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FINAL REPORT
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SCA Follow-Up Study:

A Longitudinal Study of 2009 Second Chance Act Adult Demonstration Program Participants

(Award Number 2020-MU-MU-0014)

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Acknowledgment

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We are heavily indebted to the study participants, who agreed to share their experiences with us through the participant survey and the study's other data collection activities. We also greatly appreciate the cooperation of the seven grantees that participated in the impact study:

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

We are also appreciative of the many state and local agencies, shown in Exhibit I-4, which provided criminal justice system data for this evaluation.

We are also grateful to the many members of the study team who supported this effort. Antonio Raphael, Olivia Pham, and Colleen Bullock, at SPR, cleaned and ran analyses of the administrative data presented in the report and Jessica Muñoz, Renatta DeFever, Ava Alvarez, and Krystal Hong all assisted in obtaining and processing administrative data.

Executive Summary

Under the Second Chance Act (SCA) of 2008, the U.S. Department of Justice (DOJ) Bureau of Justice Assistance (BJA) awarded hundreds of grants under various categories of competition to state, local, and tribal governments to develop or enhance re-entry programs. In 2018, the First Step Act was signed into law, reauthorizing the Second Chance Act (SCA) in Title V, allowing for the continued financial support of the SCA program, among other reentry-focused services. While SCA programs have been operating for about 15 years, the research conducted on these programs and their services has been relatively short-term, examining outcomes on participants no more than a few years past the point of program enrollment. Until now, there has not been an examination of the long-term impacts of SCA services.

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

In 2021, DOJ's National Institute of Justice (NIJ) awarded a grant to NORC at the University of Chicago (NORC) and its partner, Social Policy Research Associates (SPR), to evaluate the long-term impacts of seven SCA Adult Demonstration Program grantees first awarded SCA funding in FY 2009 by BJA (see "SCA Impact Study Grantees" box), which continues to support and manage the SCA grant program on behalf of DOJ. NORC and SPR, along with MDRC, previously evaluated these grantees by conducting an implementation study and a randomized controlled trial impact study. The results of this earlier research were included in: an implementation study report (D'Amico et al., 2013); an 18-month impact study report (D'Amico et al., 2017), reporting on data from a follow-up survey and administrative data; and a 30-month impact study report, using only administrative data (D'Amico and Kim, 2018). The implementation study found, among other things,

that the program models for each grantee varied somewhat in their composition and mix of services. It also found that while there were no impacts observed on recidivism, a notable finding from the research at 30 months after random assignment was that the program had positive impacts on employment observed in the seventh quarter after random assignment and on earnings in the seventh and eighth quarters after random assignment.

About the Evaluation

The goal of the current longitudinal research has been to estimate the impacts of the SCA program on the outcomes of the original impact study participants over the approximately ten to twelve years following the point when these participants were first randomly assigned. Random assignment for the original evaluation commenced in the last week of 2011 and continued through March 2013. Of the 966 study participants, 606 (62.7 percent) were randomly assigned to the treatment (i.e., program) group, whose members could receive individualized SCA services, while 360 (37.3 percent) were assigned to the control group, whose members could receive all services otherwise available to them, but could not receive SCA services. Importantly, the RCT design means that our research is testing the impact of SCA services over and above whatever services might have been available to control group members and not an absence of services.

More specifically, the current research has sought to assess: 1) whether SCA participants demonstrated better long-term outcomes related to recidivism (arrest, conviction, and incarceration) than did control group members; 2) whether SCA participants demonstrated better long-term employment and earnings outcomes than control group members, and 3) whether SCA participants demonstrated better long-term outcomes on other indicators of well-being (e.g., housing, family formation, benefits, substance abuse, etc.) than control group members. To answer these questions the study team gathered both participant survey and administrative data.

- **Survey data.** Using a multi-phase process, the study team began administering a survey to study participants in late November 2021 and closed the survey on December 19, 2023. At that point, all pending respondents had been contacted by study team staff multiple times. The study team was able to complete 378 interviews (39.1 percent of all study participants) during the entire survey data collection period.
- **Criminal justice administrative data.** The study team obtained arrest, conviction, and state prison and jail incarceration data from state and local public agencies (for the counties to which participants were released or served) in the six states and seven counties in which the seven grantees operated. The study team was able to collect these data from all agencies except two (arrest data in South Dakota and jail data from Oklahoma City). These data included all periods between random assignment and nearly ten years after random assignment for all participants.
- **Employment and earnings administrative data.** The study team was able to access data from the Department Health and Human Services' National Directory of New Hires database which allowed for the analysis of employment and earnings data for study participants over a two-year period from July 2022 to June 2024.

Survey and administrative data were used to answer the first two research questions with survey data being the only source for the third research question. Due to finding no impacts related to the third research question and the low response rate for the survey, answers to the third research question are included in an Appendix to the main report.

Although the research design ensured a rigorous and unbiased estimate of impacts, the findings are subject to certain standard limitations and cautions, including: longitudinal survey sample attrition and a subsequent reduction in statistical power for detecting effects; an inability to obtain consistent or complete historical data on the criminal justice history for participants prior to random assignment; the ability of control group members to access similar, non-SCA services; variations in the seven SCA grantee program models; the use of SCA funds for general system improvements which could have

theoretically benefited both study groups; and our limited ability to generalize these study findings due to the purposeful sampling of the seven SCA grantee programs.

Impacts on Recidivism

Using administrative data for the longitudinal analysis, the study team found that in the nearly 10 years after random assignment, rates of recidivism increased overall among both treatment and control groups. Additionally, the rate of desistance (i.e., reduction in criminal behavior) did not substantially improve on average among individuals in the treatment group relative to control group members. Overall, program and control group members were equally likely to: be re-arrested, reconvicted, or re-incarcerated; have the same number of arrests, on average; and record, on average, the same total days incarcerated (including time in prisons and jails). While the differences were not significant, data suggest that control group members may have had more arrests and convictions than the treatment group. More specifically, these data show the following:

- Nearly 75 percent of all study participants had been re-arrested at least once and convicted of a new crime, at some point during the 10-year follow period, with no significant differences between program and control group members for either measure. These rates represent an increase over the 60 percent arrest rate and 45 percent conviction rate observed for both program and control groups at 30 months after random assignment.
- Approximately 70 percent of study participants had been re-incarcerated in either a prison or a jail during the 10 years following random assignment as compared to a 60 percent re-incarceration rate observed at 30 months after random assignment. There was also no significant difference between program and control group members' rates of re-incarceration.
- Over the 10-year follow-up period, there was no significant difference between program and control group members in their time to first arrest and conviction after random assignment.

Analysis of the participant survey data generally corroborated the findings from the administrative data. There were no statistically significant differences between program and control group members in terms of re-arrests or re-incarceration although there was a difference observed between the individuals incarcerated at the time that interview for the survey occurred, with control group members being more likely to be incarcerated at that time (20 percent compared to 29.7 percent). Notably, though, these data were less reliable given the relatively low response rate (39.1 percent of all study participants), which meant that it was more difficult to detect statistically significant differences and sub-group analyses with survey data were not possible.

The study team also estimated impacts on recidivism for three key subgroups—men/women, under 30/30 and over, and lower/higher risk level—at the time of random assignment. These subgroup analyses examined differences between treatment and control group members within each sub-group and between sub-groups. The analysis based on risk-level did not find any statistically significant differences, but there were significant differences for the other two groups.

- Comparisons based on age presented a fairly striking contrast. Treatment group participants who were 30 years of age or older at the time they were randomly assigned were less likely than their control group counterparts to be arrested, convicted, or incarcerated in prison during the nearly 10-year follow-up period. They were also less likely to be arrested for violent or public order crimes or to be convicted of a misdemeanor or felony, and they spent approximately 90 fewer days incarcerated than their control group counterparts. Thus, the program had a differential impact on those who were 30 and older, producing positive, and in some cases fairly large, impacts on this older group of participants.
- There were notable but more modest differences between male and female participants. Treatment group males had violent crime arrest rates nearly 10 percentage points lower than their control group counterparts, while female treatment group members actually had higher rates of arrest for violent crimes than did female control group members. The difference

between male and female subgroups is significant, indicating that the program had a differential impact on this measure between males and females. Additionally, males in the treatment group were less likely than those in the control group to have been arrested or convicted (each by 6.7 percentage points) or had a felony conviction (by 7.9 percentage points). In contrast, female treatment group members had higher rates of property and violent crime arrests and misdemeanor convictions than their control group counterparts.

These consistent findings across multiple recidivism outcome measures may indicate that intensive, reentry interventions, such as SCA programs, are more effective with older participants and perhaps with males. Why the program had such clear impacts on these two subgroups is unclear, but should be the subject of further discussion and research.

Impacts on Employment and Earnings

The longitudinal analysis using administrative data relied on NDNH earnings data from July 2022 to June 2024. In other words, it did not capture data from the years in between random assignment and this period. In analyzing these data, the study team found that approximately 45 percent of both program and control group members were employed at some time during this two-year period and about 25 percent were employed in each of these eight quarters. There were no significant differences in the rates of employment between program and control group members either overall or within each of the eight quarters. And while there was no difference between program and control groups in the total wages they earned over the full eight quarter period, in a couple of the quarters, the control group had significantly higher wages than those in the treatment group.

These findings were echoed in the sub-group and participant survey analyses. For the analysis of the NDNH data, there were no impacts observed in the rates of employment either between program and control group members or between sub-group pairings, and only a few impacts noted where control group members had higher wages than treatment group members in specific quarters. There were also

no statistically significant differences in the measures of employment captured in the survey, except that treatment group members had a slightly higher rate of employment in temporary or seasonal jobs compared to control group members (45.6 percent vs. 32.9 percent).[1]

Discussion and Conclusions

For the longitudinal analysis, the study team concludes that the promising effects of the program on employment and earnings outcomes observed during the 30-month report appear to have diminished over time rather than intensified. It is difficult to determine whether important events, including the COVID-19 global pandemic may have contributed to these diminishing effects. However, while the survey observed changes in employment patterns between the period prior to the pandemic (2019) and those after (2020 and 2021), these changes appeared to similarly affect those in the program and control groups.

Despite the lack of impacts on employment and earnings outcomes, and the lack of impacts on arrest, conviction, and incarceration rates overall, the reduced rates of recidivism observed among older participants across criminal justice outcomes and for violent arrest and felony convictions among male participants is notable. While these findings should be approached with caution given their exploratory nature, they suggest that SCA services may result in reduced involvement with the criminal justice system for at least some participants. This is not only valuable due to the harm that results from increased crime but the significant public costs in managing the criminal justice system. These findings raise several important research questions that warrant additional exploration, for example, are these findings replicable in other contexts; might these findings be different if programs were more uniform in their service delivery approach (since these seven programs were not particularly so); if more uniform, which program models would produce the greatest impacts; might there be ways to improve or shorten

[1] This finding was only observable in the survey data since the NDNH data only report earnings by quarter and not the type of employment.

the time to observe benefits with the relevant sub-groups; and can services be adapted to better address the needs of sub-groups or for measures where no impacts were observed.

The goal of this study was to produce findings that would allow policymakers at the Federal, State, and local levels to design more effective programs for integrating former prisoners into society, increasing their opportunities for employment, and reducing their rates of recidivism. We hope the findings from this long-term follow-up study provide useful information to policymakers at all levels as they consider different approaches to prisoner reentry and their likely outcomes across different populations, especially over the long term. Namely, it will be important to see if these impacts can be replicated in future research, for other groups, and improved upon by either increasing the areas where they have been shown to have an impact (such as for participants 30 and older and possibly for males), shortening the time it takes to observe impacts, and/or discovering why certain impacts seem to only emerge or subside over time. Likewise, further research is clearly needed about why these programs may not benefit the sub-groups where there were no impacts observed.

I. Summary of the Project

Under the Second Chance Act (SCA) of 2008, the U.S. Department of Justice (DOJ) Bureau of Justice Assistance (BJA) awarded hundreds of grants under various categories of competition to state, local, and tribal governments to develop or enhance re-entry programs. In 2018, the First Step Act was signed into law, reauthorizing SCA in Title V, allowing for the continued financial support of the SCA program, among other reentry-focused services. While SCA programs have been operating for about 15 years, the research conducted on these programs and their services has been relatively short-term, examining outcomes on participants no more than a few years past the point of program enrollment. There has not yet been an examination of the long-term impacts of SCA services.

In 2021, DOJ's National Institute of Justice (NIJ) awarded a cooperative agreement to NORC at the University of Chicago (NORC) and its partner, Social Policy Research Associates (SPR), to evaluate the long-term impacts of seven SCA Adult Demonstration Program grantees first awarded SCA funding in FY 2009 (see "SCA Impact Study Grantees" box) by the Bureau of Justice Assistance (BJA), which continues to support and manage the SCA grant program on behalf of DOJ. NORC and SPR, along with an additional partner, MDRC, previously evaluated these grantees by conducting an implementation study and a randomized controlled trial (RCT) impact study, with random assignment beginning in December 2011 (or approximately two years after the grantees began operating their SCA programs) and ending in March 2013. The results of this earlier evaluation were included in an implementation study report (D'Amico et al., 2013), an 18-month impact study report (D'Amico et al., 2017), based on a follow-up survey and administrative data, and a 30-month impact study report based on administrative data (D'Amico and Kim, 2018).

This report details the findings from this long-term, follow-up evaluation It summarizes the findings based on a new wave of data collection completed by NORC and SPR (hereafter, the "study team") on the same group of individuals who participated in the original RCT impact study. NORC and SPR collected and analyzed new data on recidivism, employment, and other outcomes for these participants through both a survey and administrative data at points that were between 10 and 12 and a half years from when participants were first enrolled in the original RCT impact study.

Major Goals, Objectives & Research Questions

A notable finding from the earlier SCA research at 30 months after random assignment was that the program had positive impacts on employment observed in the seventh quarter and on earnings in the seventh and eighth quarters after random assignment. The employment findings were modest, and there were no impacts observed on recidivism (D'Amico and Kim, 2018). There is considerable research pointing to the positive associations between employment and reduced recidivism (Lageson & Uggen, 2012; Ramakers et al., 2016), including at least one long-term study (Skardhamar & Telle, 2012), and especially when that work is "high quality" or somewhat more stable (Crutchfield & Pitchford, 1997; Schnepel, 2017; Uggen, 1999) or when involving a vocational training component (Lacoe & Betesh, 2019). This study was designed to determine whether the impacts on employment for SCA participants observed in the earlier research have persisted (or perhaps grown) over time, which would be a notable benefit of the program. Furthermore, this prior research suggests the possibility that these positive impacts on employment could be associated with reduced recidivism. Finding that SCA reduced recidivism over time would also be a notable finding.

The goal of the current longitudinal research has been to examine the outcomes of the original impact study participants, over the approximately ten to twelve years following the point when these participants were first randomly assigned, and to estimate the impacts of the SCA Adult Demonstration

Program over that time or at discrete periods of time during that follow-up period. As part of this research, the study team sought to answer the following research questions:

1. Do SCA participants from the seven grant programs in the impact study (SCA participants) demonstrate better long-term outcomes related to recidivism (e.g., arrest, conviction, and incarceration) than a similar group of eligible program applicants who did not receive individualized SCA services (the control group)?
2. Do SCA participants demonstrate better long-term employment and earnings outcomes than the control group?
3. Do SCA participants demonstrate better long-term outcomes on other indicators of well-being (e.g., housing, family formation, benefits, substance abuse, etc.) than the control group?

Research Design, Methods, Analytical and Data Analysis Techniques

Below we provide a brief overview of our design, methods, and analytic approach to the study. We provide a more detailed description of the methods used to complete the SCA longitudinal impact evaluation in multiple appendices.

Research Design and Methods

To answer these research questions, our team used a combination of primary survey data collected directly from the 966 randomly assigned program and control group members (study participants), and extant administrative data gathered from federal, state, and local public agency databases on these same individuals.

Random Assignment Process

As described in the earlier SCA evaluation reports (D'Amico et al., 2013; D'Amico et al., 2017; D'Amico and Kim, 2018), SCA grants were awarded in 2009, and programs began enrolling their participants in late 2009-early 2010. For the study, random assignment took place between December 2011 and March 2013, with the starting and ending point varying by grantee. To be eligible to participate in the SCA Adult Demonstration Program, individuals had to be:

- 18 years of age or older;
- convicted as an adult;
- imprisoned in a state, local, or tribal prison or jail; and
- classified as being at medium or high risk of recidivism.

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

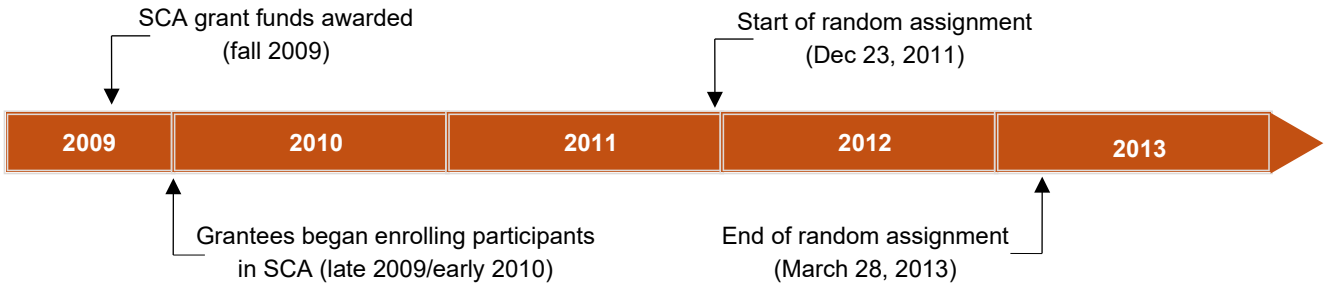
- Individuals most recently incarcerated in jail, prison, or both;
- Individuals from a specific demographic group (e.g., based on age or sex), and
- Those returning to a specific community or neighborhood.

Once determined eligible for SCA participation according to the grantee's established criteria, an individual was provided with an orientation to SCA program services and to the RCT impact study. Those who consented to participate in both the program and the study were then randomly assigned, using an online system, to either the treatment (i.e., program) group, whose members were eligible to participate in SCA services, or the control group, whose members were not eligible to receive SCA services but who could participate in other services within their facilities and/or communities.

Grantees began random assignment at slightly different points in time, between December 23, 2011 and May 8, 2012. Grantees continued random assignment until they reached their enrollment

targets, ending in March 2013. All grantees conducted random assignment for at least eight months, and during this time a total of 966 individuals were randomly assigned to the two study groups. The timeline for the grantees and the study is displayed in Exhibit I-1.

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



In addition, congruent with their different service designs and grant amounts, each grantee had a different enrollment target and, based on other factors worked out with the study team, the rate of random assignment to the treatment group varied. Most grantees assigned approximately 60 percent of program eligible individuals to the treatment group and 40 percent to the control group. However, two grantees randomly assigned approximately 75 percent of applicants to the treatment group and 25 percent to the control group. The number of eligible applicants that each grantee enrolled in the treatment and control groups is shown in Exhibit I-2.

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

	Total	Treatment	Control
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

	Total	Treatment	Control
South Dakota	202	120	82
TOTAL	**966**	**606**	**360**

Source: D'Amico et al., 2017.

Each grantee also determined when in the transition from incarceration to release random assignment would occur. Some grantees randomly assigned individuals well before release and provided individualized pre-release services during that time. Other grantees generally relied on the institutions' pre-existing pre-release services and began individualized SCA services only as close to or after the release date. Finally, some grantees only randomly assigned participants post-release. Overall, approximately 55 percent of participants were enrolled in the study three or more months prior to release, about one quarter (28 percent) within three months of release, and 17 percent after release. Regardless, random assignment always occurred just before an individual's involvement with their SCA program.[2]

Survey Data Collection

Consistent with the original survey data collection approach, the study team administered the survey data collection for the current, longitudinal study in three phases: pilot, primary, and final phases. Each phase began the same way: the study team sent a letter to each eligible respondent requesting that they call to complete the survey by telephone.[3] The letter also informed the respondent of an "early bird" bonus provided within the first two weeks of the data collection period. The first phase, pilot data collection, began in late November 2021 and comprised a random sample of 50 non-

[2] This distinction in point of random assignment is notable as it points to key differences in the ways grantees enrolled participants into their programs. While it could affect a person's time to release, and thus their ability to become employed or to recidivate, there is no indication that the variation in random assignment point affected this study. Due to random assignment, program and control group members should have had the same profiles and thus, on average, the same time to release. Chapter II shows that program and control group members were similar to one another in their baseline profiles (i.e., participants were successfully randomized). Chapter III shows that time to first arrest and conviction among treatment and control group members was the same on average. Random assignment occurred for all programs close enough in time to allow for a lengthy period of impact observation.

[3] Respondents who were known to be incarcerated or deceased were excluded from the mailing and did not receive a letter.

incarcerated respondents. There were ten completes during the pilot, six of which were early bird completes. Pilot data collection had a 20 percent response rate. The second phase, primary data collection, began in late February 2022. We sent out 520 early bird letters and received 32 early bird completes. Administrative delays in project approval required the study team to split all respondents with a California address out into the third phase of data collection. The third and final phase was comprised of 151 respondents. This phase started in June 2022 and had five early bird completes. During all three phases of data collection, Field Interviewers made outreach to respondents eligible for the survey.

The study team closed the survey data collection on December 19, 2023. At that point, staff had attempted contact with all pending respondents multiple times. We were able to complete 378 interviews during the entire data collection period, from November 2021 through December 2023. Completed responses were not necessarily evenly distributed between treatment (250) and control groups (128). A detailed breakdown of respondent status is shown in Exhibit I-3.

Exhibit I-3: Survey participants by respondent status

Case Status	Number	Total
Complete		378
Complete via Phone	244	
Complete via In Person	74	
Complete In Jail/Prison	60	
Incomplete		537
Incarcerated	68	
Unreachable	355	
Refusal	4	
Unlocatable	65	

Case Status	Number	Total
Deceased Respondent	45	
Out of Scope		51
Language Barrier	2	
Deceased*	49	
Total		966

*Respondents were deceased prior to survey administration.

Modifications from Prior Survey. The survey instrument for this longitudinal data collection was based on the survey used in the original evaluation of the SCA program (18-months after random assignment), focusing on long-term recidivism, long-term employment and income, education gains, and other measures of participant well-being, including access to housing, post-release and family support, future outlook, and other related topics. However, the survey was somewhat adapted to be responsive to potential personal changes experienced by respondents (e.g., family composition) and wide-reaching societal challenges (e.g., the COVID-19 pandemic) during the longer follow-up period. In discussions with NIJ about how best to capture participant outcomes within the context of these potentially impactful situational shifts, the study team decided to ask participants about their recent experiences in more than one time period: in 2019 and more recently at the time of interview. The two reference periods were an attempt to better understand differences in respondent outcomes over time, including the potential impact of the pandemic on respondent experiences.

Administrative Data

The administrative data needed for the evaluation included criminal justice data and employment and wage data, each obtained from different federal, state, and local public agencies. Unfortunately, data obtained during the earlier evaluation efforts were not available for this analysis due to the terms of those data sharing agreements. Furthermore, while the study team had contacts and knowledge of

which agencies housed the data needed from the earlier research, many of the agencies had undergone re-organizations, were experiencing staffing issues, or had implemented policy changes, requiring new approaches to data collection than were conducted previously.

The process was as follows. First, we contacted agencies with which we previously worked, identifying the current research request procedures or, as needed, information on the new agencies or new request procedures. Second, the study team submitted data requests. Third, study team members obtained security clearances and data use agreements, as required. Concurrently, the study team took steps to prepare for the data exchange, including compiling sample lists and setting up secure data transfer and storage procedures. Finally, depending on each agency's data system, matching was conducted using criminal justice IDs, social security numbers, names and birthdates, sex, race/ethnicity, or combinations of these identifiers.

Through this effort, for each of the seven SCA grantees (across six states), we obtained three different types of administrative criminal justice records.

- **Arrest and conviction data.** These data included arrest dates, charge types, conviction dates, types of conviction, and severity (e.g., felony or misdemeanor). We obtained these data from state police departments, administrative offices of the courts, and/or state departments of justice. Depending on the state, these data were housed within one or two different agencies.
- **State prison incarceration data.** These data included admit and release dates, among other variables, within state prisons. We requested these data from state departments of corrections.
- **County jail incarceration data.** These data included admit and release dates, among other variables, within county (and city) jails. We requested these data from sheriff's departments or jail administrators in the county of release for SCA participants.

We were able to obtain most, but not all, of the requested data.[4] First, while we requested criminal justice records for as far back as possible prior to random assignment up through the time of the match, some agencies were not able to provide, or provided only limited, historical data due to the extent to which they retained historical records. Second, some agencies did not have the administrative capacity to fulfill our request despite the study team offering to provide funds for the provision of data and otherwise meeting all requirements. As shown in Exhibit I-4, we were able to obtain arrest and jail records for six grantees and conviction and prison incarceration records for all seven grantees. Due to differing times at which data matching occurred, we were able to obtain at least nine years and seven months of criminal justice system data for all the participants for whom we have data.

Exhibit I-4: Criminal Justice Data Obtained for the SCA Impact Study

State (Grantee)	Arrest and conviction data	State prison incarceration data	Jail incarceration data
California (San Francisco and San Mateo County)	California Department of Justice	California Department of Corrections and Rehabilitation	San Francisco and San Mateo County Sheriff's Departments
Kentucky	Kentucky Admin Office of the Courts	Kentucky Department of Corrections	Louisville Metro Department of Corrections
Oklahoma	Oklahoma State Bureau of Investigation	Oklahoma Department of Corrections	Oklahoma County Detention Center (data not provided)
Oregon (Marion County)	Oregon State Police (arrest data not provided) and Oregon Criminal Justice Commission (conviction data provided)	Oregon Department of Corrections	Marion County Sheriff's Office
Pennsylvania (Allegheny County)	Admin Office of Pennsylvania Courts	Pennsylvania Department of Corrections	Allegheny County Department of Human Services in partnership

[4] Our efforts to locate study participants for the survey confirmed that most participants continue to reside in the same state/county where they were incarcerated at the time of the study. Of the respondents who completed the survey, 4.0% were residing outside of the state to which they were randomly assigned, while 96.0% were residing within their assigned state at the time of data collection.

State (Grantee)	Arrest and conviction data	State prison incarceration data	Jail incarceration data
			with the Allegheny County Jail
South Dakota	South Dakota State Police (arrest data not provided) and South Dakota Division of Criminal Investigation (conviction data provided)	South Dakota Department of Corrections	The Pennington and Minnehaha County Jails via the South Dakota Department of Corrections

Finally, as with our earlier research, the study team obtained employment and earnings data from the National Directory of New Hires database operated by the Administration for Children and Families (ACF) within the U.S. Department of Health and Human Services (HHS). These data are built up from states' quarterly Unemployment Insurance (UI) program wage and claimant files, federal employment files, and Directory of New Hires, and include information on covered workers' dates of hire, quarterly employment and earnings, and UI claimant benefit amounts. They do not include any information on the type of employment (e.g., part-time, full-time, or seasonal) nor would they obtain records of employment for any unreported earnings by workers or employers (i.e., “under the table” employment), which captured in the survey. The database, which only retains data for a rolling two-year period, is maintained to assist states in enforcing child support obligations for noncustodial parents, but can be used for research purposes under strictly defined circumstances. Through an agreement between HHS and DOJ, the study team gained access to NDNH data for study participants in order to calculate program impacts on employment and earnings. As part of this request, the study team received data on employment and earnings for all study participants for the two-year period starting in the third calendar quarter of 2022 through the second calendar quarter of 2024.

Analysis Techniques

The study uses an intent-to-treat framework in that we compare the outcomes of those randomly assigned to the treatment group to the outcomes of those assigned to the control group. Random assignment 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 treatment group and those to whom they are being compared. Because of the pre-random assignment equivalence between the groups, estimation methods can be relatively simple. Using simple t-tests, we can compare the mean differences in outcomes between the groups to identify the effects of being assigned to SCA. This approach was applied in our analysis of administrative employment and recidivism data, as all participants were included. For the survey data, we built off this simple approach in several ways. First, we weighted the survey sample to account for the fact that the probability of assignment to the treatment group was not constant across the grantee sites. When using survey data, we also weighted analyses to account for nonresponse bias (Details of the survey weighting are described in Appendix D).

Because random assignment effectively neutralizes the impact of pre-existing characteristics, we calculate impacts as the simple difference in means between the treatment 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 sex, 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. Statistical methods and results from the additional models are described in Appendix E.

We also estimated impacts separately for subgroups based on pre-existing characteristics of participants. 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 identified the following three subgroups, also used in our prior research:

- **Sex.** 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 specific to women (Berman 2005, Bloom et al. 2003). A program's effectiveness may vary between women and men, depending on how well it addresses their specific needs.
- **Age.** 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 (or a decrease in criminal behavior), 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.** Sex 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). The study's SCA grantees determined risk by using validated assessment instruments. 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. All those eligible for SCA were

expected to be at medium or high risk of recidivism, making this classification a relative ranking within a truncated range.

Limitations

Although the research design ensured a rigorous and unbiased estimate of impacts and random assignment was effectively executed (as discussed in the next chapter), the findings are subject to certain standard limitations and cautions described below.

Longitudinal Sample Attrition

One limitation for the study is longitudinal survey sample attrition and a subsequent lack of sample size and statistical power for detecting effects between the treatment and control groups in the survey findings. The original study was designed for an 18-month follow-up with an expected 80 percent response rate in the survey. The 18-month survey had 789 responses and an 81.7 percent response rate consistent with expectations. The long-term follow-up survey received 378 responses and a response rate of 39.1 percent of the randomly assigned sample. The expected minimal detectable effect for the long-term follow-up survey in employment is fifteen percentage points.

Missing or Non-Continuous Administrative Data

As noted above, the new administrative data sources were incomplete in a few ways. First, while continuous from the point of random assignment, the study team was unable to obtain consistent historical data on criminal justice history for participants. However, the study team was able to obtain these historical data in the prior analysis and refers to those to confirm the integrity of random assignment. Second, as noted above, we were unable to obtain data from two criminal justice agencies.[5] Third, the limited time frame of the employment data prevented the team from examining

[5] The impact analysis does not include arrest data for South Dakota participants or jail data for Oklahoma participants.

employment history over the full follow-up period, so the findings of this analysis are limited to just a two year period (from July 2022 to June 2024) that is well after the point of randomization. We are unable to provide information about the time period between the eighth quarter following random assignment (as examined in the earlier evaluation) and approximately ten to twelve years after random assignment.

Limitations Due to Program and Study Design

Certain aspects of random assignment and program design introduced limitations in the interpretation of the findings.

- **Control-group members were allowed to access re-entry services.** Those randomly assigned to the treatment 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, therefore, compares the effectiveness of SCA at these sites relative to the services that are otherwise available, rather than comparing it to the absence of any services.
- **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 (e.g., prison programming) that could have benefited all those returning from incarceration (including the control group) to some degree. Because these changes were general system improvements, it was not practical to deny control-group members access to them. The study, therefore, captures the effect of the personalized services that SCA provided, rather than general system improvements.
- **The individual SCA program models varied in important ways.** The seven SCA grantees that participated in this study used their SCA funds to implement somewhat different program models. The programs varied both in terms of target population and services offered, so this study looked at the impact of the mix of services, rather than the impacts of one single intervention. Because of small sample sizes in each grantee site, it was not practical to estimate grantee-specific impacts, so observations were pooled across the seven grantees.

- **Data collection efforts were most limited to the states where the SCA programs originally operated.** The study team focused survey location efforts and administrative data collection to those states (California, Kentucky, Oklahoma, Oregon, Pennsylvania, and South Dakota) where the SCA programs were located. Furthermore, jail data were only obtained from the counties to which participants were released or in which programs operated, which was limited to one or two counties within each of the six program states. Focusing resources in these states and counties allowed the study team to successfully obtain data for a more substantial portion of the sample and since obtaining data from additional counties and states would have resulted in substantial costs for relatively low levels of additional data (only 4.0 percent of survey respondents were located outside of these states). However, because data collection efforts were limited to these areas, our final respondent group may not include individuals who left these states where they were enrolled in SCA programs.
- **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 it was 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 SCA grantees. Moreover, BJA made SCA awards in subsequent fiscal years and under different categories of competition; therefore, the results of this study cannot be generalized to those other grantees.

Expected Applicability of the Research

The goal of this study was to produce findings that will allow policymakers at the Federal, State, and local levels to design more effective programs for integrating former prisoners into society, increasing their opportunities for employment, and reducing their rates of recidivism. Building on the findings from the earlier research, NORC and SPR anticipate that the findings from this long-term

follow-up study may provide useful information to policymakers at all levels as they consider different approaches to prisoner reentry and their likely outcomes across different populations, especially over the long term. Findings should also contribute to more effective practices for conducting long-term research on justice system involved populations. NORC and SPR hope that these findings will contribute to the current policy debate on how best to support formerly incarcerated individuals who are reentering society.

Outline for the Remainder of the Report

The remainder of the report proceeds as follows. The next chapter provides additional background information on the SCA programs, including the services provided to participants, and details about how random assignment was implemented and the results. Chapter III addresses the evaluation's first research question and describes the impact of SCA programs on criminal justice system outcomes, specifically the arrests, convictions, and periods of incarceration experienced by study participants during a nearly 10-year long period following random assignment. Chapter IV addresses the evaluation's second research question and describes the impact the programs had on participants' employment and earnings. The impacts of the program on other outcomes beyond recidivism and employment, including housing, health, and child support, as described in the third research question, are provided in Appendix B. Chapter V summarizes our findings, presents the study's conclusions, and offers lessons for future programming and research.

II. Background

This chapter provides background information for the longitudinal research discussed in this report. It describes the general challenges facing individuals reentering their communities and provides additional information on the seven SCA Adult Reentry Demonstration grantee programs that participated in the randomized controlled trial (RCT) impact study. It describes the grantee organizations, how they implemented their programs, and the process by which the RCT impact study was implemented, including the enrollment of individuals. The information summarized in this chapter on the program and the RCT impact study implementation is based on information presented in the three prior SCA evaluation reports (D'Amico et al., 2013; D'Amico et al., 2017, and D'Amico and Kim, 2018).

Challenges Facing Those Reentering Society

At the end of 2022, approximately 5.4 million individuals were under the supervision of an adult correctional system in the U.S., accounting for nearly 2 percent of all U.S. residents (Bureau of Justice Statistics, 2022). Reentry into society after incarceration presents significant challenges for individuals, particularly in securing stable employment. A criminal record can make it challenging to find work, as many employers are hesitant to hire individuals with felony convictions. Research shows that approximately 60 percent of formerly incarcerated individuals remain unemployed one year after their release (Couloute & Kopf, 2018). Even when employment opportunities are available, the jobs obtained are often low-wage positions with limited benefits, perpetuating economic instability and increasing the likelihood of recidivism (Western et al., 2015; Wang & Bertram, 2022).

Housing is another critical barrier for formerly incarcerated individuals. Many face restrictive housing policies, including public housing bans for those with felony convictions and discrimination from private landlords. A lack of stable housing exacerbates the challenges of reentry, as homelessness or

housing insecurity increases the risk of violating parole conditions and reoffending (Geller & Curtis, 2011; Francis et al., 2023). Reintegration into society becomes far more difficult without a safe and consistent living environment.

Additionally, social stigma and community reintegration pose emotional and psychological hurdles. Formerly incarcerated individuals often face rejection from family and community members, creating a sense of isolation and discouragement (Hirschfield & Piquero, 2010). This stigma can also lead to mental health challenges, including depression and anxiety, or exacerbate pre-existing conditions, which further complicate the reentry process. Accessing mental health services can be difficult, especially for those without health insurance or stable income (Mallik-Kane & Visher, 2008; Galletta et al., 2021).

Finally, systemic barriers such as limited access to education and healthcare can hinder successful reentry. Many incarcerated individuals lack the necessary educational qualifications to pursue higher-paying jobs, and educational opportunities within prisons are often inadequate or unavailable (Travis et al., 2014). Healthcare access, particularly for mental health and substance abuse treatment, is essential for successful reintegration, yet it remains out of reach for many due to financial and systemic obstacles (Freudenberg et al., 2005).

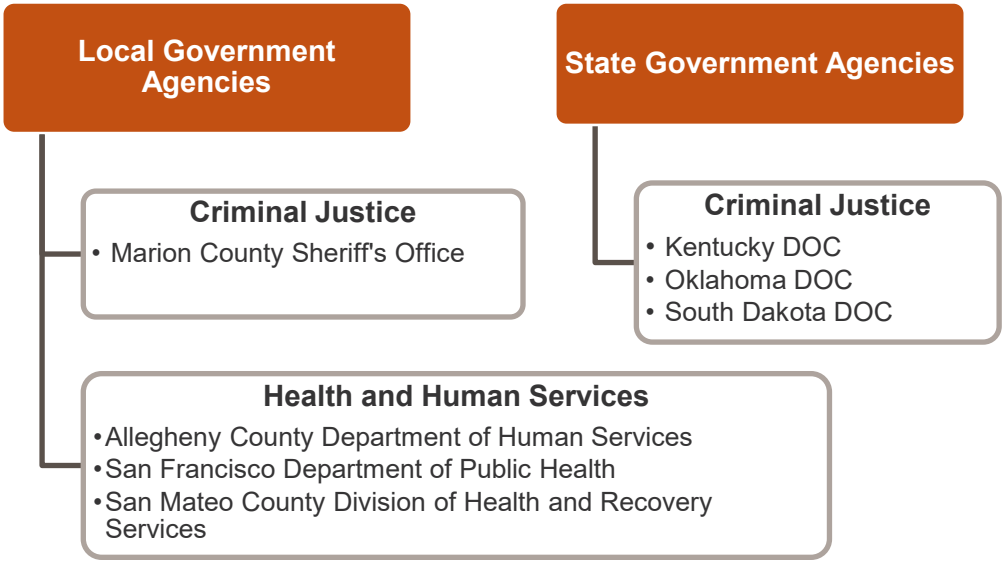
Addressing these challenges among formerly incarcerated individuals requires coordinated efforts across policy, community support, and rehabilitation programs to reduce recidivism and promote successful reentry. The long-term impact of these challenges underscores the need for sustainable solutions. SCA funding was intended to help address these and other reentry issues for this particularly challenged population.

SCA Grantee Overview

BJA began issuing SCA Adult Reentry Demonstration grants in fiscal year (FY) 2009 to state and local governments and federally recognized Indian tribes with the goal of helping them implement strategies to address the challenges faced by adults returning to their communities after incarceration. The seven grantees introduced in the last chapter were awarded their initial grants as part of that first round of FY 2009 grant funding although they were also awarded several rounds of continuation funding. Exhibit II-1 lists the seven grantees and organizes them by grantee type: three were state departments of corrections (DOCs); one was a county sheriff's office; and three were local government social services or health agencies. The evaluation's earlier implementation study found that these distinctions mattered in that DOCs generally had probation or parole officers (POs) carry out SCA's post-release case management functions. These individuals had the intrinsic challenge of balancing supervisory and rehabilitative functions and were sometimes slower to gain the trust of SCA participants. However, they were better able to coordinate with jail and prison staff, and having a parole officer as a case manager was beneficial for ensuring that participants kept their case management appointments. Social services agencies, by contrast, had wider networks of social service agencies with whom they could partner but weaker connections with prisons and jails. At the same time, participant retention could be a challenge for them in that program participants sometimes did not take advantage of the case management offered. Despite these observed differences in implementation, the earlier impact analyses did not find any difference in criminal justice impacts when considering grantee type (i.e., criminal justice agency or health and human services agencies) as a subgroup.

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

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



Source: D'Amico et al., 2013

SCA Program Services

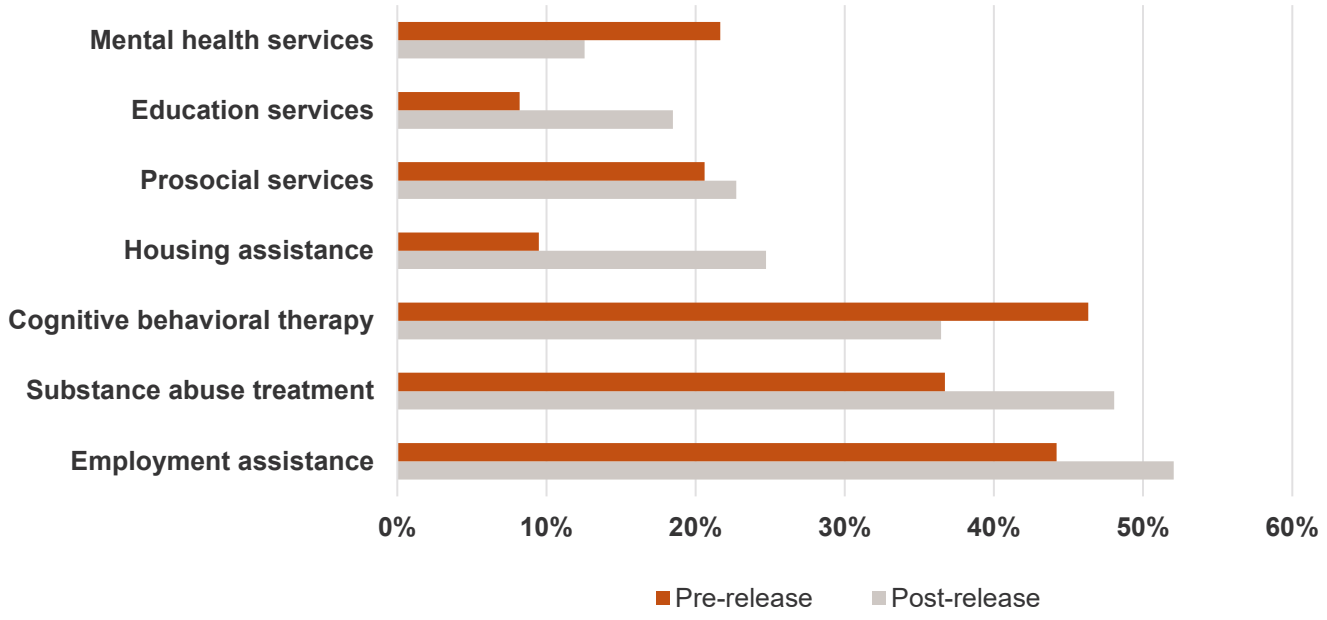
Grantees used their SCA grant funds to fill gaps in existing reentry services in their communities at the time, as well as expand their service capacity. Regardless of how they got there, one core component of grantee programs was the use of various types of risk and needs assessment tools to both establish program eligibility and determine services for participants. For example, one grantee used a proxy indicator (based on age, age at first arrest, and number of arrests) to establish program eligibility. However, once the individual was enrolled in SCA, a validated risk assessment tool was administered to develop a reentry plan. Another grantee used a risk assessment tool at the outset, but also used a formal needs assessment tool for ongoing case planning.

For six of the seven grantees, another core service component was case management. Its purpose was to help prevent program participants recidivism by providing individualized support and coordinating their access to services based on identified needs and risk factors. The case managers were either POs (for DOC grantees) who commonly had reduced caseloads and extra training provided through the grant or came from social services agencies and had more traditional case management

backgrounds (e.g., social workers, counselors). In the latter case, SCA participants might also have been required to (separately) report to a PO. In contrast, the seventh grantee implemented a different program model in which participants attended a structured set of classes that took place full-time, Monday through Friday, during the first 12 weeks after release, followed by a period of "aftercare," consisting of one- to two-hour sessions for an additional 12 weeks. 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.

In addition to these services, the seven impact study grantees also provided program participants with one of seven types of program services. The rates at which impact study program participants were provided these services is shown below in Exhibit II-2, followed by a definition of these services as laid out in the original SCA grant solicitation in Exhibit II-3 (U.S. Department of Justice 2009, pp. 2-3). Employment assistance, cognitive behavioral therapy, and substance abuse treatment were the most common services provided, with nearly one-half of SCA program participants who were in the impact study having received employment assistance and cognitive behavioral therapy and more than one-third having received substance abuse treatment while still incarcerated. These three services were also the most commonly provided through SCA after release. These data also showed that approximately 25 percent of program participants in the impact study participated in SCA for more than one year, and another 37 percent participated for between six and twelve months. A little less than 40 percent participated for up to six months or less.

Exhibit II-2: Incidence of Pre-Release and Post-Release Services for Those in the Treatment Group



Source: Grantee MIS data, presented in D'Amico, et al., 2017

Exhibit II-3: Types of Program Services

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

Source: D'Amico et al., 2013

Another key finding from the implementation study was that some grantees provided these services directly, while others provided them through various program partners, either on a fee-for-service basis (formal partnerships) or through unfunded referrals (informal partnerships). Exhibit II-4 summarizes the nature and focus of these approaches as well as their advantages and limitations.

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 made formal arrangement with provider for services	No specific terms or agreement; SCA program staff provided referrals
Priority for Participants	The service was exclusively for SCA participants	SCA participants were given priority over others	SCA participants were like all others seeking services
Advantages	Specifically tailored to SCA participants; grantee-controlled access and engagement	Provided SCA participants with priority access; services were coordinated by the program	Most flexible, least costly, and allowed SCA staff members to use any service provider available in the community
Limitations	Grantee lacked resources and expertise to deliver all services directly	Typically cost the program money	SCA participants had the same access to services as others; little formal follow-up on participant involvement in services

Source: D'Amico et al., 2013

Prior SCA Evaluation

The previous evaluation of the seven FY 2009 SCA Adult Demonstration grantees was conducted from 2010 to 2018 and included an RCT impact study, an implementation study, and a cost study. Random assignment began in December 2011 and ended in March 2013, enrolling 966 individuals in the study. Of these, 606 (62.7 percent) were assigned to the study's treatment group, while 360 (37.3

percent) were assigned to the control group.[6] Treatment group members were able to participate in individualized SCA services while control group members could receive any reentry services otherwise available in their communities, but were ineligible for individualized SCA services.

This prior impact evaluation measured the program's effects on participant outcomes at 18 and 30 months after random assignment, revealing several notable findings.

- *In terms of overall implementation, grantees used SCA funds to expand their capacity for reentry services.* All grantees allocated funds to provide services after individuals were released from incarceration, with some enhancing pre-release services. Case management was a common service component, provided by either parole officers with reduced caseloads or staff members from social services agencies or community-based organizations. Additional services included employment assistance, substance abuse treatment, and cognitive behavioral therapy.
- *The treatment group was significantly more likely than the control group to have received a variety of services.* These included a re-entry plan, a trusted case manager, job search assistance, cognitive behavioral therapy, and other needed services. However, at the end of 18 months, individuals in the treatment group reported having just as many unmet service needs as those in the control group.
- *Individuals in the treatment group experienced better long-term employment and higher earnings than those in the control group.* In the second year after random assignment, the SCA treatment group reported consistently higher employment rates and, towards the end of the observation period, earned 83 percent more than the control group.
- *In the 30 months following random assignment, individuals in the treatment group were no less likely to be re-arrested, reconvicted or re-incarcerated than those in the control group.* Their time to re-arrest or re-incarceration was not shorter, and they did not have fewer total days incarcerated

[6] While 973 individuals were randomly assigned, one grantee lost the signed consent forms for seven individuals, who were then dropped from the study.

(including time in both prisons and jails). Additionally, they had a slightly greater total number of re-arrests and reconvictions, which may have been due to enhanced case management increasing the likelihood of detecting new offenses.

This earlier research also identified several possible reasons why being assigned to the treatment group improved employment and earnings but did not reduce recidivism. First, the service differential between the treatment and control groups was most pronounced for employment-related assistance, with more modest differences (though statistically significant) for other services. This suggested that control group members were often able to access similar services elsewhere, highlighting the fact that the study measured the impacts of the grantees' services relative to the alternative services available, rather than having no services available at all. Second, the SCA funds provided did not appear adequate to address the many complex needs of individuals returning from incarceration. Third, there were limitations in the grantees' service models; most emphasized case management, and prior research has indicated that casework alone is not particularly effective as a reentry approach.

Composition of Study Participants

Exhibit II-5 shows the key characteristics of treatment and control group members measured at the time of random assignment. As expected, those in the treatment and control groups had similar demographic characteristics. Nearly 80 percent of participants in both groups were male. Approximately half of participants were White, and nearly one-third were African American. Additionally, approximately 15 percent in both study groups were American Indian/Alaska Native, and around 10 percent identified as Hispanic. Roughly half of the participants in each study group were 30 years old or younger, while one quarter were older than 40.

In terms of other characteristics and background, the study groups were very similar. About one-quarter of participants in both groups had not obtained a high school diploma or GED. A small

percentage in both groups (around six percent) had attended some college. While nearly all participants had been employed at some time prior to random assignment, approximately half of the study sample had been employed at the time of incarceration that preceded random assignment, usually full-time. The other half were not employed at the time of incarceration. Just over 10 percent of respondents reported having a disability (defined as a condition limiting one's physical activity or type of work). Nearly all study participants spoke English as their primary language.

Importantly, there were virtually no statistically significant differences between the treatment and control groups, except for a modest difference in the percentage who worked at some point prior to random assignment.

Exhibit II-5: Background Characteristics of Treatment and Control Group Members

	Treatment	Control	Difference
Demographic Characteristics			
Sex			
Female	21.8	19.9	1.9
Male	78.2	80.1	-1.9
Race and Ethnicity			
White	52.3	49.0	3.3
African American	31.2	33.8	-2.7
American Indian/Alaska Native	13.2	15.6	-2.4
Hispanic	10.2	9.2	1.1
Hawaiian Native/Pacific Islander	1.8	2.4	-0.6
Asian	0.9	1.7	-0.8
Age			
18 to 21	8.4	9.3	-0.8
22 to 25	17.8	20.6	-2.8
26 to 30	23.7	23.3	0.4
31 to 35	15.7	12.9	2.8

	Treatment	Control	Difference
36 to 40	8.2	11.2	-2.9
41 to 50	18.6	17.6	1.0
51 or more	7.5	5.2	2.3
Highest Degree Attained			
Less than high school degree or GED	25.0	23.2	1.8
GED	44.9	43.4	1.4
High school diploma	24.4	27.1	-2.8
Some college	5.7	6.2	-0.5
Employment Characteristics			
Worked sometime in the past	93.0	88.8	4.2**
Employment status at time of most recent incarceration prior to random assignment			
Was employed full time	32.7	33.3	-0.6
Was employed part time	14.4	15.4	-1.0
Was not employed	52.9	51.3	1.6
Other Characteristics			
Has a disability	13.6	11.6	2.0
English as a primary language	98.7	98.8	-0.1
Sample Size	606	360	

Note: Numbers in the first two columns represent the percentage of study participants with the characteristics in question; the third column represents the difference between the two (treatment group value minus control group value). Estimates were weighted to equalize the odds of selection into the groups and, where appropriate, to account for potential survey nonresponse bias. The sample sizes shown are for items taken from the baseline information as collected in the prior research.
Sources: Data in this table come from sources collected during the prior research, including the BIF, except for the information on English as a primary language, which comes from the survey.
*/**/*** Statistically significant at the.1/.05/.01 level.

In addition, Exhibit II-6 reports on the criminal history of study participants in the period prior to random assignment. According to these data, members of both the treatment and control groups were arrested and incarcerated a similar number of times prior to random assignment, and their offense categories were comparable. The length of their most recent prior sentence was also similar, with just over half of both groups serving sentences longer than two years. One difference was that 88 percent of treatment group members were randomly assigned while still incarcerated, compared to 83 percent.[7] of the control group.[8]

Exhibit II-6: Criminal History of Treatment and Control Group Members

	Treatment	Control	Difference
Number of separate times arrested in the 10 years prior to random assignment[a]			
1 to 2	14.8	15.2	-0.4
3 to 5	21.4	23.3	-1.9
6 to 10	26.3	25.4	0.9
11 or more	37.5	36.1	1.4
Most serious arrest offense in the 10 years prior to random assignment[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

7 Whether an individual was incarcerated at the time of random assignment was determined by comparing spells of incarceration measured from administrative data to the date of random assignment. At the time these baseline data were collected and analyzed, we did not have prison and jail data for jurisdictions nationwide. Therefore, these estimates likely undercounted the actual percentage incarcerated at the time of random assignment. Survey data from the prior research show that the percentage who self-reported that they were incarcerated on the date of random assignment is approximately 88 percent for both the program and control groups.

8 Whether an individual was incarcerated at the time of random assignment was determined by comparing spells of incarceration measured from administrative data to the date of random assignment. At the time these baseline data were collected and analyzed, we did not have prison and jail data for jurisdictions nationwide. Therefore, these estimates likely undercounted the actual percentage incarcerated at the time of random assignment. Survey data from the prior research show that the percentage who self-reported that they were incarcerated on the date of random assignment is approximately 88 percent for both the program and control groups.

	Treatment	Control	Difference
Number of separate times incarcerated in prison or jail any time prior to random assignment[b]			
1	11.1	13.3	-2.2
2 to 4	38.3	34.2	4.2
5 or more	50.5	52.5	-2.0
Type of crime for which most recently incarcerated prior to random assignment[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 random assignment[b]			
Less than 90 days	3.6	4.8	-1.2
At least 90 days but less than 6 months	6.6	7.3	-0.7
At least 6 months but less than 1 year	14.1	13.2	0.8
1 year to 2 years	21.0	20.2	0.8
More than 2 years	54.7	54.5	0.3
Total days incarcerated in prison or jail in 10 years prior to random assignment[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 random assignment[c]	87.8	83.1	4.6**

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

Sources: Data in this table come from sources collected during the prior research: [a]=Administrative data; [b]=BIF; [c]=Both administrative data and the study's random assignment system.
[#] The sum across the categories exceeds 100 percent because multiple types of crimes could have been recorded.
*/**/*** Statistically significant at the.1/.05/.01 level.

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

Summary of Earlier Evaluation Findings

The evaluation's 18-month impact study report (D'Amico et al., 2017) highlighted several important implementation and impact study findings that may have influenced the limited impacts observed for the program at that time. These findings may also be relevant when interpreting the findings discussed in this report.

- **SCA funds led to important system changes, but fidelity to the reentry model funded by BJA was incomplete.** The earlier implementation study found an important contrast between services under SCA and those provided before the grants began. More attention was given to reentry planning, additional funds were available for services, partnerships were strengthened, and, especially after release from incarceration, participants gained access to case management services that were intended to be more robust than those typically available. Further, for DOCs, SCA grant funds allowed a continued shift away from strictly an enforcement mindset toward a rehabilitative philosophy. Nevertheless, full implementation of the originally proposed service model was found to be incomplete in several ways. In particular, the continuum of care from incarceration to release was less developed in some sites than in others. Furthermore, none of the grantees had sufficient funding to directly deliver all of the services that participants needed. As a result, they relied heavily on unfunded referrals to meet many of these needs. Where unfunded referrals were used, coordination with the grantee's program was often limited, and case managers were not always able to track whether participants accessed the referred services. Given the reliance on unfunded

referrals for many post-release services - and challenges with participant retention in some locations - earlier evaluation reports noted that not all SCA participants received all of the services deemed necessary.

- **The treatment group was significantly more likely than the control group to receive an array of reentry services.** This finding was based on the results of the 18-month participant survey. For example, program participants were more likely to report receiving case management assistance than control group members. This included participants' self-reports of receiving help with reentry, having a reentry plan, and identifying someone who went out of their way to support them. Individuals in the treatment group were also significantly more likely to receive assistance finding a job, cognitive behavioral therapy, housing assistance, outpatient substance abuse treatment, and prosocial services. However, being in the treatment group had no significant effect on the likelihood of receiving other mental health services or education and vocational training.
- **Despite the grantees' programs having a significant impact on service receipt, participants in the treatment group still reported many unmet service needs at 18 months after random assignment, compared to those in the control group.** Results from the 18-month participant survey also indicated that program participants reported service needs that were comparable to those of the control group. For example, approximately two-thirds of both groups reported a need for additional housing assistance and job placement support. More than half reported wanting further access to health services, education, and job training. Additionally, over one-third of both groups indicated a need for family reunification services, substance abuse treatment, and mental health services.

III. Impacts on Recidivism

The previous SCA evaluation found that the SCA program had no impact -or in some cases, a negative impact- on program participants' criminal justice outcomes at both 18 and 30 months after random assignment. The current longitudinal evaluation provides an opportunity to examine the long-term effects of the SCA program on these same outcomes, tracking the same participants for up to nearly 10 years after their initial assignment.

Summary of Prior Evaluation Findings

The prior SCA evaluation assessed the program's impact on participants' arrests, convictions, parole and probation status, and incarcerations (in either prison or jail). This analysis used both administrative and survey data at 18 months following random assignment, and administrative data only at the 30-month mark. The 18-month follow-up period was limited because most study participants were still incarcerated at the time of random assignment and some participants were not released for six or more months after enrolling in the study (D'Amico et al. 2017). This was improved upon in the 30-month analysis (D'Amico and Kim, 2018). While the current longitudinal analysis examines impacts up to nearly 10 years after random assignment -using both administrative and survey data - it is important to note that the earlier 18- and 30-month analyses found no impact of the SCA program on any criminal justice outcomes. More specifically:

- **Assignment to the treatment group did not reduce the probability of recidivism.** At both 18 and 30 months after random assignment, and across all data sources, individuals in the treatment group were equally, or sometimes more likely, than those in the control group to be re-arrested, re-convicted, experience new parole or probation violations, or be re-incarcerated in either prison or jail.

Similarly, their time to first re-arrest or re-incarceration (based on administrative data) was no shorter, and the total number of days incarcerated (in either prison or jail) was no lower.

- **The 30-month analysis found an increase in the overall rates of recidivism since the 18-month analysis**: 1) nearly 60 percent of those in both the treatment and control groups were re-arrested, and approximately 45 percent were reconvicted; 2) approximately 60 percent were re-incarcerated, most of which were jail incarcerations; and 3) excluding the time between the random assignment date and initial release, study participants spent approximately 140 days incarcerated.
- **The 30-month analysis revealed that the treatment group experienced a higher number of re-arrests and reconvictions.** While the effect was small, it was statistically significant. In other words, the program had a negative impact on recidivism, as member of the treatment group were more likely than those in the control group to be arrested and convicted within the 30 months following random assignment.
- **The 30-month analysis showed at best, modest subgroup differences.** Generally, assignment to the treatment group did not reduce recidivism for any of the subgroups we examined. The only subgroup difference observed was that treatment group membership may have increased involvement with the criminal justice system younger individuals, but not for those who were older at the time of random assignment.
- **Assignment to the treatment group did not have an impact on the timing of release from incarceration.** Although 85 percent of study participants were incarcerated at the time of random assignment - and there was some uncertainty about whether program enrollment might affect release - the 30-month analysis found no impact on the timing or likelihood of initial release. Both the treatment and control groups were released at the same rate, with an average time to release of six months. By the 30-month mark, nearly all participants in both groups had been released.

Primary Analysis of Administrative Data

For the current longitudinal analysis, the study team was able to obtain recidivism data for nearly ten years following random assignment for all study participants.[9] Below, we describe the levels of recidivism in the data, as well as any impacts between the treatment and control groups.

The process of randomization is intended to equalize the treatment and control groups on all measurable and unmeasurable characteristics. Given this differences observed between the two groups can be attributed to the program intervention being studied. For this reason, outcomes are typically measured from the date of random assignment, and study participants are generally not sub-setted based on events occurring following random assignment. Following this approach, we calculated impacts on recidivism for the full sample measured in the nearly ten years since random assignment.

Exhibit III-1 presents the results on recidivism during this period.

- Nearly 75 percent of both the treatment and control groups were re-arrested sometime in the nearly ten years since random assignment. There were no significant difference between the groups.
- There was an average of approximately six arrests per person. Approximately 60 percent of participants in both groups were arrested for a public order offense (63 percent of the treatment group and 66 percent of the control group), making it the most common reason for arrest. Arrests for violent crimes were less frequent.
- Approximately 75 percent of study participants were convicted of a new crime.[10] The rate of reconviction and the number of overall convictions did not differ significantly between the groups.

[9] Because the timing of random assignment varied across participants, as did the time periods covered by state criminal justice data, the length of follow-up differs somewhat across individuals. However, the minimum period for which we have data on all participants is nine years and seven months.

[10] Given that most individuals were incarcerated at the time of random assignment, we assume that convictions occurring after random assignment generally reflect new crimes. However, this assumption may not hold in all cases.

- Reincarceration rates did not differ significantly between the groups—approximately 70 percent of participants in both groups had been re-incarcerated in either a prison or jail.
- Including the time from random assignment to initial release for those incarcerated at the time of random assignment, study participants spent an average of approximately 170 day in jail and 465 days in prison during the follow-up period. There was no significant difference between the groups in total days incarcerated.

Exhibit III-1: Impacts on Recidivism for the Full Sample

	Treatment	Control	Difference
Re-arrests			
Re-arrested (%)	73.6	77.4	-3.8
Average number of re-arrests	5.8	6.2	-0.4
Re-arrests by offense type (%)[a]			
Public Order Arrest	62.6	66.3	-3.7
Drug Arrest	47.0	49.1	-2.1
Property Arrest	49.6	45.2	4.4
Violent Arrest	32.5	36.9	-4.4
Reconvictions			
Convicted of a new crime (%)	71.4	74.5	-3.1
Average number of reconvictions	2.9	2.8	0.1
Felony Conviction	50.2	55.1	-4.9
Misdemeanor Conviction	60.9	61.8	-0.9
Incarcerations (prison or jail)			
Was re-incarcerated in prison or jail (%)	70.6 (N=612)	72.0 (N=361)	-1.4

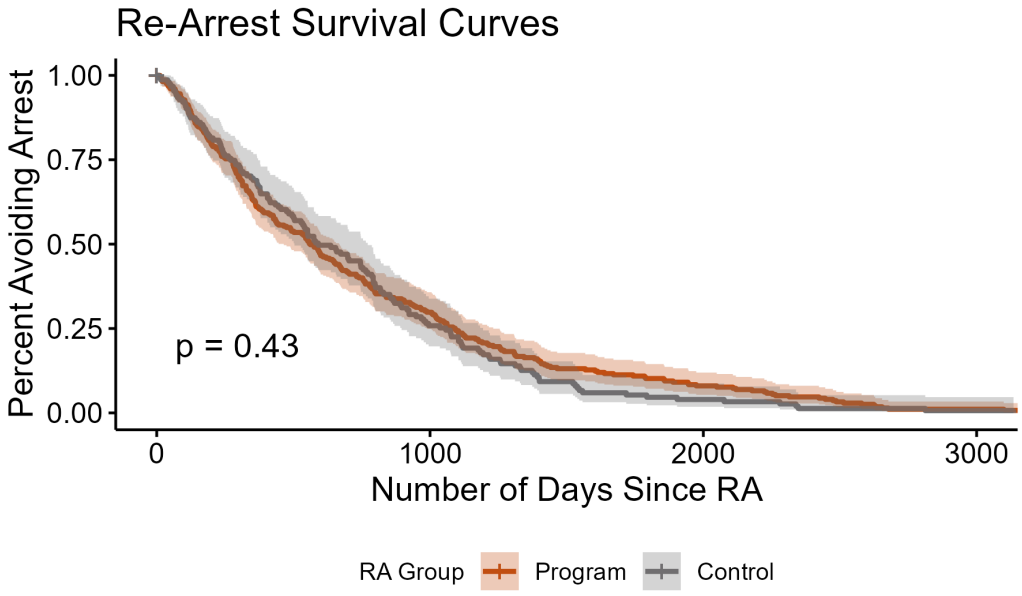
	Treatment	Control	Difference
Experienced a new jail incarceration (%)	68.0 (N=538)	68.1 (N=301)	0.1
Experienced a new prison incarceration (%)	48.0 (N=606)	51.4 (N=360)	-3.4
Total days incarcerated in prison	434.4 (N=606)	500.5 (N=360)	-66.1
Total days incarcerated in jail	184.9 (N=538)	154.9 (N=301)	30.0
Sample Size for Arrest Data	492	279	
Sample Size for Conviction Data	612	361	

Note: Numbers in the first two columns represent outcomes measured for the nearly ten-year period following the date of random assignment for the treatment and control groups. The third column represents the difference between the first two columns.
Source: Administrative data from state and local agencies.
[a] The sum across categories exceeds the percentage ever arrested because individuals can be arrested more than once and with different arrest charges during the follow-up period.
*/**/*** Statistically significant at the.1/.05/.01 level.

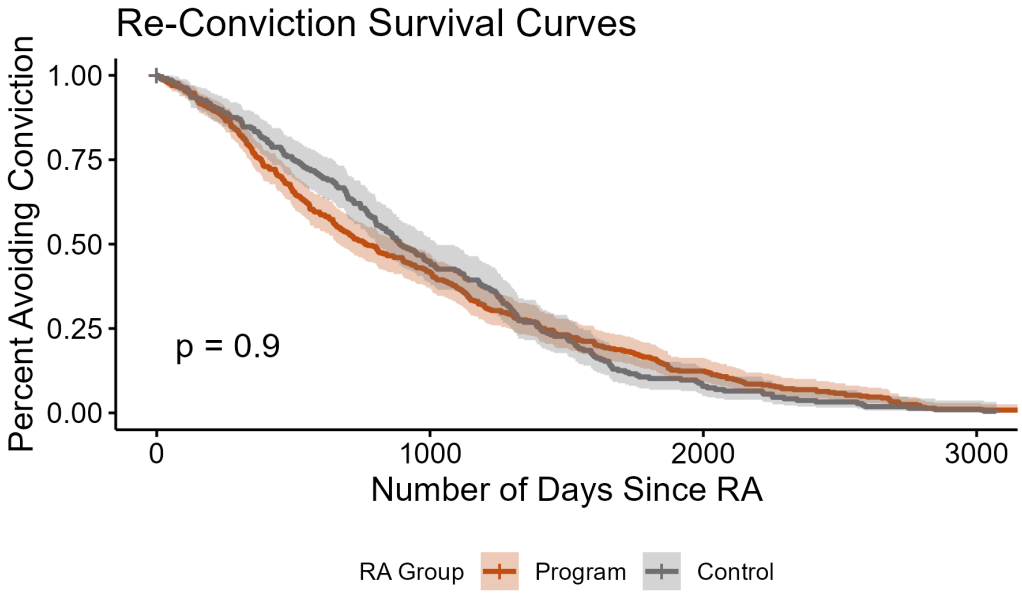
Overall, the results suggest that the program did not have a clear impact on participants' recidivism.

We also calculated the time from the date of random assignment to the first re-arrest or re-conviction. The cumulative frequency distributions of the first occurrence plotted by time elapsed are shown in Exhibit III-2 for both the treatment and control groups. The trajectories run almost completely in parallel and the gap between the groups is very small. There are no significant differences between the groups in the cumulative percentage of individuals with an occurrence at any point during the follow-up period.

Exhibit III-2: Risk Curves for Re-arrest and Re-conviction During the Follow-Up Period for Treatment and Control Group Members



N. Program = 377
N. Control = 196



N. Program = 497
N. Control = 278

Note: The plotted lines represent the cumulative percentage of participants by the first occurrence of the event following the random assignment date.
Source: Administrative data from state and local agencies.
*/**/*** Statistically significant at the.1/.05/.01 level.

Additional Analysis of Administrative Data

Using administrative data, we also calculated the impacts on recidivism for three key subgroups: sex, age, and risk level. Exhibit III-3 display results for sex and age. The numbers tabulated in the exhibits are the *impacts* of being assigned to the treatment group—that is, the difference in outcomes between those assigned to the treatment and control groups *within* each subgroup. Asterisks denote whether this difference is statistically significant, and the † symbol denotes whether the difference in the impact of being assigned to the treatment group is significantly different between the subgroup pair (for example, whether being assigned to the treatment group has a different effect for females than for males).

As shown in the exhibit, assignment to the treatment group did not consistently result in significant improvements in desistance for males or females. Exceptions to this general rule include the finding that those in the treatment group were less likely to be arrested for a violent offense or convicted of a felony. The subgroup comparison reveals that the reduced likelihood of violent crime arrests is driven by males in the program group. Specifically, male treatment group members had violent crime arrest rates nearly 10 percentage points lower than their control group counterparts, whereas female treatment group members had higher rates of violent crime arrests compared to female control group members. The difference between these two subgroups is significant, indicating that the program had a differential impact on this measure for males and females. Additionally, males in the treatment group were less likely than those in the control group to be arrested, convicted (each by 6.7 percentage points), or have a felony conviction (by 7.9 percentage points). In contrast, female treatment group members did not experience these positive impacts; instead, they had higher rates of property and violent crime arrests, as well as misdemeanor convictions, compared to their control group counterparts.

Age-based comparisons reveal a striking contrast. While individuals under 30 at the time of random assignment show little difference between the treatment and control groups, those 30 and older exhibit clear signs of a positive impact of the program. Specifically, treatment group members who were 30 years or older at the time of random assignment were less likely to be arrested or convicted of a crime compared to their control group counterparts. Furthermore, these "older" treatment group members were less likely to be arrested for violent or a public order crimes and less likely to be convicted of a felony. They were also somewhat less likely to be incarcerated in prison, spending approximately 90 fewer days in incarceration compared to their control group counterparts. Nearly all these impacts were significantly different from those observed in the age group comparison (i.e., those younger than 30 at random assignment), indicating that the program had a differential effect on individuals aged 30 and older, producing positive, and in some cases, quite large impacts on this older group of participants.

Exhibit III-3 does not include a comparison between individuals at low risk for recidivism prior to random assignment and those at high risk. This is because there were no differences either between the treatment and control groups within these risk levels or in comparisons across the risk levels.

Exhibit III-3: Impacts on Recidivism by Sex and Age

	Sex		Age	
	Female	Male	Less than 30	30 or Older
Arrests				
Arrested (%)	7.4	-6.7*	3.1†	-7.5***
Average number of arrests	1.4	-0.9	0.8	-1.1
Arrests by offense type (%)				
Violent crime	12.9*†	-8.8**	2.0†	-7.8**

	Sex		Age	
	Female	Male	Less than 30	30 or Older
Property crime	17.7**	1.0	8.9	2.1
Drug crime	3.7	-3.7	1.2	-3.8
Public order crime	4.1	-5.7	10.0†	-10.9**
Convictions				
Convicted of a new crime (%)	10.2	-6.7**	2.5†	-7.4*
Average number of convictions	0.6	-0.1	0.5	-0.3
Misdemeanor conviction	15.7**	-5.3	3.6	-4.1
Felony conviction	1.3	-6.6*	-3.2†	-5.9**
Incarcerations (prison or jail)				
Was re-incarcerated in prison or jail (%)	-2.7	-1.0	5.0	-6.2*
Experienced a new jail incarceration (%)	0.9	-0.6	5.2	-4.9
Experienced a new prison incarceration (%)	5.7	-5.8	4.1†	-9.0**
Total days incarcerated	15.5	-34.9	38.1†	-89.4*
Sample Size	P=133 C=77	P=479 C=284	P=273 C=168	P=339 C=193

Note: Numbers in the exhibit represent the impact estimates. A positive number denotes that the incidence or mean value is higher for the treatment group than for the control group; a negative number denotes that the incidence or mean value is higher for the control group.
Source: Administrative data from state and local criminal justice agencies.
*/**/*** The difference between the treatment and control groups within the subgroup is statistically significant at the.1/.05/.01 level.
† The difference in the impact of SCA between subgroups in the subgroup pair is statistically significant at the.05 level (the symbol is placed by the impact estimate of the first group of the subgroup pair).

Analysis of Survey Data

The long-term follow-up survey complements the analysis of recidivism based on administrative data. However, a key limitation of the survey is its considerably smaller sample size, which restricts the ability to detect statistically significant differences between treatment and control groups. As a result, few meaningful differences emerge in the survey data, and the limited sample size prevents reliable subgroup analyses. Unlike the administrative data, which tracked arrests, convictions, and incarcerations over nearly 10 years following random assignment, the survey measured outcomes on experiences during 2019.

As shown in Exhibit III-4, a higher proportion of treatment group members reported being re-arrested and formally charged with a new crime in 2019 compared to the control group, although these differences were not statistically significant. Similarly, a greater share of treatment group members reported being re-incarcerated in 2019, but again, the differences were not statistically significant.

Treatment group members were significantly less likely to be incarcerated at the time of survey completion (i.e., interview) than control group members. The difference is particularly notable given the survey's small sample size, underscoring the strength of the finding. At the time of their survey interview, only 20 percent of treatment group members were incarcerated, compared to 29.7 percent of the control group - a difference of 9.7 percentage points, or roughly one-third lower for the treatment group. This result offers important context for interpreting the higher rates of re-arrest and re-incarceration by treatment group members in 2019. Taken together, the findings suggest that program participants may be serving shorter sentences. This interpretation aligns with administrative data, which showed that control group members were more likely to be re-arrested for more serious crimes. The survey also asked respondents whether they were incarcerated in 2020 and 2021. In 2020, incarceration rates remained significantly lower for the treatment group (26 percent) than the control

group (35 percent). By 2021, the pattern persisted - 28 percent of treatment group versus 34 percent of the control group- but the difference was no longer statistically significant.

Exhibit III-4: Impacts on Recidivism from Survey Data

	Treatment	Control	Difference
Re-Arrests			
Re-Arrested in 2019 (%)	21.6	19.5	-2.1
Average number of arrests in 2019 (%)	0.34	0.31	-.03
Arrested for a parole violation in 2019 (%)	7.6	6.3	-1.3
Formally charged with a new crime in 2019 (%)	18.6	13	-5.6
Re-Incarcerations			
Re-incarcerated in prison or jail in 2019 (%)	33.6	32	-1.6
Average number of times re-incarcerated in 2019	0.28	0.25	-.03
Currently incarcerated at interview (%)	20.0	29.7	9.7**
Sample Size	250	128	

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

Summary

Over a nearly ten-year period following initial random assignment, relatively few overall impacts were observed. In terms of administrative data, there were no significant differences between the treatment and control groups in their rates of arrest, conviction, or incarceration. Similarly,, there were no differences in the average number of these incidents or in the total time spent incarcerated during the follow-up period. Among survey respondents, the only statistically significant difference was that control group members were more likely than treatment group members to be incarcerated at the time of their interview.

Although overall program impacts were limited, subgroup analyses indicated that certain participants benefited more from the intervention. Specifically, individuals who were 30 years of age or older at the time of random assignment experienced notably better outcomes than their control group counterparts. They were less likely to be arrested, convicted, or incarcerated in prison during the follow-up period. They also had lower rates of arrest for violent or public order crimes, were less likely to be convicted of a misdemeanor or felony, and spent total days incarcerated. Furthermore, male participants were less likely than females to be arrested for a violent crime or to receive a felony conviction. These findings suggest that the SCA programs included in this study had a positive impact, particularly for older participants - and to some extent, for males - by reducing their subsequent criminal involvement on nearly all measures.

IV. Impacts on Employment and Earnings

The prior SCA evaluation offered reasons to be optimistic about the long-term effects of the SCA program on employment and earnings. Although the 18-month impact study found no positive impacts on these outcomes based on both survey and administrative data (D'Amico et al. 2017), the 30-month report identified positive impacts. Specifically, it found increased employment in the seventh quarter following random assignment, as well as higher earnings in both the seventh and eighth quarters (D'Amico and Kim, 2018). This chapter explores the longer-term impacts of the SCA program on employment and earnings, focusing on a follow-up period approximately 10 to 12 years after participants were randomly assigned.

Summary of Prior Evaluation Findings

The prior SCA evaluation reported varying results across different time periods but ultimately suggested a positive trend in employment and earnings outcomes. The 18-month impact report found that treatment group members had higher employment rates and earnings than the control group; however, these differences were not statistically significant (D'Amico et al. 2017). This finding held true across both the administrative data from the National Directory of New Hires (NDNH) database and the evaluation's participant survey data. Although the evaluation was not able to collect additional rounds of survey data, it did obtain extended NDNH data. These data enabled analysis of employment and earnings outcomes for 755 of the 966 study participants at up to eight quarters (24 months) following the quarter of random assignment (D'Amico and Kim, 2018).[11] This extended analysis produced the following observations.

[11] Due to the timing of release from incarceration of participants and a fixed window of data from NDNH, the study team was only able to analyze impacts on employment and earnings for this subsample of 755 study participants. The 30-month impact report demonstrated the baseline equivalence of program and control group members in this subsample.

- **Participation in the treatment group was associated with increased employment.** Treatment group members were more likely to be employed in the seventh (32.7 vs 26.5 percent) and eighth quarters (31.9 vs. 29 percent) after random assignment.[12] The difference observed in the seventh quarter was statistically significant, while the difference in the eighth quarter was not.
- **Treatment participation increased earnings.** On average, individuals in the treatment group earned $780 more than those in the control group during the seventh quarter after random assignment ($1,988 vs. $1,208). In the eighth quarter, the earnings gap widened, with treatment group members earning $1,026 more on average than control group members ($2,268 vs. $1,242). These differences represent earnings increases of approximately 65 and 83 percent and were statistically significant in both quarters, indicating that the program had a growing positive impact on participants' earnings over time.
- **Modest differences were observed across subgroups**. Assignment to the treatment group was generally associated with a greater probability of employment and increased earnings in both the seventh and eighth quarters across the various subgroups examined in the evaluation. However, most of these differences were not statistically significant, likely due to the limited sample sizes within each subgroup.

Primary Analysis of Administrative Data

For the longitudinal analysis of employment and earnings in this evaluation, the study team obtained NDNH administrative data and participant survey data. The NDNH administrative data covered a fixed two-year period, from the third quarter of 2022 through the fourth quarter of 2024.

[12] NDNH reports data in calendar quarters (that is, whether a person was employed in a calendar quarter and how much they earned that quarter). The prior and current evaluations treat the quarter in which random assignment occurred as Quarter 0, and measure employment and earnings in the subsequent quarters. For someone randomly assigned very early in Quarter 0, the period of time up through the seventh and eight quarters covers 22 to 27 months after a person's date of random assignment whereas someone randomly assigned very late in Quarter 0 would equate to 19 to 24 months after that person's date of random assignment.

Because study participants were enrolled on a rolling basis – through random assignment between December 23, 2011 and May 8, 2012 - these administrative data do not reflect the same time intervals following random assignment for all sample members. Given the length of time that elapsed since random assignment, the analysis focuses on measuring employment and earnings for all participants for the full two-year period for which NDNH data are available. In addition, employment and earnings are examined within this timeframe to capture trends over time.

Key findings from these analyses include the following:

- Approximately 45 percent of both treatment and control group participants were employed at some time during the eight quarters for which NDNH data were available.
- In each of the eight quarters, about 25 percent of participants in both the treatment and control groups were employed. There were no statistically significant differences between the groups overall or in any individual quarter.
- While there was no overall difference in total wages earned by treatment and control group participants across the full eight-quarter period, the control group had significantly higher earnings than the treatment group in a few individual quarters.

Results from our primary analysis of employment and earnings data are shown in Exhibit IV-1.

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

	Treatment	Control	Difference
Employment			
Ever Employed	45.0	45.6	0.6
Total Number of Quarters Employed	2.1	2.2	0.1
Employed Q3 2022	28.2	28.5	0.3
Employed Q4 2022	29.6	30.4	0.8

	Treatment	Control	Difference
Employed Q1 2023	27.4	29.3	-1.9
Employed Q2 2023	27.6	27.6	0.0
Employed Q3 2023	27.9	26.8	1.1
Employed Q4 2023	22.8	22.3	0.5
Employed Q1 2024	27.6	28.2	-0.6
Employed Q2 2024	23.3	23.1	0.2
Wages			
Total Wages, Eight Quarters	$18,748	$22,688	-$3,940
Total Wages Q3 2022	$2,262	$2,714	-$452
Total Wages Q4 2022	$2,610	$2,750	-$140
Total Wages Q1 2023	$2,366	$2,955	-$589
Total Wages Q2 2023	$2,190	$2,895	-$705*
Total Wages Q3 2023	$2,419	$2,997	-$578
Total Wages Q4 2023	$2,068	$1,987	$81
Total Wages Q1 2024	$2,833	$4,024	-$1,191**
Total Wages Q2 2024	$2,001	$2,365	-$364
Sample Size	602	355	

Note: Numbers in the first two columns represent outcomes measured for the approximately ten-to-twelve-year period following the date of random assignment for the treatment and control groups; the third column represents the difference between the first two columns.
Source: Administrative data is from NDNH.
*/**/*** Statistically significant at the.1/.05/.01 level.

Additional Analysis of Administrative Data

As with the recidivism data presented in the previous chapter, we also examined the impact of program assignment on employment and earnings for three key subgroups: sex, age and risk level.

Exhibit IV-2 displays the results of these analyses. Consistent with the prior chapter, the figures in the exhibit represent the *impact* of being assigned to the treatment group — that is, the difference in outcomes between treatment and control groups *within* each subgroup. Asterisks indicate whether the differences are statistically significant, while the † symbol denotes whether the impact of program assignment differs significantly between subgroup pairs (e.g., whether the effect differs for females compared to males). As with the recidivism findings, results for participants classified as low or high risk of recidivism prior to random assignment are not included in Exhibit IV-2. These findings are omitted because there were no meaningful differences, either between the treatment and control groups within these risk levels or in comparisons across risk levels.

Exhibit IV-2: Impacts on Recidivism by Sex and Age

	Sex		Age	
	Female	Male	Less than 30	30 or Older
Employment				
Ever Employed	-9.7	1.8	1.6	-2.4
Total Number of Quarters Employed	-0.5	0.1	-0.0	-0.0
Employed Q3 2022	-9.4	2.2	0.5	-0.6
Employed Q4 2022	-6.8	0.8	1.5	-3.0
Employed Q1 2023	-9.4	0.1	0.1	-3.8
Employed Q2 2023	-8.1	2.1	2.5	-2.4
Employed Q3 2023	-5.9	3.0	2.9	-0.5
Employed Q4 2023	-4.3	1.8	-2.7	4.0
Employed Q1 2024	-7.0	1.1	-2.1	1.1
Employed Q2 2024	-2.2	0.8	-3.9	4.6

	Sex		Age	
	Female	Male	Less than 30	30 or Older
Wages				
Total Wages, Eight Quarters	-$8,463*	-$2,739	-$3,582	-$4,185
Total Wages Q3 2022	-$1,038	-$297	-$294	-$597
Total Wages Q4 2022	-$760	$24	$260	-$537
Total Wages Q1 2023	-$754	-$545	-$337	-$837
Total Wages Q2 2023	-$1,312*	-$544	-$312	-$1,101*
Total Wages Q3 2023	-$1,933*	-220	-$402	-$745
Total Wages Q4 2023	-$293	$181	-$395	$592
Total Wages Q1 2024	-$1,729	-$1,049	-$817	-$1,566
Total Wages Q2 2024	-$645	-$289	-$1,284**	$607
Sample Size	P=126 C=74	P=476 C=281	P=300 C=182	P=302 C=173

Note: Numbers in the exhibit represent the impact estimates. A positive number denotes that the incidence or mean value is higher for the treatment group than for the control group; a negative number denotes that the incidence or mean value is higher for the control group.
Source: Administrative data is from NDNH.
*/**/*** The difference between the treatment and control groups within the subgroup is statistically significant at the.1/.05/.01 level.
† The difference in the impact of SCA between subgroups in the subgroup pair is statistically significant at the .05 level (the symbol is placed by the impact estimate of the first group of the subgroup pair).

As shown in the exhibit, there were no measurable impacts of the program on employment across any of the subgroups. Similarly, few significant differences were observed in the wage data. In a few cases, control group members earned higher wages in specific quarters, but these generally appear to be isolated. Overall, the lack of effects observed in the full sample is largely reflected across the key subgroups of interest.

Analysis of Survey Data

As noted in Chapter I, the survey sample consists of 378 study participants who completed the long-term follow-up survey. Due to the small sample size, only large differences between the treatment and control groups are likely to be statistically significant. As a result, the survey results are best interpreted in conjunction with the administrative data findings. It is also important to note that the survey measures employment outcomes specifically for the 2019 calendar year.

A smaller share of treatment group respondents reported being employed during 2019 compared to the control group - a difference of 6.2 percentage points – but this difference was not statistical significance. Among those who were employed in 2019, respondents in both groups reported similar hourly wages and comparable rates of full-time versus part-time employment. Treatment group members reported working an average of 2.2 more weeks than control group members, though this difference was also not statistically significant. They were more likely to report working in temporary or seasonal jobs, off-the-books jobs, and working multiple jobs. These factors may account for the higher number of weeks worked. The difference in the rate of temporary, seasonal, or off-the-books jobs was marginally significant at the 0.10 level, while the difference in the percentage of participants holding multiple jobs did not reach statistical significance.

Exhibit IV-3: Impacts on Employment from Survey Data

	Program	Control	Difference
Employed in 2019 (%)	61.8	68.0	-6.2
Of those Employed in 2019			
Average Weeks Worked	38.3	36.1	2.2
Employed Full-Time (%)	80.0	80.5	-.5
Employed Part-Time (%)	20.0	19.5	.5

	Program	Control	Difference
Employed in Temporary or Seasonal Jobs, or Off-the-Books (%)	45.6	32.9	12.7*
Multiple Jobs (%)	36.1	30.5	5.6
Hourly Rate of Pay ($)	16.0	16.5	.5
Sample Size	250	128	

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

The survey also collected employment data for 2020 and 2021. Employment rates declined for both treatment and control group members in 2020 and remained low in 2021. In 2020, 49 percent of treatment group respondents reported being employed, compared to 47 percent of control group members. In 2021, the rates were 51 percent for the treatment group and 52 percent for the control group. Among those employed in 2020, a higher proportion were working part-time (27 percent of program group), and the average number of weeks was lower (36.2 weeks for the treatment group), likely reflecting the effect of the COVID-19 pandemic on employment. The pandemic appears to have affected both groups similarly, with comparable employment patterns across nearly all outcomes and no statistically significant differences between the treatment and control groups.

Summary

As with the recidivism findings, the analysis of employment and earnings nearly ten years following random assignment revealed few overall impacts in the full sample. There were no significant differences between the treatment and control groups in the percentage employed or in any of the specific quarter of the NDNH data. Total wages earned were also generally similar across groups, with the exception of a few quarters in which control group members had significantly higher average wages. Additionally, the program showed no measurable impact on employment or earnings within any of the subgroups examined.

The survey data showed similar trends, with some modest differences between the study groups, most of which were not statistically significant. When examining the effects of the pandemic on employment, self-reported employment rates declined for both treatment and control group members in 2020 and remained low in 2021 compared to 2019 levels. However, both groups appear to have been similarly impacted by the pandemic.

V. Discussion and Conclusions

This report describes the results of estimating the long-term impacts of seven programs that received grants through an early round of funding authorized under the SCA Adult Demonstration Program. Impacts were estimated using a random assignment design. Individuals who were screened and determined eligible for SCA were randomly assigned either to receive individualized SCA program services (the treatment group) or to receive other re-entry services available outside the specially funded SCA services (the control group). The differences in outcomes between the two groups were then analyzed to identify any program impact.

Summary of Impact Findings

The previous 18-month and 30-month evaluation reports highlighted that individuals randomly assigned to the treatment group were significantly more likely to receive a wide range of re-entry services compared to those in the control group. These services included the development of individualized case plans, cognitive behavioral therapy, job search assistance, substance abuse treatment, housing assistance, and mentoring. However, despite receiving these services, treatment group participants reported having many unmet needs. Specifically, 18 months after random assignment, more than half of both the treatment and control group members expressed a desire for additional support in areas such as housing assistance, job placement, job training, health services, and educational services.

These earlier reports also examined the impacts on recidivism and employment up to 18 and 30 months after random assignment. As described in those reports, treatment group members were no less likely than control group members to be re-arrested, reconvicted or re-incarcerated. There were no differences in the time to re-arrest or re-incarceration, nor did the treatment group have fewer total days

incarcerated (including time spent in both prisons and jails). In fact, treatment group members had a slightly higher total number of re-arrests and reconvictions, which may reflect enhanced case management that could have increased the likelihood of catching new offenses. However, by the seventh quarter after random assignment, treatment group members had better employment outcomes compared to the control group. By the seventh and eighth quarters, treatment group members also had higher earnings than those in the control group.

This report extends these findings by examining outcomes approximately 10 to 12 years after random assignment.

Approximately 10 years after random assignment, the treatment group did not show improved desistance compared to the control group. Treatment group members were no less likely to be re-arrested, reconvicted or re-incarcerated than control group members. Their average number of re-arrests was also similar, and they did not have substantially fewer total days incarcerated (including time in both prisons and jails). Previous reports showed that the treatment group had higher rates of re-arrest and reconvictions, possibly due to enhanced case management, which may have increased the likelihood of detecting new offenses. However, nearly 10 years out from enrollment, these differences had disappeared, and the two groups showed similar patterns of recidivism.

The employment and earnings improvements observed among the treatment group in the 30-month study did not persist in the long-term follow-up period. By the end of the 30-month follow-up, the treatment group had higher employment rates and average earnings than the control group. However, 10 to 12 years after random assignment, these differences diminished. Approximately 45 percent of both treatment and control group participants were employed at some point during the long-term follow-up period and total wages earned were similar, with no statistically significant differences between the groups.

Significant differences were observed across subgroups due to age and being male. For this long-term analysis, we compared the estimated impacts across several subgroups: males versus females, individuals 30 years of age or older versus those younger than 30, and those with lower versus higher risk of recidivism. While no impacts were found based on recidivism risk, notable findings emerged within the other two subgroups, particularly among those who were older at the time of enrollment in the study. Study participants who were 30 years of age or older at the time of random assignment were less likely than their control group counterparts to be arrested, convicted, or incarcerated during the follow-up period. They also were less likely to be arrested for violent or public order crimes, less likely to be convicted of a misdemeanor or felony, and spent fewer days incarcerated overall. Furthermore, while the findings for sex were less clear and consistent, males were less likely than females to be arrested for a violent crime or to have a felony conviction.

These consistent findings across multiple recidivism outcome measures suggest that intensive, reentry interventions, such as SCA programs, may be more effective with older participants. Additionally, there is some evidence that the program may have helped steer males away from more serious crimes. While the reasons behind the program's clear impacts on older participants and its partial effects on males remain unclear, this warrants further discussion and research.

Conclusions

In the 30-month report, we concluded that the SCA grant funds helped grantees enhance their existing programs and strengthen their partnerships. The funds were used to expand capacity, allowing for greater support to more individuals than would have been possible without the grant. These improvements to the service models developed by the grantees may have contributed to better outcomes. However, we also found that resources remained insufficient to meet all service needs. As a result, not all participants appeared to receive a full continuum of care or an adequate dosage of

services. Additionally, grantees often struggled to ensure that participants received all the services they were assigned, particularly when services were delivered by partner organizations.

In our long-term follow-up, we found that the promising effects of the program on employment and earnings observed in the 30-month report appear to have diminished over time rather than strengthened. It is difficult to determine whether important events, such as the global pandemic, which impacted the job market in various regions, contributed to these diminishing effects. However, while we observed changes in employment patterns between the pre-pandemic period (2019) and the post-pandemic years (2020 and 2021), these changes appeared to affect both the treatment and control groups similarly.

Another possible explanation for the lack of persistence in early impacts on employment and earnings is our reliance on a narrow reference period (2019 to 2021 for the survey and 2022 to 2024 for the administrative data), which limited our ability to capture a comprehensive picture of study participants experiences over time. For example, our survey findings suggest that treatment group members were significantly less likely to be incarcerated at the time of survey completion compared to control group members. Additionally, the treatment group was less likely to be incarcerated in subsequent years (2020 and 2021). We speculated that these findings might suggest that treatment group members are serving shorter sentences on average than control group members. While small sample sizes may account for the lack of statistically significant findings during the long-term follow-up period, another possible explanation is our reliance on a narrow window of time. This limited period may not fully capture the typical experiences of program participants and does not provide a full picture of their employment and recidivism outcomes over time.

Despite the lack of overall impacts on employment, earnings, arrest, conviction, and incarceration rates, the reduced rates of recidivism observed among older participants across criminal justice outcomes, as well as for violent arrests and felony convictions among male participants, are

noteworthy. While these findings should be interpreted with caution due to their exploratory nature, they suggest that the SCA program may reduce involvement with the criminal justice system for at least some participants. This is significant not only because of the harm caused by increased crime, but also due to the significant public costs associated with managing the criminal justice system. Given the substantial costs involved with criminal justice involvement, particularly incarceration, even modest reductions in recidivism are likely to generate financial benefits that outweigh program's costs (Redcross et al, 2012; Foley et al., 2018). These findings raise several important research questions that warrant additional exploration: Can these findings be replicated in other contexts? Are there ways to improve or shorten the time required to observe benefits in the relevant subgroups? Can services be adapted to better address the needs of subgroups or for areas where no impacts were observed?

Future Research

The goal of this study was to generate findings that would help policymakers at the Federal, State, and local levels design more effective programs for reintegrating former prisoners into society, increasing their employment opportunities, and reducing recidivism rate. We hope the findings from this long-term follow-up study offer valuable insights to policymakers as they evaluate different approaches to prisoner reentry and consider their potential outcomes across different populations, especially over the long term. It will be important to determine whether these impacts can be replicated in other contexts or for different groups, and whether they can be enhanced by expanding the areas where effects have been observed, shortening the time required to see impacts, and/or discovering why certain impacts only emerge or diminish over time. Likewise, further research is needed to explore why these programs may not benefit the subgroups where no impacts observed.

One recommendation for future longitudinal research is to plan such studies from the start and build in the capacity to gather data throughout the entire follow-up period. In the case of the current project, the opportunity for long-term follow-up emerged only after the initial research was completed.

As a result, the study team was unable to gather interim employment data or conduct the necessary outreach to maintain contact with participants for survey data collection. Building in periodic check-ins with evaluation participants to maintain engagement and ensure updated contact information would have substantially benefited this study. The long gaps between data collection periods required intensive efforts to locate participants, which resulted in attrition as many could not be found. Utilizing short, targeted reminders or maintaining contact with the agencies providing administrative data would likely have increased participation rates and improved the ability to capture data in later rounds of the study.

While further research is clearly needed, NORC and SPR are pleased to contribute these findings to ongoing discussions about how to best support formerly incarcerated individuals as they reenter society.

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Appendix B: Impacts on Other Participant Outcomes

The survey included multiple questions about other outcomes beyond recidivism and employment, including housing, health, and child support. About half of the respondents were currently living in their own house, apartment, or room at the time of the survey. Over two-thirds were contributing rent or to the mortgage for their housing. These rates were similar across the treatment and control groups. Respondents in the treatment group were more likely to report they were living in section 8 or public housing (18.5% program to 10.9% control); this difference is marginally significant at the p<.1 level. Twice as many program respondents reported receiving assistance from an organization to find housing (5% to 2.5%); this difference does not reach statistical significance.

Exhibit B-1: Impacts on Other Well-Being Outcomes from Survey Data

	Treatment	Control	Difference
Housing			
Currently living in own house, apartment, or room (%)	49.4	49.2	.2
Contributing to rent or mortgage (%)	72.2	69.7	2.5
Living in Section 8 or Public Housing (%)	18.5	10.9	7.6*
Received assistance from an organization to find housing (%)	5.0	2.5	2.5
Health			
Physical health is good or very good (%)	69.2	75	-5.8
Physical health limits work or other activities moderately, quite a bit, or extremely (%)	32.0	22.7	-9.3
Mental health is good or very good ($)	60.9	61.7	-.8

	Treatment	Control	Difference
Emotional problems interfere with normal work moderately, quite a bit, or extremely (%)	29.7	27.3	-2.4
Child Support			
Required to pay child support (%)	34.2	44.3	10.1
Among respondents paying child support			
Required to pay through agency enforcement	90.0	70.4	-19.6
Requirement to pay through agency affected ability to find work	22.5	18.5	-4
Average child support per child	$231	$209	22
Others helped respondent care for or support children	42.5	44.4	-1.9
Sample Size	250	128	

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

Self-reports of physical, mental, and emotional health were similar across treatment and control groups with no statistically significant differences. Respondents in the treatment group were more likely to report physical health limits work or other activities moderately, quite a bit, or extremely (32% program to 22.7% control). This difference was not statistically significant.

A higher share of respondents in the control group reported they were required to pay child support (44.3% control to 34.2% program); however, this difference was not reach statistically significant. Of those respondents, those in the treatment group are more likely to be required to pay through agency enforcement (90% to 70.4). This difference is marginally statistically significant at the p<.1 level. Both treatment and control groups reported similar rates of this requirement affecting ability to find work, average child support payments per child, and receiving help from others to care for and support children.

Appendix C: Implementing Random Assignment

This appendix describes the way random assignment was implemented. It first discusses changes to the 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 II, 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 random assignment, grantees were to provide a study orientation to applicants and obtain informed consent, and only then could they conduct random assignment.

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 treatment 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 random assignment to grantee staff and observed at least one study orientation session to be sure that procedures were being followed correctly.

Appendix D: Survey Methods

Data collection for the SCA study began with the adaptation and development of the survey instrument in the spring of 2021. The survey instrument for this longitudinal data collection was based on the survey used in the original evaluation of the SCA program (18 months after random assignment). These adaptations occurred collaboratively across the research team and with NIJ to be responsive to changing situations (e.g., family composition) of participants and wide-reaching challenges (e.g., the COVID-19 pandemic). Based on a discussion with NIJ and in thinking further about the data needed to answer the research questions, NORC decided to use two reference periods: 2019 and current (at the time of the interview). Key adaptations from the original survey included affording participants an opportunity to report on their recent experiences in more than one time period (2019 and current) to account for personal outcomes affected by COVID-19. By reporting on more than a single time period, current and pre-pandemic experiences were captured to better understand the pandemic's potential impact on respondent experiences. Overall, the survey focuses on long-term recidivism, long-term employment and income, education gains, and other measures of participant well-being, including access to housing, post-release and family support, future outlook, and other related topics. The survey instrument was programmed in Dooblo, and all modules were tested. Dooblo is a questionnaire authoring software used for mobile survey administration. NORC staff completed the survey in person or via phone using an Android tablet.

NORC administered the survey data collection for the current, longitudinal study in three phases: pilot, primary, and final phases. Details of the pilot phase are provided below. Following the pilot phase, as NORC moved forward with preparing for the primary data collection, there were several administrative delays, and a decision was made to separate the remaining participants into two groups: those with a California address and those without. The primary data collection group moved forward in

February 2022, following the early bird letter pattern described in the pilot phase below. The California group participants began data collection in June 2022.

Pilot Data Collection

Before the pilot data collection, NORC began pre-field locating the evaluation participants. First, searches on state and federal departments of corrections web pages were conducted. NORC found that 99 evaluation participants were incarcerated during the searches. NORC also conducted a batch locating search using a Lexis Nexis online locating database called Accurint. Accurint is a pay-for-use service with the functionality to conduct locating searches on a person and their relatives, neighbors, and possible associates. The batch search looked for new information for the evaluation participants and cross-checked the evaluation participant list with death records. A NORC Field Manager, Field Interviewer, and Research Associate conducted the pre-field locating searches. NORC successfully found updated contact information, primarily for evaluation participant contacts and relatives. While yielding newly found information, the individual locating searches took longer than expected. This resulted in a change to the original survey plan. NORC divided the respondents into two groups: a pilot data collection group and the main data collection group. The pilot group consisted of a random sample of 50 non-incarcerated evaluation participants. This allowed for the locating searches of the remaining participants to continue without impeding the data collection timeline.

In preparation for pilot data collection, NORC developed a field interviewer training manual, a training agenda, and training slides, and then trained the NORC staff on interview completion. The pilot data collection was staffed with one Field Manager and one Field Interviewer. Pilot data collection was carried out in several phases, starting with an Early Bird invitation letter, moving to outbound telephone calls, texts, and emails, and sending ad hoc letters.

An Early Bird invitation letter was sent on November 22, 2021. Two versions of the invitation were mailed via regular USPS mail. Invitation letters were mailed directly to 43 pilot respondents and to respondent contacts for 7 of the pilot respondents. The 43 respondents who were mailed the invitation letter had contact information that NORC was able to confirm during the pre-field locating. Letters were sent to respondent contacts when the pilot respondent's contact information could not be confirmed during the pre-field locating. In both versions of the invitation letter, the respondent was encouraged to call the project 1-800 line to complete the interview and receive a $50 thank you gift with a $25 bonus for calling in early. The incentives were provided in the form of a prepaid Visa card sent via regular USPS mail. Six pilot respondents completed the interview during the first two weeks of data collection. Two additional pilot respondents called in but did not complete the interview during the pilot. The Early Bird period ended on December 5, 2021.

The day after the close of the Early Bird period, the Field Manager and the Field Interviewer began outbound calls to the pilot respondents and their contacts. Outbound calls began on December 6, 2021, and ended on January 23, 2022. NORC made several calls to each pilot respondent and their contacts. While many contacts agreed to share NORC's message, some indicated that they were no longer in continual contact with the respondent and would pass along the message when they next saw or spoke with the respondent. NORC was cautious not to overwhelm contacts with excessive follow-up calls so that they would continue to be willing to share NORC's messages. NORC also sent text messages and emails to respondents and their contacts. This communication was facilitated by the Field Manager. Throughout the pilot, additional online locating searches were conducted for the pilot respondents and their contacts. If a new address was found for a respondent or contact during this locating effort, a letter was sent requesting that the pilot respondent call NORC to complete the interview or that the contact asks the pilot respondent to call NORC.

Pilot Data Collection Results

At the close of pilot data collection, there were 10 completed interviews; 6 were completed during the Early Bird period, and 4 were completed after the Early Bird period closed. One pilot respondent was found to be deceased, and 39 interviews remained pending.

Exhibit D-1: Case Status at the Close of Pilot Data Collection

Case Status	#
Complete	10
Out of Scope	1
Pending	39
Total	50

Of the 39 pending interviews, 7 pilot respondents were incarcerated, and one indicated that s/he did not want to complete the interview as s/he did not recall being part of the evaluation.

Exhibit D-2: Pending Case Status at the Close of Pilot Data Collection

Expanded Case Status	#
Incarcerated	7
Pending	31
Temporary Refusal	1
Locating	0
Total	39

The pilot participants who did not complete the interview during pilot data collection remained in the sample, and NORC continued to contact them during primary data collection.

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, sex 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 E: 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.

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 treatment group or control group. Random assignment, by design, enables unbiased estimates of the impact of being assigned to the treatment group by generating treatment 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 treatment and comparison groups may systematically differ in both observed and unobserved ways), which can bias impact estimates.

To verify that the treatment and control groups were statistically equivalent, means for the two groups were compared 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 treatment 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 treatment group to outcomes of the control group. In keeping with ITT, control group members could have accessed re-entry services from other sources, but could not enroll in SCA;

conversely, not all those randomly assigned to the treatment 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 having access to whatever re-entry services were normally available from other sources. The experiences of the control group provide an estimate of what would have happened to the treatment 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. 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:

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

In this equation, Group Assignment is coded 1 for those assigned to the treatment 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 sex, age, and indicators of criminal history, among others. Exhibit E-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 II were included in these regression models. Some of these characteristics were not known to be strong predictors of recidivism, were colinear 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 E-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

Variable	N	Mean	Standard Deviation
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 treatment group. Dichotomous variables in the table above were multiplied by 100, for ease of presentation. Estimates are unweighted.

Survival Analysis

Differences between group means on key recidivism outcomes 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 15 months after random assignment. At the time of the 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 III, 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 treatment 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 treatment group on the time to first arrest, conviction, and incarceration during the entire period following random assignment. A hazard ratio of 1 indicates that those in the treatment and control groups have a comparable probability of recidivism; a hazard ratio less than 1 indicates that those in the treatment 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 treatment 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 treatment 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 treatment 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.