

Evaluation of the Alameda County Justice Restoration Project

Final Report

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Executive Summary

The Alameda County District Attorney’s Office, Alameda County Administrator’s Office, La Familia Counseling Service (LFCS), Building Opportunities for Self-Sufficiency (BOSS), Reinvestment Fund, and Third Sector partnered to create and implement the Alameda County Justice Restoration Project (ACJRP). ACJRP provided services to adults aged 18 to 34 on felony probation or who had been charged with felony crimes (i.e., realigned Assembly Bill 109 (AB 109) or 1170(h) felonies). The new type of service delivery offered by ACJRP included coaching services and navigation support provided by peer coaches with similar cultural, linguistic, and socioeconomic backgrounds and similar lived experience. The overall goal of the project was to reduce recidivism by providing intensive resources such as individualized peer coaching and case management.

ACJRP was also unique in that it was a Pay for Success (PFS) project, with seed funding awarded by the Board of State and Community Corrections through a competitive grant program. A PFS project is an innovative financing mechanism that shifts financial risk from a traditional funder, usually a government, to private or foundation funders who provide upfront capital to a service provider in order to scale an evidence-based program that aims to improve outcomes for a vulnerable population. In a PFS project, if an independent evaluation demonstrates that the program achieved agreed-upon outcomes, then the investment is repaid by the traditional funder. If the evaluation shows that the program did not achieve the agreed-upon outcomes, the funder takes the loss. Because the government pays for a new program only if it meets the agreed-upon outcomes and milestones, PFS projects are an innovative new strategy to help ensure that publicly funded services produce their intended outcomes. PFS projects also build and amplify the evidence base for promising programs, thereby advancing evidence-based policymaking.

WestEd was contracted to conduct a rigorous evaluation of ACJRP that focused on both program implementation and the program’s impact on recidivism.¹ We have integrated the

¹ WestEd also conducted the independent evaluation of the recidivism rates needed to calculate the outcome payment for the PFS study; this evaluation was a subset of the larger ACJRP evaluation. The PFS outcome payment was based on the achievement of the project outcome recidivism metric, which was calculated using a different methodology than was used for this evaluation report’s impact study. Based on the calculation of this project outcome metric, there was a large reduction in recidivism rates for the completed group compared with the randomized control group. Therefore, the full \$1.37 million investment was repaid to Reinvestment Fund. The details of the PFS outcome payment are described in an August 2021 report. This report describes the larger

formal evaluation and the policy and research reflections into this report. Findings from the implementation evaluation will provide ACJRP partners and future service providers information that can support future implementation of reentry programs similar to ACJRP. The impact evaluation employed a rigorous randomized controlled trial (RCT) study design to address whether ACJRP led to decreases in recidivism outcome measures.

The PFS study was conducted from August 2018 to August 2021. A total of 450 eligible individuals were randomized into either the participant group (n = 257) or control group (n = 193) for a 12-month recruitment period from August 2018 to August 2019. Each individual was observed for 24 months, starting from their randomization date, to assess whether they recidivated, as measured by a new felony or misdemeanor arrest in the state of California and a new charge, conviction, sentence, or probation violation in Alameda County. Thus, the measurement period was from August 2018 to August 2021.

Table ES1 summarizes the various groups in the study. The sections on the impact and implementation evaluations in this executive summary provide further information about the groups.

Table ES1. Summary of Groups in the ACJRP Evaluation

Group	Definition	Strengths and Importance	Drawbacks
Participant group	Those referred to or randomized into the participant group to receive services (n = 257).	<ul style="list-style-type: none"> • Represents everyone who received the opportunity to participate in the program. • The group of interest to researchers and evaluators, as they are the best group to examine for experimental studies. 	<ul style="list-style-type: none"> • Includes everyone who was referred to ACJRP, even if they didn't enroll in the program. As such, this group may not have as much practical relevance to practitioners and decision-makers.
Ever enrolled group	Those who were referred to or randomized into the participant group and actually enrolled into ACJRP (n = 154).	<ul style="list-style-type: none"> • Includes everyone who received ACJRP services and therefore captures the reach of the ACJRP program in terms of total individuals served and all services provided. • Relevant to those who are interested in implementing a similar program. 	<ul style="list-style-type: none"> • This group includes everyone who enrolled into ACJRP, even if they didn't complete the program. ACJRP had a high attrition rate, with 79 percent of enrollees not completing the 18-month program. As such, a large portion of this group does not represent the fully designed ACJRP program experience.

ACJRP implementation and impact evaluations, for which more rigorous methodologies were used to assess recidivism impacts.

Group	Definition	Strengths and Importance	Drawbacks
Completed group	Those in the participant group who completed the 18-month ACJRP program (i.e., the ever enrolled group excluding the previously enrolled group; <i>n</i> = 33).	<ul style="list-style-type: none"> • This group represents the full ACJRP program experience. • The most relevant group to examine if interested in the effect of the full ACJRP program. • Relevant to researchers, evaluators, and those who are interested in implementing a similar program. 	<ul style="list-style-type: none"> • For ACJRP, 33 individuals completed the program, a small subset (21 percent) of all individuals who enrolled into ACJRP. • Those who completed the program may differ in unknown ways (e.g., more motivated to not recidivate) compared with those who never enrolled or enrolled but didn't complete ACJRP. As such, the results for the completed group may have limited generalizability.
Intent-to-treat (ITT) sample	A type of analysis that compares the outcomes of all individuals based on the group they were originally assigned to. For this study, the ITT analysis compared those who were assigned to or randomized into the participant group (regardless of whether they participated in ACJRP; <i>n</i> = 257) to all individuals randomized into the control group (<i>n</i> = 193).	<ul style="list-style-type: none"> • The most rigorous method used to evaluate a program, specifically using experimental methods such as RCTs. • The group of interest to researchers and evaluators, as they are the best group to examine for experimental studies. 	<ul style="list-style-type: none"> • Same drawbacks as for the ever enrolled group in that not many individuals referred to ACJRP enrolled into the program (60 percent of those who were referred to ACJRP enrolled into the program). • For ACJRP specifically, the ITT sample may not be the best test of the program because of the low enrollment and the fact that not enough services were provided to the randomized participant group.

Group	Definition	Strengths and Importance	Drawbacks
<p>Treatment-on-the-treated (TOT) sample</p>	<p>A type of analysis that compares the outcomes of individuals according to the treatment they actually received (regardless of the group they were assigned to originally). For this study, the TOT analysis compared the completed group (individuals who completed the 18-month program; $n = 33$) with a matched subset of control group individuals ($n = 33$) who were similar on background characteristics (prior criminal history, age, sex, and race/ethnicity).</p>	<ul style="list-style-type: none"> • Same strengths as the completed group. • The TOT sample includes a matched comparison group and allows researchers to use experimental methods (e.g., quasi-experimental designs, or QEDs) to examine program effects, although this research design is not as rigorous as the ITT sample used in RCTs. 	<ul style="list-style-type: none"> • Same drawbacks as for the completed group. • Due to the small TOT sample size ($n = 66$), the TOT recidivism analyses were statistically underpowered. Therefore, it is not surprising that most of the TOT results were not statistically significant.

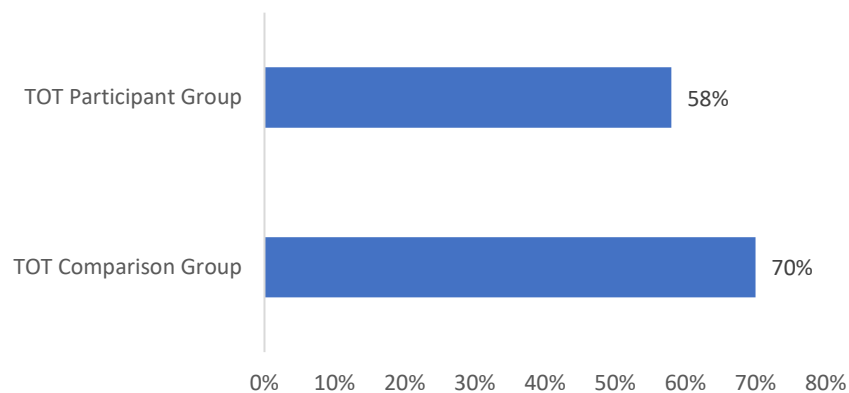
Although the impact study found both positive and negative results, these results should be interpreted alongside the data limitations of the recidivism measures (see the section “Impact Evaluation’s Data Limitations”). Additionally, results from the implementation evaluation provide rich qualitative descriptions of ACJRP outcomes beyond the recidivism measures, such as the ACJRP participants’ experiences in the program, their successes during and after program completion, and the effect the ACJRP program had on the peer coaches’ journey to rehabilitation.

Impact Evaluation

For the RCT, WestEd used an intent-to-treat (ITT) analysis that compared the randomized participant group (regardless of the extent to which they participated in ACJRP) and the control group to assess the impact of the ACJRP program on recidivism outcomes. The RCT is the most rigorous way to examine the impact of ACJRP. WestEd also used a quasi-experimental design (QED) that included a treatment-on-the-treated (TOT) analysis to compare those who

completed the 18-month ACJRP program (completed group) with a matched comparison group identified from the control group. The TOT analysis is a less rigorous approach because participants who completed the program may differ from the matched comparison group in unknown ways (e.g., be more motivated to not recidivate). The study found mixed results:

- **Statewide measures:** Completing the ACJRP program was associated to a statistically significant extent with lower recidivism. As shown in Figure ES1, 58 percent of the completed group had a new misdemeanor or felony arrest in the state over the two-year period compared with 70 percent of the matched comparison group. After accounting for background characteristics (prior criminal history, race/ethnicity, and sex), individuals who completed the 18-month ACJRP program were 65 percent less likely to have a new arrest in the state in the 24 months after being randomized into the study than were individuals in the matched comparison group. However, there were no statistically significant differences between the randomized participant and control groups' recidivism rates.
- **County measures:**
 - After accounting for background characteristics, there were no statistically significant differences between the randomized participant and control groups along two recidivism measures: new convictions and probation violations in Alameda County. There were also no statistically significant differences between the completed group and the matched comparison group on new convictions and probation violations in Alameda County.
 - For new charges and sentences (detentions and supervisions) in Alameda County, individuals in the randomized participant group were more likely to recidivate than were individuals in the randomized control group after accounting for background characteristics. In addition, the difference between the participant and control groups' risk for recidivism varied by race/ethnicity. For new charges and supervision sentences, Black, Indigenous, and people of color (BIPOC) control group individuals had the lowest risk of recidivism, followed by White participant and control group individuals; BIPOC participant group individual had the highest risk. For new detention sentences, White participant group individuals had the lowest risk of recidivism, followed by BIPOC and White control group individuals; BIPOC participant group individuals had the highest risk. The negative RCT results with the randomized participant and control groups were not found in the QED sample; there were no statistically significant differences between the completed group and the matched comparison group along those three recidivism measures.

Figure ES1. Arrest Rates by TOT Participant Group and TOT Comparison Group

Impact Evaluation's Data Limitations

WestEd strongly urges caution when interpreting the impact evaluation's results due to the low uptake of ACJRP services, specifically when interpreting the ITT results (which include the full $n = 450$ randomized sample), as the program effect may be washed out because the ITT sample also included participants who did not complete the program. Additionally, WestEd urges caution when interpreting the TOT results (which include the $n = 66$ sample with the completed group and matched comparison group). Although the completed group from the TOT sample completed the program, the findings may not be very generalizable because they represent a small portion of the individuals who were offered program services and are different from the population of offenders in Alameda County.

Regarding the ITT findings, of the 257 individuals randomized into the participant group (i.e., had the opportunity to receive ACJRP program services), 61 percent (157 individuals) ever enrolled into ACJRP and the remaining 39 percent (100 individuals) did not join ACJRP. The ITT analyses, which are the standard to conduct when evaluating an intervention with an RCT, may not be a good test of ACJRP because not enough services were provided to the randomized participant group. The group of participants served and/or completing the program was not large enough to evaluate with the RCT study design. Low enrollment among the randomized participant group could explain the null or nonsignificant results for the ITT analyses (specifically for arrests, convictions, and probation violations). That is, it is possible that there were no statistically significant findings for ACJRP along these recidivism measures because of the 257 participant group individuals in the ITT analyses, almost 40 percent of whom did not receive any ACJRP services at all.

A similar caveat is also applicable to the TOT analyses, in that the TOT sample was very small ($n = 66$ total) and therefore statistically underpowered. Statistical power is a statistical test's ability to correctly detect a statistically significant finding if an intervention truly has an effect. In

general, studies with larger sample sizes have more statistical power, whereas studies with smaller sample sizes have lower statistical power (Cohen, 1988). For this study, the TOT sample was small because it includes the 33 individuals who completed the 18-month ACJRP program and the 33 matched comparison group individuals. Therefore, it is not surprising that most of the TOT results were not statistically significant. This study's TOT analyses would need a larger sample size (i.e., a larger completed group and therefore a larger matched comparison group) in order to have more power to detect a statistically significant effect if the ACJRP program had one.

It is critical that the negative or unfavorable ACJRP results must be interpreted with data limitations in mind. The impact evaluation had two levels of recidivism measures—statewide (arrest data from the California Department of Justice [CA DOJ]) and countywide (charges, convictions, sentences, and petition violations data from Alameda County's Consolidated Records Information Management System). One of the eligibility requirements for the study was that individuals had to live in Alameda County. However, this eligibility criterion was later revoked in order to open ACJRP program services to individuals experiencing homelessness. Participant group individuals who enrolled into ACJRP inherently had to remain in Alameda County in order to participate continuously in the program, go to court visits, and receive program services. On the other hand, control group individuals did not engage in any programming and may have left the county during the study's 24-month observation period, especially given the national trend during the COVID-19 pandemic of individuals leaving areas with high housing prices, such as California's Bay Area. Control group individuals who left the county could have been recidivating outside of Alameda County, but their criminality would have been picked up only by the CA DOJ data, not by the Alameda County-specific measures.

Additionally, one must appear in court to be charged and subsequently sentenced. Court notices are typically sent via mail. **Bench warrants become an issue when individuals cannot be found because they have moved, been evicted, or are experiencing homelessness or other unstable living conditions. On the other hand, it is easier to find individuals if they are participating in a program, such as ACJRP, and are therefore more likely to appear for court visits—a common activity mentioned by both peer coaches and ACJRP participants in the interviews.** Because the participant group was more likely to stay in the county and also more likely to go to court, it is possible that they were more likely to have a new charge, sentence, or probation violation in the county. The statewide CA DOJ data circumvents the issue of losing contact with the control group. That is why CA DOJ arrests was the only outcome measure for the PFS study's outcome payment. It is noteworthy that the evaluation study's negative ITT results were found only for the county-specific measures (i.e., and not for the statewide arrest data). Readers should keep in mind that the county-specific measures may not reflect an accurate assessment of the control group's criminality if they left Alameda County and/or were less likely to appear for court notices.

The significant interaction term in the ITT analyses indicated that the difference between the participant and control groups' risk for recidivism varied by race/ethnicity; BIPOC participant group individuals had the highest recidivism rates compared with White participant and control

group individuals and BIPOC control group individuals. The history of racial and ethnic disparities in the criminal justice system have been well documented, with much research demonstrating that the disparities continue today (Hinton et al., 2018; The Sentencing Project, 2018). As a case moves through the criminal justice system, there are many decision points made by different parties (e.g., whether a law enforcement officer decides to make a formal arrest or whether a case is moved forward in court or deferred). Explicit and implicit biases can play a role at each of these decision points. The combination of potential disparities in the criminal justice system, control group individuals possibly leaving the county, and the participant group individuals being more likely to appear for court are possible explanations for the significant interaction finding.

Implementation Evaluation

WestEd used service provision data from LFCS, data collected from focus groups and interviews with peer coaches and peer coach supervisors, data collected from interviews with ACJRP participants, and other data to understand how ACJRP services were implemented. Results indicate that the ACJRP program was implemented well with the participants who completed the program. According to peer coaches, peer coach supervisors, and ACJRP participants interviewed, ACJRP appeared worthwhile in that it helped both ACJRP participants and the peer coach staff through the process of reentering their communities.

The implementation findings indicated that ACJRP provided a range of services:

- Despite delays in funding when ACJRP was first implemented—and, subsequently, a client-to-peer-coach ratio that was higher than intended—the ACJRP program enrolled 154 participants over the span of 12 months. This represented 60 percent of the 257 individuals who were randomized to receive the opportunity to enroll into ACJRP.
- Of the 154 participants who enrolled into ACJRP, 33 individuals (21 percent) completed the 18-month program, indicating challenges with retention for the long-term program.
- The majority of the ACJRP participants (approximately 75 percent) were 25 to 34 years old, male, and either Black or African American or Hispanic.
- Participants who completed the ACJRP program received on average a total of 212.9 hours of services from LFCS: 162.3 hours from direct services and 50.6 hours from indirect services or support. This 212.9 hours is equivalent to approximately 27 days of services over the span of 18 months.
- In contrast, those who enrolled into ACJRP but did not complete the program received on average a total of 55.0 hours of services from LFCS: 37.0 hours from direct services and 18.0 hours from indirect services or support. This 55.0 hours is equivalent to approximately 7 days of services. Those in the previously enrolled group were engaged in the ACJRP program for five months on average.

- For those who completed ACJRP, 74 percent of the direct service provision time was for prosocial companion and structured leisure activities, which included mentoring and the ACJRP group sessions. This aligns with the finding that peer coaching is at the heart of the ACJRP program model.
- Of the time LFCS staff spent on administrative tasks that supported those who completed ACJRP, most of the time was spent on legal support (23 percent), basic needs and social services (21 percent), prosocial companions such as family members (19 percent), housing (16 percent), and document support (10 percent).
- ACJRP participants also received supports, such as housing funds, gift cards, and coenrolled in the Reentry Employment Program, which was provided by a partnership between LFCS and BOSS and included services such as résumé building, job searches, interview preparation, and work readiness training.

The ACJRP not only provided employment supports and opportunities to the participants, it also provided employment supports and opportunities to formerly incarcerated individuals who were hired to serve as peer coaches for the participants. These supports are critical, as employment is an important factor that reduces the likelihood of recidivism (Bahr et al., 2010; Bonta & Andrews, 2017; Decker et al., 2015; Holzer et al., 2002; Lockwood et al., 2012). The findings indicate that ACJRP leveraged and trained peer coaches in the following ways:

- At the height of the ACJRP program, LFCS employed 12 staff members to support the program and its participants.
- According to peer coaches and peer coach supervisors, their responsibilities included mentoring or being role models, sharing their experiences, conducting check-ins with participants, providing on-call support, assisting with systems navigation, connecting participants with resources, and accompanying participants on court visits.
- All peer coaches and peer coach supervisors participated in a certified 10-day peer specialist credential training, which resulted in them becoming certified in Medicare, Medi-Cal, and peer support in criminal justice settings. Completing the certified peer specialist credential training demonstrates the peer coaches' expertise and can open up a wider range of future employment opportunities for them, such as working with agencies that can be reimbursed by government funders. Peer coaches also received on-the-job scaffolded peer mentoring onboarding that lasted from two to four weeks. They received additional training, including training on HIPAA, motivational interviewing, documentation, mandated reporting, de-escalation, and specific topics (e.g., reentry, suicide prevention, trauma).

Many ACJRP participants reported that their lives would have been different if they had never joined ACJRP. ACJRP showed them a way out of drugs and crime. Some participants reported that they were never ones to “plan anything” and that ACJRP provided them the scaffolding and skills they needed to identify their personal goals and make plans to meet the goals. They

valued ACJRP services and resources such as job skills training, the peer coaches with lived experiences, the ACJRP group motivational classes, financial resources to help with meeting basic needs, support for courts visits, and documentation support. **ACJRP participants' success stories include obtaining permanent housing and employment, reducing their felonies, learning lifelong coping skills, staying away from old friends, being a better person, and having more stability in their lives.** When participants were asked about areas for program improvement, they recommended clearer communication about ACJRP program completion and their deferred entry of judgement and probation status, additional support after program completion, better housing options, additional staff members for participants to contact with questions, adding more options for classes in order to accommodate more schedules, and more substance use disorder treatment.

Peer coaches and supervisors described deeply rewarding and multifaceted benefits from being a peer coach, despite experiencing challenges. **Peer coaches received certified training that will help with their future employment prospects, were motivated to continue on their rehabilitation journey, and were “walking evidence” that successful reentry is possible. The peer coach position made peer coaches see themselves differently, as role models to their clients as well as to the community. Peer coaches spoke overwhelmingly of how their work permeated their personal lives, yielding benefits such as improved relationships with friends and family as a result of their work.**

Many peer coaches and peer coach supervisors also discussed challenges such as wanting to help participants make changes before the participants were ready, managing relationship with clients, and being able to be fully “off the clock.” When asked about supports that would help peer coaches improve in their roles, individual suggestions included basic case management training, a manual of resources or strategies that peer coaches can refer to after training is completed, training on working with various populations such as the LGBTQ+ community, and training or resources to help address the stigma associated with seeking mental health services.

The report ends with a discussion on lessons learned and factors to take into consideration when implementing a program similar to ACJRP (i.e., a reentry program that leverages peer coaches with lived experiences), such as the peer coach model, training, and program retention (e.g., shortening the 18-month requirement of consistent engagement). We describe the cross-sector collaborations that facilitated the implementation of ACJRP and how the successes and partnerships from the ACJRP work have informed the development of a new diversion program in Alameda County. Finally, we end with a discussion of lessons learned regarding PFS studies and gaps in statewide criminal justice data.

Project Description

The Alameda County District Attorney’s Office (ACDAO), Alameda County Administrator’s Office, La Familia Counseling Service (LFCS), Building Opportunities for Self-Sufficiency (BOSS), Reinvestment Fund, and Third Sector partnered to create and implement the Alameda County Justice Restoration Project (ACJRP). ACJRP provided services to adults aged 18 to 34 on felony probation or who had been charged with felony crimes (i.e., realigned Assembly Bill 109 (AB 109) or 1170(h) felonies). The new type of service delivery offered by ACJRP included coaching services and navigation support provided by peer coaches with similar cultural, linguistic, and socioeconomic backgrounds and similar lived experience. The overall goal of the project was to reduce recidivism by providing intensive resources such as individualized peer coaching and case management. WestEd was contracted to provide external evaluation services of the project.

ACJRP was also unique in that it was a Pay for Success (PFS) project, with seed funding awarded by the Board of State and Community Corrections through a competitive grant program. A PFS project is an innovative financing mechanism that shifts financial risk from a traditional funder, usually a government (in this case, Alameda County under the leadership of ACDAO), to private or foundation funders (Reinvestment Fund) who provide upfront capital to a service provider (LFCS) to scale an evidence-based program in order to improve outcomes for a vulnerable population. In a PFS project, if an independent evaluation (in this case, conducted by WestEd) demonstrates that the program (ACJRP) achieved agreed-upon outcomes (i.e., reduced recidivism), then the investment is repaid by the traditional funder. If the evaluation shows that the program did not achieve the agreed-upon outcomes, the funder takes the loss. An intermediary or project manager (Third Sector) conducts, facilitates, and advises the overall process. A fiscal manager (BOSS) structures the financial deal and solicits investors to provide the up-front capital. Harvard Kennedy School of Government Performance Lab served as the government advisor early in the development of the project. Because the government pays for a new program only if it meets the agreed-upon outcomes and milestones, PFS projects are an innovative new strategy to help ensure that publicly funded services produce their intended outcomes. PFS projects also build and amplify the evidence base for promising programs, thereby advancing evidence-based policymaking.

ACJRP Theory of Change

ACJRP was designed to be a peer-based program with an individualized peer coaching model wherein individuals were paired with peer coaches who had similar lived experiences as those in the reentry population had (e.g., had experienced incarceration and/or substance abuse directly or through a family member). Having peer coaches with comparable life histories and experiences as program participants helps build trust and effective communication while also providing a role model for success (Bauldry et al., 2009). The peer coaches hired by LFCS were trained to provide intensive case management and navigation support that were tailored to the needs and goals of each participant.

ACJRP’s individualized peer coaching model was designed to address the key contributing factors to recidivism: (1) chronic unemployment and poverty; (2) substance abuse; (3) criminogenic thinking; (4) limited access to a wide array of services and supports such as subsidized housing, mental and physical health care, and education; and (5) lack of positive peer relationships and role models.

ACJRP’s theory of change is that the individualized peer coaching model promotes participants’ uptake of services that support behavioral health, physical health, employment, housing, and other domains as a result of the coaching (Bahr et al., 2010; Osher et al., 2003). The peer coaches motivate participants to use the services and help participants navigate the various service systems by using a strength-based, client-centered, trauma-informed approach that meets the participants where they are.

Individuals eligible for ACJRP can be broadly summarized as adults aged 18 to 34 who represented a subset of the AB 109² realignment population: individuals who were on felony probation at the time of study enrollment or who were charged with certain felony crimes.³ In order to enroll into ACJRP, eligible individuals voluntarily agreed to a plea bargain, which included pleading no contest to the qualifying felony conviction or felony probation violation and a deferred entry of judgment (DEOJ) granted by the judge. For the DEOJ to lead to a dismissal of the new crime or felony probation violation, the individual was required to meet both of the following two conditions: (1) successfully participate and complete the 18-month ACJRP and (2) not recidivate (i.e., be charged with a new crime in Alameda County) during the 24-month measurement period (i.e., the 24 months after being randomized into the study or, for individuals who were in custody during study randomization, the 24 months after being released). If the referred individual and their defense attorney agreed to both terms, LFCS staff then contacted the individual to complete ACJRP enrollment, which included intake and a written agreement for ACJRP participation.

² AB 109 established the California Public Safety Realignment Act of 2011, which allows for “current non-violent, non-serious, and non-sex offenders” who, after they are released from a California state prison, are to be supervised at the local county level (County of Los Angeles, 2021). The goal of AB 109 is to meet the U.S. Supreme Court order to reduce the state prison population in California.

³ See the section “Identification of Eligible Individuals” in this report for additional details on ACJRP eligibility criteria.

During intake, a peer coach supervisor conducted an initial interview and assessed the participant's strengths, needs, and risk using the Adult Needs and Strengths Assessment (ANSA; Praed Foundation, 2020), assessed their mental health with the PTSD Checklist for DSM-5 (PCL-5; Weathers et al., 2013), and conducted additional assessments as needed (e.g., assessments related to mental health, substance use disorder [SUD] needs, oral comprehension and literacy). Based on the information gathered, the peer coach supervisor or peer coach developed an individualized treatment plan for transitioning the individual back into the community. Individualized treatment plans included administering the dosage and intensity of various ACJRP services, offering navigation support for standard probation services provided as part of ACJRP, connecting the individual with housing or employment services, and establishing contact with the individual's family or other social supports.

"I didn't have anything—a birth certificate or identification. I didn't have a job. I didn't have anything. So I made goals that I was going to get all of these things—get a job, keep a job. I haven't been one to plan anything, so having another person to help you with that was helpful."

—ACJRP participant

After undergoing the enrollment process, the peer coaches carried out the process underlying ACJRP's theory of change by providing the core services of ACJRP's individualized peer coaching model, which were not a part of standard probation services. The individualized peer coaching model included the following three core services:

- **Consistent outreach and coaching** (e.g., employing motivational interviewing to connect with hard-to-reach individuals, meeting with participants in any location, meeting participants where they are at mentally, helping participants identify their own goals for reentry in various life domains);
- **Service linkage and navigation support** (e.g., helping participants navigate existing county and community services, scheduling intake appointments with referred providers, arranging transportation, attending joint meetings, consulting with providers regarding participants' needs); and
- **Intensive case management** (e.g., developing individualized treatment plans; engaging participants via phone or in person at least two times per week to provide intensive case management services [peer coaches]; and providing supports that address the key contributing factors to recidivism as appropriate [peer coaches, SUD counselors, employment counselors]).

In addition to the individualized peer coaching model’s core services, ACJRP supporting services included individual, group, and family therapy; intrinsic motivation enhancement; SUD treatment; community-based services; and additional budgeted supports. The community-based services consisted of existing resources not provided by LFCS—some of which were funded by AB 109—that were referred to ACJRP participants as part of navigation support. These services included housing resources, employment services (provided through AB 109 contracted service providers such as BOSS), educational support services (provided through partnerships with, for example, local adult schools and community colleges), public health services, behavioral health services, legal assistance services (e.g., Alameda County Clean Slate Program, Alameda County Prop 47 coordinator), and broader reentry services. LFCS also had additional supports budgeted to help participants overcome barriers to accessing supports, such as right-to-work documentation, work clothes, educational supplies, emergency food, emergency shelter/hotel vouchers, and transportation passes. The additional budgeted supports were limited and provided as needed.

“My life, looking back, could have been a lot different if I hadn’t joined that program. It was much needed for me. It helped being a tool for the community in general because I know how drugs and crime are in the area. The hardest thing is to get out of it. That was my issue. Getting in the program showed me a way.”

—ACJRP participant

Research on Peer Coaching

Definition of Peer Coaching

Mentoring is an evidence-based activity marked by a one-on-one, relationship-based connection that has been shown to be effective in positively affecting social and behavioral outcomes for people. Mentoring works through developing a trusting relationship with someone who provides consistent and nonjudgmental support and guidance (Fletcher et al., 2009). Mentoring can be a helpful part of the process of reintegrating people involved in the criminal justice system into society because it can facilitate creating an interconnected support network for them (Osher et al., 2003). Evidence shows that support from mentors helps individuals follow through on substance abuse treatment, educational and vocational programs, cognitive behavioral programs, and the pursuit of other life goals (Lockwood et al., 2012). Some formerly incarcerated individuals have resisted working with mentors because they thought that someone who had never walked in their shoes could not understand the issues they were dealing with (Fletcher et al., 2009). To help individuals who are reentering their communities overcome this barrier, the use of peer mentors or peer coaches is a recommended approach (Fletcher & Batty, 2012).

ACJRP utilized a peer coaching model in which the peer coaches were formerly incarcerated individuals who had transitioned back to society successfully. Peer coaches are mentors who are approximately the same age as their mentees, have been in a situation similar to the situation their mentees are in, and/or come from a similar background as their mentees (Finnegan et al., 2010). Peer mentorship in criminal justice settings is a newer practice that yields growing evidence of the benefits of peer mentorship for adults involved in the criminal justice system (Deville et al., 2005; Fletcher & Batty, 2012; LeBel et al., 2015; Matthews et al., 2020; Portillo et al., 2017; Umez et al., 2017). The successful integration of peer coaches in rehabilitative services is most often seen in substance use treatment (e.g., Best et al., 2011; Kulik & Shah, 2016; Riggs, 2010) and mental health treatment (e.g., Gray et al., 2017). In both settings, credentialed programs help people with lived experience transition into the workforce, specifically into jobs related to assisting others affected by similar life circumstances.

Benefits of Peer Coaching

Peer coaching is a promising approach for several reasons. Research suggests that the most important aspect of the peer coach model is the shared lived experience. For instance, participants in a jail diversion program rated shared experiences and trauma experience as the two most important characteristics of peer coaches (Clark et al., 2016). Peer coaches serve as positive models of people who have overcome challenges to reentry and achieved their reentry goals (Bauldry et al., 2009; Buck, 2021; LeBel et al., 2015; Lee, 2021; Portillo et al., 2017; Randall & Ligon, 2014). The peer mentor model is seen to be more effective than other mentor models because the mentee can identify with their mentor and see that not recidivating is possible (Hunter & Kirby, 2011). The shared experience is shown to encourage individuals to envision their own success because they see others who overcame the same obstacles (Matthews et al., 2020). Having a role model such as a peer coach is also impactful for individuals because they are able to form a deep connection with someone outside their existing social network, thereby reducing the chances of recidivism (Umez et al., 2017).

In addition to serving as a model, peer coaches are seen as trusted individuals who can connect clients to services (Lynch et al., 2018; Umez et al., 2017). Research has shown that relationship-based connective services are associated with increased positive outcomes for formerly incarcerated individuals (Bahr et al., 2010; Connolly & Granfield, 2017; Heidemann et al., 2014; LeBel, 2007; Tolan et al., 2008). Similarly, it has been demonstrated that peer coaches are most effective when the mentee feels a close connection to the peer coach (Menon & Cheung, 2018). Specifically, peer mentors may be better at engaging formerly incarcerated individuals because they share information and knowledge based on experience, having overcome their own challenges of life after incarceration (Buck, 2021; Fletcher & Batty, 2012; Hunter & Kirby, 2011; Randall & Ligon, 2014). Within the justice system, peer coaches provide formerly incarcerated individuals a unique and more relatable relationship compared with their relationship with traditional staff, whom formerly incarcerated individuals may view as

authority figures who, because they have no firsthand experience, provide irrelevant advice (Devilly et al., 2005). Functionally, peer mentors act as resource brokers, and they are positioned to use a strengths-based approach that emphasizes skill building and identifying ways of coping (Buck, 2021; Nixon, 2020). Furthermore, because of the shared experience, mentees are more likely to trust the resources and referrals the mentor advises because the mentor is seen as a credible source (Bauldry et al., 2009; Devilly et al., 2005; Hunter & Kirby, 2011; Matthews et al., 2020; Portillo et al., 2017).

“I had a therapist and a lot of other people who were really well educated, but they didn’t quite get me. . . . I knew that the people I listened to were the people I related to.”

—Peer coach supervisor

The peer coaching model is also cost-effective because alternative community interventions cost less than the per-day cost of jails (Randall & Ligon, 2014). In addition, this model can reduce staff turnover and staff costs. Hutchinson and colleagues (2006) saw that the employees in their peer intervention had more flexibility in scheduling, which increased the number of people served and increased cost-effectiveness.

Benefits for Mentees

The peer coaching model yields a number of positive outcomes for mentees. When paired with social support services, peer mentorship is shown to reduce recidivism for adult and juvenile inmates (Buck, 2021; Fletcher & Batty, 2012; Fletcher et al., 2009; Jolliffe & Farrington, 2008; Lynch et al., 2018; Randall & Ligon, 2014; Sells et al., 2020; Tolan et al., 2008; Umez et al., 2017). Further, peer interventions have been found to help individuals find employment and reintegrate into society, change their behavior, build confidence and skills, and reduce social isolation (Buck, 2021; Fletcher & Batty, 2012).

There are also several examples of quasi-experimental and randomized studies on peer mentoring interventions to reduce recidivism for adults. First, Lynch and colleagues (2018) conducted an evaluation of the Arches Transformative Mentoring program in New York City, which paired mentors with prior justice system involvement with young adult probation clients aged 16 to 24. The researchers used a quasi-experimental design (QED) with a matched comparison group, in which 279 program participants were compared to a matched comparison group of 682 people who began probation at the same time. For both groups, 85 percent were male and the majority were Non-Hispanic Black and Hispanic. The felony reconviction after 24 months was 6 percent for the program participants who

received peer mentoring compared with 14 percent in the comparison group who did not receive peer mentoring.

Second, a recent pilot randomized controlled trial (RCT) study (Sells et al., 2020) evaluated whether receiving peer mentoring in addition to standard reentry services resulted in better outcomes compared with receiving standard services alone. The researchers conducted a pilot-scale RCT with 55 men who were identified by the Connecticut Department of Correction as moderate to high risk of reoffense when they were about to be released. Though the pilot study had a small sample (38 men were assigned to the experimental group, and 17 men were assigned to the control group), the researchers found that the intervention that included peer mentors caused a statistically significant decrease in parole violations, and the odds of recidivism decreased by 5 percent for mentored clients compared with nonmentored clients.

Benefits for Peer Coaches

The peer mentor model can have a positive impact on the mentor as well (LeBel et al., 2015). The inclusion of peer coaches within the criminal justice system provides an opportunity to create jobs that are open to formerly incarcerated individuals, which could potentially invert the negative history of a criminal record into a prerequisite for hiring (Riggs, 2010). This is significant, as unemployment is one of the strongest predictors of recidivism (Bahr et al., 2010; Bonta & Andrews, 2017; Decker et al., 2015; Holzer et al., 2002; Lockwood et al., 2012). Employment is an important component of successful reentry because it provides financial stability, self-sufficiency, and the ability to meet one's basic needs of shelter, food, hygiene, and transportation.

Despite clear evidence of the importance of employment to the reentry process, the reality is that employment rates for formerly incarcerated people are 15 to 30 percent below those of the general population because employers are hesitant to hire individuals with a criminal record (Pager, 2003; Petersilia, 2005; Schmitt & Warner, 2011). This stigmatization is even more apparent for formerly incarcerated individuals who are Black or Hispanic (Lockwood et al., 2012; Pager & Western, 2009). But in a role as a peer mentor, formerly incarcerated individuals can experience less stigma; develop coping strategies, prosocial attitudes, and beliefs; gain a sense of empowerment; and build communication skills (Adair, 2005; Hunter & Kirby, 2011; LeBel et al., 2015; Lopez-Humphreys & Teater, 2020).

Challenges to Peer Coaching

Although there are demonstrated positive outcomes with peer support programs, some studies note the challenges of implementing this model. These challenges include difficulty recruiting and maintaining peer mentors, competence, confidentiality, appropriate boundaries, and the support and management required for peer mentors (Buck, 2021; Devilly et al., 2005; Fletcher & Batty, 2012; Hunter & Kirby, 2011; Lee, 2021).

Overview of the Evaluation

WestEd conducted a rigorous evaluation of ACJRP that focused on both program implementation and the program's impact on recidivism. We have integrated the formal evaluation and the policy and research reflections into this report. The implementation evaluation examines the characteristics of the ACJRP participants, the types of services provided, the order in which services were provided, standard probation services provided to the control and participant groups, the roles and responsibilities of the peer coaches, challenges to implementation, and how the challenges were overcome. Findings from the implementation evaluation will provide ACJRP partners and future service providers information that can support future implementation of reentry programs similar to ACJRP. The impact evaluation employed a rigorous RCT study design to address whether ACJRP led to decreases in recidivism outcome measures. Findings from the impact evaluation will provide ACJRP partners and others with information they can use to determine the magnitude of impact and for whom ACJRP was most successful.

WestEd also conducted the independent evaluation of the recidivism rates needed to calculate the outcome payment for the PFS study; this evaluation was a subset of the larger ACJRP evaluation. The PFS outcome payment was based on the achievement of the project outcome recidivism metric, which was calculated using a different methodology than was used for this evaluation report's impact study. Based on the calculation of this project outcome metric, there was a large reduction in recidivism rates for the completed group compared with the randomized control group. Therefore, the full \$1.37 million investment was repaid to Reinvestment Fund. The details of the PFS outcome payment are described in an August 2021 report. This report describes the larger ACJRP implementation and impact evaluations, for which more rigorous methodologies were used to assess recidivism impacts.

ACJRP was first piloted with 12 individuals from September to December 2017 (pilot period) and with another 12 individuals from March to May 2018 (ramp-up period). The pilot and ramp-up periods allowed ACDAO, LFCS, and WestEd to test all aspects of the program, including randomization into the study and enrollment into the program, before the full PFS evaluation study.

The full PFS evaluation study was conducted from August 2018 to August 2021. A total of 450 eligible individuals were randomized into either the participant or control group for a 12-month recruitment period from August 2018 to August 2019. Each individual was observed for 24 months, starting from their randomization date, to assess whether they recidivated, as measured by a new felony or misdemeanor arrest in the state of California and a new charge, conviction, sentence, or probation violation in Alameda County. Thus, the measurement period was from August 2018 to August 2021. WestEd collected and analyzed data from various sources (ACJRP service provision data, state- and county-level recidivism data, Alameda County Probation Department (ACPD) data, and qualitative data from ACJRP participants, peer coaches, and peer coach supervisors) from August 2018 to September 2022.

The implementation and impact evaluations are described in detail in the next two sections. Though the impact study found both positive and negative results, these results should be interpreted alongside the data limitations of the recidivism measures (see the section “Impact Evaluation’s Data Limitations”). Additionally, results from the implementation evaluation provide rich qualitative descriptions of ACJRP outcomes beyond the recidivism measures, such as the ACJRP participants’ experiences in the program, their successes during and after program completion, and the effect the ACJRP program had on the peer coaches’ journey to rehabilitation. The report concludes with a discussion on lessons learned and factors to take into consideration when implementing a program similar to ACJRP, such as the peer coach model, training, and program retention. Also described are the cross-sector collaborations that facilitated the implementation of ACJRP and how the successes and partnerships of the ACJRP work have informed the development of a new diversion program in Alameda County. Finally, the report ends with a discussion on lessons learned regarding PFS studies and gaps in statewide criminal justice data. Definitions of key terms for ACJRP and the evaluation study are available in Appendix A.

Implementation Evaluation

The implementation evaluation provides data about how the program was implemented, the types of services and supports provided, who received services, the standard probation services provided to the control and participant groups, the role of peer coaches in the reentry program, the factors that challenged implementation of the program, and the program successes as experienced by the participants and peer coaches. The implementation evaluation's research questions are as follows:

1. What were the number and demographic characteristics (age, sex, and race/ethnicity) of the individuals participating in ACJRP? Were there differences in demographic characteristics across the five groups that participated in the study (control group, participant group, ever enrolled group, previously enrolled group, and completed group)?
2. What were the most common ACJRP services and supports provided?
3. What was the typical order in which ACJRP services were provided?
4. What were the roles and responsibilities of the peer coaches? What training and supports did they receive?
5. What were the probation program referrals and program services provided to the control and participant groups?
6. What challenged implementation of ACJRP? How were challenges overcome?
7. What were peer coaches' and ACJRP participants' perceived successes of ACJRP?

Methods

Data Sources

The implementation evaluation included data from six sources: ACDAO's DALITE, the California Department of Justice (CA DOJ) Criminal Offender Record Information (CORI) database, service provision data from LFCS, interviews and focus groups with LFCS peer coaches and peer coach supervisors, interviews with ACJRP participants, and service provision data from ACPD. Additional detailed, technical information on the data sources can be found in Appendix B.

Data Analysis

WestEd employed content analysis to analyze the qualitative peer coach and peer coach supervisor interview (Charmaz, 2006) and employed grounded theory to analyze the focus group data (Miles et al., 2014). This analysis identified themes that emerged from groups of codes. We used a composite narrative analysis technique (Willis, 2019) for the participant interviews to create a narrative that combined the experiences of multiple interviewees into one account, allowing us to draw out themes. Quantitative data analysis for the implementation evaluation included summary statistics for describing (a) the frequency and types of probation and ACJRP services provided and (b) the characteristics of the individuals at the various points of the ACJRP program pipeline (eligible for program, program enrollment, program completion). Additional detailed, technical information on the data analysis plan can be found in Appendix B.

Results

The results that follow are organized by the implementation evaluation questions.

Characteristics of ACJRP Participants

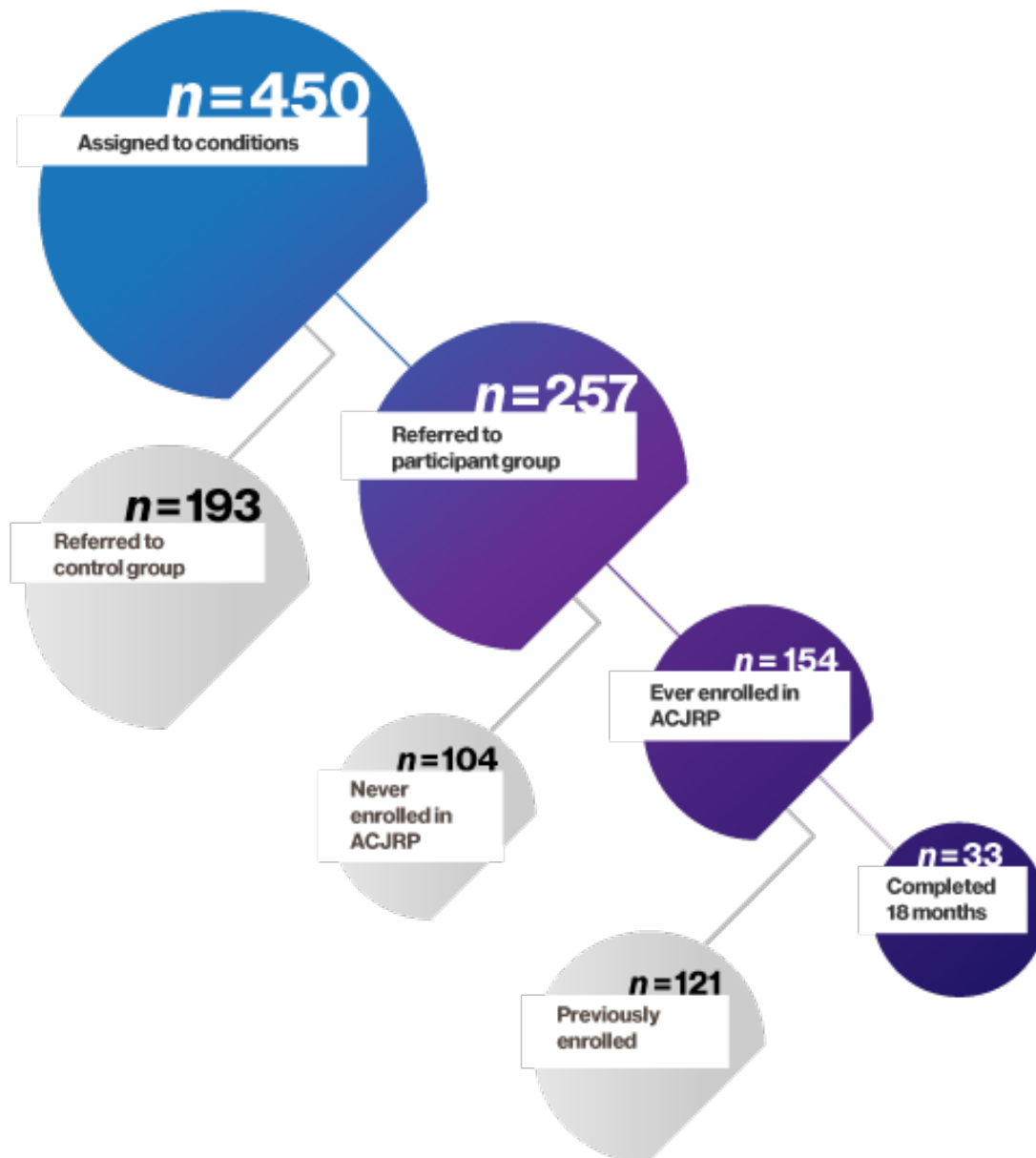
WestEd used the CA DOJ data to provide the demographic breakdown of the five groups that participated in the study (see Appendix A for detailed definitions of the groups):

- **Control group:** those referred to or randomized into the control group
- **Participant group:** those referred to or randomized into the participant group
 - **Ever enrolled:** those who had ever enrolled into the program services offered by LFCS
 - **Previously enrolled:** those who lost contact with ACJRP for 12 consecutive weeks
 - **Completed:** those who completed the 18-month ACJRP program

When examining the service provision hours, WestEd also disaggregated the results by age.

Figure 1 depicts how the groups are related to one another, with the total number of ACJRP participants through the randomization phase of the study shown at the top of the diagram and the program enrollment and completion phases shown at the bottom. There were 450 individuals enrolled in the study and randomly assigned to conditions, with 193 individuals randomly assigned to the control group and 257 individuals randomized to the participant group. Of those 257 individuals referred to the participant group, 104 never enrolled into ACJRP and 154 did enroll. Of the 154 that enrolled into ACJRP, 33 individuals successfully completed 18 months in the program and the 121 individuals who did not complete the program were considered previously enrolled.

Figure 1. Groups Participating in the ACJRP Study



In all five groups, the majority of individuals were 25 to 34 years old (Table 1). In all groups except the completed group, approximately 75 percent of the individuals were 25 to 34 years old. In the completed group, only 61 percent were 25 to 34 years old.

Table 1. Age at Randomization, Control and Participant Groups

Age	Control Group (n = 193)	Participant Group: Referred (n = 257)	Participant Group: Ever Enrolled (n = 154)	Participant Group: Previously Enrolled (n = 121)	Participant Group: Completed 18 Months (n = 33)
18 to 24 years old	25%	26%	27%	23%	39%
25 to 34 years old	75%	74%	73%	77%	61%

The majority of individuals in all groups (73 to 82 percent) were male (Table 2).

Table 2. Sex, Control and Participant Groups

Sex	Control Group (n = 193)	Participant Group: Referred (n = 257)	Participant Group: Ever Enrolled (n = 154)	Participant Group: Previously Enrolled (n = 121)	Participant Group: Completed 18 Months (n = 33)
Female	24%	20%	20%	18%	27%
Male	76%	80%	80%	82%	73%

For all groups, approximately 70 percent were either Black or African American or Hispanic, followed by White and Other Race (Table 3). The completed group had a statistically significantly (i.e., *p*-value less than .05) higher percentage of individuals of Other Race compared with the previously enrolled group (*p* = .04).

Table 3. Race, Control and Participant Groups

Race/Ethnicity	Control Group (n = 193)	Participant Group: Referred (n = 257)	Participant Group: Ever Enrolled (n = 154)	Participant Group: Previously Enrolled (n = 121)	Participant Group: Completed 18 Months (n = 33)
Black or African American	39%	34%	32%	30%	39%
Hispanic	27%	39%	40%	42%	30%
White	25%	19%	19%	21%	12%
Other Race	9%	8%	9%	7%*	18%*

*The completed group had a statistically significantly higher percentage of individuals of Other Race than the previously enrolled group ($p = .04$).

Note. Other Race includes American Indian, Asian Indian, Chinese, Filipino, Hawaiian, Korean, Other Race, Other Asian, and Vietnamese. Percentages may not sum to 100 percent due to rounding.

Common ACJRP Services and Supports Provided

Table 4 provides the amount of service hours the various participant groups received from LFCS, specifically direct face-to-face time receiving services from LFCS. Those who completed 18 months of the program received on average approximately 160 hours of direct services. In contrast, those who enrolled into ACJRP but did not complete the program received on average 37 hours of direct services. When disaggregating by age, younger participants (aged 18 to 24) received on average approximately 29 more hours of direct services than did older participants (aged 25 to 34). This is consistent with the finding that the completed group was younger than the other groups.

Table 4. Service Provision Time Participant Groups Received from LFCS (Hours)

Service	Ever Enrolled (n = 154)	Previously Enrolled (n = 121)	Completed 18 Months (n = 33)	Aged 18–24 (n = 41)	Aged 25–34 (n = 113)
Prosocial companion and structured leisure activities	55.6	31.5	120.1	71.9	49.2
Employment/workforce development	6.0	1.9	16.5	9.7	4.5
Behavioral health	4.8	2.5	11.3	4.9	4.8
Housing (retention)	1.9	0.2	6.3	0.9	2.3
Substance abuse (ACJRP session)	2.1	0.8	5.8	3.5	1.5
Education	0.7	0.1	2.4	1.0	0.6
Total	71.1	37.0	162.3	92.0	63.0

Consistent with the program’s core service to provide consistent outreach and coaching, prosocial companion and structured leisure activities made up the majority of service provision time. Table 5 further examines these types of activities. Mentoring and the ACJRP group sessions each accounted for approximately half of the prosocial companion and structured leisure activities service provision time for almost all groups. The completed group received the most service provision time, receiving on average 71 hours of mentoring and almost 50 hours of ACJRP group sessions.

Table 5. Service Provision Time Participant Groups Received from LFCS, Prosocial Companion and Structured Leisure Activities Subcategories (Hours)

Service	Ever Enrolled (n = 154)	Previously Enrolled (n = 121)	Completed 18 Months (n = 33)	Aged 18–24 (n = 41)	Aged 25–34 (n = 113)
Mentoring	30.7	15.4	70.9	37.4	28.0
ACJRP group session	24.6	15.9	48.4	34.4	20.8
Structured recreational and leisure activities	0.1	0.0	0.5	0.2	0.1
Other	0.2	0.2	0.3	0.0	0.2
Total	55.6	31.5	120.1	72.0	49.1

Figures 2 and 3 provide the same information as Tables 4 and 5, respectively, but by percentage for only the completed group.⁴ For the completed group, 74 percent of the direct service provision time was for prosocial companion and structured leisure activities (Figure 2). Most of this time was for mentoring (59 percent) and the ACJRP group sessions (40 percent) (Figure 3). This finding is in line with the fact that peer coaching is at the heart of the ACJRP model. The remaining service provision time the completed group received was for employment/workforce development (10 percent), behavioral health (7 percent), housing (4 percent), ACJRP group sessions related to substance abuse (4 percent), and education (1 percent).

⁴ The percentage breakdown of service provision time for all five participant groups (ever enrolled, previously enrolled, completed 18 months, aged 18–24, and aged 25–34) can be found in Tables C1 and C2 (Appendix C).

Figure 2. Service Provision Time the Completed Group Received from LFCS (Percentages)

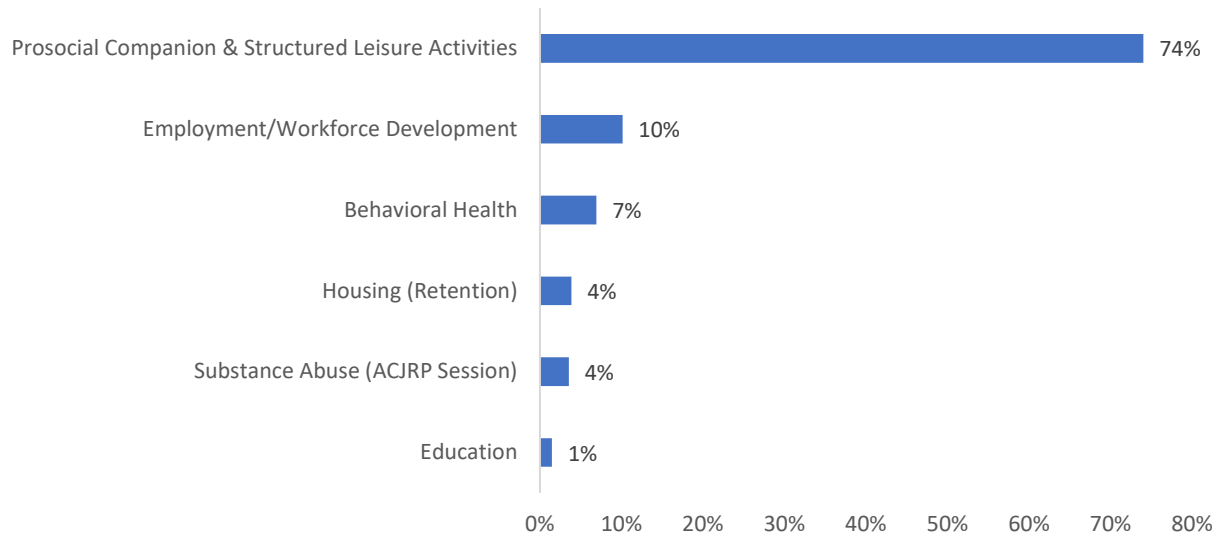
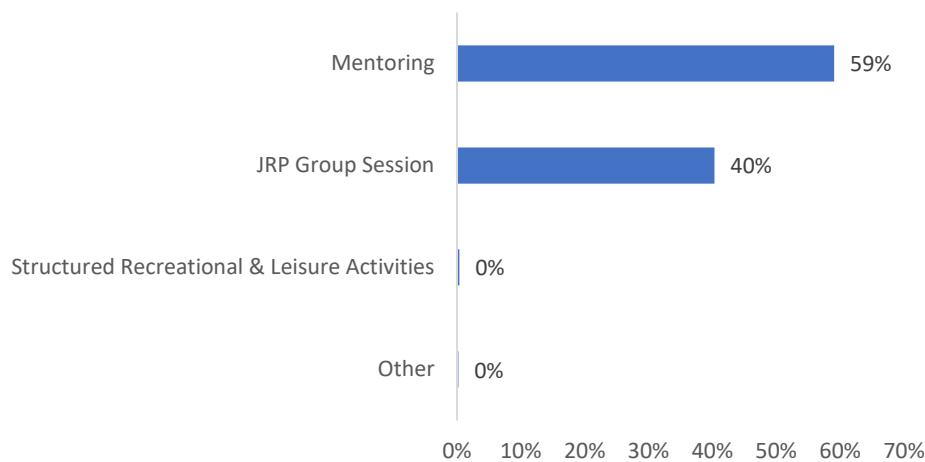


Figure 3. Prosocial Companion and Structured Leisure Activities Time the Completed Group Received from LFCS (Percentages)



Note. Percentages may not sum to 100 percent due to rounding.

Table 6 shows the time LFCS staff spent on administrative tasks that supported the participants. These tasks are the *indirect* services LFCS provided, as opposed to the direct services shown in Tables 4 and 5 and in Figures 2 and 3. For example, for participants who completed the 18-month program, LFCS spent on average 11.4 hours providing indirect legal support in the courts that did not include face-to-face time with the participants (e.g., working with judges). LFCS spent on average 10.5 hours providing indirect basic needs and social services support,

such as completing necessary administrative paperwork. LFCS staff also spent on average 9.6 hours providing indirect prosocial companion support, such as communicating with and providing support to family members and significant others of ACJRP participants. In contrast, those who enrolled into ACJRP but did not complete the program received on average 18.0 hours of indirect services.

In summary, participants who completed the 18-month program received, on average, a total of 212.9 hours of services from LFCS: 162.3 hours from direct services and 50.6 hours from indirect services or support. This 212.9 hours is equivalent to approximately 27 days of services over the span of 18 months. In contrast, those who enrolled into ACJRP but did not complete the program received, on average, a total of 55.0 hours of services from LFCS: 37.0 hours from direct services and 18.0 hours from indirect services or support. This 55.0 hours is equivalent to approximately 7 days of services. Those in the previously enrolled group were engaged in the program for five months on average.

Table 6. Time LFCS Staff Spent on Administrative Tasks Supporting Participants (Hours)

Task/Service	Ever Enrolled (n = 154)	Previously Enrolled (n = 121)	Completed 18 Months (n = 33)	Aged 18–24 (n = 41)	Aged 25–34 (n = 113)
Legal	10.3	9.9	11.4	9.7	10.5
Basic needs and social services	4.6	2.4	10.5	6.7	3.7
Prosocial companion (family)	4.7	2.8	9.6	5.3	4.5
Housing	2.6	0.6	8.0	4.7	1.8
Document support	2.3	1.2	5.2	3.2	2.0
Health	0.8	0.1	2.7	1.7	0.5
Substance abuse	1.3	0.9	2.4	0.8	1.6
Employment/workforce development (AB 109)	0.3	0.1	0.7	0.2	0.3
Financial	0.0	0.0	0.1	0.0	0.0
Total	26.9	18.0	50.6	32.3	24.9

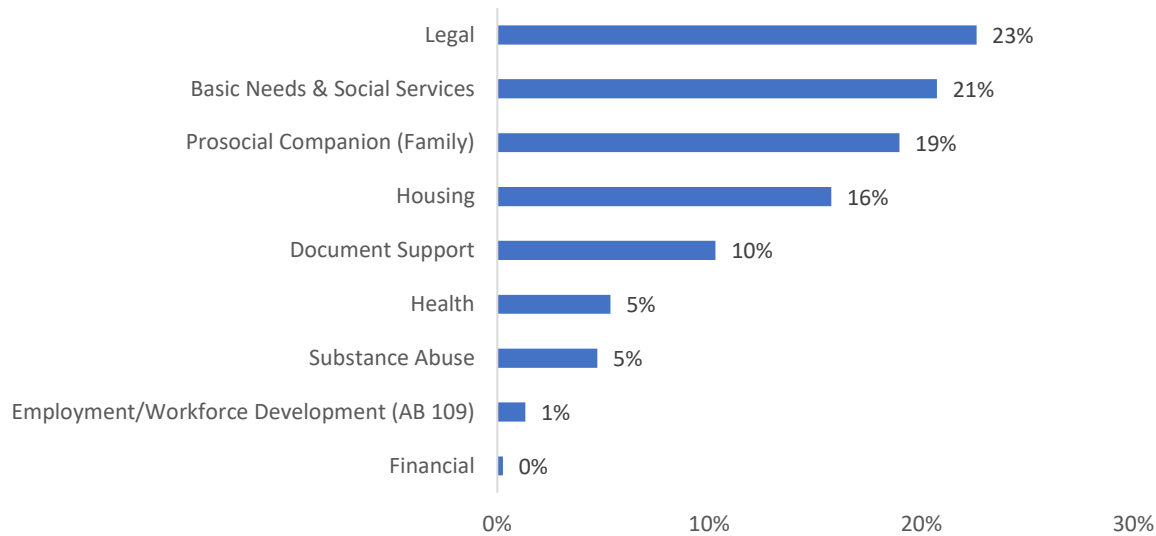
Table 7 and Figure 4 provide the same information as Table 6 but by the percentage of total time. For the completed group, 23 percent of the time was spent on legal support, whereas approximately half of the time (56 percent) was spent on legal support for the previously enrolled group. Typically, legal support was provided at the beginning of the program, during program enrollment. Thus, it is not surprising that for the previously enrolled group, a higher proportion of the time was spent on legal support, as this group did not receive the full program. In contrast, for those who completed the 18-month program, the proportion of time spent on legal support was similar to the time spent on basic needs and social services and prosocial companion supports.

Table 7. Time LFCS Staff Spent on Administrative Tasks Supporting Participants (Percentages)

Task/Service	Ever Enrolled (n = 154)	Previously Enrolled (n = 121)	Completed 18 Months (n = 33)	Aged 18–24 (n = 41)	Aged 25–34 (n = 113)
Legal	41%	56%	23%	32%	45%
Basic needs and social services	17%	14%	21%	20%	15%
Prosocial companion (family)	17%	15%	19%	17%	17%
Housing	9%	3%	16%	14%	6%
Document support	8%	6%	10%	9%	8%
Health	3%	1%	5%	5%	2%
Substance abuse	5%	5%	5%	2%	6%
Employment/workforce development (AB 109)	1%	1%	1%	1%	1%
Financial	0%	0%	0%	0%	0%

Note. Percentages may not sum to 100 percent due to rounding.

Figure 4. Time LFCS Staff Spent on Administrative Tasks Supporting the Completed Group (Percentages)



LFCS also provided various housing resources to participants. Of the 33 participants who completed the 18-month program, 12 participants (36 percent) received on average \$2,205.42 each in Kaiser housing funds. Six participants (18 percent) received an average of \$5,008.33 each in Prop 47 funds. Six participants received nine ACJRP housing vouchers total, with an average of \$639.86 housing funds per voucher.

Many ACJRP participants also coenrolled in the Reentry Employment Program (REP) that is provided in partnership between LFCS and BOSS. REP provides job readiness services such as helping individuals build their resumes, conduct job searches, prepare for interviews, and complete work readiness training. WestEd obtained REP participation data from LFCS. One important caveat regarding these data is that LFCS has REP data only for the ACJRP participants who live in south Alameda County; thus, the REP data must be interpreted with that limitation in mind. Of the 33 individuals who completed the 18-month program, 15 were from south Alameda County and were eligible to participate in REP. Of these 15 eligible participants, 10 participants (67 percent) participated in REP. On average, REP participants received \$166.00 for support services, to be used for transportation, food, or purchasing interview clothing. They also received on average 73.5 hours of job training.

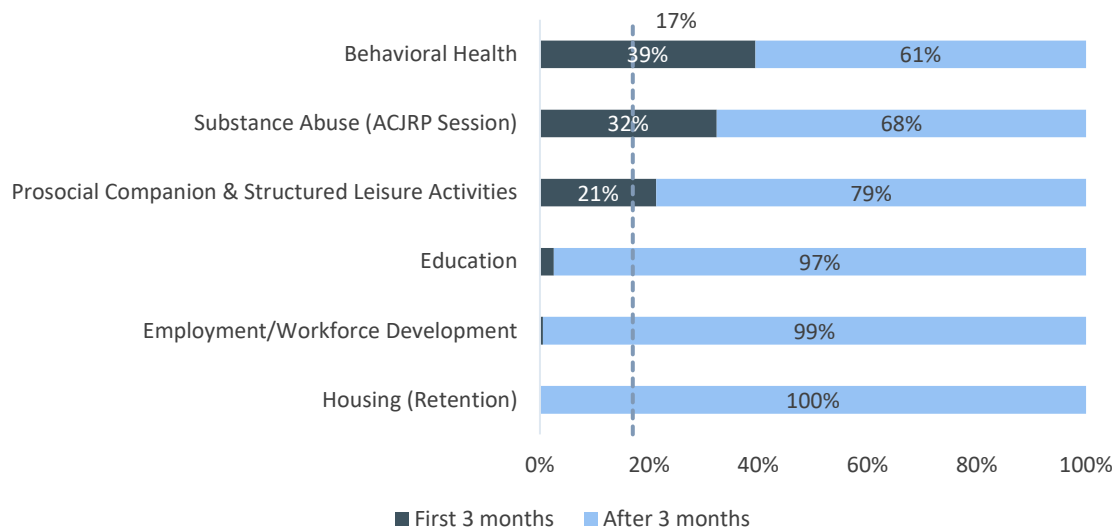
Of the 10 ACJRP members of the completed group who participated in REP, six participants (60 percent) obtained subsidized employment after completing REP. Of these six participants, half of them (50 percent) obtained a job with benefits, one participant (17 percent) obtained a job that did not have benefits, and the remaining two participants (33 percent) did not report benefits information. All six participants (100 percent) still had a job at the 180-day mark from their job start date.

Order of ACJRP Services Provided

To further understand what the implemented ACJRP program looked like, WestEd examined the order in which LFCS services were provided. For each of the service provision categories, WestEd examined the proportion of service provision time that was provided in the first three months of program enrollment compared with the proportion of service provision time provided after the first three months. For instance, if a service was provided at a fairly constant rate throughout the 18-month program, 17 percent of the service provision time would be provided in the first three months of the program ($3/18 = 0.17$) and 83 percent of the service provision time would be provided in the remaining months ($15/18 = 0.83$). Thus, 17 percent was used as a reference point to compare the proportions of service provision time.

Of the direct services that participants received from LFCS staff, behavioral health and substance abuse (ACJRP group session) services were frontloaded in the first three months of the program (Figure 5). Thirty-nine percent of all behavioral health and 32 percent of all substance abuse service provision time the completed group received was in the first three months of the program. In contrast, almost all education (97 percent), employment/workforce development (99 percent), and housing (retention) (100 percent) services were provided after three months of program enrollment.

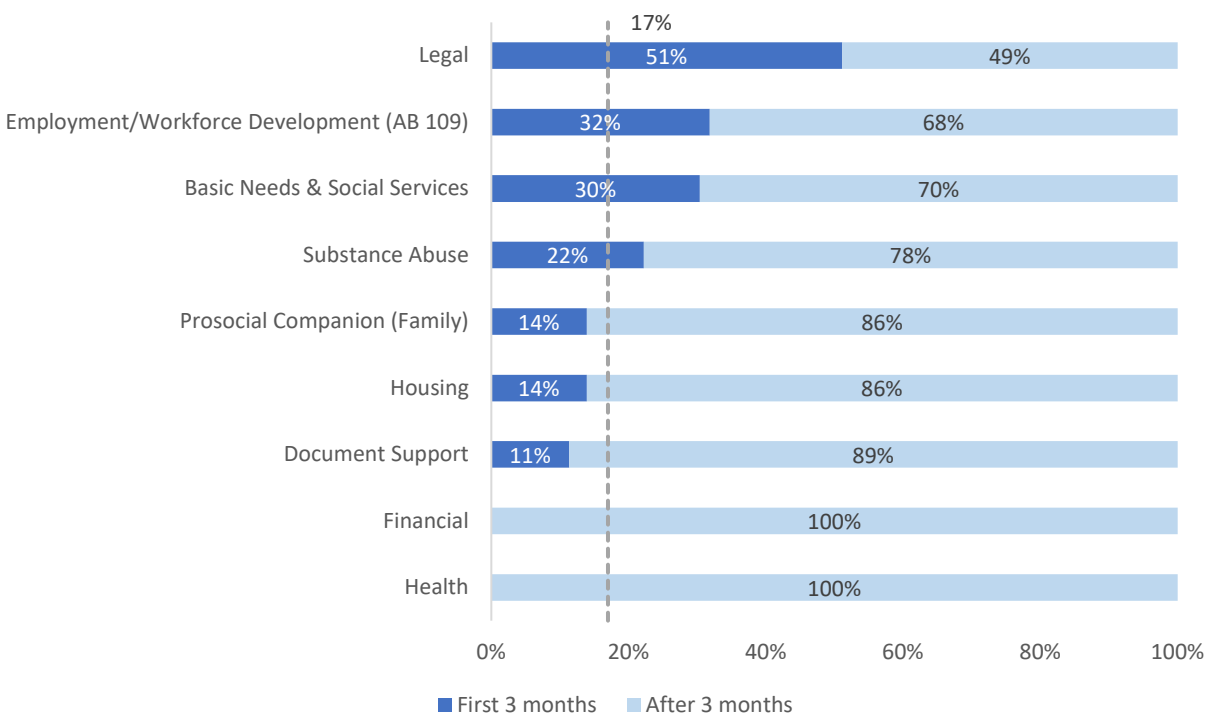
Figure 5. Proportion of Service Provision Time the Completed Group Received from LFCS Before and After Three Months of Program Enrollment



Note. The line labeled “17 percent” represents a reference point for examining the order in which services were provided. If a service was provided at a fairly constant rate throughout the 18-month program, 17 percent of the service provision would be provided in the first three months of the program ($3/18 = 0.17$) and 83 percent of the service provision time would be provided in the remaining months ($15/18 = 0.83$). A proportion of a service’s provision time that is greater than 17 percent provided in the first three months indicates that the service was frontloaded near program enrollment. Percentages may not sum to 100 percent due to rounding.

Of the time LFCS staff spent on administrative tasks that supported participants (indirect services), time spent on legal, employment/workforce development (AB 109), basic needs and social services, and substance abuse was frontloaded in the first three months of the program (Figure 6). The time LFCS staff spent providing legal support (51 percent), employment/workforce development (AB 109) (32 percent), basic needs and social services (30 percent), and substance abuse services (22 percent) to the completed group was focused in the first three months of the program. This is in line with the qualitative interview findings with peer coaches that described the legal supports they provided in the courts during program enrollment (see the section Peer Coach Roles and Trainings in this report). In contrast, the majority of document support services (89 percent) and all financial and health services (100 percent) were provided after the first three months of program enrollment.

Figure 6. Proportion of Time LFCS Staff Spent on Administrative Tasks That Supported the Completed Group Before and After Three Months of Program Enrollment



Note. The line labeled “17 percent” represents a reference point for examining the order in which services were provided. If a service was provided at a fairly constant rate throughout the 18-month program, 17 percent of the service provision would be provided in the first three months of the program ($3/18 = 0.17$) and 83 percent of the service provision time would be provided in the remaining months ($15/18 = 0.83$). A proportion of a service’s provision time that is greater than 17 percent provided in the first three months indicates that the service was frontloaded near program enrollment.

Peer Coach Roles and Training

At the time that WestEd interviewed four peer coaches, each had held their position as peer coach for an average of 9 months (the range was 3 to 12 months). The two peer supervisors had been in their position for 13 and 24 months.

Becoming a Peer Coach or Supervisor

Half of the peer coach and supervisor interviewees were former clients of another LFCS program and came into their roles by LFCS staff recommendation. Of the remaining three interviewees, two were referred to LFCS through word of mouth without having been a client first, and one was a peer coach for another LFCS program and transferred into the ACJRP program.

When asked to describe the most important skills for a peer coach to have, lived experience ($n = 5$) and interpersonal and social skills ($n = 4$) were the most frequently mentioned. Interviewees underscored that interpersonal and social skills—such as patience, being a good listener, open-mindedness, and the ability to communicate, lead, and manage people—were essential to the position.

“Once you navigate through different situations yourself, it’s much easier to help someone figure out their way.”

—Peer coach

Peer Coach Roles and Responsibilities

According to the peer coaches, most of their time was spent on mentoring, being a role model, and sharing their experiences, which is in line with peer coaching being at the heart of the ACJRP model and with the service provision data that shows that prosocial companion and structured leisure activities accounted for approximately three-quarters of the direct service provision time (Figure 2). Peer coaches explained that their activities on any given day varied, though common responsibilities included participant check-ins, on-call support, systems navigation assistance (e.g., completing necessary paperwork for obtaining an ID or food stamps, working with Child Protective Services), resource connections, and court visits. Client check-ins, conducted in person or by phone, were fundamental to the coaches’ work, as they determined the schedule of each week or even a given day. Peer coaches also frequently relied on phone calls to conduct quick status checks and determine client needs for the week.

“[Our work includes] everything from helping [clients] get an ID or Social Security card to getting housing, food, or clothing.”

—Peer coach

Echoing the findings from the peer coaches’ interviews and ACJRP service provision data, program participants shared in interviews that they regularly interacted with their peer coaches throughout the program. They reported that they started with face-to-face check-ins once or twice a week, and then, after a while, the check-ins were conducted through calls or texts several times a week. Participants shared that they would go to the LFCS office for anything they needed from their peer coach, or they would see their peer coach at the ACJRP group motivational class. Generally, participants shared that coaches helped with anything they needed.

All four peer coaches interviewed indicated that they served as on-call support for their clients. In addition to assisting clients with navigating various systems, peer coaches helped their clients access resources as requested or required. Peer coaches drove clients who did not have their own transportation, found housing for clients and their families, and served as a source of support for various situations.

“One of my clients was unfairly terminated from social services. I know that he has a learning disability and that he’s homeless. He finished his documentation showing that he needed assistance. He was cut off [from] his food stamps and his money [because] he needed to complete the quarterly QR7 form. He wasn’t familiar with it. They handed it to him and told him to complete it and turn it in. . . . He took the paper and never went back. When I went to the hearing, I advocated for him. I told the judge that any person with a learning disability won’t ask for help. They’re embarrassed that they can’t do it. There should be something in the system that offers help to fill out the form. I was able to help him get nine months of back pay. I was super excited for him.”

—Peer coach

Peer coaches and peer coach supervisors indicated in the interviews that court visits were a crucial service they provided. Staff members provided transportation to clients’ court dates, prepared progress reports for each court hearing, and advocated for their clients in court. Court visits were so important that one peer coach’s responsibilities focused largely on court visits. One peer coach recommended that an additional staff position be created to focus solely on court visits.

“It was nice to have someone there—especially when my court dates came up—to guide me through the process and just be there with me. The judge I had the check-ins with was nice, but it was also nice to have someone there also standing by your side saying, ‘Yes, this person is doing well, has the same job, and is doing what is expected of her.’”

—ACJRP participant

Peer Coach Training

All peer coaches participated in a certified 10-day peer specialist credential training that taught strategies specific to the role, addressed issues unique to the peer coach model, provided hands-on activities, and taught how to set boundaries to prevent burnout. The training focused on (1) developing peer support skills for use in the workplace, (2) the exploration and development of personal recovery, and (3) supporting individuals in recognizing their strengths, responsibilities, and accountability as certified peers. Completing the training resulted in the peer coaches becoming certified in Medicare, Medi-Cal, and peer support in criminal justice settings. The training prerequisites included a high school diploma or GED equivalent and lived experience with recovery, and a certificate was issued upon completion of the course. Completing the certified peer specialist credential training demonstrates the peer coaches’ expertise and can open up a wider range of future employment opportunities for them, such as working with agencies that can be reimbursed by government funders. Peer coaches also received additional trainings, including training on HIPAA, motivational interviewing, documentation, mandated reporting, and de-escalation. Additionally, an external trainer provided one-on-one personal peer coach trainings and trainings focused on specific topics (e.g., reentry, suicide prevention, trauma, justice reform).

LFCS also had a scaffolded peer mentoring system over two to four weeks that included onboarding new peer coaches by first having them help in the office and then shadowing or going on “ride-alongs” with other peer coaches as they worked with clients. Next, peer coaches worked on specific processes with clients (e.g., résumé writing) and then, finally, obtained a caseload of their own clients. After the initial training, peer coaches and supervisors were also encouraged to individually seek out trainings and continuing education opportunities (e.g., life coach, community, and social service certification programs).

“[The 10-day peer specialist training] gave us detailed information about being a peer specialist. I learned how to let our clients partner with us and let them come up with their own solutions and be part of the brainstorming. [Before that] I was a fixer. I would come in and ask, ‘What do you think about doing A, B, C, and D?’ It might be good, but that’s not what they wanted. It made me self-reflect.”

—Peer coach

Social Supports for Peer Coaches and Supervisors

Interviewees named social supports, specifically their coworkers and supervisor, as reliable resources when experiencing challenges. Coworkers were a major source of support, whether through weekly team meetings ($n = 5$), offering to switch incompatible or difficult clients ($n = 3$), a group text message between all agency staff ($n = 2$), or encouraging self-care ($n = 2$). Three of the four peer coaches mentioned their supervisor specifically as a source of support. One peer coach noted that she relied on her supervisor as fallback support if she was not available for a client: “[My supervisor]’s phone is on 24 hours a day. Anything I ever need, I can call [my supervisor]. [My supervisor] will make it happen.” Another peer coach noted that his supervisor was very supportive of peer coaches taking time off for self-care. Peer coaches cited weekly check-ins and one-on-one time with supervisors as important.

Suggested Improvements From Peer Coaches and Supervisors

When asked about supports that would help peer coaches improve in their roles, individual suggestions included basic case management training that covers the structure of intake and check-in meetings, a manual of resources or strategies that peer coaches can refer to after training is completed (e.g., “I’m stuck here. My client didn’t show up. What do I do?”), training on grant writing to support the agency in obtaining additional resources for the clients, training on working with various populations such as the LGBTQ+ community, and training or resources to help address the stigma associated with seeking mental health services. One peer coach noted that training specifically on how to help clients understand the benefits of working with mental health counselors would be helpful because there is often stigma around seeking mental health services.

When asked, “What do you think is important for supervisors of peer coaches to know in order to best support peer coaches?” supervisors and peer coaches suggested that supervisors should know the peer coaches’ life experiences and possible triggers in order to best match (or unmatch) peer coaches with clients ($n = 3$), be firm but humble and understanding ($n = 2$), have open communication with peer coaches ($n = 2$), meet all of the clients ($n = 1$), and ask peer coaches about their preferences regarding supervision style ($n = 1$). Additionally, one supervisor

thought that having more accountability or structure for providing critical feedback could be beneficial in pushing the peer coaches' practices.

Probation Program Referrals and Program Services Provided

WestEd examined data on ACPD program referrals and subsequent program enrollment in order to understand the business-as-usual condition (i.e., the control group condition). Being able to describe the types of probation services the control group was receiving (i.e., the counterfactual condition) helped clarify what types of services were available to individuals who were reentering the community but not participating in ACJRP and what ACJRP is being compared with. WestEd also examined the ACPD data to assess the types of ACPD program referrals and program services the participant group received. This examination provided a broader understanding of the types of services and resources the participant group received from both ACJRP and ACPD.

Approximately 15 percent of the study sample (68 of 450 individuals) received services from ACPD during the study's 24-month observation period: 32 of the 257 individuals in the participant group (12 percent) and 36 of the 193 individuals in the control group (19 percent). In total, these 68 individuals were referred to ACPD programming 86 times. There were only two cases in which the referred individual was not enrolled in the service, which means that, as a result, these 68 individuals received ACPD programming services on 84 different occasions.

The results showed that both ACPD program referrals (Table 8) and enrollments (Table 9) were the same across the participant and control groups. Overall, the services individuals were referred to and enrolled in the most frequently were employment services (35 percent of total referrals and total enrollment), education services (17 percent of total referrals and total enrollment), the reentry program (10 percent of total referrals and 11 percent of total enrollment), housing services (8 percent of total referrals and total enrollment), and client engagement/For Us By Us (FUBU) (7 percent of total referrals and total enrollment).

Table 8. Probation Program Referrals by Randomization Group (Percentages)

Program	Control Group (n = 36)	Participant Group (n = 32)	Total (n = 68)
California Department of Corrections and Rehabilitation (CDCR) transportation	5%	2%	4%
Career technical education (CTE)	0%	11%	6%
Client engagement/ For Us By Us (FUBU)	7%	7%	7%
Cognitive behavior	2%	0%	1%
Domestic violence	0%	2%	1%
Education services	20%	16%	17%
Employment services	32%	38%	35%
Family reunification	5%	7%	6%
Health services	0%	2%	1%
Housing services	12%	4%	8%
Mental health services	0%	2%	1%
Reentry court	2%	0%	1%
Reentry program	12%	9%	11%
Substance abuse counseling	2%	0%	1%

Note. Percentages may not sum to 100 percent due to rounding.

Table 9. Probation Program Services Received by Randomization Group (Percentages)

Program	Control Group (n = 36)	Participant Group (n = 32)	Total (n = 68)
California Department of Corrections and Rehabilitation (CDCR) transportation	5%	2%	4%
Career technical education (CTE)	0%	11%	6%
Client engagement/ For Us By Us (FUBU)	8%	7%	7%
Cognitive behavior	3%	0%	1%
Domestic violence	0%	2%	1%
Education services	20%	14%	17%
Employment services	30%	39%	35%
Family reunification	5%	7%	6%
Health services	0%	2%	1%
Housing services	13%	5%	8%
Mental health services	0%	2%	1%
Reentry court	3%	0%	1%
Reentry program	13%	9%	11%
Substance abuse counseling	3%	0%	1%

Note. Percentages may not sum to 100 percent due to rounding.

There were two instances in which the participant group and the control group differed regarding the number of ACPD program services referred or received. First, the largest difference between the two groups was in career technical education (CTE). Whereas 11 percent of the individuals in the participant group were referred to and enrolled in CTE services, no individuals from the control group (0 percent) were either referred to or enrolled in CTE services. Second, the analysis unearthed differences between the two groups in regard to employment services, which were by far the most common services individuals were referred to and enrolled in. A larger share of the participant group was referred to (38 percent) and enrolled in (39 percent) the services compared with the share of the control group referred to (32 percent) and enrolled in (30 percent) the services.

Nonetheless, there were no statistically significant differences between the participant and control groups in ACPD program referrals and enrollments. Specifically, chi-square tests indicated that there were no statistically significant associations between the randomization groups and ACPD program referrals: $X^2(13, N = 86) = 13.37, p = .42$. The same was true for the ACPD enrollment data; there was no statistically significant association between the randomization groups and ACPD program service enrollment: $X^2(13, N = 84) = 13.91, p = .38$.

Implementation Challenges

COVID-19 Pandemic

To better understand the impacts of the COVID-19 pandemic on the ACJRP participants and how LFCS adapted their reentry services and responded to emergent participant needs in response to the pandemic, WestEd conducted a focus group with peer coaches and their supervisors in May 2020. With public health mandates in place, all interviewees mentioned that the lack of physical, face-to-face interaction with their clients was difficult. Many of the peer coaches' responsibilities are best, and often only, practiced in person. Additionally, peer coaches' inability to see and support their clients in person was the most frequently mentioned impact of the pandemic.

In addition to lack of direct contact with clients, the pandemic affected participants' needs for support in several ways:

Employment changes and effects on basic needs and housing. While LFCS pivoted to operating remotely and peer coaches were able to retain their jobs, the same could not be said for the ACJRP participants. The majority of peer coaches spoke of their clients' loss of employment though, notably, one peer coach had a client who gained employment in an essential industry. Overall, however, there was a heightened urgency for assistance in securing basic needs such as housing, utilities, toiletries, and food for ACJRP participants who experienced job loss.

Court closures and work related to legal matters. Peer coaches emphasized the difficulties of court closures as a result of the pandemic. The pandemic's impact on this area of their work was twofold: the court closures hindered peer coaches' immediate work, and, subsequently,

their work accumulated, creating a backlog of work to catch up on when courts reopened. A peer coach supervisor described his anticipation of this backlog after courts reopening: “The challenge of thinking about the number of progress report letters—I just hope they don’t put it all on one day. We have a lot of work to look forward to.”

SUD treatment. Public health concerns during the pandemic, such as social distancing and symptom checking, changed the nature of in-person services. Peer coaches noted that the ongoing developments of best practices for in-person services and care made SUD treatment, an already complex process, more challenging.

“Another experience through Center Point and the regular process of SUD treatment—[it’s] been a challenge; everyone’s experience is different. It’s a way different process than normal. Way longer than normal. They have new standards and rules that they have to go by due to COVID-19.”

—Peer coach

LFCS used several strategies to adapt rapidly to the changing needs of ACJRP participants as best they could. First, LFCS prioritized ACJRP participants’ urgency for basic needs. In addition to continuing the distribution of LFCS’s existing services, such as providing financial assistance (e.g., retail gift cards, gas cards, transportation cards), they increased the frequency of their food drives from every other week to weekly. One peer coach and peer coach supervisor continued to provide essential field support during the shelter-in-place ordinance.

“We have clients struggling with housing, but we have a lot of resources. One client in particular was a barber. He’s not able to work due to the outbreak. He’s been reaching out to us on a regular basis. He was one of those clients that never needed anything. We were able to provide help with housing, help with toiletries. He’s in a good place now.”

—Peer coach supervisor

Another main strategy LFCS used to respond to ACJRP participants’ employment loss was to co-enroll them in LFCS’s sister program for reentry employment, REP. REP aims to support the successful and sustainable transition of formerly incarcerated individuals back into the community by working with them on building résumés, conducting job searches, completing job applications, preparing for job interviews, and obtaining work-ready clothing.

“Luckily, REP has been able to coenroll ACJRP participants who have lost their jobs. I’ve been able to coenroll one of my clients. As soon as the shelter-in-place started, my client lost her job. She’s been working with [REP]. They’re doing an amazing job. ACJRP participants are finding employment during the shelter-in-place.”

—Peer coach

LFCS also adapted to the pandemic by reducing in-person interaction, instead making use of other methods to conduct program activities. For example, to work on required forms and obtain participant signatures, LFCS quickly shifted to using a mail-in order form to continue to aid participants in securing food support services.

To mitigate the restrictions of the public health mandates, interviewees explained that their work relied increasingly on social media and digital communication services such as Instagram Live and Zoom. ACJRP quickly pivoted to hosting group motivational sessions on Instagram Live, a feature on the popular mobile app Instagram that allows users to livestream and engage with viewers via chat. The classes held on Instagram Live were archived on LFCS’s Instagram account, allowing participants who could not attend to review the classes in their own time. In interviews, program participants said the classes were still beneficial despite being held over Instagram Live and that they still wanted to attend classes. In addition, the conferencing application Zoom enabled peer coaches to meet remotely for staff meetings. Though LFCS put in incredible effort to provide timely services online via digital tools, ACJRP participants experiencing homelessness or those without access to digital devices or a stable internet connection may not have been able to access the same services.

LFCS staff also developed “homework assignments” for ACJRP participants as another strategy for providing coaching remotely. These assignments were take-home worksheets with questions that prompted participants to reflect on their experience as they sheltered in place and to make plans for themselves. Plans were designed based on their responses to the questions in the assignments, which touched on topics such as mitigating substance use and developing and meeting career goals.

“Making plans was something that was brought out of her [participant and] that [never came up during our discussions]. The homework assignment has been very helpful. With the shelter-in-place, all they have is time on their hands. They can realize and help with what’s going on.”

—Peer coach

Lastly, there were some unexpected positive outcomes of LFCS pivoting in response to the pandemic. Although the shelter-in-place ordinance forced peer coaches to utilize new tools and platforms, they saw having to do so as a silver lining and wanted to continue to use Instagram and Zoom after the pandemic. Peer coaches viewed the new tools as providing opportunities to be more accessible to ACJRP participants who have busier schedules. They also hoped to continue to use Zoom occasionally after the pandemic because not having to travel better accommodates staff schedules.

Despite the pandemic's impact on in-person work with the ACJRP participants, peer coaches also noted that their frequency of engagement with participants increased. With COVID-19 information changing rapidly at the city, county, state, and federal levels, the peer coach-participant relationship became more critical than ever, with participants relying on peer coaches to keep them updated about changes to the shelter-in-place ordinance and resources. ACJRP participants who were experiencing homelessness increased their reliance on peer coaches for information and support, potentially due to lack of social services or outreach.

"I had clients that weren't keeping in contact, not nearly as much as they do now. They depend on us way more for information. Everyone is in crisis mode. I have homeless clients who really be needing stuff. It's for sure making them call more. That's a bright side to me."

—Peer coach

Challenges Faced by Participants

Though the ACJRP participants interviewed described mainly positive experiences while in the program, they also shared some negative experiences and recommendations for improvements. Among those recommendations were the following:

Communicate more clearly about program completion. Two of the four interview participants who completed the 18-month ACJRP program noted that they were surprised when they finished the program, that the peer coaches suddenly told them they had met all the requirements for completion. Participants may be better able to prepare for transitioning out of the program if they have a way to track their progress in meeting the program requirements.

Communicate more clearly about DEOJ and probation status. Two of the four interview participants noted that they did not know the status of their probation after they had completed the program or whether the crime they had been arrested for was still on their record.

Provide transitional support. Multiple participants described a lack of support after program graduation. They noted that peer coaches had communicated poorly toward the end of the program or did not respond when contacted, although participants were told they were allowed to keep contact with peer coaches. One participant noted that they missed their court date shortly after program graduation due to the lack of support.

Improve the quality of vet housing. One participant noted that the ACJRP program provided housing but that it was in a high-traffic drug area, making it difficult for those who were recovering from an SUD. Though the participant said that the peer coaches made sure she did not suffer from SUD issues before placing her, she suggested that safer housing options were needed, especially for younger female ACJRP participants.

Provide participants with a second contact to reach out to. Some participants suggested having a second peer coach or additional ACJRP staff to contact for more information. Participants noted that peer coaches may have been overwhelmed at times, during which they were not as helpful as they had been, or that access to an LFCS administrative staff member would have been another useful resource.

Add more class options. Participants sometimes had difficulty attending the ACJRP group motivational class because of the traveling distance or because the classes were offered only once a week. One participant suggested offering more class times throughout the week so they could attend.

Place more emphasis on SUD treatment. Interviewees described seeing other ACJRP participants struggling with SUD issues and suggested that this was a reason some other participants did not successfully complete ACJRP.

Challenges Faced by Peer Coaches

Challenges peer coaches faced in their roles were like those in other occupations related to working with incarcerated individuals reentering their communities, particularly when individuals do not want to or are unready to make changes. The most common challenges discussed included lack of client readiness to make changes ($n = 4$), client disappearance ($n = 3$), client recidivism ($n = 3$), managing relationships with clients ($n = 3$), and lack of resources for clients ($n = 2$).

Two peer coaches and both supervisors noted their struggle with their desire to “fix” the clients’ situations rather than allowing clients to take ownership of their growth process. The peer specialist training taught peer coaches not to “tell” clients what to do but instead to make suggestions and empower client agency.

Three of the four peer coaches also discussed the challenge of never being fully “off the clock.” Clients contacted the peer coaches at all hours of the day, and peer coaches continued to think about work during off hours.

“They always say if we need a day off, we could take a day off. But you don’t wanna take a day off; you’re gonna fall behind on your clients. I wanna be the best person I can be, so I kept my phone on 24 hours. But I learned from a training, you got to turn your phone off at 5 p.m. I asked, ‘But what if it’s an emergency?’ They said, ‘They could call 911.’ I learned to cut off that phone at 5 p.m.”

—Peer coach

LFCS also described delays in funding when ACJRP was first implemented. They did not have the full resources and were not able to hire as many staff at the beginning of program implementation, resulting in a higher client-to-peer-coach ratio than their goal of 14:1.

Implementation Successes

There were many successes with the implementation of the program. Participants received an array of services and overall had a positive perception of the services they received. In addition, peer coaches reported that their job positively affected them.

Participant Experience

As described in detail in Appendix B, WestEd conducted interviews with four individuals who completed ACJRP. The interview data were transcribed and analyzed using the composite narrative methodology, wherein several individual interviews are combined to tell a single story. The composite narrative methodology allows WestEd to convey the richness of each individual participant’s account while also maintaining the participant’s confidentiality. The resulting composite narrative that follows is based solely on interview data, and all quotations come directly from interviews. The only change has been presenting data from several interviews as if they came from a single individual. The narrative describes in their words how the participants experienced the various phases of the ACJRP program (e.g., enrollment, working with peer coaches, the ACJRP group classes, program completion) and their successes with ACJRP.

Composite Narrative of ACJRP Participants

Juan is a 22 year-old man who was born and raised in the San Francisco Bay Area. He helps support his children and is the primary caretaker for his father. Juan was an ACJRP participant from May 2018 to November 2019, and he completed the program.

Prior to ACJRP, I was in a bad place. I was in and out of jail and on probation for a few years, going back to the same stuff. I didn't have a job or a consistent place to live. I struggled with my mental health. I was in court, about to go to jail again, when my public defender told me about ACJRP. He explained that the program could help people stop committing more crimes by giving them resources. He told me if I agreed to the program, I could get out of jail. If I completed the program, the court would reduce my felony to a misdemeanor. ACJRP came at the right time and pretty much saved me. I wanted to change my life—be a better person, a better father. I was tired of being in and out of jail. I didn't want to stay in a bad place. I just needed help. What sounded the best was help finding housing and work—also the fact that it was a paid program.

Enrollment was easy. After I agreed to participate, a La Familia staff member walked in and clearly explained the program expectations and requirements about face-to-face contact, phone calls, texts, and staying out of trouble for two years. I filled out the paperwork, then went to the judge. I felt like I had enough time and information to make a decision about enrolling.

Right at the beginning of the program, I met with La Familia staff to make short-term and long-term goals. They asked me questions to help figure out my goals. My goals included getting a stable job, going back to school, making sure I had stable housing, and helping my family. I definitely wanted to stay out of jail too. Overall, I just wanted to be a better person. Thinking back, making goals was a key part of my success in the program. Based on my goals, they structured the program to make sure I had what I needed.

I got help with anything I needed, regardless of what it was. They gave me some job skills training, helped with my résumé and finding a job. The job preparation courses were free. They paid us for participating. They also connected me with someone to talk to for therapy. Throughout the program, they helped financially too—rent money, stipends, and gift cards for gas and groceries. They even offered professional clothing if I needed it. All the financial support, especially the stipend, showed that they recognized that we were trying.

La Familia staff made me very comfortable. They were good at answering the phones or texts, being there when I needed them. I was paired with a peer coach and had regular check-ins with them. My peer coach did a fantastic job. It surpassed my expectations—they were kind, helpful, and we had a good connection. They were also accommodating to things going on in my personal life, especially when I had stuff with my family—as long as I kept doing a check-in call.

When I checked in, my peer coach would ask me how I'm doing, how my job was, and how I was emotionally. I could go to my peer coach for help with anything and they would go out of their way to help me if I needed it. They helped me find a place to live, get my ID and Social Security card, complete a food stamps application, and they provided information I needed. They would also help with goal setting if I needed it and check in on the progress. They also gave me emotional support and helped me open up despite the trauma I have in my past.

My peer coach helped a lot with court too. They reminded me of court dates, met me at court, offered rides if I needed them, checked in with me at court, and provided progress reports to the court staff. I really appreciated them guiding me through the court process, especially having someone else vouching for me in the court room and saying that I'm doing well and how I'm trying to turn my life around. It was really nice to know that someone was there for you and reminded you that they were there for you.

My peer coach was a role model to me. I was receptive to my coach's advice because they know both sides of the criminal justice system. Even though we hadn't been through the same things, it was easier for me to listen to them rather than to a person who doesn't know what they're talking about. Having a peer coach with their own experience being in jail opened my eyes and helped me reframe how I viewed myself. I saw my peer coach and thought, I need to be like them, be sober, be around others. It helped me turn my life around and get on the path to choosing something different.

Another requirement of the program was to attend this weekly group class led by Kevin Grant. It was like group therapy and motivational speaking—kind of like a Narcotics Anonymous class. The talks were informational and inspirational. We learned about the criminal justice system, and Kevin would lead us through different scenarios that come up in life that could be difficult for us to get through. It was a safe space for us to talk, which isn't normal for people who come from my background. Even my family came to some of the group meetings. Them attending was actually more helpful because it wasn't just me there; they pushed me to keep going.

Overall, meeting the program requirements was easy for me. I had to stay on schedule with check-ins with my peer coach and go to my court dates. It was flexible to my life circumstances. I had trouble getting to the classes after work because of my schedule, so I couldn't go as often for a while. La Familia understood that work was a priority, and they were able to adjust the requirements as needed. But when they got a second La Familia office location, that helped so I could go more often.

The two years went by quickly. Graduating from ACJRP felt good. I got a gift card, and my peer coach and I talked about the goals I set, the goals I completed, and the ones I need to continue to work on. I didn't expect this, but I got a certificate from La Familia for graduating, which was cool. I was told that I can continue to attend the group meetings, but I can't use other services. I still keep in touch with my peer coach.

I absolutely recommend ACJRP to other people. Being able to reduce my felony was so helpful. ACJRP gives you a second chance, which helps a lot in life. The classes, the gift cards, and the peer coaches were on point. I matured in the program and learned lifelong coping skills to get through everyday life situations. To be honest with you, if I didn't do ACJRP I would probably be homeless or still in jail. But I was able to move out of temporary housing and have stability all the way until now, running my own business and being a better person. I worked hard to be in this good position. I kept myself away from old friends. I'm off probation now. My success is that I'm happy with where I'm at. My mind is stronger with ACJRP's help—I made goals and I met them.

Flexibility of the Program

Unprompted, three out of the four participants mentioned that the flexibility of the ACJRP program helped facilitate their success. For instance, participants with other life circumstances—such as conflicting work schedules or caring for a dependent adult—initially could not make it to court dates or the weekly motivational classes. However, the peer coaches were flexible and understood that obtaining and maintaining employment was important to the participants. Peer coaches worked with participants to develop a plan so that they could succeed both inside and outside of ACJRP.

“When I explained to my peer coach what I had going on at home, he became more involved in my life and helped me through it. My mom got breast cancer, and he helped me through it with my court dates and everything too. He put court dates in times when there wasn't going to be too many doctors' appointments. The whole goal was getting off probation, and it happened.”

—ACJRP participant

Peer Coaches' Successes

Although the ACJRP program and evaluation were designed to focus on program participants, ACJRP also had an impact on the peer coaches. Peer coaches received certified training that will help with their future employment prospects, served as role models for the community, were motivated to continue on their rehabilitation journey, and were “walking evidence” that successful reentry is possible. Similar to the ACJRP participants, peer coaches were still working on their own rehabilitation journeys, and employment as a peer coach and the certified training helped them on their journey.

Despite experiencing challenges, peer coaches and supervisors described deeply rewarding and multifaceted benefits of being a peer coach. Almost all interviewees (five of six) named client growth as the biggest reward of being a peer coach or supervisor. Peer coaches and supervisors noted that any movement, big or small, was progress forward.

“I have a homeless family. . . . I found them a place to live. When their son came in and was jumping around yelling, ‘This is my room!’ that made me feel good.”

—Peer coach

Peer coaches spoke overwhelmingly of how their work permeated their personal lives. All four peer coaches described experiencing personal growth, and three of the four peer coaches noted that their relationships with friends and family improved as a result of their work. One supervisor described his role as being an “honor” outside of work: “From the outside looking in, my mom’s proud of me. My family is really proud of me. It’s a good thing.” The peer coaches expressed how their job as a peer coach made them see themselves differently as a role model, knowing that their clients were depending on them.

“Since I been a peer coach, I been a better son, a better father. It brought a lot of understanding to my life, seeing other people’s issues. As a role model, it makes me walk a better line. I can’t do nothing stupid; I can’t mess up. I got 35 clients depending on me. . . . I see my community too. They say, ‘I seen you in court. You working with the DA [district attorney]? You working in court? YOU?!’ [Comedic disbelief.] It’s good for them to see an average street guy, not being no snitch. . . . I keep my work badge on; I keep my ACJRP shirt on. It’s like being a role model for the community.”

—Peer coach

Two peer coaches stated that being a role model in their community was another important reward of their work. They felt that their role and being able to give back to their community served as “walking evidence” that change is possible. One peer coach used to struggle with substance use; she explained that in her role as a peer coach, she runs into individuals who knew her from her past life. These individuals knew of the transformation she had achieved.

“I’m a real-life walking miracle. I made it. So can you. . . . I still have lots of friends and family struggling with substance abuse and incarceration, so when they see me working with DAs in the courts, they’re just like, ‘Oh my God.’ There’s even people I know who are enrolled in ACJRP. . . . They see how well you’re doing. It’s emotional. That’s the best experience for me.”

—Peer coach

Impact Evaluation

To assess the impact of ACJRP, WestEd utilized an RCT design. Individuals eligible for the study were randomly assigned to either the participant group (i.e., they received the opportunity to participate in the ACJRP programming) or the control group (i.e., business-as-usual condition). The RCT design was chosen because it is the most rigorous of designs. RCTs improve internal validity (Shadish et al., 2002). Specifically, when randomization is implemented well, the resulting participant and control groups are similar at baseline prior to the start of services for the participant group. Thus, changes seen at follow-up or after randomization are attributable to the participant group participating in the intervention (in this case, ACJRP). WestEd conducted baseline equivalence testing to ensure similarity between the participant and control groups.

The impact evaluation's research questions ask about the effectiveness of ACJRP in reducing recidivism:

1. Do individuals who participated in ACJRP recidivate less compared to control group individuals (as measured by a new felony or misdemeanor arrest in the state of California and a new charge, conviction, sentence, or probation violation in Alameda County)? How did this vary by age, gender, and race/ethnicity?
2. Do individuals who successfully completed the 18-month ACJRP program recidivate less compared to individuals in a matched comparison group (as measured by a new felony or misdemeanor arrest in the state of California and a new charge, conviction, sentence, or probation violation in Alameda County)?⁵

Methods

Study Design

The ACJRP evaluation relies on an RCT design to assess program impacts. Based on power analyses that estimated the sample size needed to identify statistically significant differences, the targeted sample size for the evaluation design was projected to be 450 individuals

⁵ Variation by age, gender, and race/ethnicity was not examined in the treatment-on-the-treated (TOT) analyses due to the small sample size (33 enrolled group individuals who completed the 18-month ACJRP program and 33 matched comparison group individuals).

randomized into either the participant or control group. One of the original study eligibility criteria was that individuals had to live in Alameda County. During the recruitment stage of the study, LFCS observed that this eligibility criterion excluded individuals experiencing homelessness, thereby preventing this population from accessing the available program services. The project partners later revoked this eligibility criterion to expand the eligibility pool, especially given the housing crisis in the Bay Area and California at large. By the end of the 12-month recruitment period from August 2018 to August 2019, a total of 450 individuals were recruited and randomized into the study.

Identification of Eligible Individuals

Individuals eligible for randomization can be broadly summarized as adults aged 18 to 34 who represent a subset of the AB 109 realignment population—that is, individuals who were on felony probation at the time of study enrollment or who have been charged with certain felony crimes. Specifically, individuals were eligible to participate in the study and receive ACJRP services if they met all of the following criteria:

- Charged with, or convicted of, an AB 109 1170(h) felony and/or a felony probation violation based on a new crime rather than on a technical violation. The felony probation could not be for a strike offense, and the new violation must have been identified by AB 109 as a PC 1170(h) offense.
- Previously convicted of a nonstrike felony at least once. To focus on recidivism and to increase eligibility, the qualifying previous conviction need not currently count as a “felony prior,” so reduced felony priors of all kinds, including juvenile felony priors, were allowed. A strike prior conviction (a violent or serious felony as defined in PC 667.5 and 1192.7) or a prior conviction resulting in Penal Code Section 290 registration requirements, disqualified individuals from study eligibility.
- Between 18 and 34 years old at the time their eligibility was reviewed.
- Live in Alameda County.⁶

Randomization Process

Randomization of eligible individuals into either the participant or control group was conducted on a rolling basis from August 2018 to August 2019 to accommodate the continuous nature of enrollment. WestEd conducted the randomization process independently. At the beginning of the study, WestEd utilized a 1:1 ratio for randomization (i.e., one individual randomized to the participant group for every individual randomized to the control group). In February 2019, the randomization ratio was updated to 2:1 (i.e., two individuals randomized to the participant group for every individual randomized to the control group) in order to enroll and provide ACJRP services to more individuals. Detailed information on the randomization process can be found in Appendix B.

⁶ This eligibility criterion was later revoked in order to open ACJRP program services to individuals experiencing homelessness.

Consent

After ACDAO received from WestEd the list of individuals who were randomized into the participant group, ACDAO notified the relevant district attorneys of the individuals referred to the participant group so that the district attorneys could obtain their consent for ACJRP enrollment. Simultaneously, ACDAO also notified LFCS staff of the names and court schedules of individuals who were randomized into the participant group in order to optimize intake and enrollment. LFCS staff attending court notified the relevant defense attorneys of the client eligibility.

During the consent process, the defense attorneys or district attorneys explained to individuals who were randomized into the participant group that if they chose to enroll into ACJRP, they must voluntarily agree to a plea bargain, which included pleading no contest to the qualifying felony conviction or felony probation violation and a DEOJ granted by the judge. As described in the section “ACJRP Program Description and Theory of Change,” for the DEOJ to lead to a dismissal of the new crime or felony probation violation, the individual was required to meet both of the following two conditions: (1) successfully participate and complete the 18-month ACJRP and (2) not recidivate (i.e., be charged with a new crime in Alameda County) during the 24-month measurement period. If the referred individual and their defense attorney agreed to both terms, LFCS staff then contacted the individual to complete ACJRP enrollment, which included intake and a written agreement for ACJRP participation.

The consent process typically took place while individuals were in court or in custody at one of the county or city jails, though LFCS staff could also contact individuals out of custody after, or away, from court. Individuals randomized into the control group were not approached for consent, as they were in the business-as-usual condition.

Sample

A total of 467 individuals were assessed for eligibility, but 17 individuals did not meet the inclusion criteria and were excluded from the study before randomization (Appendix D, Figure D1). Thus, 450 individuals were eligible for random assignment. A total of 257 individuals (57 percent) were assigned to the participant group, and 193 individuals (43 percent) were assigned to the control group—reflecting that the randomization ratio (participant group:control group) was updated from 1:1 to 2:1 in the middle of the study’s recruitment period. Of the 257 individuals who were randomized into the participant group, 154 individuals (60 percent) enrolled into ACJRP services. Of the 154 individuals who enrolled into ACJRP, 33 individuals (21 percent) completed the 18-month program. Of the remaining enrolled 121 individuals who did not complete the 18-month program, two individuals decided immediately after enrollment to serve time instead and 119 individuals disengaged from the program (i.e., had no contact with ACJRP for 12 consecutive weeks).

Identification of Matched Comparison Groups

WestEd conducted a quasi-experimental treatment-on-the-treated (TOT) analysis (i.e., individuals who successfully completed the 18-month program) to investigate the impact of

receiving the full suite of ACJRP services. WestEd identified the participant group individuals who completed the program, and then compared this group to a matched subsample of individuals selected from the control group. As such, 33 participant group individuals completed the 18-month program and were in the TOT sample (referred to as “TOT participant group” from this point forward). Propensity score matching with replacement (Guo & Fraser, 2010) was conducted three times: once for the CA DOJ arrest analyses, once for the Alameda County convictions analyses, and once for the remaining Alameda County recidivism measures (charges, sentences, and probation violations). WestEd conducted the matching separately because the three outcomes had different sample sizes due to varying missing outcome data. Following suggestions from the What Works Clearinghouse (WWC), if there are multiple analysis samples, baseline equivalence must be established for each analysis sample (U.S. Department of Education, 2020). Propensity score matching with replacement allows a control group individual to be matched multiple times (i.e., with multiple different enrolled group individuals). Thus, WestEd used weights for the baseline equivalence tests and impact analyses, with the comparison group individuals weighted by the number of times they were matched to the TOT participant group.

After matching, a subset of individuals from the control group (referred to as the “TOT comparison group” from this point forward) was selected to match the TOT participant group individuals. The resulting analysis is quasi-experimental in nature, as matching only creates groups that are similar on known characteristics. After matching, the TOT analysis included 33 participant group individuals and 33 control group individuals (a total of 66 individuals) for all three outcomes (arrests, convictions, and the remaining Alameda County recidivism measures: charges, sentences, and probation violations). All participant group individuals who did not complete the 18-month program were excluded from this analysis. Detailed information on the matching process can be found in Appendix B.

Baseline Equivalence Testing of Participant and Control Group Comparisons

ITT sample. Following suggestions from the WWC, WestEd examined whether there was baseline equivalence for each outcome’s analysis sample (U.S. Department of Education, 2020). According to Ho and colleagues (2007), an acceptable level for minimizing bias between groups is a difference between the intervention and comparison groups on measured characteristics prior to the intervention that is less than a quarter of a standard deviation (i.e., an effect size of 0.25). No unacceptable group differences emerged between the participant and control groups for all three analysis samples. Following suggestions by the WWC, differences in baseline characteristics that are between 0.05 and 0.25 standard deviations must be statistically adjusted (U.S. Department of Education, 2020). Thus, race/ethnicity and sex were included as covariates in the ITT analyses of all three outcomes. Detailed information on the baseline equivalence testing and results can be found in Appendix B.

TOT sample. Baseline equivalence for the TOT sample of each of the three outcomes was also assessed. Results indicated that the TOT participant group individuals and the matched TOT comparison group individuals were similar on baseline characteristics. In some cases (number

of felony arrests in the year prior to study randomization, race/ethnicity, and sex), effect sizes were greater than 0.05, indicating it was necessary to include statistical adjustments to satisfy baseline equivalence. However, none of the effect sizes was greater than 0.25; thus, the samples are considered to have baseline equivalence. Detailed information on the baseline equivalence testing and results can be found in Appendix B.

Data Sources

The impact evaluation included secondary administrative data that came from three sources: the CA DOJ CORI database, ACDAO's DALITE case management system, and ACDAO's Consolidated Records Information Management System (CRIMS). WestEd used CA DOJ CORI data to measure the main project outcome (new felony and misdemeanor arrests in the state within each individual's 24-month observation period) and to create a measure of each individual's prior criminal history (number of felony arrests in the state in the year prior to study randomization) to use as a covariate in the recidivism analyses.⁷ WestEd used ACDAO data to measure the project's nonoutcome metrics (charges, convictions, sentences, and probation violations in Alameda County). Additional detailed, technical information on the data sources can be found in Appendix B.

Analysis Plan

WestEd employed survival analysis (also known as event history analysis) for the impact analyses on recidivism outcomes within the 24-month observation period. Specifically, WestEd used Kaplan-Meier group comparisons of survival times and Cox regression models (i.e., Cox proportional hazard models) to examine the effects of ACJRP on time to recidivism (Lee, 1992). The goal of the survival analysis was to determine whether differences existed between the participant and control groups in terms of their recidivism rates and the amount of time that passed before they recidivated.

The Kaplan-Meier procedure is a descriptive approach and cannot include covariates (i.e., individuals' background characteristics) as the Cox regression models can. Using the study's data, a downward curve was calculated for the participant and control groups as more individuals recidivated over time. The Kaplan-Meier procedure is typically done before the Cox regression model and examines how the curves for the participant and control groups differ (Kaplan & Meier, 1958). The Kaplan-Meier procedure produces the Wilcoxon test and the log-rank test, both of which compared the number of events (e.g., new arrest) in each group during the 24-month observation window with the expected number if there were no differences

⁷ WestEd originally planned on using baseline risk assessment scores from ACPD (i.e., risk assessment scores from before study randomization) as a covariate for the recidivism analyses. However, 51 percent (228 of 450) of the study sample was missing baseline risk assessment scores (control group: 47 percent, or 91 of 193, were missing data; participant group: 53 percent, or 137 of 257, were missing data). Thus, WestEd instead used the CA DOJ CORI data—which was available for all study participants—to create a variable that captured each individual's prior criminal history as a covariate for the recidivism analyses.

between the participant and control groups. The Wilcoxon test puts more weight on earlier points in time whereas the log-rank test weights all parts of the survival curves the same. Although these two tests produce p -values and therefore indicate whether the differences are statistically significant, they should be viewed as exploratory because they are not based on an analysis that statistically controls for important covariates (e.g., race/ethnicity).

Cox regression (Cox, 1972) is the most commonly used multivariate approach for analyzing time until recidivism and is the primary approach WestEd used to determine whether the time until recidivism differed to a statistically significant extent across the participant and control groups. The Cox regression models determine group differences (between the participant and control groups) while controlling for other factors (e.g., age). The Cox regression results were expressed as hazard ratios. As an example of a hazard ratio less than 1.00, if the treatment variable had a hazard ratio of 0.90, that would indicate that compared with the control group, the participant group was associated with a 10 percent decrease in the hazard rate, or the propensity for recidivism. As an example of a hazard ratio greater than 1.00, if the treatment variable had a hazard ratio of 1.20, that would indicate that compared with the control group, the participant group was associated with a 20 percent increase in the hazard rate, or the propensity for recidivism. WestEd employed listwise deletion in the survival analysis models and excluded individuals with missing values on the outcome measure from the analyses. Additional detailed, technical information on the data analysis plan can be found in Appendix B.

Results

The sections that follow present evaluation findings for each of the recidivism measures: arrests in the state of California and charges, convictions, sentences (detentions and supervisions), and probation violations in Alameda County. For each section, the results for the randomized (i.e., ITT sample) participant and control groups are presented first. These results are followed by the subgroup analyses for the ITT sample that examined whether the ACJRP treatment effect differed by sex, age, or race/ethnicity. Lastly, the TOT participant group and matched comparison group results that examined the effect of completing the 18-month ACJRP program are presented.

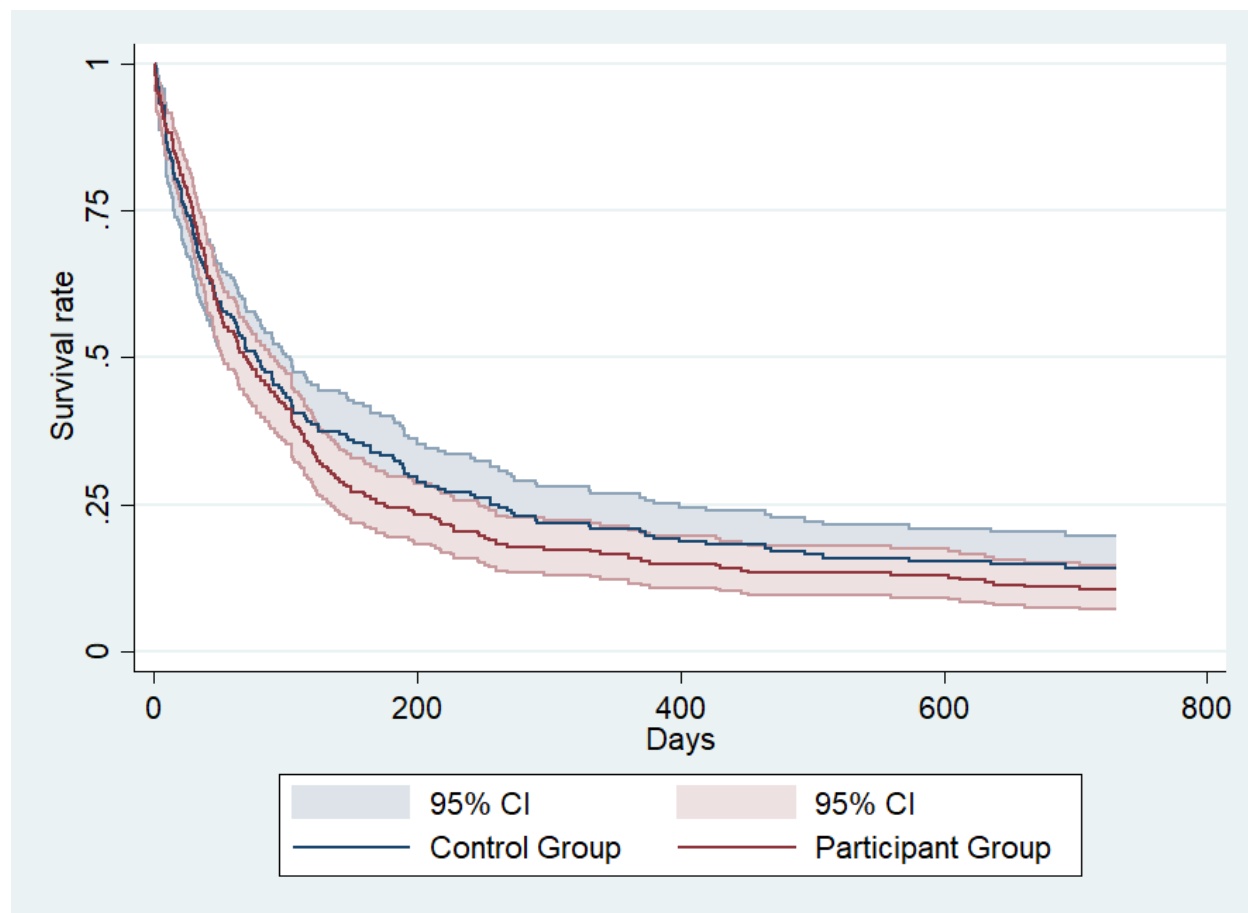
Statewide Recidivism Measures

Arrests

The arrest analysis sample included 446 individuals (participant group = 254, control group = 192) because there was missing arrest data. The vast majority of both the participant and control groups were arrested for a new felony or misdemeanor in California during the 24-month observation period. The exploratory tests (i.e., the Wilcoxon test and the log-rank test) indicated that there were no statistically significant differences in the time to recidivism between the participant and control groups ($p = .58$ and $p = .27$, respectively). By the end of the 24 months after each individual's randomization date, 89 percent of the participant group and

86 percent of the control group had been arrested for a new felony or misdemeanor. Figure 7 presents a graph of the Kaplan-Meier survival rate for new felony or misdemeanor arrests in the state. As shown in the figure, the majority of the arrests for both the participant and control groups occurred during the first 200 days during the observation period. At the end of the 24-month observation period, the survival rate for the participant group was 11 percent and the survival rate for the control group was 14 percent.

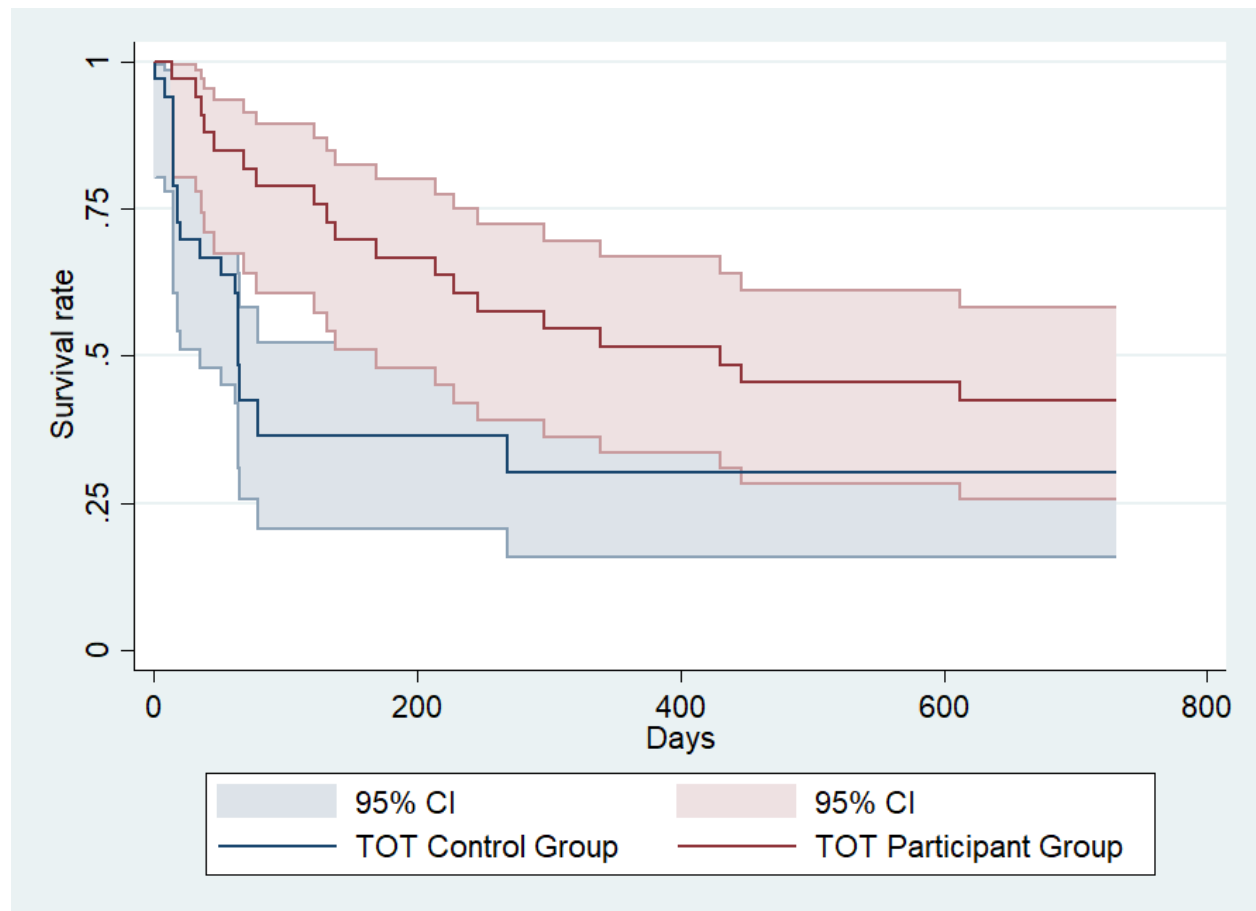
Figure 7. Arrest Survival Rates Over Time by ITT Participant and Control Groups



The Cox regression models, which were WestEd’s primary method for assessing ACJRP’s effect on time to recidivism, indicated that there were no statistically significant differences between the participant and control groups in the overall hazard rate, or the risk for recidivism, after accounting for race/ethnicity and sex (hazard ratio = 1.11, $p = .32$; see Table C3 in Appendix C for detailed results from the Cox regression models). The difference between the participant and control groups’ risk for recidivism also did not vary by a statistically significant extent by sex, age, or race/ethnicity (Tables C4–C6), indicating that the non–statistically significant impact of ACJRP was observed for all subgroups of participants.

Figure 8 presents a graph of the Kaplan-Meier survival rates for new felony or misdemeanor arrests in the state for the TOT participant group and matched comparison group. At the end of the 24-month observation period, fewer individuals from the TOT participant group had been arrested compared with the TOT comparison group. Specifically, 58 percent of the TOT participant group and 70 percent of the TOT comparison group had been arrested for a new felony or misdemeanor in the state. In other words, the survival rate for the TOT participant group was 42 percent, and the survival rate for the TOT comparison group was 30 percent. The exploratory Wilcoxon test and the log-rank test suggested that the time to recidivism for the TOT participant group was greater than the time to recidivism for the TOT comparison group and that this difference was statistically significant ($p = .006$ and $p = .03$, respectively).

Figure 8. Arrest Survival Rates Over Time by TOT Participant and Comparison Groups



Additionally, the results from the Cox regression confirmed the Kaplan-Meier results and indicated that at any particular time in the 24-month observation period, individuals who completed the 18-month ACJRP program were 65 percent less likely to have a new arrest in the state than were individuals in the matched TOT comparison group after accounting for prior

criminal history, race/ethnicity, and sex. This difference was statistically significant (hazard ratio = 0.35, $p = .004$; Table C7).

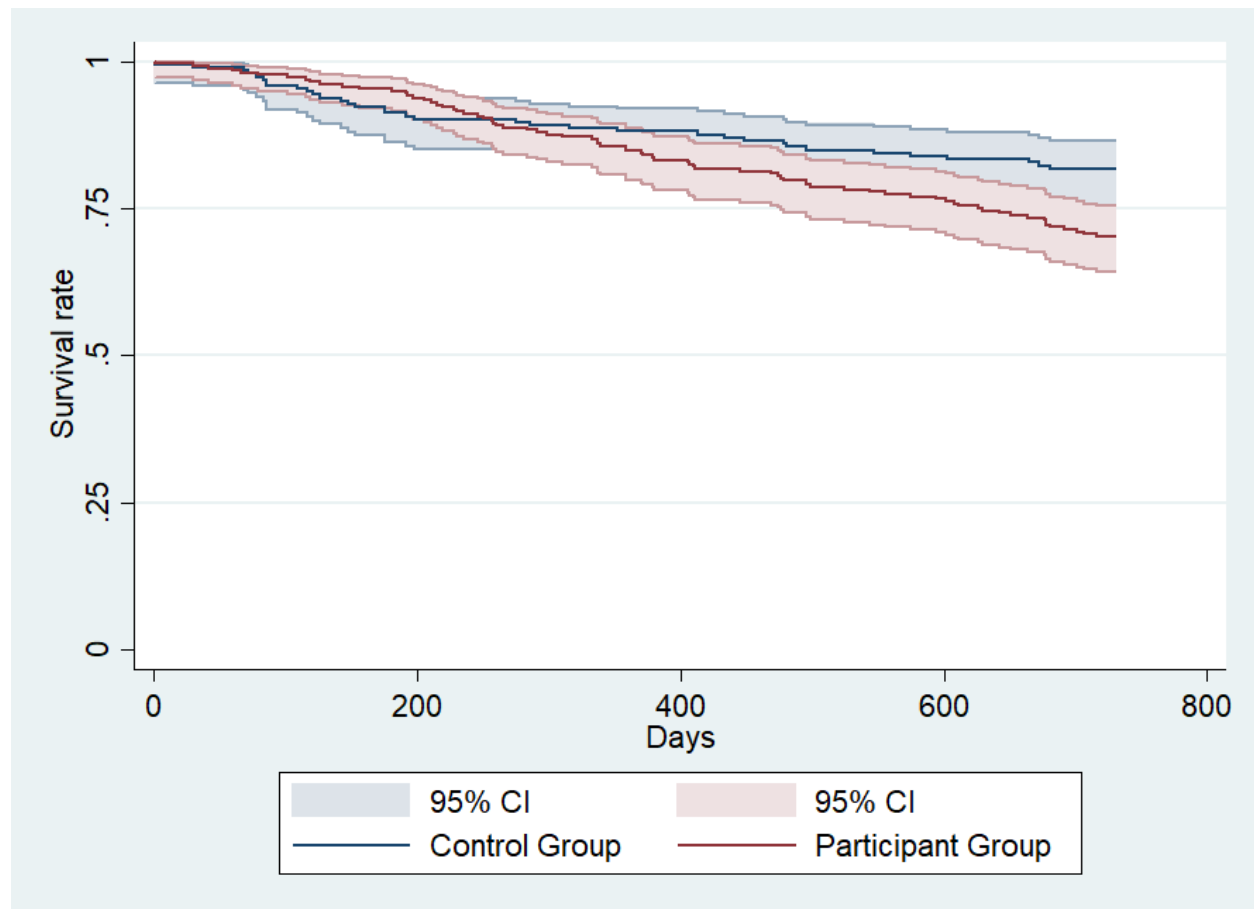
County-Level Recidivism Measures

What follows are the recidivism results for the county-level recidivism measures (charges, convictions, detention and supervision sentences, and probation violations). We caution that the county-level results should be interpreted with data limitations in mind. First, participant group individuals who enrolled into ACJRP inherently had to remain in Alameda County in order to participate continuously in the program, whereas the control group did not engage in any programming and may have left the county during the pandemic. Therefore, the control group's criminality may not have been picked up by the Alameda County-specific measures. Second, one must appear in court to be charged and subsequently sentenced. Court notices are typically sent via mail to home addresses. Because the participant group was more likely to stay in the county and also more likely to go to court as part of ACJRP program activities, it is possible that they were more likely to have a new charge, sentence, or probation violation in the county. The county-specific measures may not reflect an accurate assessment of the control group's criminality if they left the county and/or were less likely to appear for court notices. Additional information is available in the section "Impact Evaluation's Data Limitations."

Charges

The charges analysis sample included 450 individuals (participant group = 257, control group = 193), as there was no missing data. Figure 9 presents a graph of the Kaplan-Meier survival rate for new charges in Alameda County. By the end of the 24 months, 30 percent of the participant group and 18 percent of the control group had a new charge in Alameda County. In other words, the survival rate for the participant group was 70 percent, and the survival rate for the control group was 82 percent. The exploratory Wilcoxon test and the log-rank test suggested that the time to recidivism for the participant group was shorter than the time to recidivism for the control group and that this difference was statistically significant ($p = .02$ and $p = .01$, respectively).

Figure 9. Charge Survival Rates Over Time by ITT Participant and Control Groups



Similarly, the results from the Cox regression confirmed the Kaplan-Meier results and indicated that at any particular time in the 24-month observation period, individuals in the participant group were 64 percent more likely to have a new charge in Alameda County than were individuals in the control group after accounting for race/ethnicity and sex. This difference was statistically significant (hazard ratio = 1.64, $p = .02$; Table C8).

Additionally, there was a statistically significant interaction term, which indicated that the difference between the participant and control groups' risk for recidivism varied by race/ethnicity (hazard ratio = 2.78, $p = .046$; Table C11). The relative risk hazards indicate the relative risks for recidivism, with the following groups listed in order from the lowest to highest risk for recidivism: Black, Indigenous, and people of color (BIPOC) control group individuals

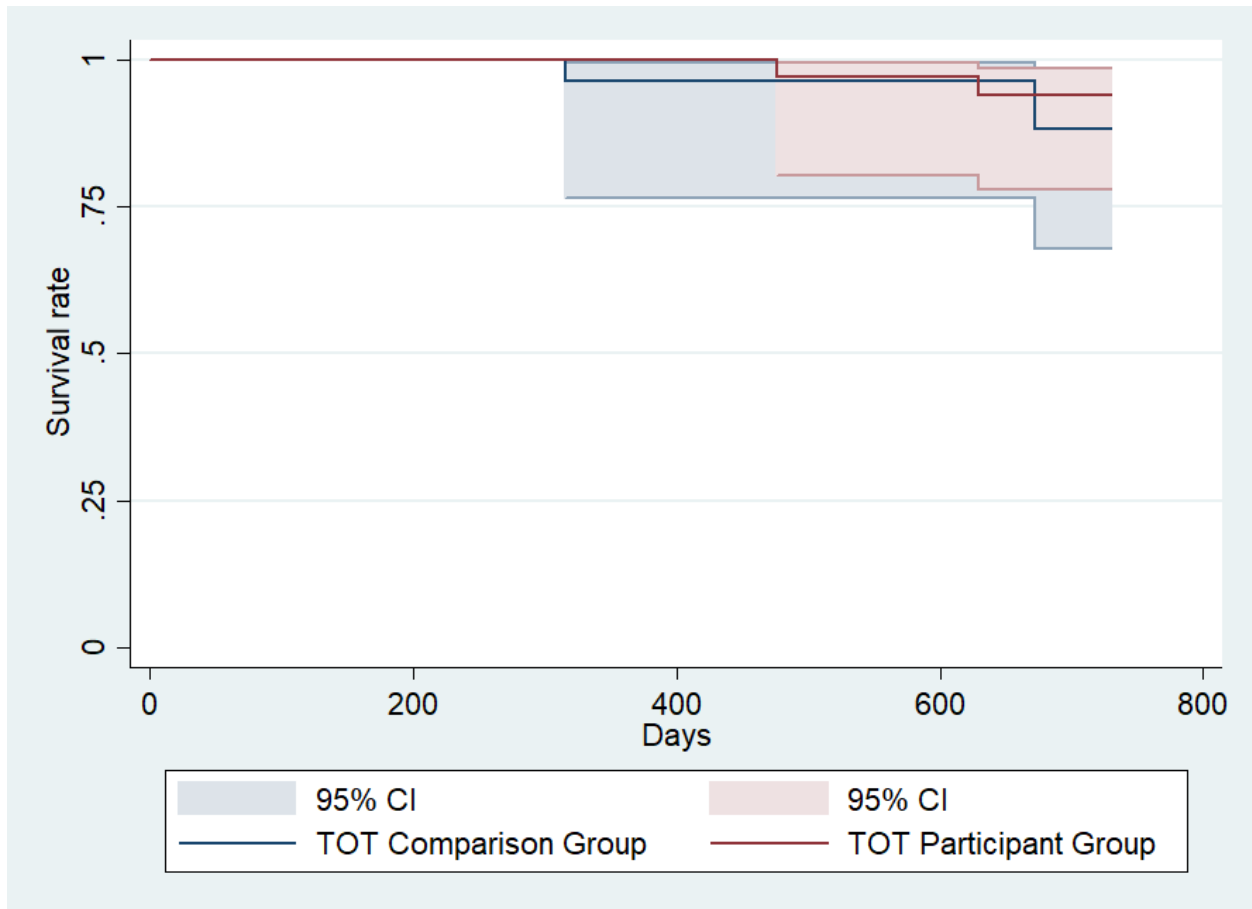
(relative risk = 0.84), White participant group individuals (relative risk = 0.86), White control group individuals (relative risk = 1.18), and BIPOC participant group individuals (relative risk = 1.70).⁸ The difference between the participant and control groups' risk for recidivism did not vary by a statistically significant extent by sex or age (Tables C9 and C10).

Figure 10 presents a graph of the Kaplan-Meier survival rates for new charges in Alameda County for the TOT participant group and matched comparison group. As shown in the figure, not many TOT participant and comparison group individuals received new charges during the observation period. Specifically, by the end of the 24 months, 6 percent of the TOT participant group and 12 percent of the TOT comparison group recidivated, as measured by a new charge in Alameda County. In other words, the survival rate for the TOT participant group was 94 percent, and the survival rate for the TOT comparison group was 88 percent. The exploratory Wilcoxon and log-rank tests indicated that there were no statistically significant differences in the time to recidivism between the TOT participant group and matched comparison group ($p = .48$ and $p = .46$, respectively). The Cox regression model that would have assessed the effect of completing the 18-month ACJRP program on time to recidivism after accounting for background characteristics was not appropriate for the data because the rate of recidivism was too low for this type of analysis.⁹ The data showed that none of the individuals in the TOT participant group and matched comparison group recidivated within the first 310 days of the observation period.

⁸ Additional analyses revealed that the significant randomization group \times BIPOC interaction result was found for Black or African American individuals (hazard ratio = 3.20, $p = .049$) and Other Race individuals (hazard ratio = 7.06, $p = .03$). No significant interaction results were found for Hispanic individuals (hazard ratio = 2.18, $p = .18$).

⁹ The Cox regression model was overfitting the data.

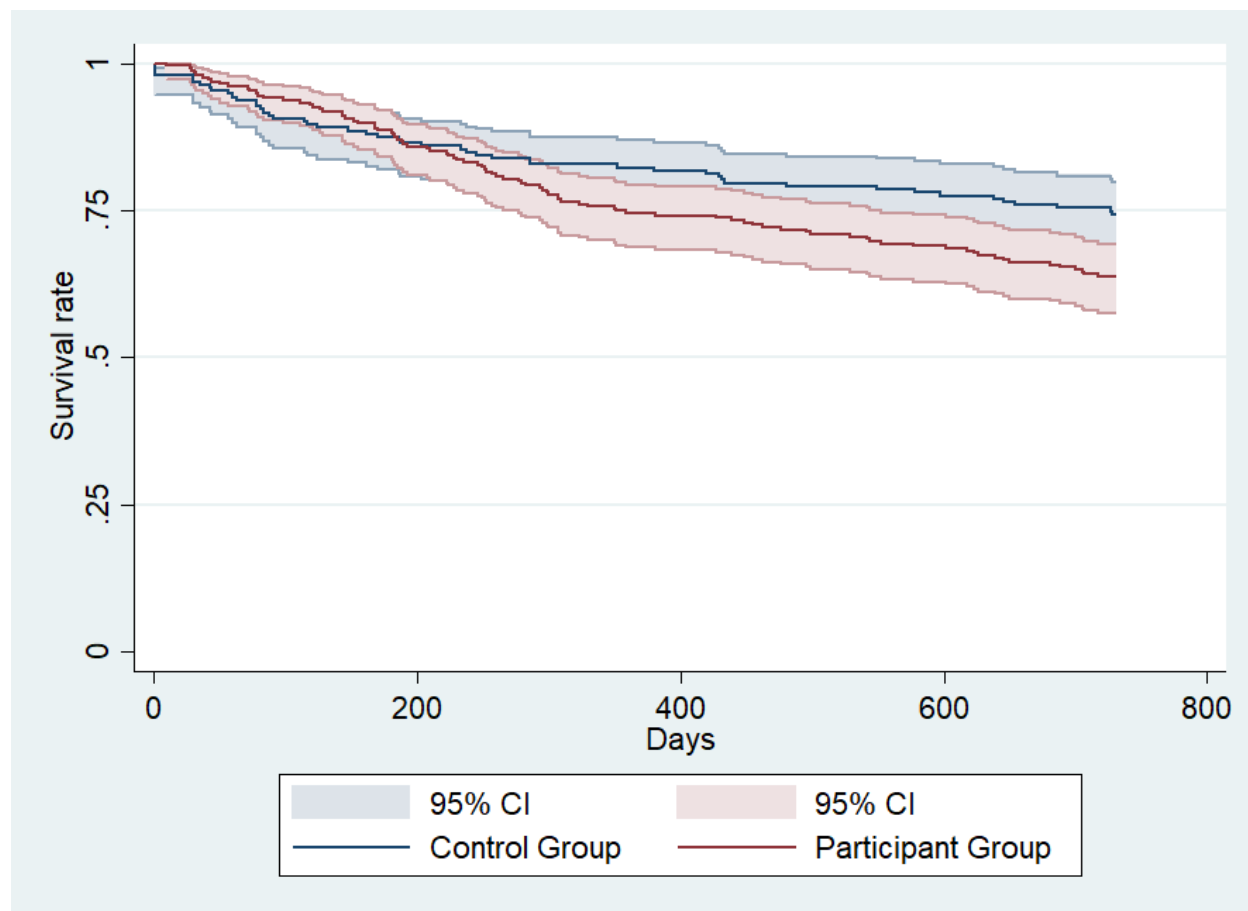
Figure 10. Charge Survival Rates Over Time by TOT Participant and Comparison Groups



Convictions

The convictions analysis sample included 447 individuals (participant group = 255, control group = 192) because the final conviction status of three individuals was missing. Figure 11 presents a graph of the Kaplan-Meier survival rate for new convictions in Alameda County. By the end of the 24 months, 36 percent of the participant group and 26 percent of the control group had a new conviction in Alameda County. In other words, the survival rate for the participant group was 64 percent, and the survival rate for the control group was 74 percent. The exploratory Wilcoxon test and the log-rank test indicated that the time to recidivism for the participant group was shorter than the time to recidivism for the control group and that this difference was statistically significant ($p = .0497$ and $p = .03$, respectively).

Figure 11. Conviction Survival Rates Over Time by ITT Participant and Control Groups

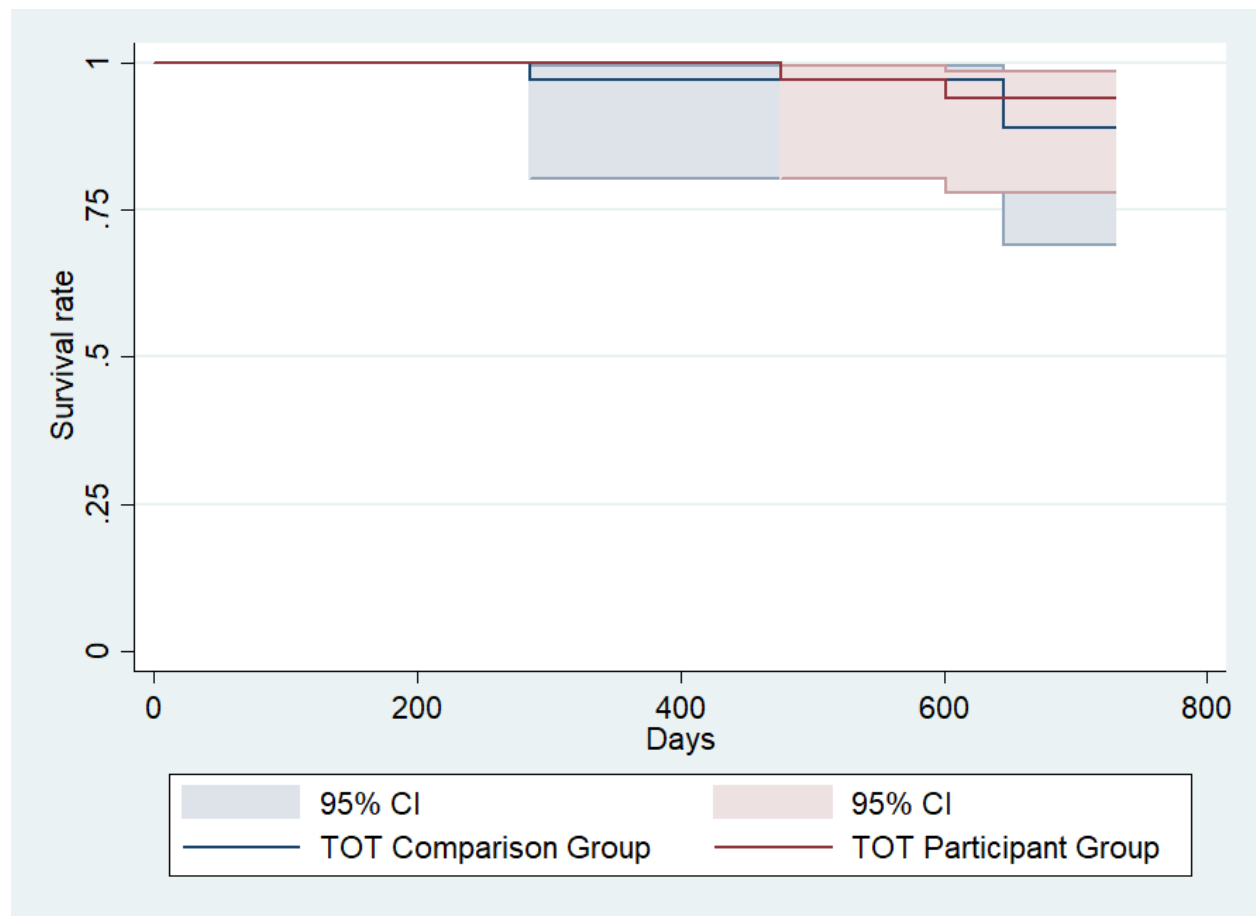


However, the Cox regression model indicated that there were no statistically significant differences between the two groups’ risk for recidivism (hazard ratio = 1.39, $p = .07$; Table C12). At any particular time in the 24-month observation period, individuals in the participant group were 39 percent more likely to have a new conviction in Alameda County than were individuals in the control group after accounting for race/ethnicity and sex. The difference between the participant and control groups’ risk for recidivism also did not vary by a statistically significant extent by sex, age, or race/ethnicity (Tables C13–C15), indicating that the non–statistically significant impact of ACJRP was observed for all subgroups of participants.

Figure 12 presents a graph of the Kaplan-Meier survival rates for new convictions for the TOT participant group and matched comparison group. As shown in the figure, not many TOT participant and comparison group individuals received new convictions during the observation period. Specifically, by the end of the 24 months, 6 percent of the TOT participant group and 11 percent of the TOT comparison group recidivated, as measured by a new conviction in Alameda County. In other words, the survival rate for the TOT participant group was 94 percent, and the survival rate for the TOT comparison group was 89 percent. The exploratory Wilcoxon and log-rank tests indicated that there were no statistically significant

differences in the time to recidivism between the TOT participant group and matched comparison group ($p = .49$ for both). The Cox regression model that would have assessed the effect of completing the 18-month ACJRP program on time to recidivism after accounting for background characteristics was not appropriate for the data because the rate of recidivism was too low for this type of analysis. The data showed that none of the individuals in the TOT participant group and matched comparison group recidivated within the first 280 days of the observation period.

Figure 12. Conviction Survival Rates Over Time by TOT Participant and Comparison Groups

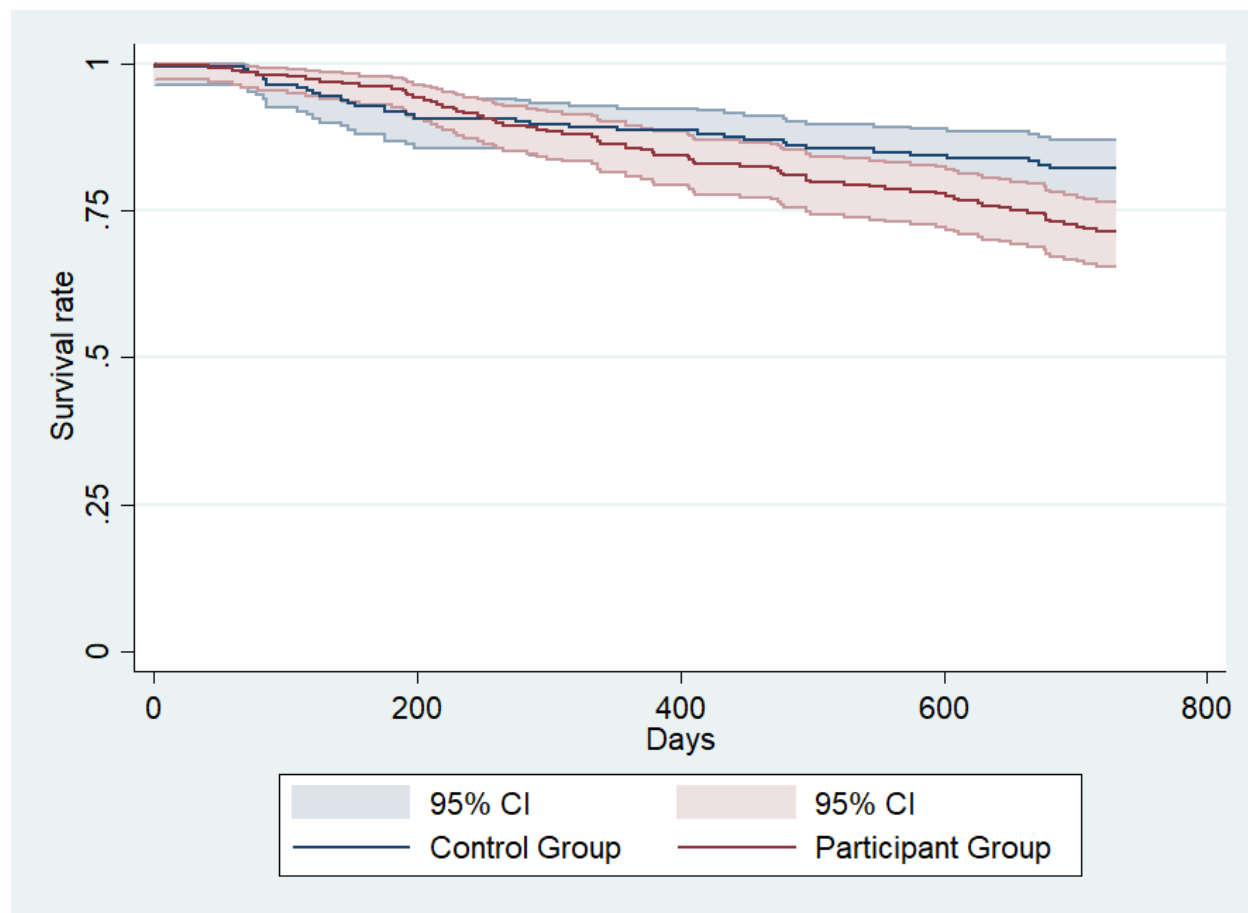


Sentences (Detentions)

The detentions analysis sample included 450 individuals (participant group = 257, control group = 193), as there was no missing data. Figure 13 presents a graph of the Kaplan-Meier survival rate for new detention sentences in Alameda County. By the end of the 24 months, 29 percent of the participant group and 18 percent of the control group had a new detention sentence in Alameda County. In other words, the survival rate for the participant group was 71 percent, and the survival rate for the control group was 82 percent. The exploratory Wilcoxon test and the

log-rank test indicated that the time to recidivism for the participant group was shorter than the time to recidivism for the control group and that this difference was statistically significant ($p = .02$ and $p = .01$, respectively).

Figure 13. Detention Sentence Survival Rates Over Time by ITT Participant and Control Groups



Similarly, the results from the Cox regression confirmed the Kaplan-Meier results and indicated that at any particular time in the 24-month observation period, individuals in the participant group were 60 percent more likely to have a new detention sentence than were individuals in the control group after accounting for race/ethnicity and sex. This difference was statistically significant (hazard ratio = 1.60, $p = .03$; Table C16).

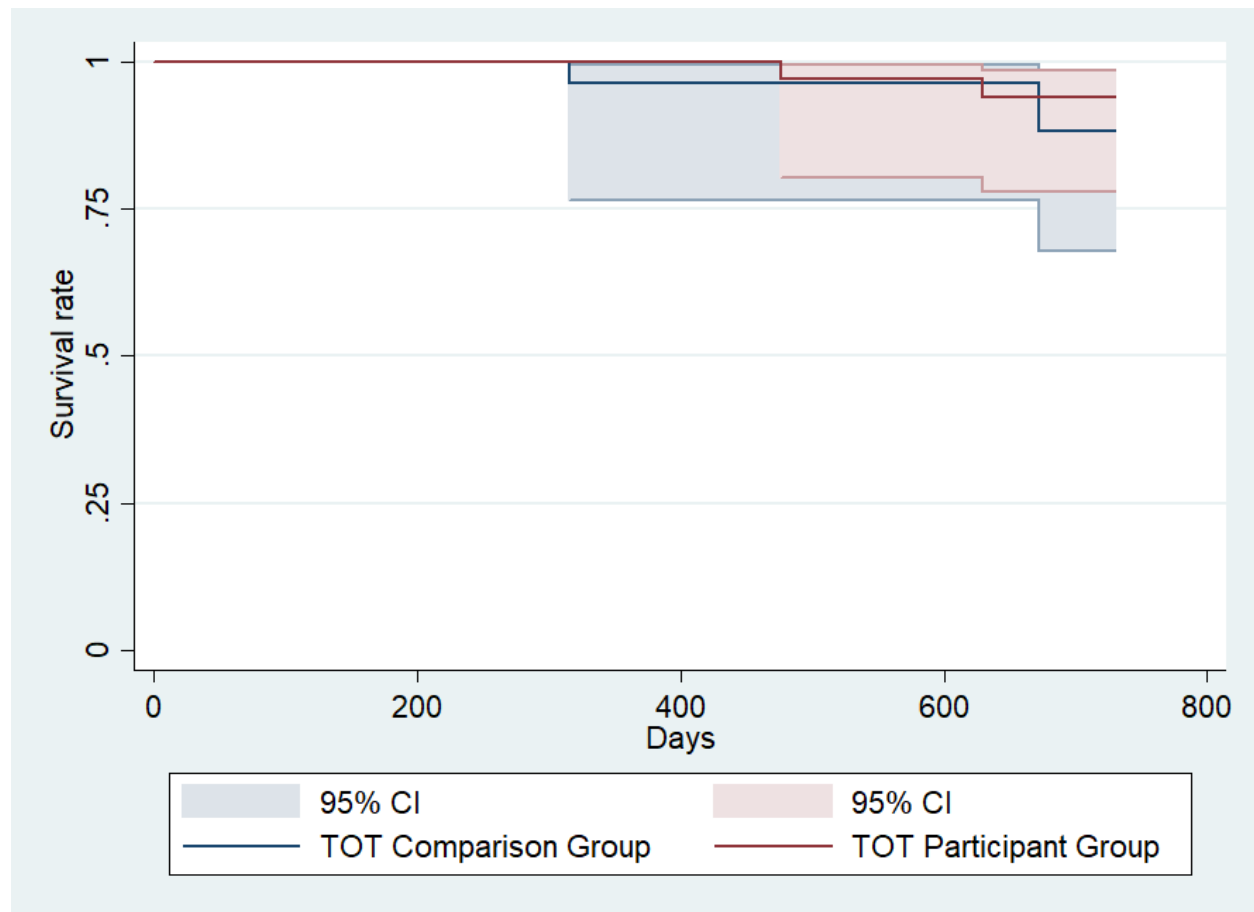
Additionally, there was a statistically significant interaction term, which indicated that the difference between the participant and control groups' risk for recidivism varied by race/ethnicity (hazard ratio = 3.20, $p = .03$; Table C19). The relative risk hazards indicate the relative risks for recidivism, with the following groups listed in order from the lowest to highest risk for recidivism: White participant group individuals (relative risk = 0.73), BIPOC control group individuals (relative risk = 0.78), White control group individuals (relative risk = 1.15), and

BIPOC participant group individuals (relative risk = 1.60).¹⁰ The difference between the participant and control groups' risk for recidivism did not vary by a statistically significant extent by sex or age (Tables C17 and C18).

Figure 14 presents a graph of the Kaplan-Meier survival rates for new detention sentences in Alameda County for the TOT participant group and matched comparison group. As shown in the figure, not many TOT participant and comparison group individuals received new detention sentences during the observation period. Specifically, by the end of the 24 months, 6 percent of the TOT participant group and 12 percent of the TOT comparison group recidivated, as measured by a new detention sentence in Alameda County. In other words, the survival rate for the TOT participant group was 94 percent, and the survival rate for the TOT comparison group was 88 percent. The exploratory Wilcoxon and log-rank tests indicated that there were no statistically significant differences in the time to recidivism between the TOT participant group and matched comparison group ($p = .48$ and $p = .46$, respectively). The Cox regression model that would have assessed the effect of completing the 18-month ACJRP program on time to recidivism after accounting for background characteristics was not appropriate for the data because the rate of recidivism was too low for this type of analysis. The data showed that none of the individuals in the TOT participant group and matched comparison group recidivated within the first 310 days of the observation period.

¹⁰ Additional analyses revealed that the significant randomization group \times BIPOC interaction result was found for Black or African American individuals (hazard ratio = 3.44, $p = .04$) and Other Race individuals (hazard ratio = 6.77, $p = .04$). No significant interaction results were found for Hispanic individuals (hazard ratio = 2.63, $p = .11$).

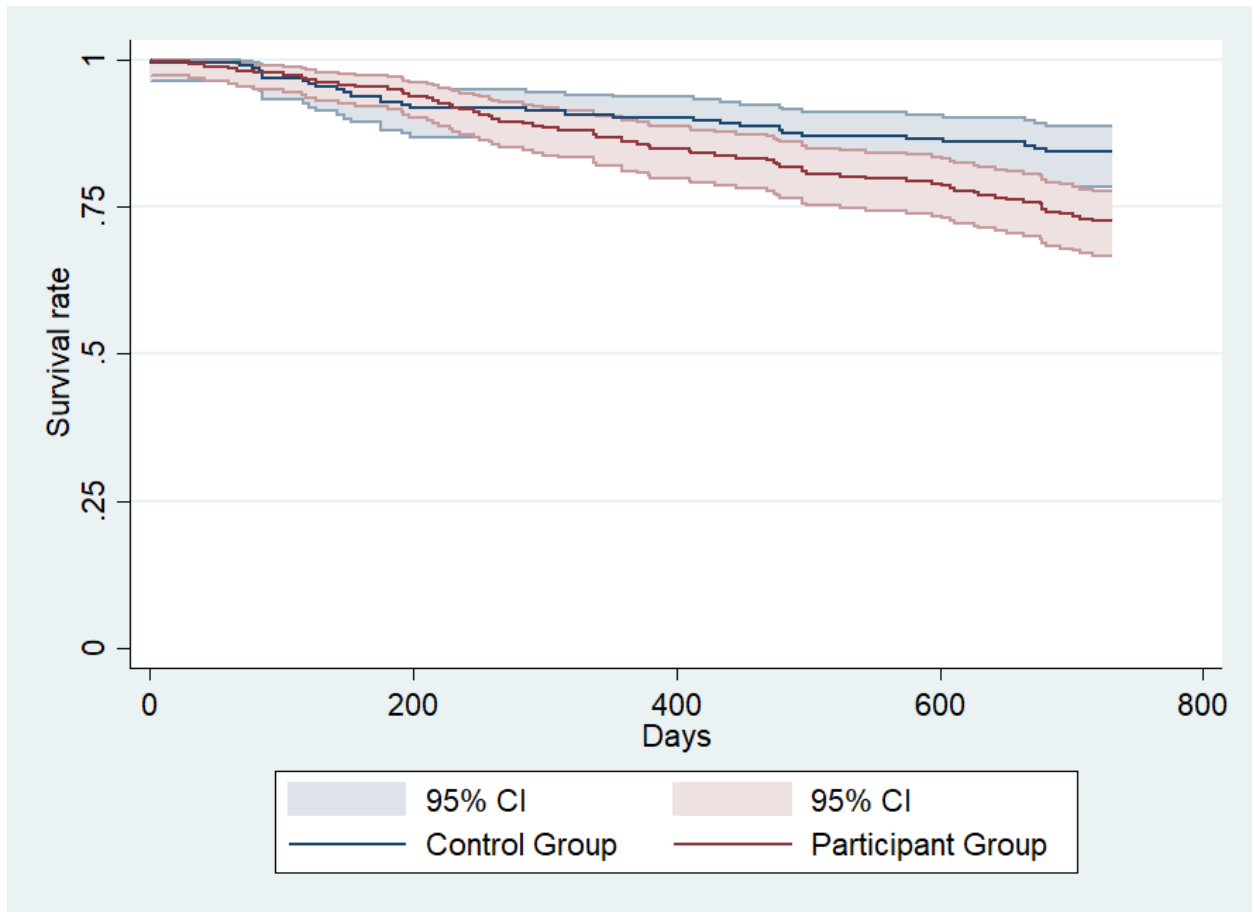
Figure 14. Detention Sentence Survival Rates Over Time by TOT Participant and Comparison Groups



Sentences (Supervisions)

The supervision sentences analysis sample included 450 individuals (participant group = 257, control group = 193), as there was no missing data. Figure 15 presents a graph of the Kaplan-Meier survival rate for new supervision sentences in Alameda County. By the end of the 24 months, 27 percent of the participant group and 16 percent of the control group had a new supervision sentence in Alameda County. In other words, the survival rate for the participant group was 73 percent, and the survival rate for the control group was 84 percent. The exploratory Wilcoxon test and the log-rank test suggested that the time to recidivism for the participant group was shorter than the time to recidivism for the control group and that this difference was statistically significant ($p = .009$ and $p = .005$, respectively).

Figure 15. Supervision Sentence Survival Rates Over Time by ITT Participant and Control Groups



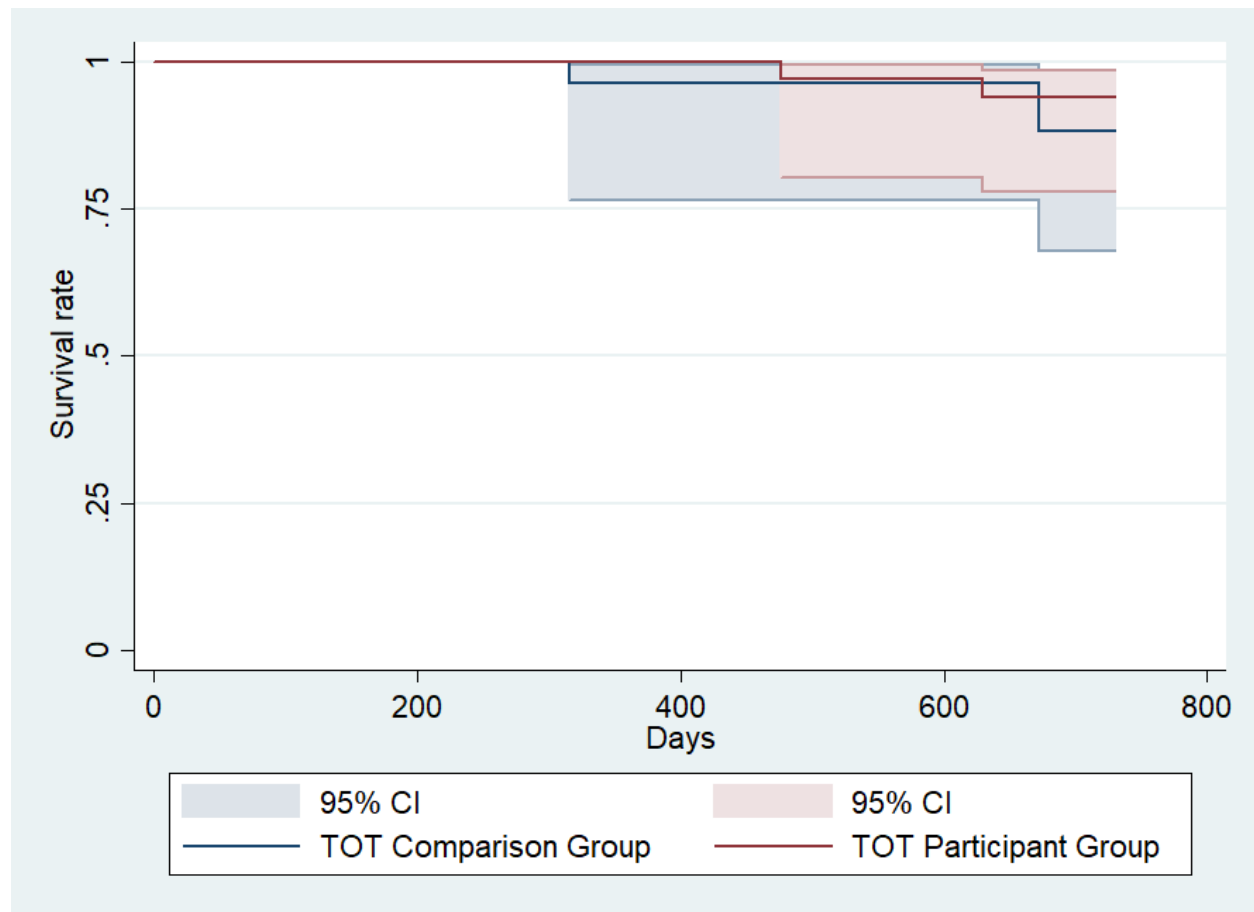
Similarly, the results from the Cox regression confirmed the Kaplan-Meier results and indicated that at any particular time in the 24-month observation period, individuals in the participant group were 80 percent more likely to have a new supervision sentence than were individuals in the control group after accounting for race/ethnicity and sex. This difference was also statistically significant (hazard ratio = 1.80, $p = .008$; Table C20).

Additionally, there was a statistically significant interaction term, which indicated that the difference between the participant and control groups' risk for recidivism varied by race/ethnicity (hazard ratio = 4.42, $p = .007$; Table C23). The relative risk hazards indicate the relative risks for recidivism, with the following groups listed in the order from lowest to highest risk for recidivism: BIPOC control group individuals (relative risk = 0.54), White participant group individuals (relative risk = 0.55), White control group individuals (relative risk = 0.98), and BIPOC participant group individuals (relative risk = 1.34).¹¹ The difference between the participant and control groups' risk for recidivism did not vary by a statistically significant extent by sex or age (Tables C21 and C22).

Figure 16 presents a graph of the Kaplan-Meier survival rates for new supervision sentences in Alameda County for the TOT participant group and matched comparison group. As shown in the figure, not many TOT participant and comparison group individuals received new supervision sentences during the observation period. Specifically, by the end of the 24 months, 6 percent of the TOT participant group and 12 percent of the TOT comparison group recidivated, as measured by a new supervision sentence in Alameda County. In other words, the survival rate for the TOT participant group was 94 percent, and the survival rate for the TOT comparison group was 88 percent. The exploratory Wilcoxon and log-rank tests indicated that there were no statistically significant differences in the time to recidivism between the TOT participant group and matched comparison group ($p = .48$ and $p = .46$, respectively). The Cox regression model that would have assessed the effect of completing the 18-month ACJRP program on time to recidivism after accounting for background characteristics was not appropriate for the data because the rate of recidivism was too low for this type of analysis. The data showed that none of the individuals in the TOT participant group and matched comparison group recidivated within the first 310 days of the observation period.

¹¹ Additional analyses revealed that the significant randomization group \times BIPOC interaction result was found for Black or African American individuals (hazard ratio = 5.24, $p = .009$), Hispanic individuals (hazard ratio = 3.54, $p = .049$), and Other Race individuals (hazard ratio = 7.52, $p = .03$).

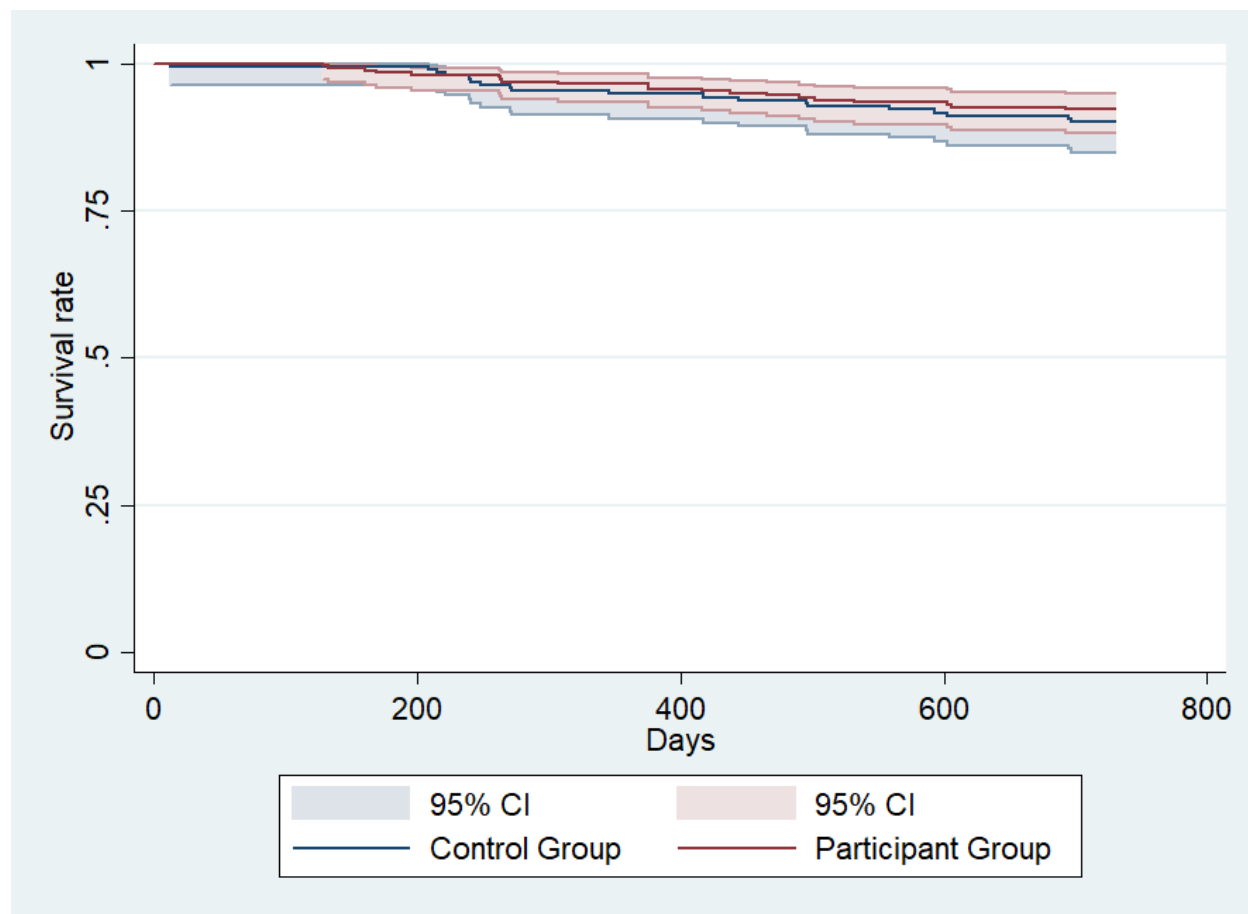
Figure 16. Supervision Sentence Survival Rates Over Time by TOT Participant and Comparison Groups



Probation Violations

The probation violations analysis sample included 450 individuals (participant group = 257, control group = 193), as there was no missing data. Figure 17 presents a graph of the Kaplan-Meier survival rate for new probation violations in Alameda County. By the end of the 24 months, 8 percent of the participant group and 10 percent of the control group had a new probation violation in Alameda County. In other words, the survival rate for the participant group was 92 percent, and the survival rate for the control group was 90 percent. The exploratory Wilcoxon test and the log-rank test indicated that there were no statistically significant differences in the time to recidivism between the participant and control groups ($p = .45$ and $p = .44$, respectively).

Figure 17. Probation Violation Survival Rates Over Time by ITT Participant and Control Groups

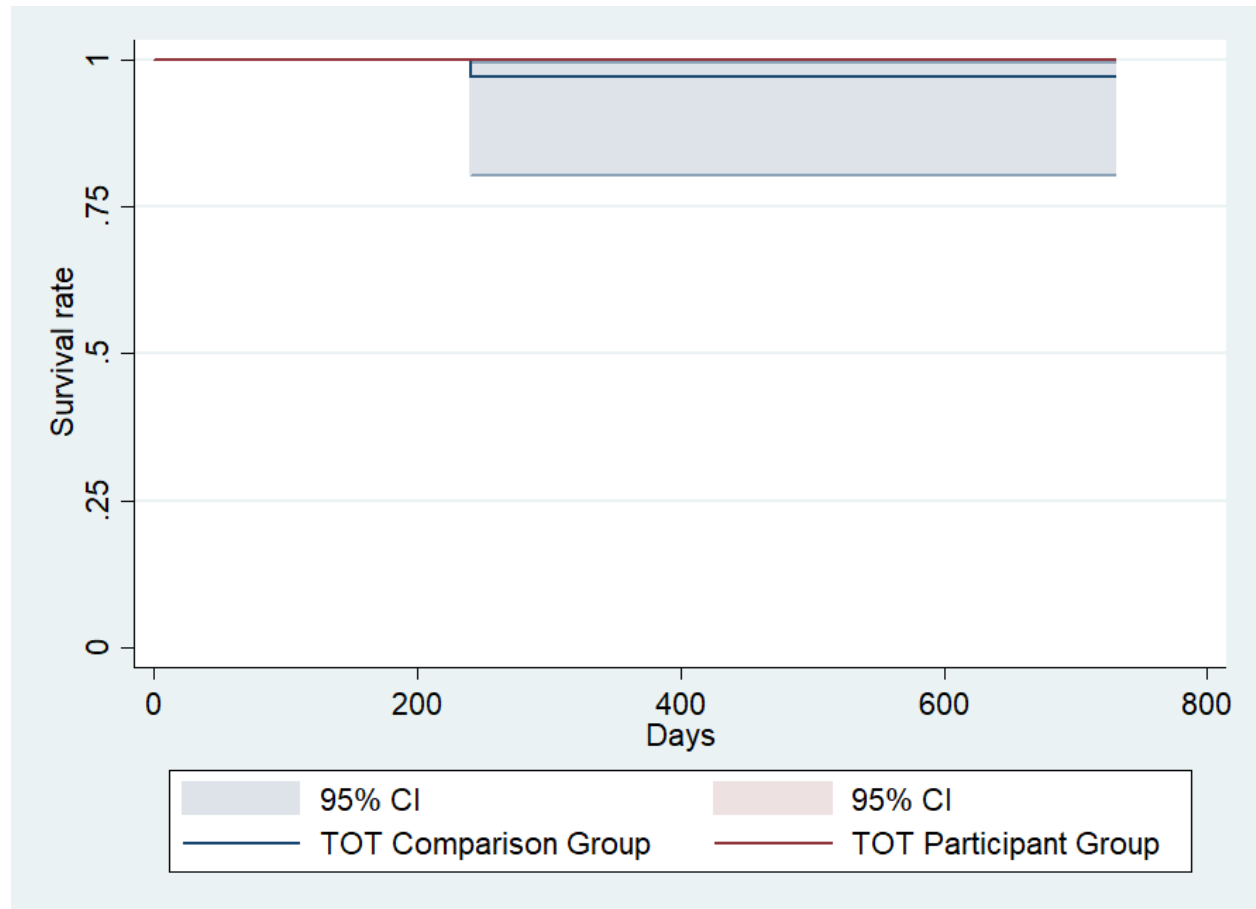


The results from the Cox regression models, which were WestEd’s primary method for assessing ACJRP’s effect on time to recidivism, confirmed the Kaplan-Meier results and indicated that there were no statistically significant differences between the participant and control groups’ risk for recidivism after accounting for race/ethnicity and sex (hazard ratio = 0.78, $p = .44$; Table C24). The difference between the participant and control groups’ risk for recidivism also did not vary by a statistically significant extent by sex, age, or race/ethnicity (Tables C25–C27), indicating that the non–statistically significant impact of ACJRP was observed for all subgroups of participants.

Figure 18 presents a graph of the Kaplan-Meier survival rates for new probation violations in Alameda County for the TOT participant group and matched comparison group. As shown in the figure, not many TOT participant and comparison group individuals received new probation violations during the observation period. Specifically, by the end of the 24 months, 8 percent of the TOT participant group and 10 percent of the TOT comparison group had a probation violation in Alameda County. In other words, the survival rate for the TOT participant group was 92 percent, and the survival rate for the TOT comparison group was 90 percent. The

exploratory Wilcoxon and log-rank tests indicated that there were no statistically significant differences in the time to recidivism between the TOT participant group and matched comparison group ($p = .32$ for both). The Cox regression model that would have assessed the effect of completing the 18-month ACJRP program on time to recidivism after accounting for background characteristics was not appropriate for the data because the rate of recidivism was too low for this type of analysis. The data showed that none of the individuals in the TOT participant group and matched comparison group recidivated within the first 240 days of the observation period.

Figure 18. Probation Violation Survival Rates Over Time by TOT Participant and TOT Comparison Groups



Discussion

Though the impact study found both positive and negative results, these results should be interpreted alongside the data limitations of the recidivism measures (see the section “Impact Evaluation’s Data Limitations”). Additionally, results from the implementation evaluation provide rich qualitative descriptions of ACJRP outcomes beyond the recidivism measures, such as the ACJRP participants’ experiences in the program, their successes during and after program completion, and the effect the ACJRP program had on the peer coaches’ journey to rehabilitation.

What follows is a summary of the findings of the impact and implementation evaluations. Lessons learned is then discussed, as well as factors to take into consideration when implementing a program similar to ACJRP (i.e., a reentry program that leverages peer coaches with lived experiences), such as the peer coach model, training, and program retention. We describe the cross-sector collaborations that facilitated the implementation of ACJRP and how the successes and partnerships from the ACJRP work have informed the development of a new diversion program in Alameda County. Finally, we end with a discussion of gaps in statewide criminal justice data and of lessons learned regarding PFS studies.

Summary of Impact Findings

For the RCT, WestEd used an ITT analysis that compared the randomized participant group (regardless of the extent to which they participated in ACJRP) and the control group to assess the impact of the ACJRP program on recidivism outcomes. The RCT is the most rigorous way to examine the impact of ACJRP. WestEd also used a QED that included a TOT analysis to compare those who completed the 18-month ACJRP program (completed group) with a matched comparison group identified from the control group. The TOT analysis is a less rigorous approach because participants who completed the program may differ from the matched comparison group in unknown ways (e.g., be more motivated to not recidivate). The study found mixed results:

- **Statewide measures:** Completing the ACJRP program was associated to a statistically significant extent with lower recidivism. By the end of the 24-month observation period, 58 percent of the completed group had a new misdemeanor or felony arrest in the state compared with 70 percent of the matched comparison group. After accounting for background characteristics (prior criminal history, race/ethnicity, and sex), individuals who completed the 18-month ACJRP program were 65 percent less likely to have a new

arrest in the state in the 24 months after being randomized into the study than were individuals in the matched comparison group. However, for statewide arrests there were no statistically significant differences between the randomized participant and control groups' recidivism rates.

- **County measures:**
 - After accounting for background characteristics, there were no statistically significant differences between the randomized participant and control groups along two recidivism measures: new convictions and probation violations in Alameda County. There were also no statistically significant differences between the completed group and the matched comparison group on new convictions and probation violations in Alameda County.
 - For new charges and sentences (detentions and supervisions) in Alameda County, individuals in the randomized participant group were more likely to recidivate than were individuals in the randomized control group after accounting for background characteristics. In addition, the difference between the participant and control groups' risk for recidivism varied by race/ethnicity. For new charges and supervision sentences, BIPOC control group individuals had the lowest risk of recidivism, followed by White participant and control group individuals; BIPOC participant group individuals had the highest risk. For new detention sentences, White participant group individuals had the lowest risk of recidivism, followed by BIPOC and White control group individuals; BIPOC participant group individuals had the highest risk. The negative RCT results with the randomized participant and control groups were not found in the QED sample; there were no statistically significant differences between the completed group and the matched comparison group along those three recidivism measures.

Impact Evaluation's Data Limitations

WestEd strongly urges caution when interpreting the impact evaluation's results due to the low uptake of ACJRP services, specifically when interpreting the ITT results (which include the full $n = 450$ randomized sample), as the program effect may be washed out because the ITT sample also included participants who did not complete the program. Additionally, WestEd urges caution when interpreting the TOT results (which include the $n = 66$ sample with the completed group and matched comparison group). Although the completed group from the TOT sample completed the program, the findings may not be very generalizable because they represent a small portion of the individuals who were offered program services and are different from the population of offenders in Alameda County.

Regarding the ITT findings, of the 257 individuals randomized into the participant group (i.e., had the opportunity to receive ACJRP program services), 61 percent (157 individuals) ever enrolled into ACJRP and the remaining 39 percent (100 individuals) did not join ACJRP. The ITT analyses are the standard analyses to conduct when evaluating an intervention with an RCT, and the participant group represents the group of interest to researchers and evaluators because it is the best group to examine for experimental studies. However, the ITT analyses may not be a good test of ACJRP because not enough services were provided to the randomized participant group. The group of participants served and/or completing the program was not large enough to evaluate with the RCT study design. Low enrollment among the randomized participant group could explain the null or nonsignificant results for the ITT analyses (specifically for arrests, convictions, and probation violations). That is, it is possible that there were no statistically significant findings for ACJRP along these recidivism measures because of the 257 participant group individuals in the ITT analyses, almost 40 percent of whom did not receive any ACJRP services at all. Because only 61 percent of the randomized participant group enrolled into ACJRP, the ITT results may not have as much practical relevance to practitioners or decision-makers interested in implementing similar programs.

A similar caveat is also applicable to the TOT analyses, in that the TOT sample was very small ($n = 66$ total) and therefore statistically underpowered. Statistical power is a statistical test's ability to correctly detect a statistically significant finding if an intervention truly has an effect. In general, studies with larger sample sizes have more statistical power, whereas studies with smaller sample sizes have lower statistical power (Cohen, 1988). For this study, the TOT sample was small because it includes the 33 individuals who completed the 18-month ACJRP program and the 33 matched comparison group individuals. Therefore, it is not surprising that most of the TOT results were not statistically significant. Although the TOT group is relevant to various audiences that are interested in the effects of completing the full ACJRP program (researchers, evaluators, and those interested in implementing a similar program), this study's TOT analyses would need a larger sample size (i.e., a larger group of individuals who completed ACJRP and therefore a larger matched comparison group sample) in order to have more statistical power to detect a statistically significant effect if the ACJRP program had one.

It is critical that the negative or unfavorable ACJRP results must be interpreted with data limitations in mind. The impact evaluation had two levels of recidivism measures—statewide (arrest data from CA DOJ) and countywide (charges, convictions, sentences, and petition violations data from Alameda County's CRIMS). One of the eligibility requirements for the study was that individuals had to live in Alameda County. However, this eligibility criterion was later revoked in order to open ACJRP program services to individuals experiencing homelessness. Participant group individuals who enrolled into ACJRP inherently had to remain in Alameda County in order to participate continuously in the program, go to court visits, and receive program services. On the other hand, control group individuals did not engage in any programming and may have left the county during the study's 24-month observation period, especially given the national trend during the pandemic of individuals leaving areas with high housing and rent prices, such as California's Bay Area. Control group individuals who left the

county could have been recidivating outside of Alameda County, but their criminality would have been picked up only by the CA DOJ data, not by the Alameda County–specific measures.

Additionally, one must appear in court to be charged and subsequently sentenced. Court notices are typically sent via mail. **Bench warrants become an issue when individuals cannot be found because they have moved, been evicted, or are experiencing homelessness or other unstable living conditions. On the other hand, it is easier to find individuals if they are participating in a program, such as ACJRP, and are therefore more likely to appear for court visits—a common activity mentioned by both peer coaches and ACJRP participants in the interviews.** Because the participant group was more likely to stay in the county and also more likely to go to court, it is possible that they were more likely to have a new charge, sentence, or probation violation in the county. The statewide CA DOJ data circumvents the issue of losing contact with the control group. As discussed in detail in the “Gaps in Statewide Criminal Justice Data” section, arrests were the only reliable outcome data in the CA DOJ dataset. That is why CA DOJ arrests was the only outcome measure for the PFS study’s outcome payment. It is noteworthy that the evaluation study’s negative ITT results were found only for the county-specific measures (i.e., and not for the statewide arrest data). Readers should keep in mind that the county-specific measures may not reflect an accurate assessment of the control group’s criminality if they left Alameda County and/or were less likely to appear for court notices.

The significant interaction term in the ITT analyses indicated that the difference between the participant and control groups’ risk for recidivism varied by race/ethnicity; BIPOC participant group individuals had the highest recidivism rates compared with White participant and control group individuals and BIPOC control group individuals. The history of racial and ethnic disparities in the criminal justice system has been well documented, with much research demonstrating that the disparities continue today (Hinton et al., 2018; The Sentencing Project, 2018). As a case moves through the criminal justice system, there are many decision points made by different parties (e.g., whether a law enforcement officer decides to make a formal arrest or whether a case is moved forward in court or deferred). Explicit and implicit biases can play a role at each of these decision points. The combination of potential disparities in the criminal justice system, control group individuals possibly leaving the county, and the participant group individuals being more likely to appear for court are possible explanations for the significant interaction finding.

In response to the COVID-19 pandemic, many parts of the criminal justice system adjusted their operations to reduce in-person contact and to prevent the spread of the virus. For example, in some states individuals were released from prison early, though most of the prison population decline was due to a decline in the number of individuals admitted. A report by the Council of State Governments Justice Center (2022) found that this decline in prison admissions in 2020 was due to changes in offending behaviors, local law enforcement, community supervision, and

court operations. This study's observation period for recidivism outcomes spanned from August 2018 to August 2021, capturing the first 18 months, or 1.5 years, since California's shelter-in-place ordinance began in March 2020. As such, this study's recidivism results should be interpreted with the pandemic and its impacts on the criminal justice system in mind.

Summary of Implementation Findings

Results indicate that the ACJRP program was implemented well for those who completed the program. According to peer coaches, peer coach supervisors, and ACJRP participants who participated in focus groups and interviews, ACJRP appeared worthwhile in that it helped both ACJRP participants and the peer coach staff through the process of reentering their communities.

The implementation findings indicated that ACJRP provided a range of services:

- Despite delays in funding when ACJRP was first implemented—and, subsequently, a client-to-peer-coach ratio higher than intended, the ACJRP program enrolled 154 participants over the span of 12 months. This represented 60 percent of the 257 individuals who were randomized to receive the opportunity to enroll into ACJRP.
- Of the 154 participants who enrolled into ACJRP, 33 individuals (21 percent) completed the 18-month program, indicating challenges with retention for the long-term program.
- The majority of the ACJRP participants (approximately 75 percent) were 25 to 34 years old, male, and either Black or African American or Hispanic.
- Participants who completed the ACJRP program received on average a total of 212.9 hours of services from LFCS: 162.3 hours from direct services and 50.6 hours from indirect services or support. This 212.9 hours is equivalent to approximately 27 days of services over the span of 18 months.
- In contrast, those who enrolled into ACJRP but did not complete the program received, on average, a total of 55.0 hours of services from LFCS: 37.0 hours from direct services and 18.0 hours from indirect services or support. This 55.0 hours is equivalent to approximately 7 days of services. Those in the previously enrolled group were engaged in the ACJRP program for five months on average.
- For those who completed ACJRP, 74 percent of the direct service provision time was for prosocial companion and structured leisure activities, which included mentoring and the ACJRP group sessions.
- Of the time LFCS staff spent on administrative tasks that supported those who completed ACJRP, most of the time was spent on legal support (23 percent), basic needs and social services (21 percent), prosocial companions such as family members (19 percent), housing (16 percent), and document support (10 percent).
- ACJRP participants also received additional supports, such as housing funds, gift cards, and coenrollment in REP, which was provided by a partnership between LFCS and BOSS

and included job readiness services such as résumé building, job searches, interview preparation, and work readiness training.

The ACJRP program not only provided employment supports and opportunities to the participants, it also provided employment supports and opportunities to formerly incarcerated individuals who were hired to serve as peer coaches for the participants. These supports are critical, as employment is an important factor that reduces the likelihood of recidivism (Bahr et al., 2010; Bonta & Andrews, 2017; Decker et al., 2015; Holzer et al., 2002; Lockwood et al., 2012). The implementation findings indicate that ACJRP leveraged and trained peer coaches in the following ways:

- At the height of the ACJRP program, LFCS employed 12 staff members to support the program and its participants. The peer coaches and peer coach supervisors had diverse backgrounds. Some were former clients of other LFCS programs, some were referred to LFCS through word of mouth, and others were peer coaches for another LFCS program and transferred into ACJRP.
- According to peer coaches and peer coach supervisors, their responsibilities included mentoring or being role models, sharing their experiences, conducting check-ins with participants, providing on-call support, assisting with systems navigation, connecting participants with resources, and accompanying participants on court visits.
- All peer coaches and peer coach supervisors participated in a certified 10-day peer specialist credential training, which resulted in them becoming certified in Medicare, Medi-Cal, and peer support in criminal justice settings. Completing the certified peer specialist training demonstrates the peer coaches' expertise and can open up a wider range of future employment opportunities for them, such as working with agencies that can be reimbursed by government funders. Peer coaches also received on-the-job scaffolded peer mentoring onboarding that lasted from two to four weeks. They received additional training, such as training on HIPAA, motivational interviewing, documentation, mandated reporting, de-escalation, and specific topics (e.g., reentry, suicide prevention, trauma).

Many ACJRP participants reported that their lives would have been different if they had never joined ACJRP. ACJRP showed them a way out of drugs and crime. Some participants reported that they were never ones to “plan anything” and that ACJRP provided them the scaffolding and skills they needed to identify their personal goals and make plans to meet the goals. They valued ACJRP services and resources such as job skills training, the peer coaches with lived experiences, the ACJRP group motivational classes, financial resources (e.g., rent money, stipends, and gift cards) to help with meeting basic needs, support for court visits, and

documentation support. **ACJRP participants' success stories include obtaining permanent housing and employment, reducing their felonies, learning lifelong coping skills, staying away from old friends, being a better person, and having more stability in their lives.**

When participants were asked about areas for program improvement, they recommended clearer communication about ACJRP program completion and their DEOJ and probation status, additional support after program completion, better housing options, additional staff members for participants to contact with questions, adding more options for the ACJRP group motivational classes in order to accommodate more schedules, and more SUD treatment.

Peer coaches and supervisors described deeply rewarding and multifaceted benefits from being a peer coach, despite experiencing challenges. **Peer coaches received certified training that will help with their future employment prospects, were motivated to continue on their rehabilitation journey, and were “walking evidence” that successful reentry is possible. The peer coach position made peer coaches see themselves differently, as role models to their clients as well as to the community. Peer coaches spoke overwhelmingly of how their work permeated their personal lives, yielding benefits such as improved relationships with friends and family as a result of their work.**

Many peer coaches and peer coach supervisors also discussed challenges such as wanting to help participants make changes before the participants were ready, managing relationships with clients, and being able to be fully “off the clock.” When asked about supports that would help peer coaches improve in their roles, individual suggestions included basic case management training, a manual of resources or strategies that peer coaches can refer to after training is completed, training on grant writing, training on working with various populations such as the LGBTQ+ community, and training or resources to help address the stigma associated with seeking mental health services.

LFCS staff described how the COVID-19 pandemic impacted the reentry population, most notably job loss caused by the pandemic and its downstream effects on individuals' ability to meet basic needs such as food and housing. LFCS quickly adapted its work to best serve its participants through various methods—supporting ACJRP participants remotely (e.g., mailing documents rather than requiring in-person contact, hosting group sessions on Instagram Live), increasing workforce development support and the number of food drives, and working collaboratively with ACJRP participants to develop plans to weather the shelter-in-place ordinances and the pandemic's other challenges. Other areas of difficulty in providing reentry support during the pandemic included SUD treatment and reaching individuals experiencing homelessness who were unable to tap into digital resources and information-providing services.

Lessons Learned

What follows are lessons learned and factors to take into consideration when implementing a program similar to ACJRP (i.e., a reentry program that leverages peer coaches with lived experiences), such as the peer coach model, training, and program retention. We describe the cross-sector collaborations that facilitated the implementation of ACJRP and how the successes and partnerships from the ACJRP work have informed the development of a new diversion program in Alameda County. Finally, we end with a discussion of lessons learned regarding PFS studies and gaps in statewide criminal justice data.

Peer Coaching Model and Training

The service provision data showed that mentoring represented a large proportion of the service provision time; this aligns with the finding that peer coaching is at the heart of the ACJRP program model. The qualitative results from the implementation evaluation underscored the usefulness of certified training for the peer coaches. If peer coaching is going to be the crux of the reentry program, it is critical to continue providing high-quality peer specialist training from a certified provider paired with ongoing supports to ensure success for the peer coaches and ultimately the ACJRP participants they serve.

Peer coaches expressed that they could benefit from additional training in case management, grant writing, working with special populations such as the LGBTQ+ community, and mental health stigma and that they would welcome a reference manual of resources and strategies. The certifications that peer coaches received may benefit them in the future as they seek employment as formerly incarcerated individuals. Future reentry programs that utilize peer coaches should invest in training the coaches to ensure their success in the program and beyond. Future reentry programs may also want to examine implementation fidelity to assess adherence to the peer coaching model and peer coaching training, determine where there are deviations, and identify areas for improvement.

The peer coaches and supervisors supported the peer coach model's effectiveness, primarily due to peer coaches' ability to resonate with formerly incarcerated individuals reentering their communities. The peer coaches noted that their own reentry experiences made it easier for them to relate to their clients. And because peer coaches had often navigated the same systems themselves, it was easier for them to help others navigate these systems.

Program Retention

The TOT arrest analyses that compare the completed group with the matched comparison group showed promising results. However, retention was low, with approximately one in every five individuals who enrolled into ACJRP completing the program. One possible explanation for low retention is that asking individuals to consistently engage in a program for 18 months is too high a bar to meet for successful program completion. Another possible explanation came from the interviews with ACJRP participants, who described seeing other ACJRP participants

struggling with SUD issues and suggested that this was a reason other participants did not successfully complete ACJRP. Reentry service providers may want to consider collecting information to better understand whether there are barriers to accessing or benefiting from community-based reentry services. A possible source for this information is individuals who enrolled but did not complete the program. This information may provide insights into how reentry programs can be tailored to specific individuals according to their needs and the barriers they face.

To learn more about the barriers to program completion, WestEd attempted to interview individuals who did not complete the program but was not successful in getting them to participate in the interviews. Canvassing individuals through surveys or interviews about their awareness of various reentry services, level of satisfaction with currently available services, and perceived barriers is critical for reaching the population who may need services the most. Future studies may also want to examine and link program implementation—such as specific criminogenic risk and needs assessment tools used at program intake, incorporating criminogenic needs treatment planning, and different types of program delivery (e.g., separating lower-risk and higher-risk individuals)—with retention and program outcomes.

Though the number of completed participants was small, which limits the ability to generalize the results, there was a higher percentage of females in the completed group compared with the previously enrolled group. It is unclear whether there were particular supports that better facilitated females' success, such as LFCS's efforts to match female participants with female peer coaches. Though WestEd attempted to recruit many individuals for the ACJRP participant interview, only four individuals agreed to participate in the interviews. With a larger interview sample, we would have been able to examine trends by participant characteristics, such as whether females experienced the program differently than males did.

In addition, ACJRP targeted young adults aged 18 to 34. Although only 26 percent of those referred to the participant group were 18 to 24 years old, 39 percent of the completed group was in this age range, suggesting that younger participants were more likely to complete the ACJRP program than their older peers were. For future reentry programs, it will be important to explore whether there are specific supports for individuals aged 25 and older that are more helpful in ensuring their continued program participation.

Lastly, the study sample was racially/ethnically diverse and its racial/ethnic makeup differed from that of Alameda County's jail population. In 2019, Alameda County's jail population was approximately 38 percent Black or African American, 30 percent Hispanic, 23 percent White, and 8 percent Asian or Pacific Islander (California State Auditor, 2021). In contrast, the completed group had a larger representation of Asian or Pacific Islander individuals (15 percent) and an underrepresentation of White individuals (12 percent). Those who completed the program were more likely to be of an "other" race/ethnicity (including American Indian, Asian Indian, Chinese, Filipino, Hawaiian, Korean, Other Race, Other Asian, and Vietnamese) compared with the previously enrolled group, suggesting that those of an "other" race/ethnicity were more likely to be retained and complete the program.

Reentry program stakeholders may want to consider examining whether there are race/ethnicity differences in program enrollment and completion and investigating whether this variation is due to different needs, awareness about, or level of comfort with various services or other factors related to race/ethnicity. In particular, knowing whether individuals' use of services is tied to language or culture differences could indicate that reentry programs may want to consider adjusting their strategies for promoting reentry services to individuals and in the provision of services.

Cross-Sector Collaboration and Extensions of ACJRP Work

The program was implemented and evaluated with the participation of ACDAO, LFCS, BOSS, Reinvestment Fund, Third Sector, Alameda County Administrator's Office, and WestEd. These partners contributed to the development and implementation of ACJRP. The collaboration was notably impactful during program recruitment and enrollment:

- Streamlined data sharing among ACDAO, LFCS, and WestEd was critical in knowing who was eligible for the ACJRP program so that LFCS staff could enroll them into the program quickly.
- ACDAO granted LFCS staff specific access in the courts so that they could enroll eligible individuals while they were physically still in court.
- ACDAO included "green sheets" in dockets so that it was immediately obvious to all court staff which individuals were confirmed to be eligible for ACJRP.

The process of program implementation also showed markers of deep collaboration:

- ACDAO sponsored the certified peer coach training for LFCS staff.
- LFCS and ACDAO staff attended the multiday certified peer coach training together. Attending the training together allowed both agencies to coconstruct a better understanding of the peer coaching model and demonstrated that they were partners in the ACJRP work.
- Similar to LFCS, ACDAO adopted the peer coaching model by hiring formerly incarcerated individuals to support ACJRP. One staff member was brought on to help facilitate the ACDAO-LFCS-WestEd information sharing needed for the randomization and enrollment process. Another staff member was a community leader and was hired for his expertise in finding housing and employment opportunities.

The partnerships underlying the ACJRP program demonstrated that close collaboration between the service provider and government agency and streamlined processes for information sharing are needed for future reentry programs.

LFCS and WestEd also collaborated in building data collection tools and a data dashboard in order to collect data on the PFS metrics and improve LFCS' capacity to serve participants:

- LFCS and WestEd collaborated to create implementation data tools that LFCS used to track and improve services. What data was to be collected by the tools was determined by identifying key information of interest to LFCS (e.g., participants' needs at intake, the zip codes participants lived in, service components) through iterative, reciprocal exchange between LFCS and WestEd. This information was also needed for the evaluation of the program.
- LFCS knew they would eventually enter the intake form and service provision into spreadsheets, so they wanted to collect participant information with an electronic form so that the information could be easily collated and data could be acted on in a timely manner.
- WestEd created digital forms for LFCS that facilitated the population of data into a data dashboard, allowing LFCS to examine process and implementation portions of their logic model in real time.
- The data dashboard supported ongoing, data-driven program improvement by providing insight into the most common types of services and the “pipeline” of participants (e.g., enrollment rate, service hours spent overall for each participant; see the “Pay for Success Projects” section for more information).

As another sign of the deep partnership that stemmed from ACJRP, LFCS and ACDAO have collaborated to implement the new Community Assessment, Referral and Engagement Services (CARES) Navigation Center, also commonly referred to as the 3-D Program (for “deflect, defer, divert”). The CARES Navigation Center is an extension of ACJRP—it is a peer coach diversion program that provides services similar to those that were provided by ACJRP (referrals for housing, employment, mental health services, and SUD treatment), though for a different population: individuals suspected of committing a low-level offense but who may display mental health or SUD struggles. With this new program, when police officers come into contact with this population, individuals will be offered the option of being transported to the CARES Navigation Center to receive services from peer coaches and clinicians instead of being transported to jail. Though the PFS funding has ended for LFCS, they continue to reap the learnings and successes of ACJRP. For instance, one ACJRP peer coach was hired to be the lead support specialist for the CARES Navigation Center and was later promoted to supervisor.

Gaps in Statewide Criminal Justice Data

The lack of consistent data and a lack of electronic-data sharing across counties in California are well-documented obstacles for the systematic collection of statewide data. For the ACJRP evaluation and the PFS study, WestEd needed to be able to analyze data from various counties and the various agencies an individual encounters in the criminal justice system in order to better understand ACJRP's effect on recidivism. In WestEd's investigation of CA DOJ data, we learned that charges and sentences were not measured reliably across the state, and we found delays in the measures (i.e., delays between when the event occurred and when the data appeared in the CA DOJ data). Indeed, other researchers have found that up to 60 percent of

CA DOJ arrest records are missing disposition information due to different legal interpretations regarding whether criminal court records fall within California’s CORI statutory scheme (Rabinowitz et al., 2019). Additionally, California does not have a centralized system in which agencies enter record-level information using standardized terms. Instead, CA DOJ must rely on agencies sending information in whatever format they collect it, using their own local nomenclature. Further, some agencies send paper records, which CA DOJ staff must enter manually into their systems, causing further delays in the availability of statewide data.

Because only arrests were measured reliably across the state and continuously submitted to CA DOJ in a timely fashion, WestEd used arrests as the main outcome for the PFS study’s 24-month observation period. We had to rely on Alameda County data for the supplemental recidivism measures (i.e., charges, convictions, sentences, and probation violations) for the larger evaluation, with the caveat that these events occurred only in Alameda County. As a point of comparison, 29 percent of the arrests in the arrest recidivism analyses occurred outside of Alameda County, suggesting that a substantial proportion of recidivism along the other measures (as defined by new charges, convictions, sentences, or probation violations) were not captured in this report’s analyses due to limitations in the statewide dataset. Additionally, as discussed in the “Summary of Impact Findings” section, these recidivism measures may have reflected a higher level of missing data for the control group individuals who moved out of county, especially during the COVID-19 pandemic, a data limitation when only county-specific recidivism data are available. Researchers, policymakers, and the public are not able to evaluate crime and recidivism trends that may or may not result from reforms and policy changes if we lack the real-time electronic-data sharing with partner agencies that is necessary to accurately access these numbers.

Pay for Success Projects

PFS projects are an innovative new strategy that shifts the financial risk in order to scale evidence-based programs and ensures that publicly funded services produce their intended outcomes. These projects also build and amplify the evidence base for promising programs, thereby advancing evidence-based policymaking. Data and metrics tracking are at the heart of PFS projects. What follows are data considerations for future PFS projects.

Having various metrics in place can help gauge how program implementation is going at multiple stages of the PFS project. For ACJRP, the PFS study had metrics tied to the following project milestones: the number of individuals randomized into the study, the number of individuals who enrolled into ACJRP, the number of individuals who met the engagement threshold by meeting the target number of service provision hours within two months of enrollment, the number of active ACJRP staff, and recidivism rates. The ACJRP PFS project partners reviewed the metrics regularly, which were updated by WestEd for the monthly partners meetings. The group reviewed the metrics together, assessed how the project was doing at the various project milestones, discussed factors contributing to successes, identified barriers, and problem-solved challenges. For instance, the enrollment metric was helpful for

comparing against LFCS’s bandwidth and assessing how many more clients the ACJRP peer coaches could take on. Other metrics were useful in determining how many ACJRP participants made it past the engagement threshold and in tracking program attrition.

Future PFS studies may need to provide data technical assistance to the local service provider. WestEd built data systems for LFCS to allow tracking of who was randomized into the participant group, who was enrolled, who disengaged, and when they were approaching their 18-month completion mark. WestEd built data dashboards that updated automatically so that LFCS could see “live” data in order to keep ahead of the various PFS metrics and inform their programming. The data dashboard also included data visualizations that summarized cities and demographics served, ACJRP services provided, external service referrals made, and needs assessment data from the intake form. Service providers can leverage data dashboard charts such as these and repurpose them in presentations and reports to stakeholders in order to demonstrate the populations they serve, the needs they address, and the services they provide.

Because data and metrics are at the heart of PFS studies, it is recommended to plan the data collection strategy at the beginning of the PFS study, taking into consideration the reporting needs across the various stakeholders. Having a list of the possible data and reporting requests at the outset of the project and building those data needs into the data collection plan—as opposed to trying to meet ad hoc reporting requests—can better prepare the PFS study to provide high-quality data points and allow the project partners to leverage the data for multiple purposes. Additionally, interim reporting, such as the monthly metrics updates, can lead to discussions that help gain and maintain buy-in from partners.

Finally, frequent and routine meetings with PFS partners are critical for troubleshooting and problem-solving. PFS projects involve many players, and it takes concerted effort across multiple agencies to solve unanticipated challenges. For example, in the early stages of the ACJRP study, the metrics suggested enrollment challenges. The ACJRP partners were able to address barriers quickly, such as granting LFCS staff special access in the court so that they could enroll ACJRP clients more quickly and updating the eligibility criteria and randomization ratio so that ACJRP services could be provided to more individuals. Regular partner meetings also allowed team members to celebrate accomplishments along the way and share success stories. ACJRP peer coaches would join the partner meetings frequently to share case studies of the clients they were working with, providing rich qualitative descriptions of how the ACJRP program was serving and impacting the community and reminding all partners of the “human” side of the ACJRP work.¹²

¹² Readers can visit [ACDAO’s Justice For All podcast](#) and access the second episode, “Second Chances,” to hear about the success stories of two ACJRP participants.

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Appendix A. Key Terms

Alameda County Justice Restoration Project (ACJRP). An 18-month reentry program that provided individualized coaching, navigation support, and intensive case management from peer coaches with similar lived experiences. Broadly, individuals eligible for ACJRP were young adults aged 18 to 34 in Alameda County who were on felony probation or were charged with AB 109 or 1170(h) felonies.

control group. Those who were referred to or randomized into the control group (i.e., received the business-as-usual condition instead of ACJRP services).

completed group. Those in the participant group who completed the 18-month ACJRP program (i.e., the ever enrolled group excluding the previously enrolled group). This group is also referred to as the enrolled group.

ever enrolled group. Those who were referred or randomized into the participant group and actually enrolled into ACJRP.

intent-to-treat (ITT) analysis. A type of analysis that compares the outcomes of all individuals based on the group they were originally assigned or randomized to. For this study, the ITT analysis compared those who were assigned or randomized to the participant group (regardless of whether they participated in ACJRP) with all individuals randomized to the control group.

participant group. Those referred to or randomized into the participant group to receive ACJRP services.

peer coach. Staff who go by different names—such as peer support specialist, peer mentor, or peer navigator—who are hired to assist clients with whom they share similar life experiences of addiction, recovery, or successful reentry. For this project, LFCS employed peer coaches to provide coaching, navigation support, and intensive case management to ACJRP participants.

previously enrolled group. Those in the participant group who were enrolled into ACJRP but later disengaged from the program. Specifically, they lost contact with ACJRP for 12 consecutive weeks or more.

recidivism. For this Pay for Success project, recidivism was defined as any new felony or misdemeanor arrest in the state of California during each study participant’s 24-month measurement period (i.e., the 24 months after being randomized into the study or, for individuals who were in custody during study randomization, the 24 months after being released). Technical violations of supervision requirements or any other nonstatutory criminal conduct, infractions, or technical petition violations were not categorized as recidivism. Beyond the Pay for Success project, the full evaluation of ACJRP also examined additional measures of recidivism: new charges, convictions, sentences (detentions and supervisions), and probation violations in Alameda County.

treatment-on-the-treated (TOT) analysis. A type of analysis that compares the outcomes of individuals according to the treatment they actually received (regardless of their originally assigned group). For this study, the TOT analysis compared the completed group (individuals who completed the 18-month program) to a matched subset of control group individuals who were similar on background characteristics (prior criminal history, age, sex, and race/ethnicity).

Appendix B. Technical Methods

Data Sources

The implementation evaluation included data from six sources: Alameda County District Attorney's Office's (ACDAO) DALITE case management system, the California Department of Justice (CA DOJ) Criminal Offender Record Information (CORI) database, service provision data from La Familia Counseling Service (LFCS), interviews and focus groups with LFCS peer coaches and peer coach supervisors, interviews with Alameda County Justice Restoration Project (ACJRP) participants, and service provision data from Alameda County Probation Department (ACPD).

The impact evaluation included secondary administrative data from three sources: the CA DOJ CORI database, ACDAO's DALITE case management system, and ACDAO's Consolidated Records Information Management System (CRIMS).

ACDAO Data

WestEd used ACDAO's DALITE and CRIMS data to (1) determine whether individuals were eligible for the study, (2) obtain release dates necessary for the recidivism analyses, and (3) measure the project's nonoutcome metrics that were not tied to the Pay for Success portion of the study (i.e., charges, convictions, sentences, and probation violations in Alameda County).

DALITE is used by ACDAO for their daily criminal caseload management. It has the functionality to file a case electronically with the court's system to manage the complete lifecycle of a case. During the recruitment stage of this project, ACDAO conducted daily queries of DALITE to identify individuals potentially eligible for this study. As described in the section "Randomization Process" later in this appendix, ACDAO sent WestEd the following DALITE information for individuals who were potentially eligible for the study: name, birth date, person file number (PFN), case event number (CEN), criminal identification and information (CII) number, whether the individual had a probation violation, and whether the individual had a prior felony. The DALITE information on the individuals referred to randomization formed the basis of the study's participant database.

CRIMS is a consolidated system that is used by all Alameda County public safety and criminal justice organizations. CRIMS includes criminal records and related information from multiple databases. After the recruitment stage was completed for this study, ACDAO provided the following information for all study participants (i.e., for both the participant and control

groups): PFN, CEN, arrest dates, release dates, reasons for release, conviction dates, probation violation dates, charge dates, sentenced detention dates, and sentenced supervision dates. WestEd used individuals' PFNs to merge the CRIMS data with the study participant database.

For the recidivism analyses, WestEd had to know for each individual whether they were in custody at the time they were randomized into the study. An individual's length of custody starts from the arrest date and ends at the release date. ACPD provided WestEd with the study participants' arrest and release data for any releases that occurred starting in July 2018 (one month before enrollment into the study began). WestEd used two variables—each participant's PFN and the CEN from the criminal event that made the participant eligible for the study—to merge the CRIMS custody data with the study participant database.

There were multiple custody scenarios. In the first scenario, an individual was not taken into custody for the criminal event that made them eligible for the study (i.e., the person was arrested, identified as eligible for the study and then randomized, was never taken into custody, and returned to the community). Thus, that individual did not have a release date associated with that criminal event. However, individuals can have multiple charges and thereby can have multiple simultaneous overlapping custody cycles. In the second scenario, an individual was arrested, identified as eligible for the study and then randomized, and remained in custody for some time before being released. Thus, for that individual, the release date for the study's criminal event was the final end of their custody. In the third scenario, an individual was arrested and taken into custody for the event that made them eligible for the study but was also in custody for another charge. Although that individual could have been released earlier for the criminal event that made them eligible for the study, they remained in custody for a longer period of time for the other charge. Thus, the later release date serves as the actual date the individual is released back to the community.

Using this sample of custodies, WestEd created a binary variable for whether an individual was in custody at the time of study randomization (0 = not in custody, 1 = in custody). For those who were in custody, WestEd used the final release date as the clock start date for the 24-month observation period for the recidivism analyses. If an individual's 24-month observation ended past August 2021, WestEd used their recidivism status (did or did not recidivate) as of August 2021 for the recidivism analyses (Lee, 1992).

The CRIMS database tracks individual-level criminal history data, including charges, convictions, sentences (detentions and supervisions), and probation violations. This study followed all individuals for 24 months. To examine recidivism, for each study participant, WestEd excluded any events (i.e., new charges, convictions, sentences, and probation violations) outside of the 24-month observation period starting from each participant's clock start date (either the randomization or release date). WestEd also excluded the event that made the individual eligible for the study from all analyses (arrests, charges, convictions, sentences, and probation violations) so that, for example, the qualifying arrest would not be mistakenly identified as recidivism in the charges analyses. Using this sample of events, WestEd created a binary

variable for whether an individual recidivated within the 24-month observation period (0 = did not recidivate, 1 = recidivated) for each of the different types of recidivism outcomes. WestEd calculated the time until recidivism as the number of days between the clock start date and the recidivism date (i.e., charge date, conviction date, detention sentence date, supervision sentence date, and probation violation date).¹³

CA DOJ Data

WestEd used CA DOJ CORI data (1) to describe the characteristics of the individuals participating in ACJRP, (2) to assess baseline equivalence between the participant and control groups, (3) as the source for covariates in the analytic models, and (4) to measure the main project outcome: new felony and misdemeanor arrests in the state within each individual's 24-month observation period. CA DOJ provided WestEd with data on study participants' demographic characteristics (race/ethnicity and sex). WestEd used the participants' first name, middle name, last name, suffix, and birth date to merge the CA DOJ data with the study's participant database.

Binary variables were created for analytic purposes. The following dummy variables (0 = no, 1 = yes) were created to include in analytic models: whether an individual was male; whether an individual was Black, Indigenous, or a Person of Color (BIPOC); and whether the individual was 25 to 34 years old at the time of randomization (0 = 18 to 24 years old, 1 = 25 to 34 years old).

The race/ethnicity categories in the CA DOJ included American Indian, Asian Indian, Black or African American, Chinese, Filipino, Hawaiian, Hispanic, Korean, Other, Other Asian, Vietnamese, and White. For analytic purposes, race/ethnicity was dichotomously coded as a binary variable for whether the participant was BIPOC (0 = White, 1 = BIPOC).

The CA DOJ CORI database tracks individual-level criminal history data, including arrest date, type of arrest (e.g., misdemeanor, felony), city and county of arrest, and the offense description. To examine recidivism, for each study participant, WestEd excluded any arrests outside of the 24-month observation period starting from each participant's clock start date (either the randomization or release date). WestEd also excluded any arrests that were not misdemeanors or felonies (e.g., infractions). In addition, using the offense descriptions, WestEd excluded any court-related arrests from the recidivism analyses (e.g., bench warrants or failures to appear). Using this sample of arrests, WestEd created a binary variable for whether an individual recidivated within the 24-month observation period (0 = did not recidivate, 1 = recidivated). WestEd calculated the time until recidivism as the number of days between the clock start date and the arrest date.

WestEd originally planned on using baseline risk assessment scores from ACPD (i.e., risk assessment scores from before study randomization) as a covariate for the recidivism analyses

¹³ For individuals who recidivated multiple times, the date from the first recidivating event (measured by CRIMS and/or CA DOJ data) was used to calculate time until recidivism.

because risk assessment scores should predict recidivism. However, 51 percent (228 of 450) of the study sample was missing baseline risk assessment scores (control group: 47 percent, or 91 of 193, were missing data; participant group: 53 percent, or 137 of 257, were missing data). Thus, WestEd instead used the CA DOJ CORI data—which was available for all study participants—to create a variable that captured each individual’s prior criminal history as a covariate for the recidivism analyses. Specifically, WestEd calculated for each individual the number of felony arrests in the year prior to study randomization. WestEd excluded any arrests that were not felonies (e.g., misdemeanors, infractions) and any court-related arrests (e.g., bench warrants or failures to appear).

LFCS Data

WestEd collaborated with LFCS to develop an electronic data collection tool to track enrollment and service provision data. What data was to be collected by the tool was determined by identifying key information of interest through iterative, reciprocal exchange between LFCS and WestEd. Data from the data collection tool presented unduplicated counts of participants served by LFCS. Additionally, data from the data collection tool allowed WestEd to report the number of hours LFCS provided for each service, the number of hours for services overall, and the frequency and duration of coaching sessions. LFCS also provided WestEd data on additional resources they provided ACJRP participants, including housing funds and Reentry Employment Program (REP) participation data.

ACPD Data

WestEd used data from ACPD to describe the business-as-usual, or the counterfactual, condition (i.e., probation services and service referrals that control group individuals received) and probation service referrals and programming that participant group individuals received in addition to ACJRP services. ACPD also provided WestEd with risk assessment scores to be used as a covariate in the analytic models. However, due to the high level of missing data, WestEd did not include the risk assessment scores in the analyses but, instead, used criminal history information from the CA DOJ.

ACPD provided WestEd with data on service referrals (type and date) and service participation (type and date). WestEd used the individuals’ PFNs to merge the ACPD data with the study’s participant database. The resulting merged data file contained both the individuals’ randomization group status (participant or control group) and data on ACPD program service referrals and program enrollment (i.e., ACPD program services provided to all individuals in Alameda County, regardless of ACJRP participation). Of the 450 individuals in the ACJRP study, 187 individuals (42 percent) received referrals from ACPD for program services and/or enrolled into the program services. Of the 187 individuals who received ACPD program referrals and/or enrolled into program services, 113 individuals (60 percent) were individuals from the participant group and 73 individuals (39 percent) were from the control group.

There were four steps undertaken in the ACPD data cleaning process. For the first step, individuals in the merged dataset who did not receive referrals to any ACPD program services were excluded from the ACPD program services analysis. Next, in instances where a single individual had multiple rows of data for the same program referral, the rows with duplicate program referral data were collapsed. This resulted in a deduplicated dataset that contained unique program referrals for each participant. For the third step, cases with the same program service referral but different enrollment dates were also filtered out; the cases with the earliest enrollment dates were retained. Finally, the data set was filtered to ensure program referrals and enrollments occurred within 24 months of each individual's randomization date. Thus, only ACPD program referrals and enrollments that occurred during the study's 24-month observation period were included in the analysis.

After the cleaning process, the final analysis sample included 68 individuals (15 percent) of the study sample (68 of 450). Of the 68 individuals, 32 individuals from the participant group and 36 individuals from the control group were referred to at least one ACPD program service during the 24-month observation period. In total, these 68 individuals were referred to ACPD programming 86 times. There were only two cases in which the referred individual was not enrolled into the service, which means that as a result, these 68 individuals received ACPD programming services on 84 different occasions.

Interviews and Focus Group with LFCS Staff

WestEd conducted one-hour interviews with all LFCS peer coaches and peer coach supervisors (four peer coaches and two peer coach supervisors) in October 2019. The purpose of the interviews was to glean information about the career pathways of the peer coaches and supervisors, the peer coach model, and the training and supports received. WestEd took the lead in developing the interview protocols, and LFCS's program director reviewed the interview protocols and provided additional interview questions related to the impacts of the peer coaching position on the peer coaches' lives. After the protocols were developed, a peer coach supervisor provided feedback and the protocols were revised.

Each peer coach and supervisor participated in a 60-minute semistructured interview. One supervisor participated in a 90-minute interview because their interview included eliciting their feedback on the peer coach interview protocol. The tenure of peer coaches in their current position was an average of 9 months (the range was 3 to 12 months). The two peer coach supervisors had been in their position for 13 and 24 months. All interview participants provided informed consent prior to participating in the interviews. Participants were given a gift card for their participation. One research team member conducted the interview with a second research team member taking notes during the interview.

WestEd also conducted a one-hour virtual focus group over Zoom with four peer coaches and two peer coach supervisors in May 2020 to learn about pandemic-related adjustments to program implementation and to hear from LFCS staff about how the pandemic had impacted ACJRP participants.

Interviews with ACJRP Participants

In November 2021, WestEd conducted 45-minute phone interviews with four individuals who had completed the ACJRP program. The purpose of the interviews was to learn about the perceived effects, successes, and challenges of ACJRP and to gather suggestions from ACJRP participants.

WestEd took the lead in developing the semistructured interview protocols. The interview protocol was informed by prior research (Labriola et al., 2018) and included questions regarding ACJRP participants' understanding of program requirements, decision to participate in ACJRP, services received, perceptions of peer coaches, perceived program benefits and challenges, pandemic impacts, and suggestions for improvement. After the protocols were developed, a former ACJRP peer coach assisted with interview recruitment by contacting previous ACJRP participants, which resulted in a convenience sample. If the ACJRP participants were interested in participating in the interview, the peer coach shared the participants' contact information with a WestEd staff member for interview scheduling. WestEd originally planned on interviewing two groups of ACJRP participants—those who had completed the ACJRP program and those who had started but did not complete the program (i.e., previously enrolled). Though the peer coach reached out to previously enrolled individuals, none were interested in participating in the interviews. Thus, WestEd was able to interview only those who completed the ACJRP program. All interviewees had completed their 24-month observation period for the study. LFCS was not involved in facilitating the interviews or the qualitative analysis and did not have access to the qualitative interview data.

Participants were informed both verbally and in writing that participation in the interview was voluntary and that they could opt out of the interview at any time. In addition, WestEd reminded participants that the evaluation was not interested in a specific peer coach, their names would not be included in the interview notes, and the interview notes would not be shared with LFCS. If an interviewee indicated any sign of discomfort during the interview, the interviewer reminded them that their participation was voluntary and they could share what they are comfortable with. At the end of the interview, WestEd asked interviewees if they consented to WestEd using a direct quote without their name in the report. Interviewees received a \$50 digital Target gift card after completing their interview. Interviews were conducted by pairs of WestEd researchers. One researcher facilitated the interview while another staff took detailed notes.

Study Design

Randomization Process

Randomization of eligible individuals into either the participant or control group was conducted on a rolling basis from August 2018 to August 2019 to accommodate the continuous nature of enrollment. ACDAO had two sources of referrals for individuals eligible for the study. The first referral source was a daily query of ACDAO's DALITE case management system. The second referral source was for defendants who had had charges reduced, subsequently making them eligible for the study. The charging deputy would then refer the defendant to ACJRP. In contrast to the first referral source, the second source was not an automated process.

Every day ACDAO securely transferred a list of eligible individuals to WestEd for study randomization. The list included each individual's name, birth date, PFN, CEN, CII number, whether the individual had a probation violation, and whether the individual had a prior felony. WestEd randomly assigned individuals either to the participant or control group using Stata's random number generator. At the beginning of the study, WestEd utilized a 1:1 ratio for randomization (i.e., one individual randomized to the participant group for every individual randomized to the control group). In February 2019, the randomization ratio was updated to 2:1 (i.e., two individuals randomized to the participant group for every individual randomized to the control group). This change was made in order to enroll and provide ACJRP services to more individuals. After randomization, WestEd transferred back to ACDAO the same list they had sent WestEd, but the new list included only individuals who were randomized into the participant group (i.e., those who were randomized to the control group were dropped from the new list). WestEd typically conducted randomizations within 30 minutes of receiving the list of eligible individuals so that ACDAO and LFCS could start the ACJRP enrollment process while the individual was still in court.

Identification of Matched Comparison Groups

WestEd conducted a quasi-experimental treatment-on-the-treated (TOT) analysis (i.e., individuals who successfully completed the 18-month program) to investigate the impact of receiving the full suite of ACJRP services. WestEd identified the participant group individuals who had completed the program, and then compared this group to a matched subsample of individuals selected from the control group. As such, 33 participant group individuals completed the 18-month program and were in the TOT sample (referred to as "TOT participant group" from this point forward). WestEd used Stata's psmatch2 module to conduct propensity score matching. Propensity score matching attempts to create equivalent treatment and comparison groups by summarizing relevant pretreatment, or baseline, characteristics of each individual into a single-index variable (the propensity score) and then matching individuals in the comparison group to individuals in the treatment group based on values of the single-index variable (Rosenbaum & Rubin, 1983). WestEd included the following baseline characteristics in

the propensity score matching analysis: past criminal history (number of felony arrests in the state in the year prior to randomization), race/ethnicity, sex, and age at randomization.

Propensity score matching was conducted three times: once for the CA DOJ arrest analyses, once for the Alameda County convictions analyses, and once for the remaining Alameda County recidivism measures (charges, sentences, and probation violations). WestEd conducted the matching separately because the three outcomes had different sample sizes due to varying missing outcome data. Following suggestions from the What Works Clearinghouse (WWC), if there are multiple analysis samples, baseline equivalence must be established for each analysis sample (U.S. Department of Education, 2020).

Using propensity score matching with replacement (Guo & Fraser, 2010), a subset of individuals from the control group (referred to as the “TOT comparison group” from this point forward) was selected to match the TOT participant group individuals. The resulting analysis is quasi-experimental in nature, as matching only creates groups that are similar on known characteristics. Propensity score matching with replacement allows a control group individual to be matched multiple times (i.e., with multiple different enrolled group individuals). Thus, WestEd used weights for the baseline equivalence tests and impact analyses, with the comparison group individuals weighted by the number of times they were matched to the TOT participant group. After matching, the TOT analysis included 33 participant group individuals and 33 comparison group individuals (a total of 66 individuals) for all three outcomes (arrests, convictions, and the remaining Alameda County recidivism measures: charges, sentences, and probation violations). Because matching was conducted with replacement, there was an equal number of TOT comparison group individuals in all analyses.¹⁴ All participant group individuals who did not complete the 18-month program and unmatched control group individuals were excluded from the TOT analyses.

Baseline Equivalence Testing of Participant and Control Group Comparisons

Statistical tests were conducted to assess the equivalence of the participant and control groups that resulted from randomized assignment (ITT sample) and from propensity score matching (TOT sample). A series of t-tests was conducted with group membership (participant versus control) as the independent variable and each of the demographic variables (criminal history, age at randomization, sex, and race/ethnicity) as the dependent measure.

ITT Sample

Following suggestions from the WWC, WestEd examined whether there was baseline equivalence for each outcome’s analysis sample (U.S. Department of Education, 2020). According to Ho and colleagues (2007), an acceptable level for minimizing bias between groups is a difference between the intervention and comparison groups on measured characteristics prior to the intervention that is less than a quarter of a standard deviation (i.e., an effect size of

¹⁴ All TOT analyses used the frequency weights option in Stata to create equal sample sizes.

0.25). Per WWC guidelines (U.S. Department of Education, 2020), WestEd calculated effect sizes for continuous variables (e.g., number of felony arrests in the year prior to randomization) using Hedges's g and calculated effect sizes for binary variables (e.g., sex) using Cox's index. No unacceptable group differences emerged between the participant and control groups for all three analysis samples (Table B1 for the arrests sample, Table B2 for the convictions sample, and Table B3 for the charges, sentences, and probation violations sample). Following suggestions by the WWC, differences in baseline characteristics that were between 0.05 and 0.25 standard deviations were statistically adjusted (U.S. Department of Education, 2020). Thus, race/ethnicity and sex were included as covariates in the ITT analyses of all three outcomes.

Table B1. Baseline Equivalence on Demographics Between Participant and Control Group Individuals: Arrests Analysis Sample

Demographic	Control Group: MEAN (n = 192)	Control Group: SD (n = 192)	Control Group: MIN (n = 192)	Control Group: MAX (n = 192)	Participant Group: MEAN (n = 254)	Participant Group: SD (n = 254)	Participant Group: MIN (n = 254)	Participant Group: MAX (n = 254)	Diff.	p-value	ES
Number of felony arrests in the year prior to randomization	2.26	1.68	0	10	2.26	1.59	0	10	0.00	0.98	0.00
BIPOC	0.74	0.44	0	1	0.81	0.39	0	1	0.07	0.09	0.16
Male	0.76	0.43	0	1	0.80	0.40	0	1	0.04	0.33	0.09
Age 25–34 at randomization	0.75	0.43	0	1	0.74	0.44	0	1	-0.01	0.81	-0.02

Note. N = 446. Although 450 individuals were randomized into the study, four individuals were excluded from the recidivism analysis sample due to missing CA DOJ data. All variables, except for number of prior felony arrests, are dichotomous variables (0 = no, 1 = yes), with means interpreted as percentages (e.g., 76 percent of the control group was male).

Table B2. Baseline Equivalence on Demographics Between Participant and Control Group Individuals: Convictions Analysis Sample

Demographic	Control Group: MEAN (n = 192)	Control Group: SD (n = 192)	Control Group: MIN (n = 192)	Control Group: MAX (n = 192)	Participant Group: MEAN (n = 255)	Participant Group: SD (n = 255)	Participant Group: MIN (n = 255)	Participant Group: MAX (n = 255)	Diff.	p-value	ES
Number of felony arrests in the year prior to randomization	2.29	1.75	0	10	2.25	1.60	0	10	-0.04	0.80	-0.02
BIPOC	0.74	0.44	0	1	0.81	0.39	0	1	0.07	0.09	0.16
Male	0.76	0.43	0	1	0.80	0.40	0	1	0.04	0.37	0.09
Age 25–34 at randomization	0.74	0.44	0	1	0.74	0.44	0	1	0.00	0.93	-0.01

Note. N = 447. Although 450 individuals were randomized into the study, three individuals were excluded from the recidivism analysis sample due to missing final convicted disposition outcome data. All variables, except for number of prior felony arrests, are dichotomous variables (0 = no, 1 = yes), with means interpreted as percentages (e.g., 76 percent of the control group was male).

Table B3. Baseline Equivalence on Demographics Between Participant and Control Group Individuals: Charges, Sentences, and Probation Violations Analysis Sample

Demographic	Control Group: MEAN (n =193)	Control Group: SD (n =193)	Control Group: MIN (n =193)	Control Group: MAX (n =193)	Participant Group: MEAN (n = 257)	Participant Group: SD (n = 257)	Participant Group: MIN (n = 257)	Participant Group: MAX (n = 257)	Diff.	p-value	ES
Number of felony arrests in the year prior to randomization	2.29	1.75	0	10	2.25	1.59	0	10	-0.05	0.78	-0.03
BIPOC	0.75	0.44	0	1	0.81	0.39	0	1	0.07	0.09	0.16
Male	0.76	0.43	0	1	0.80	0.40	0	1	0.04	0.30	0.10
Age 25–34 at randomization	0.75	0.44	0	1	0.74	0.44	0	1	0.00	0.94	-0.01

Note. N = 450. There was no missing data. All variables, except for number of prior felony arrests, are dichotomous variables (0 = no, 1 = yes), with means interpreted as percentages (e.g., 76 percent of the control group was male).

TOT Sample

Baseline equivalence for the TOT sample of each of the three outcomes was also assessed. Results indicated that the TOT participant group individuals and the matched TOT comparison group individuals were similar on baseline characteristics. Tables B4, B5, and B6 present the baseline equivalence test results between the TOT participant and TOT comparison groups for the three outcomes. In some cases (number of felony arrests in the year prior to study randomization, race/ethnicity, and sex), effect sizes were greater than 0.05, indicating that it was necessary to include statistical adjustments in the impact analyses to satisfy baseline equivalence requirements (U.S. Department of Education, 2020). However, none of the effect sizes were greater than 0.25; thus, the samples are considered to have baseline equivalence.

Table B4. Baseline Equivalence on Demographics Between TOT Participant and Comparison Group Individuals: Arrests Analysis Sample

Demographic	TOT Comparison Group MEAN (n = 33)	TOT Comparison Group SD (n = 33)	TOT Comparison Group MIN (n = 33)	TOT Comparison Group MAX (n = 33)	TOT Participant Group MEAN (n = 33)	TOT Participant Group SD (n = 33)	TOT Participant Group MIN (n = 33)	TOT Participant Group MAX (n = 33)	Diff.	p-value	ES
Number of felony arrests in the year prior to randomization	1.70	1.70	0	10	1.58	1.15	0	6	-0.12	0.74	-0.08
BIPOC	0.94	0.24	0	1	0.88	0.33	0	1	-0.06	0.40	-0.21
Male	0.76	0.44	0	1	0.73	0.45	0	1	-0.03	0.78	-0.07
Age 25–34 at randomization	0.61	0.50	0	1	0.61	0.50	0	1	0.00	1.00	0.00

Note. N = 66. There was no missing data. All variables, except for number of prior felony arrests, are dichotomous variables (0 = no, 1 = yes), with means interpreted as percentages (e.g., 76 percent of the TOT comparison group was male).

Table B5. Baseline Equivalence on Demographics Between TOT Participant and Comparison Group Individuals: Convictions Analysis Sample

Demographic	TOT Comparison Group MEAN (n = 33)	TOT Comparison Group SD (n = 33)	TOT Comparison Group MIN (n = 33)	TOT Comparison Group MAX (n = 33)	TOT Participant Group MEAN (n = 33)	TOT Participant Group SD (n = 33)	TOT Participant Group MIN (n = 33)	TOT Participant Group MAX (n = 33)	Diff.	p-value	ES
Number of felony arrests in the year prior to randomization	1.70	1.70	0	10	1.58	1.15	0	6	-0.12	0.74	-0.08
BIPOC	0.94	0.24	0	1	0.88	0.33	0	1	-0.06	0.40	-0.21
Male	0.76	0.44	0	1	0.73	0.45	0	1	-0.03	0.78	-0.07
Age 25–34 at randomization	0.61	0.50	0	1	0.61	0.50	0	1	0.00	1.00	0.00

Note. N = 66. There was no missing data. All variables, except for number of prior felony arrests, are dichotomous variables (0 = no, 1 = yes), with means interpreted as percentages (e.g., 76 percent of the TOT comparison group was male).

Table B6. Baseline Equivalence on Demographics Between TOT Participant and Comparison Group Individuals: Charges, Sentences, and Probation Violations Analysis Sample

Demographic	TOT Comparison Group MEAN (n = 33)	TOT Comparison Group SD (n = 33)	TOT Comparison Group MIN (n = 33)	TOT Comparison Group MAX (n = 33)	TOT Participant Group MEAN (n = 33)	TOT Participant Group SD (n = 33)	TOT Participant Group MIN (n = 33)	TOT Participant Group MAX (n = 33)	Diff.	p-value	ES
Number of felony arrests in the year prior to randomization	1.70	1.70	0	10	1.58	1.15	0	6	-0.12	0.74	-0.08
BIPOC	0.94	0.24	0	1	0.88	0.33	0	1	-0.06	0.40	-0.21
Male	0.76	0.44	0	1	0.73	0.45	0	1	-0.03	0.78	-0.07
Age 25–34 at randomization	0.61	0.50	0	1	0.61	0.50	0	1	0.00	1.00	0.00

Note. N = 66. There was no missing data. All variables, except for number of prior felony arrests, are dichotomous variables (0 = no, 1 = yes), with means interpreted as percentages (e.g., 76 percent of the TOT comparison group was male).

Data Analysis

Qualitative Data

After the peer coach and peer coach supervisor interview notes were cleaned and finalized, the notes were uploaded to Dedoose (Version 8.0.35) for coding and analysis. WestEd used a content analysis approach, whereby coders iteratively read through all narratives, inductively derived codes for emergent themes, compiled a list of codes, and analyzed for patterns (Charmaz, 2006). First, one analyst reviewed a sample of interview notes, developed a codebook, and applied the codebook. A second analyst reviewed the same sample of interviews and codes to see if there was agreement. The second analyst also suggested additional codes. Following agreement on the codes and codebook, the analysts developed exemplar quotes for each of the codes. After reaching agreement on the draft codebook, the first analyst coded all interviews and added additional codes that emerged from the remaining interview narratives. The second analyst reviewed and coded the same interview notes using the finalized codebook.

The final codebook consisted of codes associated with how individuals chose the peer coach or supervisor role, job responsibilities, trainings and supports received and desired, challenges and supports inherent to the work, self-care, and the impact and importance of the peer coach model. We found seven overarching themes that are reflected in the report: becoming a peer coach or supervisor, trainings for peer coaches and supervisors, peer coaches' roles and responsibilities, challenges peer coaches faced, positive impacts of peer coach work, social supports for peer coaches and supervisors, and suggested additional trainings.

For the peer coach focus group data, WestEd employed grounded theory—a series of cumulative coding cycles and analytic memoing to develop major categories for theory generation (Miles et al., 2014)—to analyze the focus group transcript. Toward this end, WestEd used descriptive coding of transcript data and created categories of major themes. These themes were then organized into similar clusters and aligned to the specific implementation evaluation questions to ensure the results were focused on needed information (Patton, 2002). Combined, the clusters of themes alongside the questions allowed for drawing conclusions, through the perspective of the peer coaches and peer coach supervisors, about pandemic-related adjustments to program implementation and how the COVID-19 pandemic impacted ACJRP participants.

For the interviews with ACJRP participants, WestEd took a summative approach to analysis and used the composite narrative methodology (Willis, 2019) to highlight the experience of ACJRP participants. The interview notes were double-coded by WestEd researchers, specifically coding for participants' experiences, thoughts, and feelings in order to capture what participating in the program was like for ACJRP participants. After researchers generated codes under the three parent codes, each excerpt was categorized as a participant characteristic, occurring before the program, in the program, or after the program. "In program" codes were further categorized as

program supports, group classes, goal setting, peer coach/staff, and transitioning out/program completion. Other categories were “negative” to specifically identify negative experiences or ideas for program improvement, and “enrollment” to identify experiences, thoughts, and feelings at the time of program enrollment.

Based on the codes, the researchers generated a composite narrative, which is a fictional narrative based on real details obtained from the interview data. Composite narratives are a helpful analysis output because they convey emotional truth from participants, which is particularly insightful for complex, situated accounts. The resulting composite narrative is an account that combines the experiences of multiple interviewees into one and allows for themes to be drawn out. It is important to note that the composite narrative does not assume motivations and feelings; any comments of this nature are taken directly from the interviews. To contextualize the report’s quantitative data, the analysts also incorporated quotes throughout the report to highlight specific aspects of an ACJRP participant’s experiences.

Quantitative Data

Quantitative data analysis for the implementation evaluation included summary statistics for describing the frequency and types of probation and ACJRP services provided and the characteristics of the individuals at the various points of the ACJRP program pipeline (eligible for program, program enrollment, program completion).

WestEd employed survival analysis (also known as event history analysis) for the impact analyses on recidivism outcomes within the 24-month observation period. Specifically, WestEd used Kaplan-Meier group comparisons of survival times and Cox regression models (i.e., Cox proportional hazard models) to examine the effects of ACJRP on time to recidivism (Lee, 1992). The goal of the survival analysis was to determine whether differences existed between the participant and control groups in terms of their recidivism rates and the amount of time that passed before they recidivated.

The Kaplan-Meier procedure is a descriptive approach and cannot include covariates (i.e., individuals’ background characteristics) as the Cox regression models can. Using the study’s data, a downward curve was calculated for the participant and control groups as more individuals recidivated over time. The Kaplan-Meier procedure is typically done before the Cox regression model and examines how the curves for the participant and control groups differ (Kaplan & Meier, 1958). The Kaplan-Meier procedure produces the Wilcoxon test and the log-rank test, both of which compare the number of events (e.g., new arrest) in each group during the 24-month observation window with the expected number if there are no differences between the participant and control groups. The Wilcoxon test puts more weight on earlier points in time whereas the log-rank test weights all parts of the survival curves the same. Although these two tests produce p -values and therefore indicate whether the differences are statistically significant, they should be viewed as exploratory because they are not based on an analysis that statistically controls for important covariates (e.g., race/ethnicity).

Cox regression (Cox, 1972) is the most commonly used multivariate approach for analyzing time until recidivism and is the primary approach WestEd used to determine whether the time until recidivism differed to a statistically significant extent across the participant and control groups. The Cox regression models determine group differences (between the participant and control groups) while controlling for other factors (e.g., age). To account for the continuous nature of the randomization process over the 12-month period, WestEd used a fixed effects model, which included a dummy variable for each randomization week.¹⁵ Using fixed effects disentangled the effects of time of randomization from the individual effects on the participant-level recidivism outcomes and controlled for time-related omitted variables and unobservables (Murnane & Willett, 2011). Below is an example equation for the Cox regression model with fixed effects for randomization week predicting the treatment effect on time to recidivism:

$$h(t) = h_0(t) \exp (b_1X_1 + b_2X_2 + \dots + b_pX_p),$$

where $h(t)$ is the expected hazard at time t , $h_0(t)$ is the unspecified baseline hazard and represents the hazard when all of the predictors (X_1, X_2, X_p) are equal to zero, b_1 represents the estimated treatment effect for the dichotomously coded ACJRP treatment variable (0 = control group, 1 = participant group), and b_2 to b_p represent the associations between the covariates (e.g., dummy variable for each randomization week) and the outcome. Depending on the results of the baseline equivalence tests, the suite of covariates could have included the individual's number of felony arrests in the year prior to study randomization, race/ethnicity (0 = White, 1 = BIPOC), sex (0 = female, 1 = male), and age at the time of randomization (0 = 18 to 24 years old, 1 = 25 to 34 years old).

The Cox regression results were expressed as hazard ratios. As an example of a hazard ratio less than 1.00, if the treatment variable had a hazard ratio of 0.90, that would indicate that compared with the control group, the participant group was associated with a 10 percent decrease in the hazard rate, or the propensity for recidivism. As an example of a hazard ratio greater than 1.00, if the treatment variable had a hazard ratio of 1.20, that would indicate that compared with the control group, the participant group was associated with a 20 percent increase in the hazard rate, or the propensity for recidivism. WestEd employed listwise deletion in the survival analysis models and excluded individuals with missing values on the outcome measure from the analyses. Statistical analyses were conducted in Stata using the survival analysis suite *st* commands.

¹⁵ For the ITT analyses, the study randomization weeks were collapsed into multiples of four weeks so that all observations within each collapsed category were not all within the participant group or control group. Given this rationale, the last eight study weeks were collapsed into one category. For the TOT analyses, the study randomization weeks were collapsed into multiples of 12 weeks and the last 14 study weeks were collapsed into one category.

To investigate differential treatment effects—that is, whether ACJRP effects differed by participant characteristics (age, race/ethnicity, and sex)—WestEd included interaction terms between the randomization status and participant characteristics in the Cox regression models. Interaction terms of statistical significance (i.e., p -values less than .05) would indicate that program effects differed by, for example, participant age (e.g., older participants benefited more from ACJRP than younger participants did).

Appendix C. Additional Tables

Table C1. Service Provision Time Participant Groups Received from LFCS (Percentages)

Service	Ever Enrolled (n = 154)	Previously Enrolled (n = 121)	Completed 18 Months (n = 33)	Aged 18–24 (n = 41)	Aged 25–34 (n = 113)
Prosocial companion and structured leisure activities	78%	85%	74%	78%	78%
Employment/workforce development	8%	5%	10%	11%	7%
Behavioral health	7%	7%	7%	5%	8%
Housing (retention)	3%	1%	4%	1%	4%
Substance abuse (ACJRP session)	3%	2%	4%	4%	2%
Education	1%	0%	1%	1%	1%

Note. Percentages may not sum to 100 percent due to rounding.

Table C2. Service Provision Time Participant Groups Received from LFCS, Prosocial Companion and Structured Leisure Activities Subcategories (Percentages)

Service	Ever Enrolled (n = 154)	Previously Enrolled (n = 121)	Completed 18 Months (n = 33)	Aged 18–24 (n = 41)	Aged 25–34 (n = 113)
Mentoring	55%	49%	59%	52%	57%
ACJRP group session	44%	51%	40%	48%	42%
Structured recreational and leisure activities	0%	0%	0%	0%	0%
Other	0%	0%	0%	0%	1%

Note. Percentages may not sum to 100 percent due to rounding.

Table C3. Arrests Cox Regression for ITT Participant and Control Groups

Variable	Haz. Ratio	S.E.	z	p-value	95% Conf. Interval	
					Low	High
Treatment	1.11	0.12	0.99	0.32	0.90	1.36
BIPOC	0.87	0.11	-1.09	0.28	0.69	1.11
Male	0.99	0.12	-0.09	0.93	0.78	1.26

Note. N = 446. Missing data was 1 percent (4 of 450). All variables are dichotomous variables (0 = no, 1 = yes). WestEd used a fixed effects model, which included a dummy variable for each randomization week. The study randomization weeks were collapsed into multiples of four weeks so that all observations within each collapsed bucket were not all within the participant group or control group. Given this rule, the last eight study weeks were collapsed into one bucket. Cox regression results for the randomization weeks were excluded from the table for conciseness.

Table C4. Arrests Cox Regression, Differential Treatment Effects by Sex for ITT Participant and Control Groups

Variable	Haz. Ratio	S.E.	z	p-value	95% Conf. Interval	
					Low	High
Treatment	0.85	0.19	-0.75	0.45	0.55	1.31
Male	0.81	0.15	-1.11	0.27	0.57	1.17
Treatment × male	1.41	0.36	1.37	0.17	0.86	2.32
BIPOC	0.87	0.11	-1.16	0.25	0.68	1.10

Note. N = 446. Missing data was 1 percent (4 of 450). All variables are dichotomous variables (0 = no, 1 = yes). WestEd used a fixed effects model, which included a dummy variable for each randomization week. The study randomization weeks were collapsed into multiples of four weeks so that all observations within each collapsed bucket were not all within the participant group or control group. Given this rule, the last eight study weeks were collapsed into one bucket. Cox regression results for the randomization weeks were excluded from the table for conciseness.

Table C5. Arrests Cox Regression, Differential Treatment Effects by Age for ITT Participant and Control Groups

Variable	Haz. Ratio	S.E.	z	p-value	95% Conf. Interval	
					Low	High
Treatment	1.10	0.23	0.44	0.66	0.72	1.67
Aged 25 to 34	1.12	0.22	0.59	0.55	0.77	1.63
Treatment × aged 25 to 34	1.01	0.25	0.06	0.96	0.63	1.63
BIPOC	0.88	0.11	-1.04	0.30	0.69	1.12
Male	1.00	0.13	0.04	0.97	0.79	1.28

Note. N = 446. Missing data was 1 percent (4 of 450). All variables are dichotomous variables (0 = no, 1 = yes). WestEd used a fixed effects model, which included a dummy variable for each randomization week. The study randomization weeks were collapsed into multiples of four weeks so that all observations within each collapsed bucket were not all within the participant group or control group. Given this rule, the last eight study weeks were collapsed into one bucket. Cox regression results for the randomization weeks were excluded from the table for conciseness.

Table C6. Arrests Cox Regression, Differential Treatment Effects by Race/Ethnicity for ITT Participant and Control Groups

Variable	Haz. Ratio	S.E.	z	p-value	95% Conf. Interval	
					Low	High
Treatment	1.13	0.25	0.55	0.59	0.73	1.75
BIPOC	0.89	0.16	-0.67	0.51	0.62	1.27
Treatment × BIPOC	0.98	0.25	-0.09	0.92	0.59	1.60
Male	0.99	0.12	-0.08	0.93	0.78	1.26

Note. *N* = 446. Missing data was 1 percent (4 of 450). All variables are dichotomous variables (0 = no, 1 = yes). WestEd used a fixed effects model, which included a dummy variable for each randomization week. The study randomization weeks were collapsed into multiples of four weeks so that all observations within each collapsed bucket were not all within the participant group or control group. Given this rule, the last eight study weeks were collapsed into one bucket. Cox regression results for the randomization weeks were excluded from the table for conciseness.

Table C7. Arrests Cox Regression for TOT Participant and Matched Comparison Groups

Variable	Haz. Ratio	S.E.	z	p-value	95% Conf. Interval	
					Low	High
TOT	0.35	0.13	-2.89	0.004*	0.17	0.71
Number of felony arrests in the year prior to randomization	1.04	0.22	0.18	0.86	0.69	1.57
BIPOC	2.62	1.64	1.54	0.12	0.77	8.92
Male	1.17	0.48	0.38	0.70	0.53	2.60

Note. *N* = 66. Missing data was 0 percent. * *p* < .05. All variables, except for number of prior felony arrests, are dichotomous variables (0 = no, 1 = yes). WestEd used a fixed effects model, which included a dummy variable for each randomization week. The study randomization weeks were collapsed into multiples of 12 weeks so that all observations within each collapsed bucket were not all within the TOT participant group or matched comparison group. Given this rule, the last 14 study weeks were collapsed into one bucket. Cox regression results for the randomization weeks were excluded from the table for conciseness.

Table C8. Charges Cox Regression for ITT Participant and Control Groups

Variable	Haz. Ratio	S.E.	z	p-value	95% Conf. Interval	
					Low	High
Treatment	1.64	0.34	2.39	0.02*	1.09	2.45
BIPOC	1.29	0.32	1.02	0.31	0.79	2.10
Male	1.33	0.34	1.11	0.27	0.81	2.18

Note. $N = 450$. Missing data was 0 percent. * $p < .05$. All variables are dichotomous variables (0 = no, 1 = yes). WestEd used a fixed effects model, which included a dummy variable for each randomization week. The study randomization weeks were collapsed into multiples of four weeks so that all observations within each collapsed bucket were not all within the participant group or control group. Given this rule, the last eight study weeks were collapsed into one bucket. Cox regression results for the randomization weeks were excluded from the table for conciseness.

Table C9. Charges Cox Regression, Differential Treatment Effects by Sex for ITT Participant and Control Groups

Variable	Haz. Ratio	S.E.	z	p-value	95% Conf. Interval	
					Low	High
Treatment	1.32	0.62	0.60	0.55	0.53	3.30
Male	1.12	0.46	0.29	0.77	0.51	2.49
Treatment × male	1.30	0.68	0.51	0.61	0.47	3.62
BIPOC	1.28	0.32	0.99	0.32	0.79	2.08

Note. $N = 450$. Missing data was 0 percent. All variables are dichotomous variables (0 = no, 1 = yes). WestEd used a fixed effects model, which included a dummy variable for each randomization week. The study randomization weeks were collapsed into multiples of four weeks so that all observations within each collapsed bucket were not all within the participant group or control group. Given this rule, the last eight study weeks were collapsed into one bucket. Cox regression results for the randomization weeks were excluded from the table for conciseness.

Table C10. Charges Cox Regression, Differential Treatment Effects by Age for ITT Participant and Control Groups

Variable	Haz. Ratio	S.E.	z	p-value	95% Conf. Interval	
					Low	High
Treatment	1.67	0.71	1.20	0.23	0.73	3.83
Aged 25 to 34	1.14	0.47	0.33	0.74	0.51	2.58
Treatment × aged 25 to 34	0.98	0.48	-0.05	0.96	0.38	2.54
BIPOC	1.31	0.33	1.09	0.28	0.80	2.15
Male	1.34	0.34	1.15	0.25	0.81	2.22

Note. *N* = 450. Missing data was 0 percent. All variables are dichotomous variables (0 = no, 1 = yes). WestEd used a fixed effects model, which included a dummy variable for each randomization week. The study randomization weeks were collapsed into multiples of four weeks so that all observations within each collapsed bucket were not all within the participant group or control group. Given this rule, the last eight study weeks were collapsed into one bucket. Cox regression results for the randomization weeks were excluded from the table for conciseness.

Table C11. Charges Cox Regression, Differential Treatment Effects by Race/Ethnicity for ITT Participant and Control Groups

Variable	Haz. Ratio	S.E.	z	p-value	95% Conf. Interval	
					Low	High
Treatment	0.73	0.33	-0.70	0.49	0.30	1.77
BIPOC	0.71	0.26	-0.93	0.35	0.35	1.46
Treatment × BIPOC	2.78	1.43	2.00	0.046*	1.02	7.59
Male	1.29	0.33	1.01	0.31	0.78	2.13

Note. *N* = 450. Missing data was 0 percent. * *p* < .05. All variables are dichotomous variables (0 = no, 1 = yes). WestEd used a fixed effects model, which included a dummy variable for each randomization week. The study randomization weeks were collapsed into multiples of four weeks so that all observations within each collapsed bucket were not all within the participant group or control group. Given this rule, the last eight study weeks were collapsed into one bucket. Cox regression results for the randomization weeks were excluded from the table for conciseness.

Table C12. Convictions Cox Regression for ITT Participant and Control Groups

Variable	Haz. Ratio	S.E.	z	p-value	95% Conf. Interval	
					Low	High
Treatment	1.39	0.25	1.83	0.07	0.98	1.97
BIPOC	1.57	0.37	1.94	0.05	0.99	2.49
Male	1.63	0.38	2.09	0.04*	1.03	2.58

Note. $N = 447$. Missing data was 1 percent (3 of 450). * $p < .05$. All variables are dichotomous variables (0 = no, 1 = yes). WestEd used a fixed effects model, which included a dummy variable for each randomization week. The study randomization weeks were collapsed into multiples of four weeks so that all observations within each collapsed bucket were not all within the participant group or control group. Given this rule, the last eight study weeks were collapsed into one bucket. Cox regression results for the randomization weeks were excluded from the table for conciseness.

Table C13. Convictions Cox Regression, Differential Treatment Effects by Sex for ITT Participant and Control Groups

Variable	Haz. Ratio	S.E.	z	p-value	95% Conf. Interval	
					Low	High
Treatment	1.15	0.49	0.32	0.75	0.50	2.67
Male	1.43	0.51	1.00	0.32	0.71	2.87
Treatment × male	1.25	0.59	0.48	0.63	0.50	3.17
BIPOC	1.57	0.37	1.91	0.06	0.99	2.48

Note. $N = 447$. Missing data was 1 percent (3 of 450). All variables are dichotomous variables (0 = no, 1 = yes). WestEd used a fixed effects model, which included a dummy variable for each randomization week. The study randomization weeks were collapsed into multiples of four weeks so that all observations within each collapsed bucket were not all within the participant group or control group. Given this rule, the last eight study weeks were collapsed into one bucket. Cox regression results for the randomization weeks were excluded from the table for conciseness.

Table C14. Convictions Cox Regression, Differential Treatment Effects by Age for ITT Participant and Control Groups

Variable	Haz. Ratio	S.E.	z	p-value	95% Conf. Interval	
					Low	High
Treatment	1.25	0.43	0.63	0.53	0.63	2.46
Aged 25 to 34	0.93	0.31	-0.21	0.83	0.48	1.80
Treatment × aged 25 to 34	1.16	0.47	0.36	0.72	0.52	2.56
BIPOC	1.58	0.37	1.94	0.05	0.99	2.51
Male	1.64	0.39	2.11	0.04*	1.04	2.60

Note. *N* = 447. Missing data was 1 percent (3 of 450). * *p* < .05. All variables are dichotomous variables (0 = no, 1 = yes). WestEd used a fixed effects model, which included a dummy variable for each randomization week. The study randomization weeks were collapsed into multiples of four weeks so that all observations within each collapsed bucket were not all within the participant group or control group. Given this rule, the last eight study weeks were collapsed into one bucket. Cox regression results for the randomization weeks were excluded from the table for conciseness.

Table C15. Convictions Cox Regression, Differential Treatment Effects by Race/Ethnicity for ITT Participant and Control Groups

Variable	Haz. Ratio	S.E.	z	p-value	95% Conf. Interval	
					Low	High
Treatment	0.73	0.32	-0.73	0.47	0.31	1.70
BIPOC	1.02	0.34	0.07	0.94	0.53	1.97
Treatment × BIPOC	2.17	1.03	1.63	0.10	0.85	5.51
Male	1.61	0.38	2.02	0.04*	1.01	2.54

Note. *N* = 447. Missing data was 1 percent (3 of 450). * *p* < .05. All variables are dichotomous variables (0 = no, 1 = yes). WestEd used a fixed effects model, which included a dummy variable for each randomization week. The study randomization weeks were collapsed into multiples of four weeks so that all observations within each collapsed bucket were not all within the participant group or control group. Given this rule, the last eight study weeks were collapsed into one bucket. Cox regression results for the randomization weeks were excluded from the table for conciseness.

Table C16. Detention Sentences Cox Regression for ITT Participant and Control Groups

Variable	Haz. Ratio	S.E.	z	p-value	95% Conf. Interval	
					Low	High
Treatment	1.60	0.34	2.24	0.03*	1.06	2.41
BIPOC	1.32	0.33	1.08	0.28	0.80	2.17
Male	1.24	0.32	0.85	0.40	0.75	2.05

Note. $N = 450$. Missing data was 0 percent. * $p < .05$. All variables are dichotomous variables (0 = no, 1 = yes). WestEd used a fixed effects model, which included a dummy variable for each randomization week. The study randomization weeks were collapsed into multiples of four weeks so that all observations within each collapsed bucket were not all within the participant group or control group. Given this rule, the last eight study weeks were collapsed into one bucket. Cox regression results for the randomization weeks were excluded from the table for conciseness.

Table C17. Detention Sentences Cox Regression, Differential Treatment Effects by Sex for ITT Participant and Control Groups

Variable	Haz. Ratio	S.E.	z	p-value	95% Conf. Interval	
					Low	High
Treatment	1.29	0.60	0.55	0.58	0.52	3.23
Male	1.05	0.43	0.13	0.90	0.47	2.34
Treatment × male	1.30	0.68	0.51	0.61	0.47	3.63
BIPOC	1.31	0.33	1.05	0.30	0.79	2.15

Note. $N = 450$. Missing data was 0 percent. All variables are dichotomous variables (0 = no, 1 = yes). WestEd used a fixed effects model, which included a dummy variable for each randomization week. The study randomization weeks were collapsed into multiples of four weeks so that all observations within each collapsed bucket were not all within the participant group or control group. Given this rule, the last eight study weeks were collapsed into one bucket. Cox regression results for the randomization weeks were excluded from the table for conciseness.

Table C18. Detention Sentences Cox Regression, Differential Treatment Effects by Age for ITT Participant and Control Groups

Variable	Haz. Ratio	S.E.	z	p-value	95% Conf. Interval	
					Low	High
Treatment	1.54	0.66	1.02	0.31	0.67	3.57
Aged 25 to 34	1.10	0.46	0.22	0.82	0.49	2.48
Treatment × aged 25 to 34	1.05	0.52	0.10	0.92	0.40	2.75
BIPOC	1.34	0.35	1.15	0.25	0.81	2.22
Male	1.26	0.32	0.90	0.37	0.76	2.08

Note. N = 450. Missing data was 0 percent. All variables are dichotomous variables (0 = no, 1 = yes). WestEd used a fixed effects model, which included a dummy variable for each randomization week. The study randomization weeks were collapsed into multiples of four weeks so that all observations within each collapsed bucket were not all within the participant group or control group. Given this rule, the last eight study weeks were collapsed into one bucket. Cox regression results for the randomization weeks were excluded from the table for conciseness.

Table C19. Detention Sentences Cox Regression, Differential Treatment Effects by Race/Ethnicity for ITT Participant and Control Groups

Variable	Haz. Ratio	S.E.	z	p-value	95% Conf. Interval	
					Low	High
Treatment	0.64	0.30	-0.97	0.33	0.25	1.59
BIPOC	0.68	0.25	-1.05	0.29	0.33	1.40
Treatment × BIPOC	3.20	1.69	2.20	0.03*	1.14	9.01
Male	1.21	0.31	0.74	0.46	0.73	1.99

Note. N = 450. Missing data was 0 percent. * p < .05. All variables are dichotomous variables (0 = no, 1 = yes). WestEd used a fixed effects model, which included a dummy variable for each randomization week. The study randomization weeks were collapsed into multiples of four weeks so that all observations within each collapsed bucket were not all within the participant group or control group. Given this rule, the last eight study weeks were collapsed into one bucket. Cox regression results for the randomization weeks were excluded from the table for conciseness.

Table C20. Supervision Sentences Cox Regression for ITT Participant and Control Groups

Variable	Haz. Ratio	S.E.	z	p-value	95% Conf. Interval	
					Low	High
Treatment	1.80	0.40	2.66	0.008*	1.17	2.77
BIPOC	1.29	0.34	0.98	0.33	0.77	2.16
Male	1.13	0.29	0.46	0.64	0.68	1.86

Note. *N* = 450. Missing data was 0 percent. * *p* < .05. All variables are dichotomous variables (0 = no, 1 = yes). WestEd used a fixed effects model, which included a dummy variable for each randomization week. The study randomization weeks were collapsed into multiples of four weeks so that all observations within each collapsed bucket were not all within the participant group or control group. Given this rule, the last eight study weeks were collapsed into one bucket. Cox regression results for the randomization weeks were excluded from the table for conciseness.

Table C21. Supervision Sentences Cox Regression, Differential Treatment Effects by Sex for ITT Participant and Control Groups

Variable	Haz. Ratio	S.E.	z	p-value	95% Conf. Interval	
					Low	High
Treatment	1.30	0.61	0.56	0.57	0.52	3.25
Male	0.87	0.36	-0.35	0.73	0.38	1.95
Treatment × male	1.51	0.80	0.78	0.44	0.53	4.28
BIPOC	1.28	0.34	0.93	0.35	0.76	2.14

Note. *N* = 450. Missing data was 0 percent. All variables are dichotomous variables (0 = no, 1 = yes). WestEd used a fixed effects model, which included a dummy variable for each randomization week. The study randomization weeks were collapsed into multiples of four weeks so that all observations within each collapsed bucket were not all within the participant group or control group. Given this rule, the last eight study weeks were collapsed into one bucket. Cox regression results for the randomization weeks were excluded from the table for conciseness.

Table C22. Supervision Sentences Cox Regression, Differential Treatment Effects by Age for ITT Participant and Control Groups

Variable	Haz. Ratio	S.E.	z	p-value	95% Conf. Interval	
					Low	High
Treatment	2.02	0.90	1.57	0.12	0.84	4.84
Aged 25 to 34	1.07	0.47	0.15	0.88	0.45	2.54
Treatment × aged 25 to 34	0.86	0.44	-0.30	0.77	0.31	2.35
BIPOC	1.28	0.34	0.94	0.35	0.76	2.15
Male	1.12	0.29	0.43	0.67	0.67	1.85

Note. *N* = 450. Missing data was 0 percent. All variables are dichotomous variables (0 = no, 1 = yes). WestEd used a fixed effects model, which included a dummy variable for each randomization week. The study randomization weeks were collapsed into multiples of four weeks so that all observations within each collapsed bucket were not all within the participant group or control group. Given this rule, the last eight study weeks were collapsed into one bucket. Cox regression results for the randomization weeks were excluded from the table for conciseness.

Table C23. Supervision Sentences Cox Regression, Differential Treatment Effects by Race/Ethnicity for ITT Participant and Control Groups

Variable	Haz. Ratio	S.E.	z	p-value	95% Conf. Interval	
					Low	High
Treatment	0.56	0.27	-1.19	0.24	0.22	1.46
BIPOC	0.55	0.21	-1.56	0.12	0.26	1.16
Treatment × BIPOC	4.42	2.45	2.68	0.007*	1.49	13.10
Male	1.09	0.28	0.33	0.74	0.66	1.80

Note. *N* = 450. Missing data was 0 percent. * *p* < .05. All variables are dichotomous variables (0 = no, 1 = yes). WestEd used a fixed effects model, which included a dummy variable for each randomization week. The study randomization weeks were collapsed into multiples of four weeks so that all observations within each collapsed bucket were not all within the participant group or control group. Given this rule, the last eight study weeks were collapsed into one bucket. Cox regression results for the randomization weeks were excluded from the table for conciseness.

Table C24. Probation Violations Cox Regression for ITT Participant and Control Groups

Variable	Haz. Ratio	S.E.	z	p-value	95% Conf. Interval	
					Low	High
Treatment	0.78	0.25	-0.77	0.44	0.41	1.47
BIPOC	1.16	0.46	0.37	0.71	0.53	2.54
Male	0.90	0.35	-0.28	0.78	0.42	1.91

Note. *N* = 450. Missing data was 0 percent. All variables are dichotomous variables (0 = no, 1 = yes). WestEd used a fixed effects model, which included a dummy variable for each randomization week. The study randomization weeks were collapsed into multiples of four weeks so that all observations within each collapsed bucket were not all within the participant group or control group. Given this rule, the last eight study weeks were collapsed into one bucket. Cox regression results for the randomization weeks were excluded from the table for conciseness.

Table C25. Probation Violations Cox Regression, Differential Treatment Effects by Sex for ITT Participant and Control Groups

Variable	Haz. Ratio	S.E.	z	p-value	95% Conf. Interval	
					Low	High
Treatment	0.49	0.34	-1.02	0.31	0.12	1.95
Male	0.70	0.35	-0.73	0.47	0.26	1.84
Treatment × male	1.83	1.47	0.76	0.45	0.38	8.79
BIPOC	1.15	0.46	0.34	0.73	0.52	2.51

Note. *N* = 450. Missing data was 0 percent. All variables are dichotomous variables (0 = no, 1 = yes). WestEd used a fixed effects model, which included a dummy variable for each randomization week. The study randomization weeks were collapsed into multiples of four weeks so that all observations within each collapsed bucket were not all within the participant group or control group. Given this rule, the last eight study weeks were collapsed into one bucket. Cox regression results for the randomization weeks were excluded from the table for conciseness.

Table C26. Probation Violations Cox Regression, Differential Treatment Effects by Age for ITT Participant and Control Groups

Variable	Haz. Ratio	S.E.	z	p-value	95% Conf. Interval	
					Low	High
Treatment	0.76	0.47	-0.44	0.66	0.23	2.57
Aged 25 to 34	0.81	0.43	-0.40	0.69	0.28	2.32
Treatment × aged 25 to 34	1.02	0.74	0.03	0.98	0.25	4.24
BIPOC	1.13	0.46	0.30	0.77	0.51	2.49
Male	0.89	0.34	-0.30	0.77	0.42	1.90

Note. *N* = 450. Missing data was 0 percent. All variables are dichotomous variables (0 = no, 1 = yes). WestEd used a fixed effects model, which included a dummy variable for each randomization week. The study randomization weeks were collapsed into multiples of four weeks so that all observations within each collapsed bucket were not all within the participant group or control group. Given this rule, the last eight study weeks were collapsed into one bucket. Cox regression results for the randomization weeks were excluded from the table for conciseness.

Table C27. Probation Violations Cox Regression, Differential Treatment Effects by Race/Ethnicity for ITT Participant and Control Groups

Variable	Haz. Ratio	S.E.	z	p-value	95% Conf. Interval	
					Low	High
Treatment	0.14	0.15	-1.86	0.06	0.02	1.11
BIPOC	0.60	0.29	-1.07	0.29	0.23	1.54
Treatment × BIPOC	8.04	9.12	1.84	0.07	0.87	74.16
Male	0.87	0.34	-0.36	0.72	0.41	1.86

Note. *N* = 450. Missing data was 0 percent. All variables are dichotomous variables (0 = no, 1 = yes). WestEd used a fixed effects model, which included a dummy variable for each randomization week. The study randomization weeks were collapsed into multiples of four weeks so that all observations within each collapsed bucket were not all within the participant group or control group. Given this rule, the last eight study weeks were collapsed into one bucket. Cox regression results for the randomization weeks were excluded from the table for conciseness.

Appendix D. Number of Participants in Each Stage of the Study

Figure D1. Number of Participants in Each Stage of the Study

