of SCA participants may have been more likely to receive certain services than others, and the gap between them and their counterparts in the control group might be wider. To investigate these possible differences in impacts, this section describes exploratory analyses that estimate impacts on service receipt for subgroups.

Exhibit IV-4 presents results for three of the five subgroups introduced in Chapter I. Numbers in the exhibit represent impact estimates—that is, *the difference* between the average response of the program group compared to the control group for each subcategory listed.[28] We also formally tested whether the difference in impacts between the two subgroups *within* a category was statistically significant—for example, the impacts of being assigned to the program group for males versus females. There are virtually no statistically significant

[28] To be concise, only the impact estimates (that is, the difference in means between the program and control groups) are reported in this exhibit and others in this report presenting subgroup findings.

differences between the subgroups in the impact of program-group assignment. For example, being in the program group increased the likelihood that females would receive the range of re-entry services to about the same degree as it increased the likelihood for males. Also, being in the program group had no different impact on the need for additional services for males and females. There are also no significant differences in impacts across the age subgroups. Thus, with regard to services, being in the program group improved service access for males and females, and the younger and older, about equally.

Similarly, there are no consistent differences in the impact of being in the program group for those in different risk classifications.[29] Thus, both lower-risk and higher-risk individuals benefited from SCA about equally; that is, program-group members in each subgroup were significantly more likely than those in the control group to receive case management, cognitive change therapy, and employment-related assistance, and program impacts are about equivalent regardless of risk level. In short, SCA appears to boost service receipt by an equal extent for these two risk subgroups.

Exhibit IV-5 shows the results for the two remaining subgroups introduced in Chapter I. Being in the program group had a more positive effect for participants who were randomly assigned well before expected release from incarceration on attending classes focused on cognitive change and on getting help with finding a job. There are also some differences in impacts for different types of grantees. For example, criminal justice agencies were more likely to provide help with *looking for* a job, but social service agencies were more likely to provide help *getting* a job. Other than these differences, being in the program group had similar positive impacts for all these subgroups.

Summary

SCA significantly increased access to a wide range of re-entry services. Those assigned to the program group were more likely to receive help with re-entry from a case manager or PO and were more likely to have an individual case plan. They were more likely to report having someone who was able to answer their questions, whom they were comfortable talking to, and who went out of the way to help. They were more likely to report having someone they could turn to for advice about personal matters, and felt that this person helped them in avoiding crime. In all these ways, the difference between the program and control groups was approximately 20 percentage points.

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[29] As noted in Chapter I, however, only those determined to be at medium or high risk of recidivism were eligible for SCA, so the subgroups represent degrees of risk within a narrow range. In other words, the lower-risk subgroup does not represent low-risk individuals in an absolute sense, but only relative to others determined eligible for SCA.

Changing attitudes and values is believed to be effective in reducing recidivism (Lowenkamp et al. 2009) and many of the SCA grantees emphasized providing cognitive behavioral therapy. According to the survey, approximately 61 percent of program-group members received cognitive behavioral therapy, in comparison to just 42 percent of those in the control group.

The grantees also emphasized providing employment services. Those in the program group were more likely to have received help on how to look for a job, and they received advice on answering employers' questions about their criminal history. They felt that their case managers or enhanced POs helped them find or keep a job. In these ways as well, differences between program and control-group members were notable.

Finally, those in the program group were more likely to receive substance abuse treatment, receive housing assistance, and participate in a mentoring program and sponsored social activities.

However, despite SCA's impacts on receipt of a wide range of services, the difference in service receipt between the program and control groups is generally modest, never exceeding 25 percentage points. In other words, although SCA participants received more services, many control-group members also accessed these services. Furthermore, SCA participants, just as control-group members, reported having many unmet service needs 18 months after RA. Two-thirds of both groups wanted additional housing assistance, and more than half wanted additional job placement assistance, job training, health services, and educational services. In short, despite the fact that SCA did provide meaningful services well beyond what the typical individual being released from incarceration would have received, many additional services were apparently needed.

Exhibit IV-4:
Impacts of SCA on Re-entry Services, by Gender, Age, and Risk Subgroups

	Gender		Age		Level of Risk[c]	
	Females	Males	Less than 30	30 or Older	Lower	Higher
Case Management Services						
Got help with re-entry	22.0***	17.4***	19.2***	18.0***	21.8***	15.3***
Had an individual case plan	17.7**	22.3***	26.2***	17.8***	24.4***	22.3***
Case manager/PO able to answer questions	17.0**	22.8***	22.9***	20.8***	22.3***	21.6***
Was comfortable talking to case manager/PO	21.9***	25.0***	28.3***	20.9***	24.8***	24.5***
Had person who went out of the way to help	12.0	22.8***	20.8***	20.6***	21.9***	20.0***
Had a person to turn to for advice	15.7**	17.1***	19.4***	13.5***	24.2***	10.4**
Helpful for avoiding crime	19.3**	21.5***	22.5***	19.6***	21.8***	20.0***
Cognitive Change & Mental Health						
Attended classes to change how I act or feel	7.7	23.0***	26.5***	15.4***	15.2***	21.1***
Received other mental health services	9.9	1.8	1.4	6.0	3.6	5.2
Employment-Related Services						
Got help with job-finding skills	10.4	23.9***	19.7***	23.3***	20.1***	21.8***
Advice on answering employers' questions	22.6***	25.0***	18.7***	29.4***	24.2***	22.0***
Case manager/PO helpful in finding job	7.3	20.8***	20.6***	15.6***	17.0***	18.7***
Got help finding a job	8.9	10.6***	11.1**	10.5**	15.5***	8.3*
Got vocational training	-7.9	2.0	-0.4	-0.2	3.6	-2.3
Education Services						
Took ABE or GED classes (D21a)	0.9	-0.8	-3.3	3.4	-0.6	-0.1
Took college courses for credit (D22)	5.3	0.1	3.3	-0.5	-1.4	3.5
Earned a diploma or degree	-4.2	0.8	-1.1	1.1	-2.9	1.3
Other Services						
Received substance abuse treatment	6.2	12.2***	12.2**	9.5*	18.9***	4.4
Help getting Food Stamps	9.0	3.5	7.1	2.3	6.0	4.2
Participated in sponsored social activities	2.8	6.6*	7.7*	3.2	6.7	3.2
Participated in formal mentoring	3.9	7.9**	9.5**	4.6	3.1	7.5*
Received housing assistance	18.1***	13.3***	17.5***	11.9***	16.1***	13.1***
Help getting public assistance	11.4*	0.7	3.0	2.9	3.9	1.9
Help with child-support system[a]	6.0	1.1	2.9	2.1	-1.6	6.0*

	Gender		Age		Level of Risk[c]	
	Females	Males	Less than 30	30 or Older	Lower	Higher
Additional Services Would Have Liked[b]						
Housing support	-10.4	1.8	-2.9	1.3	-2.5	-0.8
Job placement	6.1	-3.2	-4.3	1.4	-2.5	-0.4
Health services	-2.6	3.5	-1.0	3.9	0.1	1.0
Educational services	-1.2	-2.1	-4.5	0.3	-4.5	-0.8
Job training	-7.0	-0.3	3.8	-6.3	-6.3	2.9
Advice on getting a job	1.3	2.4	6.8	-1.9	-1.3	4.8
Family reunification	5.4	-0.4	10.0* †	-7.8	0.5	-0.2
Substance abuse treatment	2.3	-4.3	-1.3	-4.7	-1.3	-2.4
Mental health services	8.1	-2.3	-2.9	1.7	1.1	-1.4
Child-support issues	-9.6	1.2	-0.1	-2.6	-2.0	-1.5
Sample Size	**171**	**617**	**365**	**423**	**368**	**388**

Notes: Numbers in the exhibit represent the impact estimates—that is, the difference within each subgroup between the program and control groups in the percent reporting affirmatively. A positive number means that more of those in the program group responded affirmatively compared to their counterparts in the control group; a negative number means that more of those in the control group responded affirmatively. Sample size represents the unweighted number who completed the survey in each group; sample sizes are slightly lower on some items due to missing data, and are lower for conditional outcomes. Subgroups are described in Chapter I.

Source: 18-month survey

[a] This is a conditional outcome, with the results restricted to those who have children. Therefore, the random assignment 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.

[b] These questions were asked only of those not continuously incarcerated since random assignment. Because release after random assignment may be determined by participation in SCA, differences in outcomes between the groups are only suggestive of true estimates of impacts.

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

† The difference in the impact of SCA between the subgroups in the 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 of SCA on Re-entry Services, by Timing of Entry and Grantee Type

	Timing of Entry		Grantee Type	
	Well Before Release	Nearer to Release	Criminal Justice	Social Service
Case Management Services				
Got help with re-entry	21.8***	10.7*	18.0***	19.5***
Had an individual case plan	26.4***	11.3*	18.8***	27.3***
Case manager/PO able to answer questions	26.3***	12.0*	20.5***	24.4***
Was comfortable talking	29.1***	15.2**	24.0***	25.6***
Had person who went out of the way to help	25.1***	12.3*	17.2***	28.4***
Had a person to turn to for advice	21.8***	9.3	15.4***	20.2***
Helpful for avoiding crime	26.0***	11.1**	20.2***	23.4***
Cognitive Change & Mental Health				
Attended classes to change how I act or feel	28.8*** †	-3.0	23.4***	12.4*
Received other mental health services	3.6	4.3	3.3	4.7
Employment-Related Services				
Got help with job-finding skills	21.3***	20.0***	27.7*** †	7.6
Advice on answering employers' questions	25.9***	23.1***	25.8***	22.1***
Case manager/PO helpful in finding a job	23.4*** †	7.0	18.7***	17.1**
Got help finding a job	11.5***	8.3	3.8 †	24.3***
Got vocational training	0.1	-0.0	-2.6	5.4
Education Services				
Took ABE or GED classes	2.0	-6.2	-1.5	2.1
Took college courses for credit	2.4	0.5	-0.7	5.7
Earned a diploma or degree	-0.1	-2.1	-0.6	0.5
Other Services				
Received substance abuse treatment	13.1***	9.8	12.3***	8.7
Help getting Food Stamps	7.2*	0.8	0.9	12.9**
Participated in sponsored social activities	6.7*	5.8	3.3	11.2**
Participated in formal mentoring program	5.3	11.0**	6.6**	8.3
Received housing assistance	17.3***	8.2**	15.4***	12.5***
Help getting public assistance	2.7	4.7	1.5	6.5
Help with child-support system[a]	0.3	5.8*	-0.1	7.0
Additional Services Would Have Liked[b]				
Housing support	-3.7	6.7	-1.2	0.2
Job placement	-0.9	-0.1	-2.2	0.4
Health services	2.6	3.3	-2.1	10.1
Educational services	-0.7	-2.7	-6.1	5.9
Job training	2.9	-8.6	-4.2	3.0
Advice on getting a job	5.7	-2.8	1.3	3.6
Family reunification	4.7	-4.8	-7.6 †	16.7***
Substance abuse treatment	0.1	-8.0	-4.9	0.6
Mental health services	2.8	-1.0	-4.7	9.1
Child-support issues	0.4	-2.7	-2.9	2.4
Sample Size	**546**	**243**	**534**	**255**

Notes: Numbers in the exhibit represent the impact estimates—that is, the difference within each subgroup between the treatment and control groups in the percent reporting affirmatively. A positive number means that more of those in the treatment group responded affirmatively compared to their counterparts in the control group; a negative number means that more of those in the control group responded affirmatively. Sample size represents the unweighted number who completed the survey in each group; sample sizes are slightly lower on some items due to missing data, and are lower for conditional outcomes. Subgroups are described in Chapter I.

Source: 18-month survey

[a] This is a conditional outcome, with the results restricted to those who have children at the time of the survey. Therefore, the random assignment 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.

[b] These questions were asked only of those not continuously incarcerated since random assignment. Because release after random assignment may be determined by participation in SCA, differences in outcomes between the groups are only suggestive of true estimates of impacts.

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

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

V. Impacts on Recidivism

Chapter IV showed that being in the program group led to significant increases in the receipt of re-entry services for program participants. Did these additional services lead to reductions in recidivism? We measure recidivism as involvement with the criminal justice system in the 18 months after random assignment that led to arrest, conviction, or incarceration. Both administrative data and survey data are used to measure recidivism for the full sample and the key subgroups. The impact of the program on re-incarceration measured for the full sample using administrative data is the confirmatory analysis for this study; other analyses are considered exploratory.

Findings in Brief

- Those in the program group were no less likely than those in the control group to be re-arrested, reconvicted or re-incarcerated. This conclusion does not change regardless of whether recidivism is measured using administrative data or survey data.
- There is some evidence that those in the program group were more likely to have probation/parole revoked and to be convicted of new crimes, possibly because of the increased supervision they experienced.
- Based on administrative data, within 18 months after RA:
  - More than 40 percent of those in the program and control groups were arrested.
  - Between 25 percent and 31 were convicted.
  - Just under half were re-incarcerated.
- There were few differences in the impact of assignment to the program group across the various subgroups.

Impacts Overall

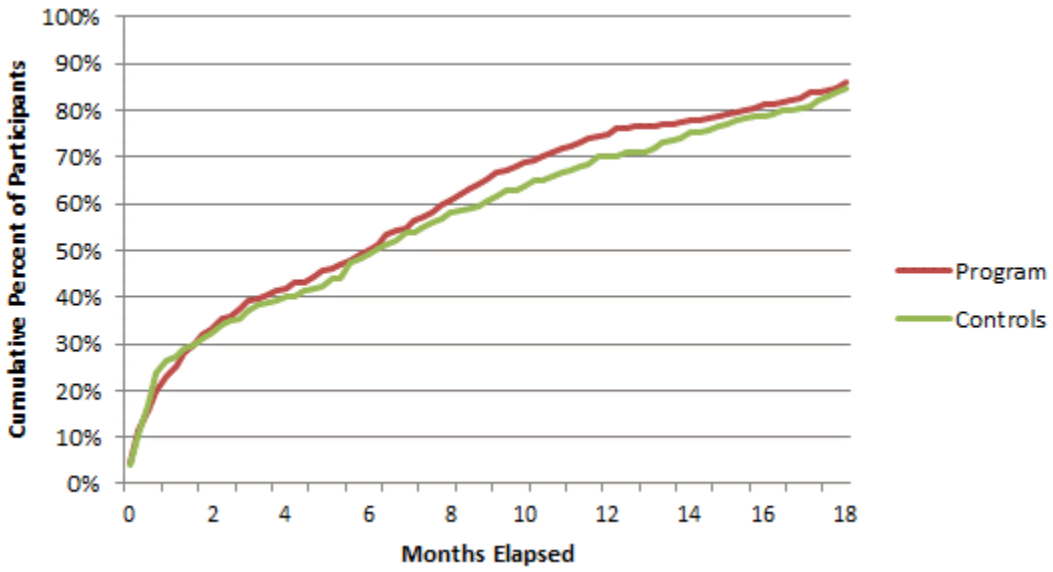
As Chapter II discussed, substantial numbers of study participants were incarcerated at the time of RA. We first explore whether assignment to the program group had an impact on the timing of release and, consequently, the duration at risk for recidivism following release. We next examine impacts on recidivism.

Impacts on Time to Release and Time at Risk

For those incarcerated at the time of RA, being in the program group could have had an impact on when an individual was released from incarceration. Exhibit V-1 examines this possibility by

showing, for those incarcerated at the time of RA, the time elapsed from RA to the release date for both the program and control groups. Among those released, the average time to release was approximately six months for both groups. There are no statistically significant differences between the groups.

Exhibit V-1: Time Elapsed from RA to Release, by Program Group



Notes: The plotted lines represent the cumulative percent of participants by the months elapsed from the RA date to the date of release. The sample is restricted to those study participants who were incarcerated at the time of RA.

Source: Administrative data from state and local agencies

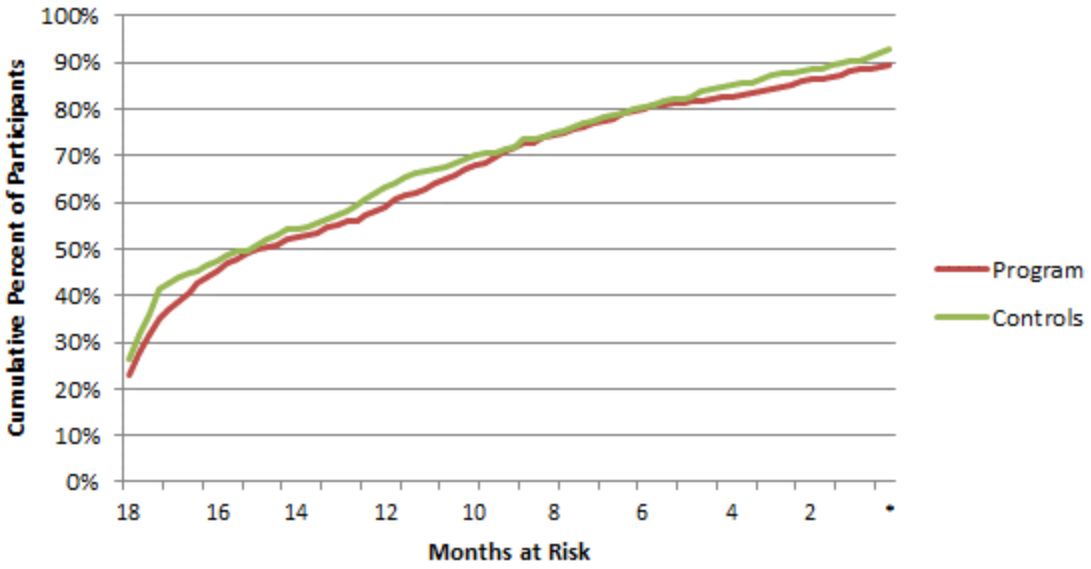
*/**/*** The difference between the program and control groups is statistically significant at the .1/.05/.01

The flip side of time to release is the duration at risk for recidivism. Because enrollment in SCA hypothetically could have had an impact on the release date, the study was designed such that all outcomes are measured for the 18 months beginning with the date of RA.[30] However, for individuals incarcerated at the time of RA, the time at risk of recidivism will be less than 18 months—and, based on the above exhibit, is indeed much less for some individuals. We therefore measured the time at risk for the program and control groups. For those who were randomly assigned after release, their time at risk is the full 18 months; for those who were randomly assigned while incarcerated, their time at risk is 18 months minus the time from RA to release.

[30] In a study using random assignment (or, indeed, any impact analysis) an event that occurs after treatment services begin is viewed as endogenous. Selecting on an endogenous variable can give rise to selectivity bias. For this reason, outcomes are measured from the date of RA.

Exhibit V-2 shows the cumulative percent of participants by time at risk for the program and control groups. Reading off the graph, the value at the intercept shows that approximately 20 percent of both groups have the entire 18 months at risk, and just under 60 percent have at least one year at risk. The lines for the two groups are largely in parallel, but slightly more control-group members have less than one month of time at risk.

Exhibit V-2: Months at Risk of Recidivism, by Program Group



Notes: The plotted lines represent the cumulative percent of participants, by the number of months outcomes were observed following release from incarceration, which represents the time at risk of recidivism.

Source: Administrative data from state and local agencies

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

Impacts on Recidivism for the Full Sample

The top panel of Exhibit V-3 reports various measures of recidivism for the program and control groups when outcomes were measured using administrative data for the period covering 18 months after RA. Based on these results, being in the program group does not appear to reduce rates of arrest, conviction, or incarceration, and, by some measures, those in the program group are more likely to have subsequent involvement with the criminal justice system. Results based on administrative data are summarized below.

- More than 40 percent of both the program and control groups were arrested sometime in the follow-up period, with no difference between the groups.
- There was an average of just over one arrest per person, with the average slightly higher for the program group than the control group.

- Approximately one-third of those in both groups were arrested with a public order offense. Those in the program group appeared somewhat more likely to have been arrested for a property crime. Violent crimes were uncommon.
- Those in the program group were more likely to be convicted of a new crime (31 percent for the program group versus 25 percent for the control group), and they have a slightly higher average number of convictions.[31]
- There is no difference between the groups in the rates of re-incarceration. A little less than half of both groups had been re-incarcerated in either a prison or jail.
- Those in both the program and control groups spent approximately 250 days in either prison or jail during the follow-up period (this includes time from the date of RA to initial release for those incarcerated at the time of RA); there was no difference between the groups.

Based on these results, there is no evidence that assignment to the program group in these seven sites decreased recidivism and, by some measures, it may even have increased it. That participation in SCA could increase recidivism is unexpected, but may reflect the finding from prior research that increased supervision can increase the likelihood of catching new offenses and violations of the terms of parole/probation when they occur (Taxman 2002, Jalbert et al. 2011).

The lower panel of Exhibit V-3 presents impacts using survey data and also provides no evidence that the program group had lower rates of recidivism. Generally, there are no significant differences between the groups across any of the measures of recidivism tabulated from the survey, with the exception that the number of revocations might be somewhat more likely for those in the program group. Noteworthy is that all the measures of recidivism calculated from survey data show a lower incidence than counterpart measures tabulated based on administrative data. This is consistent with the finding that the self-reported incidence of crime can have significant under-reporting (Thornberry and Krohn 2003, Wiegand et al. 2015).

We also calculated the time elapsed from the date of RA to the first arrest or re-incarceration. The cumulative frequency distributions of the first occurrence plotted by time elapsed are shown in Exhibit V-4 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 percentages with an occurrence at any time during the 18 months.

[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 V-3: Impacts on Recidivism for the Full Sample

	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 offense 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
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**	

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

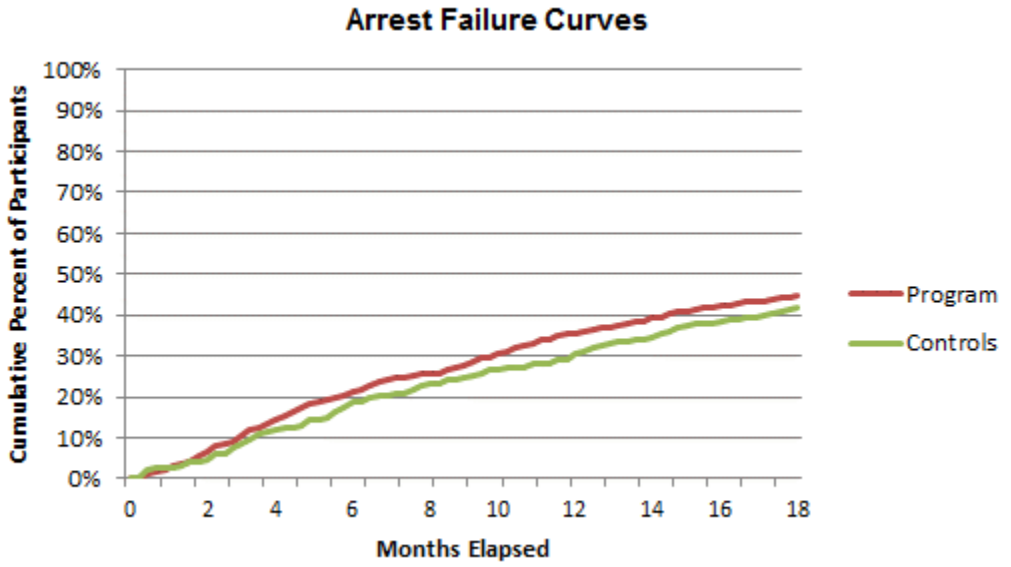
Source: Administrative data from state and local agencies and 18-month survey data

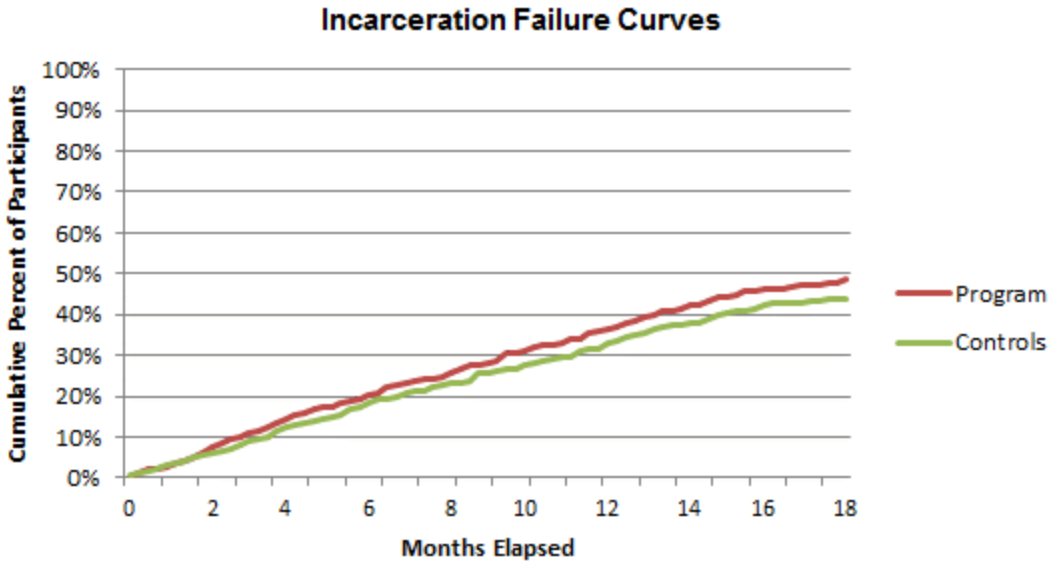
[a] The sum across categories exceeds the percent ever arrested because individuals can be arrested more than once and with different arrest charges in the 18-month follow-up period

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

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

Exhibit V-4: Risk Curves for Arrest and Re-incarceration, by Program Group





Notes: The plotted lines represent the cumulative percent 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.

Impacts on Recidivism by Time at Risk

The recidivism rates just reported are measured for the 18 months following RA for the full sample. However, we know from the discussion earlier in this chapter that the time at risk for recidivism among participants who were incarcerated at the time of RA can be much less than 18 months. In this section, we calculate recidivism rates for the subset of the sample whose outcomes are observed for at least 12 months (but no more than 18 months) following release from incarceration. These individuals were either not incarcerated at the time of RA, or were incarcerated but had less than six months elapse from the RA date to the date of release.[32] Furthermore, when calculating total days incarcerated, we excluded the days incarcerated between RA and initial release.

In comparison to Exhibit V-3, Exhibit V-5 shows that, as one would expect given that this analysis is restricted to a subset with a longer average time at risk, rates of re-arrest, reconviction, and re-incarceration are somewhat higher than for the full sample. Also, as would be expected, total days incarcerated are appreciably less because days of incarceration between RA and release are excluded from the tally. However, substantive conclusions are unchanged—assignment to the program group did not reduce recidivism and may even have increased recidivism on some dimensions.

Impacts for Subgroups

Exhibit V-6 presents impacts for three of the key subgroups. The impacts of assignment to the program group do not differ significantly by gender and they do not differ between the two risk groups—that is, being in the program group did not significantly reduce recidivism for any of these groups, regardless of whether recidivism is measured by arrests, convictions, or incarcerations, and no matter whether administrative data or survey data are used.

However, there are some significant differences in impacts for subgroups defined by age. Both administrative data and survey data present a consistent picture that assignment to the program group increased arrests and convictions for younger cohorts, and, based on survey data, also increased the incidence of parole/probation violations and re-incarcerations. However, these significant negative impacts did not appear for those who were older.

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[32] As we discussed, the date of release could be influenced by participation in SCA and, for this reason, differences between the program and control groups for this sample subset may not yield unbiased estimates of impacts. However, given that differences between the program and control groups in the time elapsed from RA to release are minimal (see Exhibit IV-1), significant bias is unlikely.

Exhibit V-5: Impacts on Recidivism, for Those with at Least One Year at Risk

	Program	Control	Difference
A. Outcomes Measured from Administrative Data			
Arrests			
Arrested (%)	60.9	55.9	5.0
Average number of arrests	2.0	1.5	0.5***
Arrests by offense type (%)[a]			
Violent crime	9.9	9.4	0.5
Property crime	31.1	19.3	11.8***
Drug crime	24.8	23.3	1.5
Public order crime	50.3	45.4	4.9
Convictions			
Convicted of a crime (%)	41.6	32.9	8.7**
Average number of convictions	0.6	0.4	0.2***
Incarcerations (prison or jail)			
Was re-incarcerated in prison or jail (%)	57.2	52.6	4.6
Experienced a new jail incarceration (%)	50.2	45.6	4.6
Experienced a new prison incarceration (%)	25.2	24.6	0.6
Total days incarcerated	86.0	83.9	2.1
Sample Size	**315**	**203**	
B. Outcomes Measured from the Survey			
Arrests			
Arrested (%)	40.4	34.4	6.0
Average number of arrests	1.0	0.8	0.2
New charges and convictions			
Formally charged with a new crime (%)	25.9	21.3	4.6
Convicted of a new crime (%)	18.6	14.1	4.6
Average number of new convictions	0.3	0.2	0.1*
Parole/probation violations			
Charged with a violation (%)	35.9	37.2	-1.3
Probation/parole revoked (%)	22.6	17.7	4.9
Incarcerations (prison or jail)			
Was re-incarcerated in prison or jail (%)	44.9	42.0	2.9
Average number of re-incarcerations	1.1	0.8	0.3
Currently incarcerated (%)	24.7	29.3	-4.6
Sample Size	**606**	**360**	

Notes: Numbers in the first two columns represent outcomes measured in the period following release from incarceration for the program and control groups; the third column represents the difference between the first two columns. The sample is restricted to those whose outcomes are observed for at least 12 months and no more than 18 months following release.

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

[a] The sum across categories exceeds the percent ever arrested because individuals can be arrested more than once and with different arrest charges in the 18-month follow-up period

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

Exhibit V-7 presents results for the remaining two subgroups. The impact of being in the program group is not significantly different between those randomly assigned well before release from incarceration and those randomly assigned near release or after, with the exception that being in the program group was more likely to increase the incidence of being charged with a parole or probation violation for those randomly assigned well before release compared to those randomly assigned later. Grantees that are associated with the state or local criminal justice system appear to be about as effective as local agencies; the differences in impacts between the two groups of grantees are not significant.

Summary

As of 18 months after random assignment, being assigned to the program group did not lead to desistance. Whether recidivism was measured by survey or administrative data, those in the program group were not less likely than those in the control group to be re-arrested, reconvicted, or re-incarcerated (in either prison or jail), and the time to their first event—an arrest or incarceration—was no shorter. This picture does not change appreciably for the various subgroups considered, except that SCA increased subsequent involvement with the criminal justice system among those who are younger but not among those who are older.

Exhibit V-6:
Impacts of SCA on Recidivism, by Gender, Age, and Risk Subgroups

	Gender		Age		Level of Risk	
	Females	Males	Less than 30	30 or Older	Lower	Higher
A. Outcomes Measured from Administrative Data						
Arrests						
Arrested (%)	10.0	1.4	9.6**	-3.1	8.9*	1.3
Average number of arrests	0.3	0.3**	0.6*** †	0.0	0.3	0.4*
Arrests by offense type (%)[a]						
Violent crime	-5.0	-1.6	-0.4	-3.9*	-2.9	-0.5
Property crime	15.4***	6.2**	10.2***	5.7	6.3*	8.9**
Drug crime	3.9	1.2	6.7*	-2.6	4.6	3.0
Public order crime	2.2	1.2	7.9*	-4.8	3.5	1.9
Convictions						
Convicted of a crime (%)	2.4	7.5**	14.4*** †	-0.7	7.8*	7.5*
Average number of convictions	-0.0	0.2***	0.3*** †	-0.0	0.1**	0.2**
Incarcerations (prison or jail)						
Was re-incarcerated in prison or jail (%)	13.2*	2.6	10.5**	0.5	7.5	4.4
Experienced a new jail incarceration (%)	10.5	1.1	9.2*	-2.0	7.8*	1.1
Experienced new prison incarceration (%)	10.2*	0.3	7.4*	-1.2	4.6	2.3
Total days incarcerated[b]	-3.8	5.3	-18.1	24.8	17.0	-9.9
Sample Size	**203**	**763**	**440**	**526**	**452**	**474**
B. Outcomes Measured from the Survey						
Arrests						
Arrested (%)	8.4	4.5	15.4*** †	-2.8	3.5	10.1*
Average number of arrests	0.2	-0.0	0.4*	-0.2	-0.1	0.2
New charges and convictions						
Formally charged with a new crime (%)	7.6	2.4	10.2**	-2.2	3.1	7.2
Convicted of a new crime (%)	4.4	2.2	9.5** †	-3.7	0.0	7.9**
Average number of new convictions	0.1	0.1	0.1** †	-0.0	0.1	0.1***
Parole/probation violations						
Charged with a violation (%)	0.3	1.9	10.1* †	-5.3	3.7	2.7
Probation/parole revoked (%)	7.6	3.9	7.4*	2.9	5.1	6.2

	Gender		Age		Level of Risk	
	Females	Males	Less than 30	30 or Older	Lower	Higher
B. Outcomes Measured from the Survey (continued)						
Incarcerated (prison or jail)						
Was re-incarcerated in prison or jail (%)	10.1**	1.4	15.0*** †	-5.6	5.9	5.8
Average number of re-incarcerations	0.3**	0.1	0.6*** †	-0.3	-0.1	0.3*
Currently incarcerated (%)	6.3*	-4.6	4.1	-7.4	-6.4	3.6
Sample Size	**172**	**617**	**365**	**424**	**368**	**388**

Notes: Numbers in the exhibit represent the impact estimates—that is, the difference within each subgroup between the incidence or mean value for the treatment group versus the control group. A positive number denotes that the incidence or mean value is higher for the treatment group than for the control group; a negative number means 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 and the 18-month survey

[a] The sum across categories exceeds the percent ever arrested because individuals can be 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.

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

† The difference in the impact of SCA between the subgroups in the 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 V-7:
Impacts of SCA on Recidivism, by Timing of Entry and Grantee Type

	Timing of Entry		Grantee Type	
	Well Before Release	Nearer to Release	Criminal Justice	Social Service
A. Outcomes Measured from Administrative Data				
Arrests				
Arrested (%)	7.1*	1.3	7.9*	-7.0
Average number of arrests	0.3***	0.4*	0.4**	0.1
Arrests by offense type (%)[a]				
Violent crime	-2.7	-1.7	-1.3	-4.9
Property crime	8.2***	9.6	8.5***	7.3
Drug crime	5.1*	-1.4	4.7*	-4.3
Public order crime	4.2	1.7	4.6	-5.5
Convictions				
Convicted of a crime (%)	10.0***	3.3	7.1*	5.0
Average number of convictions	0.1***	0.1	0.2***	0.1
Incarcerations (prison or jail)				
Was re-incarcerated in prison or jail (%)	8.4**	0.8	8.6**	-3.6
Experienced a new jail incarceration (%)	7.3*	-1.4	6.5	-4.1
Experienced new prison incarceration (%)	5.0	-1.0	5.5	-4.0
Total days incarcerated[b]	-16.2	-12.2	-2.4	8.6
Sample Sizes	**594**	**372**	**642**	**324**
B. Outcomes Measured from the Survey				
Arrests				
Arrested (%)	8.8**	-0.6	7.2*	1.3
Average number of arrests	0.2	-0.3	0.1	0.0
New charges and convictions				
Formally charged with a new crime (%)	5.9	-1.2	3.2	4.1
Convicted of a new crime (%)	4.9	-2.0	0.9	6.2
Average number of new convictions	0.1**	0.0	0.1	0.1
Parole/probation violations				
Charged with a violation (%)	7.9* †	-9.6	1.5	1.5
Probation/parole revoked (%)	5.8*	3.4	3.4	7.3*
Incarcerated (prison or jail)				
Was re-incarcerated in prison or jail (%)	7.7*	-5.0	5.0	-0.3
Average number of re-incarcerations	0.2	-0.1	0.1	0.1
Currently incarcerated (%)	-1.6	-6.4	-2.8	-2.0
Sample Size	**546**	**243**	**534**	**255**

Notes: Numbers in the exhibit represent the impact estimates—that is, the difference within each subgroup between the incidence or mean value for the treatment group versus their control group. A positive number denotes that the incidence or mean value is higher for the treatment group than for the control group; a negative number means 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 and the 18-month survey

[a] The sum across categories exceeds the percent ever arrested because individuals can be 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.

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

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

VI. Impacts on Employment and Earnings

Although the nature of the relationship between employment and recidivism is not clear (Uggen 2000, Tripoldi et al. 2009, Apel and Horney 2017), there are strong theoretical and practical reasons for believing that helping the formerly incarcerated obtain employment can improve desistance (Uggen and Staff 2001, Duran et al. 2013). Accordingly, all the grantees provided employment assistance as part of their programs, through direct service or formal or informal partnerships (see Chapter II). This chapter examines whether these efforts led to improved employment and earnings.

Two sources of data are used for measuring employment-related outcomes: survey data and NDNH administrative data. The survey, administered 18 months after RA, asked study participants whether they were ever employed after RA, whether they were employed at the time of the survey (labelled "currently employed" in this chapter) and, if currently employed, their hourly wage and whether they worked full time or had a part-time, temporary or "off-the-books" job. NDNH was used to measure whether study participants were employed anytime in the fifth and sixth calendar quarters after the quarter of RA, and, if employed, their earnings.[33]

Findings in Brief

- For the full study sample, being assigned to the program group did not improve the probability of being employed, hourly wages, or earnings.
- Assignment to the program group did not improve employment-related outcomes consistently for any subgroup.

Impacts Overall

The top panel of Exhibit VI-1 reports employment outcomes measured using survey data for the period covering 18 months after RA.

[33] NDNH measures employment and earnings in calendar quarters. The study team used these data to measure outcomes in the fifth and sixth calendar quarters after the quarter of RA—thus, for two quarters (six months) that fell sometime within the interval of 13 to 21 months after RA, depending on each participant's date of RA within the calendar quarter of RA. Due to limitations on data access, we could not measure employment and earnings using NDNH for the entire period since RA for each study participant.

Exhibit VI-1: Impacts on Employment and Earnings for the Full Sample

	Program	Control	Difference
A. Outcomes Measured from the Survey			
Ever employed since RA (%)	72.8	72.5	0.4
Currently employed (%)	33.0	34.9	-1.9
Of those currently employed[a]			
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 ($)	12.39	11.43	0.96
Sample Size	**494**	**294**	
B. Outcomes Measured from NDNH			
Employment Status (%)			
Employed anytime in the fifth quarter	35.7	33.5	2.2
Employed anytime in the sixth quarter	35.5	33.0	2.5
Employed anytime in either the fifth or sixth quarters	43.3	40.4	2.9
Earnings[b] ($)			
Earnings in the fifth quarter	1,609	1,529	80
Earnings in the sixth quarter	1,612	1,446	166
Earnings in both the fifth and sixth quarters	3,207	2,975	232
Sample Size	**602**	**355**	

Notes: Numbers in the first two columns represent the percent with the outcome, measured in the 18 months following the date of random assignment; the third column represents the difference between the first two columns. Sample sizes for analysis using survey data are reported regardless of whether the sample member is employed. One individual in the SCA program group was excluded from the calculation of NDNH earnings, because this individual's earnings were an extreme outlier.

Source: 18-month survey and the National Directory of New Hires

[a] These are conditional outcomes, with the results restricted to those who were employed at the time of the survey. Therefore, the random assignment 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.

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

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

Results based on survey data can be summarized as follows.

- Just over 72 percent of those in both the program and control groups reported that they worked sometime in the 18 months since random assignment.
- Approximately one-third of those in both groups were working at the time of the survey.
- Of those who were working at the time of the survey:
  - Approximately two-thirds of those in both groups were working full time in regular jobs.

- The average hourly rate of pay was $12.39 for the program group and $11.43 for the control group.

None of the differences between the program and control groups is statistically significant.

The lower panel of Exhibit VI-1 presents results using data from NDNH. Consistent with the findings discussed above, there are no statistically significant differences between the program group and the control group. Approximately one-third of each group was employed sometime in each of the fifth and sixth quarters after RA. Including zero earners, the average combined earnings in both the fifth and sixth quarters was $3,207 for the program group and $2,975 for those in the control group, but the difference is not statistically significant.

Impacts for Subgroups

Exhibits VI-2 and VI-3 present impact estimates for the various subgroups that were introduced in Chapter I; the numbers shown in the table are impact estimates (that is, the *differences* in outcomes between the program and control groups *within* each subgroup). Using survey data, results show that being assigned to the program group has no statistically significant impact on employment-related outcomes for any of the ten subgroups, and there are no differences in impacts within any subgroup pair. In other words, based on survey data, being in the program group did not improve employment outcomes for either males or females, for younger or older study participants, for those at various risks of recidivism, for those randomly assigned well before release from incarceration as opposed to nearer to or after release, or for those served by criminal justice or social service agencies.

Results based on NDNH tell much the same story. However, there is some evidence that males are more likely to benefit from SCA participation than females in terms of being employed in the fifth and sixth quarters after RA.

Summary

In the seven grantee sites participating in this study, assignment to the program group did not improve employment-related outcomes overall or consistently for any subgroup.

Exhibit VI-2:
Impacts of SCA on Employment and Earnings, by Gender, Age, and Risk Subgroups

	Gender		Age		Level of Risk	
	Females	Males	Less than 30	30 or Older	Lower	Higher
A. Outcomes Measured from the Survey						
Ever employed since RA (%)	-6.3	2.2	6.1	-5.0	0.7	0.9
Currently employed (%)	1.0	-2.5	-6.8	2.3	0.9	-5.6
Of those currently employed[a]						
Employed full time (%)	-2.3	6.1	-0.0	6.8	5.2	2.1
Employed part time or in temporary or seasonal jobs, or off-the-books (%)	2.3	-6.1	0.0	-6.8	-5.2	-2.1
Hourly rate of pay ($)	-0.44	1.34	-0.50	1.98	2.39*	-0.20
Sample Sizes	171	617	365	423	368	388
B. Outcomes Measured from NDNH						
Employment Status (%)					---	---
Employed anytime in the fifth quarter	-8.9†	5.0	0.2	4.5	---	---
Employed anytime in the sixth quarter	-9.4†	5.5	-0.6	6.1	---	---
Employed anytime in either the fifth or sixth quarters	-9.7†	6.2	-0.4	6.8	---	---
Earnings[b] ($)						
Earnings in the fifth quarter	-304	190	-269	419	---	---
Earnings in the sixth quarter	115	189	-288*	621*	---	---
Earnings in both the fifth and sixth quarters	-189	363	-585*	1,040	---	---
Sample Sizes	200	757	482	475		

Notes: 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 means that the incidence or mean value is higher for the control group. Subgroups are defined in Chapter I. Sample sizes for analysis using survey data are reported regardless of whether the sample member is employed. One individual in the SCA program group was excluded from the calculation of NDNH earnings, because this individual's earnings were an outlier. Due to limitations on data access, subgroup estimates of impacts could not be estimated using NDNH for those at different risks of recidivism.

Source: The 18-month survey and NDNH.

[a] These are conditional outcomes, with the results restricted to those who were employed at the time of the survey. Therefore, the random assignment 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.

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

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

† The difference in the impact of SCA between the subgroups in the 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 VI-3:
Impacts of SCA on Employment and Earnings, by Timing of Entry and Grantee Type

	Timing of Entry		Grantee Type	
	Well Before Release	Nearer to Release	Criminal Justice	Social Service
A. Outcomes Measured from the Survey				
Ever employed since RA (%)	3.1	-2.6	-0.6	2.3
Currently employed (%)	-2.9	2.0	-2.9	0.4
Of those currently employed[a]				
Employed full time (%)	1.4	7.0	1.8	9.2
Employed part time or in temporary or seasonal jobs, or off-the-books (%)	-1.4	-7.0	-1.8	-9.2
Hourly rate of pay ($)	1.36	0.28	-0.23	3.13*
Sample Sizes	545	243	534	255
B. Outcomes Measured from NDNH				
Employment Status (%)				
Employed anytime in the fifth quarter	0.9	5.9	2.8	1.2
Employed anytime in the sixth quarter	1.5	4.4	5.4	-3.3
Employed anytime in either the fifth or sixth quarters	0.3	8.3	4.7	-0.5
Earnings[b] ($)				
Earnings in the fifth quarter	131	23	205	-171
Earnings in the sixth quarter	202	148	331	-169
Earnings in both the fifth and sixth quarters	313	172	536	-384
Sample Sizes	653	304	639	318

Notes: 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 means that the incidence or mean value is higher for the control group. Subgroups are defined in Chapter I. Sample sizes for analysis using survey data are reported regardless of whether the sample member is employed. One individual in the SCA program group was excluded from the calculation of NDNH earnings, because this individual's earnings were an extreme outlier.

Source: The 18-month survey and NDNH.

[a] These are conditional outcomes, with the results restricted to those who were employed at the time of the survey. Therefore, the random assignment 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.

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

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

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

VII. Impacts on Other Outcomes

This chapter examines the impacts of assignment to the program group on a variety of outcomes not yet discussed: housing, health status, substance abuse, ability to meet child-support obligations, and total income. It reports impacts for each of these outcomes measured from the participant survey, first for the full sample, and then for key subgroups.

Findings in Brief

- Being in the program group had no impacts on housing, health status, substance abuse or ability to meet child support obligations, as measured by indicators available in the survey.
- Being in the program group may have improved the likelihood that a participant had enough income for meeting essential needs.
- There were no noteworthy differences in impacts across subgroups.

Impacts Overall

Exhibit VII-1 reports results for the full sample, and, with scant exception, there were no impacts of assignment to the program group on any of the outcomes shown.

- *Housing.* Approximately one-fourth of both the treatment and control groups were living in their own house, apartment, or room at the time of the survey, and, among those doing so, approximately half contributed money for rent or a mortgage.
- *Health status.* Questions about health and health access were asked only of those who were not incarcerated at the time of the survey. Of this group, approximately three-fourths reported that their health was good, very good or excellent. Substantial numbers needed to see a doctor or dentist but could not afford to do so. Approximately 40 percent made a visit to an emergency room. Approximately 20 percent had a health condition limiting work or other activities in the month prior to the survey; approximately 15 percent had a limiting emotional problem. There was no impact of assignment to the program group on any of these measures.
- *Substance abuse.* Questions about substance abuse also were asked only of those who were not incarcerated at the time of the survey. Relatively small numbers admitted to using illegal drugs or drinking to excess within the month prior to the survey.
- *Child support.* Of those with an order to pay child support, approximately half paid through the child-support enforcement system; others made informal payments to the custodial parent. Being in the program group did not have an impact on whether payments were made.

Exhibit VII-1: Impacts on Other Outcomes for the Full Sample

	Program	Control	Difference
Housing			
Currently living in own house, apartment, or room[a]	25.0	23.8	1.1
Contributes to the cost of rent/mortgage[b]	57.3	55.2	2.1
Health			
Health is good, very good, or excellent[c]	78.6	77.9	0.7
Needed to see a doctor but could not afford to[c]	29.9	30.1	-0.1
Needed to see a dentist but could not afford to[c]	38.1	40.2	-2.1
Made a visit to an emergency room since RA[c]	40.3	37.1	3.3
Health condition limited work or other activities in the past month[c]	20.9	19.4	1.5
Emotional problems limited work or other activities in the past month[c]	14.9	15.4	-0.5
Substance Abuse			
Used illegal drugs last month[c]	12.7	13.9	-1.2
Had 5 or more drinks in a row sometime last month[c]	12.5	14.6	-2.0
Child Support			
Paid required child support last month[d]	53.7	54.9	-1.1
Paid other support to a custodial parent[d]	39.1	47.7	-8.6
Income			
Had enough income to support self last month[c]	68.6	60.2	8.4**
Total income since RA/release:[c]			
Up to $5,000	43.9	42.9	1.0
$5,001 to $10,000	19.8	21.3	-1.5
$10,001 to $20,000	22.9	20.3	2.5
More than $20,000	13.5	15.5	-2.0
Sample Size	**495**	**294**	

Notes: Numbers in the first two columns represent the percent with the outcome, measured in the 18 months following the date of random assignment; the third column represents the difference between the first two columns.

Source: 18-month survey

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

[b] This is a conditional outcome restricted to those living in their own house, apartment, or room. Therefore, random assignment does not ensure equivalence in baseline characteristics between the groups, and differences between the groups are only suggestive of true estimates of impacts.

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

[d] 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, random assignment 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.

- *Income*. In the month prior to the survey, approximately 69 percent of those in the program group reported having enough income, compared to only 60 percent of those in the control group. This difference is statistically significant. There were no significant differences in total income in the period since RA (or release, for those incarcerated at the time of RA).

Impacts for Subgroups

Exhibits VII-2 and VII-3 present impact estimates for the subgroups. There is no evidence that the impacts discussed in this chapter were consistently different across any of the subgroups.

Summary

Based on indicators measured from the survey, assignment to the program group did not improve housing or health status, did not decrease the incidence of substance abuse, and did not increase the likelihood that a participant paid child support. There is modest evidence that being in the program group increased the adequacy of one's income for meeting necessary expenses, although there were no differences in actual reported income between the program and control groups.

Exhibit VII-2:
Impacts of SCA on Other Outcomes, by Gender, Age, and Risk Subgroups

	Gender		Age		Level of Risk	
	Females	Males	Less than 30	30 or Older	Lower	Higher
Housing						
Currently living in own house/apartment/room[a]	2.3	0.6	-1.4	2.5	-1.8	2.0
Contributes to the cost of rent/mortgage[b]	9.5	-0.0	-4.8	6.3	3.4	-3.6
Health						
Health is good, very good, or excellent[c]	2.9	0.2	1.5	1.4	2.1	0.7
Needed to see a doctor but could not afford to[c]	7.4	-2.2	-0.0	-0.8	1.1	0.1
Needed to see a dentist but could not afford to[c]	2.7	-3.6	-11.3** †	5.1	3.8	-7.2
Made a visit to an emergency room since RA[c]	0.5	-3.7	-1.8	7.1	2.5	4.7
Health condition limited work or other activities in the past month[c]	4.5	0.4	-2.8	4.1	-0.3	1.5
Emotional problems limited work or other activities in the past month[c]	2.7	-1.7	-2.6	1.0	-0.3	-1.3
Substance Abuse						
Used illegal drugs last month[c]	3.0	-2.6	0.5	-2.3	-0.9	-0.5
Had 5 or more drinks in a row sometime last month[c]	-7.2	-0.4	3.9	-5.8	-3.2	2.1
Income						
Had enough income to support self last month[c]	9.3	8.1	14.3**	3.7	12.6**	3.7
Total income since RA/release:[c]						
Up to $5,000	6.9	-0.4	0.0	2.8	-0.9	2.5
$5,001 to $10,000	-2.0	-1.5	1.3	-3.2	-4.4	0.7
$10,001 to $20,000	-2.4	3.8	1.0	3.2	5.0	-0.5
More than $20,000	-2.5	-1.9	-2.3	-2.9	0.2	-2.6
Sample Sizes	**172**	**617**	**365**	**424**	**368**	**388**

Notes: Numbers in the exhibit represent the impact estimates—that is, the difference within each subgroup between the incidence or mean value for the treatment group versus the control group. A positive number denotes that the incidence or mean value is higher for the treatment group than for the control group; a negative number means that the incidence or mean value is higher for the control group. Sample size represents the unweighted number who completed the survey in each group. Subgroups are defined in Chapter I. Results are not shown for payment of child support because subgroup sample sizes are too small for estimates to be reliable.

Source: 18-month survey

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

[b] This is a conditional outcome restricted to those living in their own house, apartment, or room. Therefore, random assignment does not ensure equivalence in baseline characteristics between the groups, and differences between the groups are only suggestive of true estimates of impacts.

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

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

† The difference in the impact of SCA between the subgroups in the 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 VII-3:
Impacts of SCA on Other Outcomes, by Timing of Entry and Grantee Type

	Timing of Entry		Grantee Type	
	Well Before Release	Nearer to Release	Criminal Justice	Social Service
Housing				
Currently living in own house, apartment, or room[a]	-0.7	5.1	-0.2	4.0
Contributes to the cost of rent/mortgage[b]	0.4	6.1	-2.1	8.6
Health				
Health is good, very good, or excellent[c]	2.5	-3.4	2.7	-3.1
Needed to see a doctor but could not afford to[c]	-2.6	4.6	-1.2	2.0
Needed to see a dentist but could not afford to[c]	-2.1	-0.8	-5.8	4.7
Made a visit to an emergency room since RA[c]	5.7	2.5	2.8	4.1
Health condition limited work or other activities in the past month[c]	0.1	5.3	-2.9	9.9*
Emotional problems limited work or other activities in the past month[c]	2.2	-5.2	-0.1	-1.5
Substance Abuse				
Used illegal drugs last month[c]	-0.8	-2.6	-1.5	-0.6
Had 5 or more drinks in a row sometime last month[c]	-3.1	-1.1	-5.1	2.7
Income				
Had enough income to support self last month[c]	8.7	9.3	3.0	16.6**
Total income since RA/release:[c]				
Up to $5,000	1.7	-6.3	3.3	-3.4
$5,001 to $10.000	-2.7	1.1	-1.1	-2.2
$10,001 to $20,000	1.1	6.6	0.4	6.4
More than $20,000	-0.0	-1.3	-2.7	-0.7
Sample Sizes	**546**	**243**	**534**	**255**

Notes: Numbers in the exhibit represent the impact estimates—that is, the difference within each subgroup between the treatment and control groups in the percent reporting affirmatively. A positive number means that more of those in the treatment group responded affirmatively compared to their counterparts in the control group; a negative number means that more of those in the control group responded affirmatively. Sample size represents the unweighted number who completed the survey in each group. Subgroups are defined in Chapter I. Results are not shown for payment of child support because subgroup sample sizes are too small for estimates to be reliable.

Source: 18-month survey

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

[b] This is a conditional outcome restricted to those living in their own house, apartment, or room. Therefore, random assignment does not ensure equivalence in baseline characteristics between the groups, and differences between the groups are only suggestive of true estimates of impacts.

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

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

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

VIII. Summary and Conclusions

This study estimated the 18-month impacts of seven programs that were awarded grants through the first round of funding under the SCA Adult Demonstration Program. Impacts were estimated using an RA design with individuals screened and eligible for SCA randomly assigned to either be allowed entry into the SCA program or receive re-entry services normally available but not enroll in SCA. The differences in outcomes between the two groups were then compared.

About the Grantees and Their Programs

According to the SCA grant solicitation, the grantees were expected to serve individuals 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 SCA awards were an important source of funds and led to system improvements. The implementation study revealed that the SCA awards helped the grantees expand re-entry services in the grantees' communities, improve partnerships with other community agencies, and strengthen the connection between pre-release and post-release services.

Case management was central. Case management was a central 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.

Beyond providing case management, each grantee used its SCA grant to support a variety of services. All the grantees devoted significant SCA resources to post-release services, and some also enhanced pre-release services. Specific services supported through SCA included cognitive behavioral therapy, employment assistance, substance abuse treatment, and housing assistance, among others. Services that a grantee could not fund through its grant were made available through unfunded referrals.

Just over one-third of those assigned to the program group received both pre-release and post-release SCA services following their program enrollment. According to the grantees' MIS data, 36 percent of those in the program group received both pre-release and post-release SCA services, 40 percent received only post-release service, and just over 20 percent received only pre-release services.

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 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.

The length of formal participation in SCA varied greatly. 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 less than six months.

Summary of Impact Findings

Outcomes were assessed for a period covering 18 months after individuals were randomly assigned. Because case management was the central focus of most grantees' programs, the impact study can be thought of as primarily representing the effects of this services. However, given the differences across grantees in service designs, this study does not test the efficacy of a specific program model.

Being in the program group led to a substantial increase in the receipt of services. Those in the program group were significantly more likely to receive a wide range of re-entry services. They were more likely to receive help with re-entry and were more likely to have an individual case plan. They were more likely to report having someone who went out of the way to help. They were also more likely to receive cognitive behavioral therapy, help with looking for a job, substance abuse treatment, housing assistance, and mentoring.

Increased access to services by those assigned to the program group did not lead to increased desistance. As of 18 months after random assignment, those in the program group did not have less involvement with the criminal justice system. Whether recidivism was measured using survey or administrative data, 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 may have been somewhat more likely to have probation or parole revoked and to have new convictions, possibly because of the increased supervision they experienced.

There were no impacts on employment-related outcomes. In the seven grantee sites participating in this study, assignment to the program group did not improve the probability of being employed in the follow-up period. In the last six months of follow-up, those in the

program group earned an average of about $3,200 and those in the control group about $3,000, but the difference between the groups is not statistically significant.

The program group may have better income adequacy. Study participants were asked about their income for the last month of the 18-month follow-up period. 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 that month.

The program had no effect on a range of other outcomes. The survey measured the adequacy of housing, health status, self-reported illegal drug use and excessive alcohol consumption, and the ability to meet child-support obligations. Being in the program group had no effect on any of these outcomes.

There are, at best, 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; that is, assignment to the program group worked about the same for each subgroup in the subgroup pair.

The study's major findings are robust to alternative model specifications and data sources. We estimated impacts as a simple difference in means between the program and control groups and using more complex statistical methods. For recidivism, the key outcome of interest, we estimated impacts using various measures of recidivism and using both administrative data and survey data, which provide independent estimates of desistance. The findings summarized above hold up to alternative model specifications and data sources.

Why Were There No Impacts on 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. Why did these additional services not improve desistance? A number of general reasons can be suggested (although not every reason applies to each grantee).

1. ***Control-group members accessed many of the same services that program-group members did, both before and after release*.** Although SCA significantly increased access to a wide range of services, the difference in service receipt between the program group and the control group was modest—at most, the program group was 25 percentage points more likely to receive a given service than the control group. For example, 61 percent of the program group reported getting help with job-finding skills, but 40 percent of the control group also reported receiving this service. Even if the

services were effective, the gap in service receipt between the groups might not be large enough to translate into differences in recidivism or other outcomes. There are several reasons why so many control group members were able to access services.

a. *Control group members had access to services available in prisons and jails after RA but while still incarcerated*. Most institutions offered courses and workshops available to their inmates without regard to SCA eligibility. Depending on the institution, these services included substance abuse treatment, adult literacy instruction, employment assistance, cognitive change therapy, and others. RA generally occurred while individuals were incarcerated, so the control group, just as the program group, had access to these services. Although SCA case managers who worked with SCA participants prior to release might have made special efforts to encourage program-group members to take advantage of these services, the services were generally available to those in the control group without restriction.

b. *A substantial proportion of control-group members got help with re-entry.* According to the participant survey, 59 percent of the control group reported that they got help with re-entry (compared to 78 percent of the program group). Whether this help was provided by a traditional PO or someone else, this individual could have provided many of the same services that SCA case managers did: assessing service needs, offering advice, providing referrals, etc. From qualitative findings, we know that SCA case managers and enhanced POs were more involved than traditional POs were in brokering services, but the difference was one of degree

c. *Grantees and their partners had other sources of funding, which were, in many cases, quite substantial*. All the grantees were required to leverage funds from multiple sources, which could include state and local funds and grants from philanthropic organizations or other sources. Similarly, the grantees' partners were existing organizations with their own funding sources and pre-existing outreach mechanisms. SCA funds, while much appreciated and valued by all the grantees and the partners the grantees funded, were often not the largest share of the organizations' budgets. These other sources of funds were not specifically earmarked for SCA participants and could have been used to serve control group members or others in need.

2. ***Given available funding, there were limitations to what SCA could do.*** Those returning from incarceration face challenges to re-entry that are many and complex (e.g., Petersilia 2003). The grantees' services could not help participants fully overcome these challenges.

a. *Funds were not adequate to directly fund all participants' needs.* Due to resource constraints, all 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.

b. *At the end of 18 months, SCA participants had many unmet needs*. Despite SCA's significant impacts on services received, those in the program group still reported

many unmet service needs 18 months after RA. Two-thirds wanted additional housing assistance, and more than half wanted additional job placement assistance, job training, health services, and educational services. In fact, at the end of the 18-month follow-up period, those in the program group were just as likely to express the need for additional services as those in the control group.

3. ***There were inherent limitations to the projects that grantees developed.*** Although the grantees used evidence on what works in developing their programs, there were limitations to their program 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; for others, it was provided by staff members from a social services agency or community-based organization. However, in their review of correctional rehabilitation approaches, Cullen and Gendreau (2000) cite 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).

   b. *It was hard to ensure that participants got the services they needed through unfunded referrals.* Many services were provided through unfunded referrals. Using unfunded referrals to provide services 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, one limitation was that there was often no way for the grantee to ensure that participants would seek out the services to which they were referred. Moreover, the quality of services provided by loosely connected partners can be uncertain.

   c. *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) argue 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) note 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 and Caveats

The SCA grant funds helped the grantees enhance their existing programs and capacity and strengthen their partnerships. 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 against all the others (Heckman et al. 2000).

At the same time, modifications to the service models that the grantees developed might help improve outcomes. Even before these impact findings were made available, the Department of Justice learned from the experiences of the grantees in this study and others that received early funding through the Adult Demonstration Program. Based on what it learned from the grantees' implementation experiences, it tightened requirements for grantees that received subsequent waves of grant 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 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 engage with participants prior to release.
- Grantees must ensure adequate dosage of cognitive-based interventions.

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

Next Steps

The findings described in this report cover 18 months after RA. This represents a relatively short observation period. Many SCA participants in this study were enrolled in SCA while they were incarcerated and were not released from custody for six or more months after RA. Therefore, the *post-release* period covered by this study is much shorter than 18 months for many individuals. As a consequence, there was limited time during the post-release observation period for program and control-group members to differentiate themselves. It is possible that these programs will be shown to be effective with a longer post-RA observation period.

The next step for the evaluation is to estimate impacts measured 30 months after RA, which may shed additional light on the programs' effectiveness. A report based on these data is forthcoming.

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Appendix A. Implementing Random Assignment

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

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 procedures 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 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. This change was not influenced by the study, but, 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 must be voluntary. Therefore, as a consequence of participating in the study, 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 SCA. 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 random assignment, every potential study participant needed to understand the research study and give 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:

- *A video.* Grantees were provided with a short video, available on DVD, which they could play at study orientation sessions. The video described the purposes of the study, the random assignment process, and what data would be collected as part of the study on each person who was randomly assigned.
- *Scripts.* Scripts for explaining the study and a frequently asked questions (FAQ) document were provided.
- *Notification materials.* Some grantees notified individuals about the results of random assignment by written correspondence. We provided the grantees with draft letters for them to use if they desired.
- *Informed consent forms.* After receiving an orientation to the study, every person being considered for random assignment needed to give written consent to participate in the study before random assignment 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 participate in SCA. (Grantees told us that no more than a few people declined to give consent and no one dropped out after being randomly assigned.)

In addition to providing the materials described above, the study team also provided each grantee with a customized procedures manual and delivered 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, random assignment occurred.

To ensure rigor in conducting random assignment, the study team developed an online random assignment system which the grantees were required to use. Each grantee staff member conducting random assignment was given a personal username and password and used these credentials to log into a secure virtual private network to access the online random assignment 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 random assignment, 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 every person randomly assigned. Those who were randomly assigned but lacking a consent form were removed from the study (seven individuals were removed for the study because their consent forms were missing).

An individual from the study team was designated as the primary site liaison for each grantee during the period that random assignment occurred and was available to provide help. The site liaison scheduled regular telephone calls with the grant manager at each site, weekly when random assignment first began and less frequently after a time. The purposes of the calls were to provide support, answer questions, and troubleshoot problems that arose. Additionally, the study team monitored sample build-up through weekly reports generated from the random assignment system and checked periodically that the program and control groups were balanced on the BIF's baseline characteristics (as would be expected if random assignment were being carried out properly). Finally, during the site visits conducted as part of the implementation study, the liaison assigned to the site 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. Survey Methods

A survey was administered to study participants (both program- and control-group members) approximately 18 months after their dates of RA. The survey asked about background characteristics, re-entry services received, involvement with the criminal justice system, and employment, among other topics. Data collection began in June 2013 and ended in early 2015, with an 82.3 percent response rate.

Locating Respondents

Participant cases were assigned to trained field interviewers 18 months after each participant's RA date. These monthly assignments for the follow-up interview began in June 2013 (18 months after the first study participant was randomly assigned), with the last assignment of participants in September 2014. Assignments ranged from 19 to 88 participants per month.

Initial efforts to locate participants made use of the contact information forms, which participants filled out at the time of RA. The contact information form asked the participants to provide information about how they could be reached for the follow-up survey and elicited contact information for up to three of each participant's significant others.

Attempting to interview the formerly incarcerated can be challenging under any circumstances, even with this contact information. Particularly for the SCA study, locating participants at follow-up was difficult because most were incarcerated at the time of random assignment. This situation often caused contact information to be incomplete because participants were unsure of their future housing situation following release. Additionally, contact information for participants could easily change during the 18-month follow-up period, because housing situations immediately following release were often temporary.

The survey data collection procedures were designed to overcome these challenges. To begin, a letter was sent to the participant's last known address. This letter:

- Described the survey and reminded participants that they had previously agreed to participate in the study,
- Noted that each participant would be provided with a $50 incentive payment for completing the survey, and
- Invited the participant to call a toll-free number to complete the interview and offered an additional $25 if the participant called within two weeks of receiving the letter.

If after two weeks the participant did not call the interviewer, then interviewers began making outgoing calls to participants. Letters were also sent to all secondary contacts (i.e., friends

and/or relatives provided on the contact information forms) with a valid address. This 'locating' letter contained the participant's name and asked the recipient to call the interviewer to provide updated contact information for the participant so that the participant could complete the interview and receive the incentive payment. If after these steps the participant had not been reached, the case was referred for more intensive locating using online databases. Outgoing calls continued using updated contact information when it was obtained.

Sometimes, queries provided evidence that the participant had been re-incarcerated. In these cases, the interviewer or other evaluation staff checked recent law enforcement or other criminal records databases to verify the participant's status. If it was confirmed that a participant had been recommitted, a formal query was submitted to the institution requesting permission to conduct the interview. If the facility gave permission, an attempt was made to interview the participant by telephone. In total, interviews were completed in facilities in six departments of corrections, 13 local jails, and the Federal Bureau of Prisons.

When all other options to find study participants were exhausted, the final step in locating participants was a site visit to the participant's assumed geographical area (based on information on the contact information form). During the visits, field staff attempted to obtain updated contact information through neighbors, significant others, friends, and employers. The field staff also offered to conduct the interview in-person if the participant was located.

Conducting the Interviews

Follow-up interviews were 40 minutes in length on average. Most interviews were completed over the telephone using computer-assisted-telephone-interviews (CATI), with computer-assisted-personal-interviews (CAPI) administered for participants that required in-person contact.

Interviews were completed with 787 of the 966 randomly assigned participants, resulting in an 82.3 percent response rate at follow-up [completed interviews/ (total sample-deceased participants)]. Of these, 258 were completed while the participant was incarcerated; among those not incarcerated, 66 interviews were completed in-person and 463 were completed by telephone. Interviews were not completed with 169 individuals for multiple reasons, including participant refusals, inability to locate the participant, lack of access to incarcerated participants, and participant deportation. The response rate was 82.2 percent for the SCA program group and 82.6 percent for the control group. The overall response rates across the seven grantees ranged from 75.4 percent to 87.3 percent (see Exhibit B-1).

Exhibit B-1: Response Rate by Site

	N of cases	Deceased	Completes	Percent Complete
Allegheny County (PA)	133	1	109	82.6
Kentucky DOC	187	4	146	79.8
Marion County (OR)	119	1	103	87.3
Oklahoma DOC	134	3	110	84.0
San Francisco (CA)	77	1	58	76.3
San Mateo County (CA)	114	0	86	75.4
South Dakota DOC	202	0	175	86.6
Total	**966**	**10**	**787**	**82.3**

Source: Survey database.

Weighting

The follow-up interview data were weighted to adjust for nonresponse. A simple first step in the weighting plan was to create a base weight, which, given the nature of this survey, was set to a value of 1.0 for each respondent. Next, we calculated the nonresponse adjustment using a propensity score approach. In this method, a logistic model was run to identify variables associated with the likelihood of responding. Multiple variables were considered for this adjustment, including treatment or control-group status, site, gender and race/ethnicity. The propensity model and estimated scores were calculated separately for the treatment and control groups. The weight is proportional to the inverse of the probability of responding.

Appendix C. Methods for Data Analysis

This technical appendix describes the statistical methods used to estimate the impacts of SCA in the seven grantee sites. It first describes the methods generally, including the simple difference in mean outcomes presented throughout the main body of the report as the estimate of impacts and alternative methods used to demonstrate the robustness of the report's major conclusions to different model specifications and methods. After describing the methods, the appendix concludes by presenting selected results from those additional analyses.

Statistical Methods 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. Random assignment, 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. Random assignment 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 the two groups were contrasted on observable background characteristics measured at baseline (see Chapter I). These characteristics included the participant's 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 as well.

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

Difference in Means

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

Regression Analysis

Regression analysis was used both to assess levels of statistical significance and as a sensitivity test in estimating impacts. Additionally, because whether an individual was incarcerated at the time of RA is included as a covariate, the regression models serve to partially control for the period at risk of recidivism following RA.

The regression analysis adds covariates to 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 (Kahan et al. 2014; Hernandez 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 continuous variables, logistic regressions are needed for assessing binary outcomes, because the OLS analysis of them violates OLS's assumptions regarding the distribution of errors.

The regression models included a vector of individual and grantee-level characteristics, as represented in Equation 1:

$$\mathbf{Y}_n = \beta_0 + \beta_1 Group\ Assignment_n + \Sigma\, \beta_p \mathbf{X}_{pn} + \varepsilon_n \qquad (1)$$

In this equation, Group Assignment is coded 1 for those assigned to the program group and 0 otherwise; β_1 provides the estimated 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 C-1 details the

individual and grantee-level characteristics included as covariates in the regression analysis and presents 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, in some cases, could introduce bias in the estimation of the treatment effect (to the extent that sample cases needed to be dropped due to missing data). Therefore, these variables were not included in the regression models.

Exhibit C-1: Descriptive Statistics of Background Characteristics Included in Regression Models

Variable	N	Mean	Standard Deviation
Male (1=yes, 0=no)	966	78.9	40.8
Age (in years)	966	33.3	10.4
Hispanic (1=yes, 0=no)	965	9.7	29.7
African-American (1=yes, 0=no)	965	31.6	46.5
Other non-white non-Hispanic (1=yes, 0=no)	965	12.6	33.2
Has at least a H.S. diploma or GED equivalent (1=yes, 0=no)	956	76.1	42.6
Incarcerated at time of random assignment (1=yes, 0=no)	966	80.5	39.6
Total years incarcerated in prior 10 years	966	2.8	2.5
Number of arrests in prior 10 years	939	10.7	10.1

Source: Baseline information forms and administrative data

Notes: In addition to the variables shown, a treatment dummy variable was also included, representing whether or not the individual was randomly assigned to the SCA program group. Dichotomous variables in the table above were multiplied by 100, for ease of presentation. Estimates are unweighted.

Hierarchical Linear Modeling

HLM is useful in analyzing data when sample members are drawn within discrete units. HLM takes into account this hierarchical structure in its estimation, 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).

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

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

$$\mathbf{Y}_n = \beta_{0j} + \beta_{1j} Group\ Assignment_n + \Sigma\, \beta_{pj} \mathbf{X}_{pn} + \varepsilon_n \quad (3)$$

$$\beta_{pj} = \Sigma \gamma_p \mathbf{Z}_{pj} + U_j$$

Equation 3 is identical to Equation 1 but with the addition of a level-2 equation, which allows estimation to vary by site j. The level-2 equation estimates site-level intercepts and slopes (β) using site-level covariates (Z_{pj}) and corresponding coefficients (γ_p).

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 V served as indicators of the program's impact on recidivism. However, indicator variables, while providing simple and easy-to-understand metrics, potentially lose nuances in the information on times to an event. For example, one individual might have been re-incarcerated 1 month after random assignment, and a second individual might have been re-incarcerated 17.5 months after random assignment. At the time of the 18-month follow-up period, both individuals are identified as having been re-incarcerated, even though there is a qualitative difference between these individuals in their time to re-incarceration. To supplement the key recidivism outcome measures reported in Chapter IV, survival analysis was conducted to examine the impact of the grantees' programs on the time until recidivism.

One approach to conducting survival analysis is using the Cox proportional hazards model (McNiel and Binder 2007). While random assignment of individuals to the program group should account for confounding variables, the hazard model 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 arrest, conviction, and incarceration during the 18 months following random assignment. 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 to 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 to 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 that has not already recidivated by a specified time has 0.75 times the chance of recidivism by the next specified time compared to an individual from the control group. The hazard ratio can be converted to probabilities (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 related indicator measures pertaining to arrest, conviction, and incarceration reported in Chapter V.

Results of the Sensitivity Analysis

The exhibits in Chapters IV through VII reported the mean outcomes for the program and control groups. Whether the difference between the groups was statistically significant was assessed using the results of the regression analysis described earlier in this appendix.

Exhibit C-2 compares this approach with two others for estimating the impacts of assignment to the SCA program group on the recidivism outcomes discussed in Chapter V. The three methods are:

- The simple difference in means (the standard model). These estimates are identical to the ones reported in Chapter V.
- Regression analysis with inclusion of the control variables listed in Exhibit C-1 and a treatment dummy, and
- HLM with inclusion of the same set of control variables.

Consistent with Chapter V, the sensitivity analysis relied on weight-adjusted data.

As revealed in Exhibit C-2, neither the inclusion of covariates nor inclusion of a multilevel framework notably altered the conclusions. Thus, the impact estimates on recidivism are robust to model specification. The ICCs were assessed to determine the need for using a multilevel model. Generally, the ICCs were considered trivial.

The sensitivity analysis was additionally carried out for other outcome measures beyond recidivism, such as services received. The results were consistent with the findings observed for recidivism and were not included in the technical appendix for concision.

Exhibit C-2: Impacts on Recidivism for the Full Sample, Using Alternative Model Specifications

	Difference in Means	Regression with Covariates	HLM with Covariates
A. Outcomes Measured from Administrative Data			
Arrests			
Arrested	3.07	3.40	4.40
Average number of arrests	0.30**	0.23*	0.25**
Arrests by offense type [a]			
Violent crime	-2.44	-1.64	-1.80
Property crime	8.06***	6.55***	7.48***
Drug crime	1.78	1.29	1.55
Public order crime	1.35	1.69	2.19
Convictions[a]			
Convicted of a crime	6.43**	5.90*	6.07**
Average number of convictions	0.13***	0.12***	0.12***
Incarcerations (prison or jail)			
Was re-incarcerated in prison or jail	4.61	4.65	5.14
Experienced a new jail incarceration	2.97	2.39	2.71
Experienced a new prison incarceration	2.33	2.70	0.71
Total days incarcerated[b]	1.31	-0.12	-1.38
B. Outcomes Measured from the Survey			
Arrests			
Arrested	5.29	5.75	6.79*
Average number of arrests	0.04	-0.01	-0.00
New charges and convictions			
Formally charged with a new crime	3.46	3.31	3.84
Convicted of a new crime	2.60	1.96	1.92
Average number of new convictions	0.06*	0.06	0.06*
Parole/probation violations			
Charged with a violation	1.51	2.03	2.60
Probation/parole revoked	4.66*	4.37*	3.74*
Re-incarcerations (prison or jail)			
Was ever re-incarcerated in prison or jail	3.23	4.41	4.79
Average number of re-incarcerations	0.11	0.07	0.07
Currently incarcerated	-2.55	-1.11	-1.61

Notes: Numbers in the table show the estimated impacts of assignment to the program group. Estimates shown in the first column represent a simple difference in-mean outcomes for the treatment and control groups; these numbers correspond to the impact estimates shown in the main body of the report; significance levels are calculated based on a t-test for differences in group means, assuming unequal variances. The second column shows regression-adjusted estimates of the treatment effect, calculated by including covariates. For outcomes that are dichotomous, logit models were used, and the treatment effect was calculated as the difference in the predicted outcome calculated separately for the treatment and control groups, estimated at the mean value of all covariates and with the predicted outcomes converted to a predicted probabilities. The third column proceeds similarly but was estimated using HLM.

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

[a] The sum across categories exceeds the percent ever arrested because individuals can be 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.

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

Exhibit C-3 reports results from the hazard models on time to first instance of re-arrest, re-conviction, and re-incarceration. There was no statistically significant difference between the program and control groups regarding time to first arrest or first incarceration. However, those in the program group had a somewhat greater chance of being convicted before the control group. These results are broadly consistent with the findings reported in Exhibit V-3, which examined the percentage of program and control-group participants who experienced re-arrest, reconviction, and re-incarceration, further evidencing the robustness of the results to model specification.

Exhibit C-3: Hazard Ratios for Time to Recidivism for the Full Sample

	Hazard Ratio	Hazard Ratio with Covariates
Time to first arrest	1.12	1.15
Time to first conviction	1.29**	1.28**
Time to first jail admission	1.11	1.14
Time to first prison admission	1.13	1.12

Notes: Numbers in the first column represent the results of the hazard model with only program participation as a predictor; the second column is a replication of the hazard model, including covariate.

Source: Administrative data from state and local agencies

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

Appendix D. 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. One approach would have been to draw 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.[34] 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 random assignment, the control group constitutes a pool for whom the determinants of recidivism in the absence of SCA can be identified.

Steps were as described below.

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), which have been found to be quite good as predictors of recidivism (Gendreau et al. 1996). 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, or five or more times), whether incarcerated at the time of RA, and total days incarcerated in the ten years prior to RA (divided by 365, to convert to fractional parts of years).

[34] 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, Kroner and Mills 2001, James 2015), different instruments need 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.

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

Exhibit D-1: Coefficients from a Logit Model Predicting the Probability of Re-incarceration within 18 Months after RA, for the Control Group

	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 ten years	.0357	.0482

Source: Administrative data and baseline information.

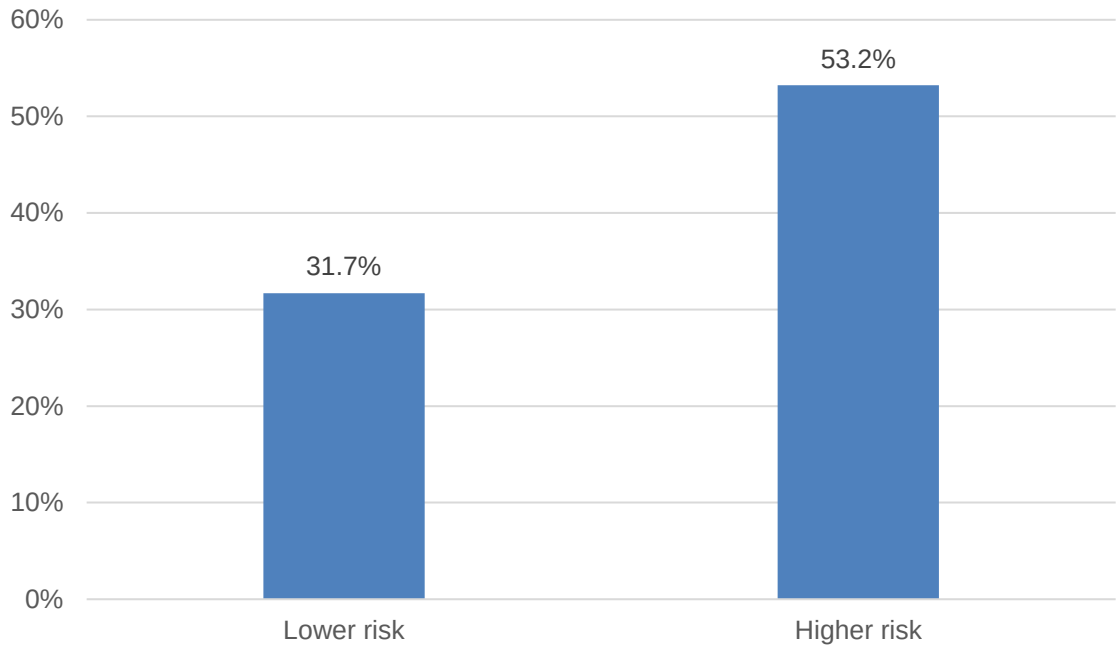
*/**/*** Statistically significant at the .1/.05/.01 level.

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. The predictive utility of this classification is demonstrated in Exhibit D-2, which shows the re-incarceration rate for the two risk groups, measured for the control group 18 months after random assignment.

Zweig et al. (2010) note that the above procedure tends to over-predict 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, 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 this constrained set.

Exhibit D-2: Percent Re-incarcerated for the Control Group, by Risk Category



Notes: The bar chart shows the percent of the control group, divided into lower-risk and higher-risk subsets, who were re-incarcerated within 18 months after RA.

Source: Administrative data and baseline information.