

A Process and Outcome Evaluation of Project Reset

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About This Report

This report, sponsored by the City University of New York Institute for State and Local Governance, presents the results from a process and outcome evaluation of the Project Reset diversion program from the program's initiation in February 2018 through January 2021. This program was originally designed to provide post-arrest, pre-arraignment diversion programs to young adults and adults with no prior criminal record who were arrested for nonviolent, low-level offenses. The program was later expanded to include participants with prior convictions. Individuals are diverted from traditional court processing and offered effective and tailored community-based responses. The program has diverted and provided services to almost 2,500 individuals. In this report, we used information collected from program staff and participant interviews and surveys, administrative data, and observations of programs to describe how the program is implemented, identify key program facilitators and barriers, illustrate participant experiences, and determine whether the model is effective in reducing risk factors for criminal legal involvement (e.g., recidivism) and whether it is cost-effective. This report should be of interest to entities across the United States interested in diversion programs.

Justice Policy Program

RAND Social and Economic Well-Being is a division of the RAND Corporation that seeks to actively improve the health and social and economic well-being of populations and communities throughout the world. This research was conducted in the Justice Policy Program within RAND Social and Economic Well-Being. The program focuses on such topics as access to justice, policing, corrections, drug policy, and court system reform, as well as other policy concerns pertaining to public safety and criminal and civil justice. For more information, email justicepolicy@rand.org.

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- Center for Court Innovation
- Osborne Association
- Young New Yorkers
- New York State Division of Criminal Justice Services (DCJS)

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Summary

Manhattan District Attorney’s Office (DANY) invested funds through its Criminal Justice Investment Initiative (CJII) to support impactful projects that improve public safety and promote a fair and efficient justice system in New York City. Between 2018 and 2022, DANY provided \$7.76 million under CJII to fund Project Reset. The Project Reset program, which began in February 2018, originally funded three different program providers to implement post-arrest, pre-arraignment diversion programs to divert young adults and adults arrested for nonviolent low-level offenses who do not have a criminal record. These individuals were offered community-based programs instead of traditional court processing, and the arrest is sealed upon successful completion. The core Project Reset program model was designed to be a one-time, two-to-three-hour session. Organizations had few specific requirements to fulfill when designing their programming, although the program request for proposals (RFP) suggested that programs include elements such as community service, restorative justice, and educational programming. In July 2019, eligibility was expanded to include participants with prior convictions. Each of the three funded programs—Center for Court Innovation, Young New Yorkers, and the Osborne Association—provides program activities to eligible participants and serves distinct geographical areas of Manhattan, New York; the Osborne Association serves Upper Manhattan, Center for Court Innovation (CCI) serves Midtown (for individuals 18 and older) and Downtown (for individuals over 21), and Young New Yorkers serves Downtown (for individuals 18–21).

Project Reset fits within CJII’s Diversion and Reentry Support Portfolio, which also funds DANY’s other investments in diversion and reentry support services to effectively address the needs and risks of young adults and adults in the justice system. The City University of New York Institute for State and Local Governance (ISLG) manages CJII, which was established by DANY in 2015. ISLG provides technical assistance, conducts oversight, measures performance, manages all CJII grantees, and oversaw this evaluation as part of CJII.

RAND conducted the evaluation of Project Reset, which sought to determine how the initiative and individual programs were implemented, whether the model was effective in reducing risk factors for criminal legal involvement (e.g., recidivism), and whether it was cost-effective. This report presents findings from the implementation assessment, a rigorous quasi-experimental outcome evaluation, and a cost analysis that answered the main five research questions listed below, as well as a set of recommendations that should help optimize program provision going forward.

1. Who were the participants of Project Reset?
2. What were the key implementation strengths and challenges experienced by the programs, and how did prosecutors and program providers overcome the challenges?
3. What is the effect of Project Reset on case outcomes and future rearrests?
4. What are the participants’ reasons for participating in diversion and their satisfaction level with program services? In what ways have program services addressed or not addressed their needs? What barriers are there to access?
5. What are the fiscal costs of early diversion programs compared to the traditional court process?

Methods

The evaluation involved seven major data collection efforts:

- **Document Review:** We conducted a document review to understand basic operation and goals of the program, including a past evaluation report of a youth pilot project of Project Reset (Dalve and Cadoff, 2019), quarterly grant reports, and applications for funding.
- **Participant Interviews:** We conducted semi-structured interviews via telephone with nine former Project Reset participants across the three program provider organizations. Interviews focused on participants' experiences in the program, including how they learned about the program, perceptions of the program, nature of their interactions with key stakeholders, facilitators and barriers to engaging with the program, and opportunities for improvement.
- **Stakeholder Interviews:** Researchers conducted interviews with staff members from DANY, the Center for Court Innovation, Young New Yorkers, and the Osborne Association from June to August 2020. Interviewees were asked for their perspective on different aspects of Project Reset, including eligibility, contact processes and case flow, and implementation barriers and facilitators, specifically focusing on the implementation components and implementation processes, including communication among partners, and program-monitoring efforts.
- **Program Observations:** To better understand how Project Reset was implemented in practice and to assess fidelity to the Project Reset model, we conducted program observations using a structured observation form. We were able to complete a total of three observations across two of the program provider organizations.
- **Participant Survey:** We surveyed participants about their experience with Project Reset and their criminal legal system beliefs from November 10, 2020, through March 19, 2021. In total, 106 people completed the survey.
- **Administrative Data:** DANY and the three program providers provided administrative data on all individuals who received a desk appearance ticket (DAT) between January 1, 2016 and January 31, 2021. This was supplemented by additional data on all *fingerprint arrests* (which includes all felonies and penal law misdemeanors) provided by DCJS. These data allowed us to construct descriptive analyses regarding the individuals who were eligible for diversion, including how the size of this population changed over time, what the biggest sources of attrition were, and what factors were most predictive of whether an eligible individual would complete diversion. These data also allowed us to implement a regression discontinuity design (where date of arrest was the running variable) to identify the causal effect of the program on case outcomes and rearrest rates.
- **Cost Data:** To compare the fiscal costs spent on providing programming to an individual versus adjudicating their case in court, we obtained program invoices and interviewed relevant stakeholders regarding the time spent on various case activities and staff wage rates. The cost analysis also required incorporating findings from both the process and outcome evaluation.

Key Findings

Based on our evaluation, we identified key findings related to program implementation, program outcomes, and the cost analysis.

Program Implementation

- **The program provided participants a range of possible benefits.** According to the qualitative data collection, individuals participated in Project Reset because it sealed their record with no fees, fines, or jail time; it eliminated interacting with the court system; and it mitigated key collateral consequences, such as losing their jobs or immigration-related issues.

- **Lack of reliable contact information and community knowledge of the program are barriers to recruitment.** Obtaining reliable and accurate contact information for participants is a key issue, as a sizeable proportion of individuals referred to Project Reset could not be reached by the provider—41 percent of individuals eligible under the original criteria and 76 percent of individuals eligible under the expanded criteria. Further, even when potential participants could be reached, they sometimes expressed concern that the program might be a scam since they had not heard of it before. These issues resulted in many eligible individuals not being provided with this diversion opportunity.
- **Inequities in program completion might compound existing disparities.** The outreach issues noted above result in White-Hispanic, Black non-Hispanic, and Black-Hispanic individuals composing 65.2 percent of the population referred to Reset, but only 58.8 percent of program completers. This represents a drop in representation of 6.4 percentage points and indicates that this program is likely to result in an increase of observed racial and ethnic disparities in the legal system, which is particularly problematic given the widespread disparities that currently exist in the legal system.
- **Stakeholder engagement is important to program success.** Program providers described the importance of their relationship with DANY in implementing the program. In particular, they cited DANY's support, availability, and willingness to hear the perspective of the providers. The Salesforce data tracking system was also an important tool for facilitating stakeholder communication, particularly with respect to making referrals and communicating the status of eligible potential participants. At the same time, it appears that additional communication with other relevant stakeholders—such as the New York Police Department (NYPD) or the legal defense organizations—could strengthen the program.
- **Program staff are key to program success.** Both program staff and Project Reset participants emphasized the importance of program staff in making Project Reset successful, including staff members' dedication and their nonjudgmental approach to working with participants. Staff willingness to evaluate and modify the curriculum in response to feedback was also noted as important. Similarly, participants seemed to benefit from the emotional and mental health support provided by Project Reset staff, and both participants and staff benefitted from that connection. As such, it is clearly important to maintain the program providers' emphasis on rapport-building as the program continues in the future, and it serves as a reminder to hire, support, and keep quality staff.
- **Participants appreciated the wraparound support provided by Project Reset.** The variety of services provided by Project Reset contributes to its success and therefore should be maintained or further developed. Participants valued referrals for outside services that had the potential to be helpful for them. In particular, participants mentioned mental health treatment and legal services. Referrals likely extended the effect of Project Reset beyond the initial light touch of the one-day intervention.
- **Participants would benefit from additional follow-up about their arrest disposition.** Some participants noted that they would have appreciated clearer follow-up after completion so that they were sure their arrest had been sealed.
- **In-person services in community-based settings promote engagement.** In addition, the fact that services were located in the offices of nonprofits—and even sometimes in local museums and other community settings—rather than courtroom-type settings helped to create a welcoming environment for participants. Although programs were able to transition services to a virtual format during the coronavirus 2019 (COVID-19) pandemic, it does appear that there are benefits of offering the program in-person when it is possible.
- **COVID-19 had a substantial effect on the provision of services.** As expected, since early 2020, the COVID-19 pandemic appeared to significantly lower the number of cases referred to Project Reset. However, there had already been a steady decline of cases pre-COVID-19 since the peak of enrollment

in mid-2018. COVID-19 also altered the ability of programs to deliver services, and each program had to pivot dramatically to provide the necessary programs to individuals.

Program Outcomes and Cost Analysis

- **Participants have positive perceptions of the program and its effectiveness.** Overall, participants voiced very positive reviews of Project Reset, its staff, and the content, and stated they would recommend it to others.
- **Program participation might be associated with improved views of the legal system.** While many respondents had a negative outlook on the police specifically and the legal system as a whole, respondents did indicate that Project Reset participation led to a more favorable outlook on the legal system. Most participants indicated that Project Reset improved their opinions of the courts and the police. Even though we cannot draw firm conclusions on the effect that Project Reset had on participant views on the justice system (we were unable to survey participants before program participation to determine original views), the qualitative interviews and survey responses suggest that the program had a positive effect.
- **The individuals who participated in the program likely would have received an adjournment in contemplation of dismissal (ACD) if their case had been adjudicated in the courts.** While both program participation (which results in a decline to prosecute) and an ACD effectively result in the same non-conviction outcome for the individual, the experience of the diversion pathway might be significantly better for the individual. ACDs require the individual to remain arrest-free and make multiple court appearances over a six-month to one-year period, and many are required to complete either programming or community service as a condition of their ACD. During this supervisory time period, the case will be visible in a criminal history check. In contrast, Project Reset allows individuals to attend programming in a supportive atmosphere that tries to tailor services to their underlying needs, and the arrest is sealed shortly after program completion without the need for a case to be docketed.
- **The program had no observable effect on the one-year rearrest rates for individuals who participated in the program.** It is important to note that the program effects we found might have been different if the referral and completion rates had been different. In particular, if the program completion rates had been higher, that would mean that somewhat riskier individuals would now be participating, and the effects of the programming on both case outcomes and rearrest rates could be different.
- **Under the existing program cost structure, the per-participant program costs were greater than the costs of traditional court adjudication. However, this relationship varied substantially by provider, and the program costs were lower than traditional court adjudication for the provider that served the largest number of clients.** The cost analysis indicated that the average per-participant cost of providing programming across all programs was \$868.87, while the average cost for the individual to have their case adjudicated in court was \$576.76. However, the cost analysis only includes money spent by the proximal agencies (DANY, defense, and the courts), and does not include the benefits that individuals might receive (e.g., through uninterrupted employment).
- **Program cost-effectiveness would improve if the ratio of program staff to participants were lower.** Providers were likely overstaffed given that their caseloads were lower than expected, which necessarily increased the per-participant costs. If program provision costs were to fall, the program would be considered cost-effective even without considering the benefits that might accrue to individuals as a result of avoiding traditional case processing. Under credible alternative scenarios whereby provider efficiency was increased—either by lowering staff levels or increasing caseloads—the fiscal cost of providing programming falls below the cost of the court pathway.

Recommendations

Based on these findings, we identified a set of recommendations for the program.

- **Increase community awareness of the program and highlight benefits to improve recruitment.** Efforts to raise the external profile of the program might help to strengthen the perception of program legitimacy, which was a concern raised by participants. Though the programs have information about Project Reset on their websites, collaborating with other relevant organizations (e.g., defense organizations) could be one potential pathway. This could include developing clear communication about benefits of the program, including the ways that it helps participants avoid collateral consequences of justice system involvement.
- **Strengthen the outreach process.** Efforts to gather reliable and accurate contact information would allow a much greater population of individuals the diversion opportunity. As these outreach issues contributed to racial disparities in completion rates, resolving this issue could also potentially result in more equitable outcomes.
- **Ensure the referral process uses eligibility criteria that are justifiable, transparent, and consistently applied.** The fact that a significant proportion of individuals are ultimately not referred after passing initial screening checks highlights the importance of ensuring that there is a clear rationale behind the criteria used, especially because existing criteria disproportionately screens out minority individuals.
- **Examine whether existing eligibility criteria result in racial or ethnic inequities in referral rates.** The current screening process relies on both automated checks and detailed case-specific checks that must be conducted by hand. If an individual passes the automated checks but is not referred to the program because they do not pass the case-specific checks, the existing data does not record the specific reason they were determined to be not eligible. In order to facilitate analysis that will identify the impact existing eligibility criteria has on racial and ethnic inequities in referral rates, the screening process either needs to be fully automated so it can easily be determined why an individual was deemed not eligible, or the precise reason the individual was screened out needs to be recorded in the data. Fully automating the screening process has additional advantages as well, as it would be more efficient and it would increase transparency into the screening process.
- **Sustain existing relationships among stakeholders and deepen relationships with other relevant stakeholders.** The strong relationships between program providers and DANY are an important program facilitator. At the same time, providers indicated that additional communication with other relevant stakeholders—such as NYPD or the defense organizations—could strengthen the program.
- **Support staff autonomy and program flexibility.** Staff dedication to delivering this program was an important facilitator, as was the ability of each program provider to draw on its existing strengths as an organization. Providing encouragement and support to staff to ensure they can continue to fulfill the mission of the program is key.
- **Continue to offer programs in in-person, community-based formats.** Prior to the pandemic, services were offered in community-based locations and group formats, which were described as strengths of the program model. Though the pandemic might create some ongoing obstacles to providing services in this format, it is worthwhile to continue offering services outside the justice system and, when possible, provide programming in person.
- **Connect participants with needed services after completion.** Participants described the value of the referrals they received through the program, and it is important that the providers continue to maintain strong referral relationships with high-quality mental health, legal, and financial services. This might include referrals provided immediately after participants complete the program, but could also include ongoing contact with program participants, such as through an alumni program. These ser-

vices can help address risk factors for future justice system involvement. Post-program services should also include better education on how participants can request documentation that their arrest has been sealed, which could be easily incorporated into the final parts of a workshop.

- **Reduce program provision costs.** The primary reason program costs were relatively high was that providers seemed to be staffed to serve a higher caseload than they received. To reduce program costs, providers could either reduce the number of staff or ensure that staff members are able to work on other projects when caseloads are low. This highlights the need for program providers to have the ability to be flexible in staffing levels. Alternatively, providers could continue with their existing staff, and there could be a coordinated effort to increase program caseloads either by reducing outreach difficulties or relaxing eligibility requirements further.
- **If participation in the program increases, further evaluation work on program effects might be necessary.** Several of the recommendations made above touch on increasing the number of participants in the program. The outcome evaluation and cost analysis conducted in this report are only able to determine the effects and cost-effectiveness of the program for the set of participants between July 2018 and June 2019. To the extent that efforts to increase the number of participants are successful, either by improving the outreach process or by relaxing eligibility requirements, the effects of the program might be different and should be evaluated.

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Introduction

The Manhattan District Attorney’s Office (DANY) has invested funds through its Criminal Justice Investment Initiative (CJII) to support impactful projects that improve public safety and promote a fair and efficient justice system in New York City. Between 2018 and 2022, DANY committed \$7.76 million under CJII to fund Project Reset. Project Reset originally funded three different program providers to implement post-arrest, pre-arraignment diversion programs to divert young adults and adults arrested for low-level nonviolent offenses who do not have a criminal record. These individuals are offered community-based programs instead of traditional court processing, and upon successful completion, the arrest is sealed. The core Project Reset program model was designed to be a one-time, two-to-three-hour session. Organizations had few specific requirements to fulfill when designing their programming, although the program request for proposals (RFP) suggested that programs should include elements such as community service, restorative justice, and educational programming. In July 2019, the eligibility was expanded to individuals with prior convictions. Each of the three funded programs, Center for Court Innovation (CCI), Young New Yorkers (YNY), and the Osborne Association, provides program activities to eligible participants and serves distinct geographical areas of Manhattan, New York; the Osborne Association serves Upper Manhattan, CCI serves Midtown (for individuals 18 and older) and Downtown (for individuals 21 and older), and YNY serves Downtown (for individuals 18–20). The program model builds on a previous version of Project Reset, focused on individuals between 16 and 17 years of age, which was associated with a reduced likelihood of violent felony rearrests at six months and new convictions at both six months and one year post-participation (Dalve and Cadoff, 2019).

Project Reset is funded through CJII’s Diversion and Reentry Support Portfolio, which funds DANY’s broader investments in diversion and reentry support services to effectively address the needs and risks of young adults and adults in the justice system. The City University of New York Institute for State and Local Governance (ISLG) manages CJII, which was established by DANY in 2015. ISLG provides technical assistance, conducts oversight, measures performance, manages all CJII grantees, and also oversaw this evaluation as part of CJII.

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5. What are the fiscal costs of Project Reset compared to the traditional court process?

Background

When there are calls to reform legal systems that are perceived to be overly punitive, one pathway that has been increasingly suggested is the use of pretrial diversion. Project Reset, the subject of this evaluation, is just one type of pretrial diversion program. While Project Reset is a post-arrest, pre-arraignment program that specifically focuses on low-level misdemeanor offenses, more generally speaking, diversion can occur at different stages of the legal system and focus on different types of offenses. Typically, diversion programming will occur at one of three stages of interaction with the criminal legal system: with law enforcement personnel (i.e., police), at the pretrial or prosecutorial phase, or via problem-solving or specialty courts (Center for Health and Justice at TASC, 2013). Diversion at these three phases differs with respect to goals, oversight mechanisms, and practices. In terms of offenders, pretrial diversion might be targeted at: (1) misdemeanor or felony offenders, (2) specific populations, such as juvenile offenders, individuals with substance abuse issues or mental health problems, or (3) low-level or first-time offenders (Labriola et al., 2018; Lange, Rehm, and Popova, 2011; and Center for Health and Justice at TASC, 2013).

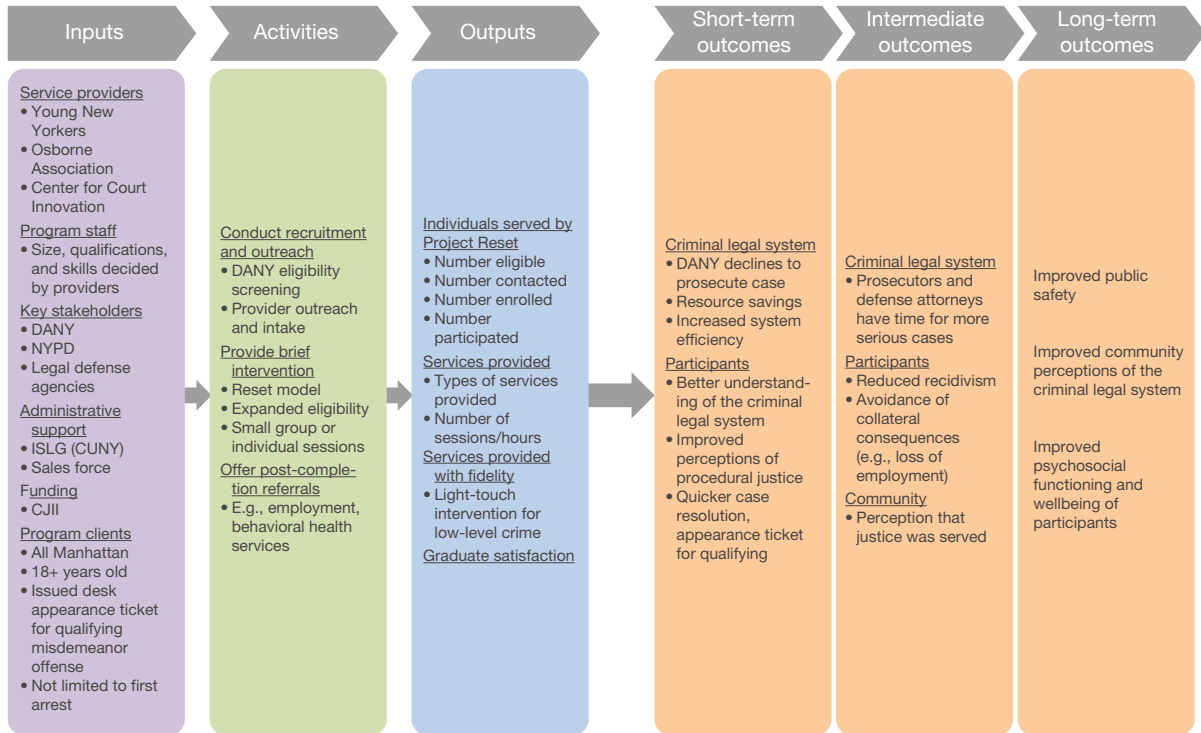
While diversion programs have been around for a while, there are very few studies that have been able to identify the causal impact of these programs using either a randomized or quasi-experimental research design.¹ Mueller-Smith and Schnepel (2021) examined a diversion practice in Harris County, Texas, focused on first-time felony defendants and found that diversion reduces reoffending rates and increases employment rates. Augustine et al. (2021) evaluated a felony diversion program in San Francisco, California, and found that referrals to diversion decrease the likelihood of future convictions. While these programs indicated beneficial effects, it is important to note they target a different population than Project Reset—namely, they are focused on felony offenses, while Project Reset is focused on low-level non-violent misdemeanor offenses. More recently, Agan et al. (forthcoming), found that the non-prosecution of a non-violent misdemeanor offense in Suffolk County, Massachusetts, significantly reduced the likelihood that individuals will be charged with new criminal complaints. However, while the setting examined by Agan et al. (forthcoming) focused on a similar set of offenses as Project Reset, the nature of the intervention is somewhat different—Project Reset requires individuals to enroll and complete programming to have their charges dropped, while Agan et al. (forthcoming) examined the impact of the prosecutor outright dropping a case without requiring anything further from individuals. The lack of research directly related to the type of intervention Project Reset presents indicates the importance of conducting this evaluation study.

Logic Model

A visual understanding of the Project Reset model is presented in Figure 1.1. The key purpose of this logic model is to help understand the path by which program inputs and outputs can affect change in participant outcomes. The model details the following components with corresponding measures: (1) inputs, (2) program activities, (3) outputs, and (4) short-term, (5) intermediate, and (6) long-term outcomes. The left-hand column of the logic model visually represents the planning and project elements of the program. The activities column represents the major components of the project, including recruitment and outreach, intervention, and referrals. The outputs and outcome columns illustrate the products of activities (outputs) or the overall outcomes of the model (outcomes). The outcomes are delineated by short-term, intermediate, and long-term outcomes.

¹ Rempel et al. (2018) examined the impact of several diversion programs using a matching estimator.

FIGURE 1.1
Project Reset Logic Model



NOTE: CUNY = City University of New York. NYPD = New York Police Department.

Purpose of This Report

This report provides a comprehensive description of how the program worked and who it served, as well as the results from the implementation and outcome evaluations and the cost analysis. The report is organized as follows:

- **Chapter 1. Introduction** provides an overview of the program and evaluation.
- **Chapter 2. Evaluation Methods** describes the methodology for the evaluation, including sources of data and the analysis plan.
- **Chapter 3. Project Reset Eligibility, Screening, and Program Overview** provides an overview of the program model and describes eligibility criteria, screening and contact procedures, and a description of program activities.
- **Chapter 4. Case Flow Through Project Reset and the Courts** presents process maps of the diversion referral and outreach process for Project Reset under the original eligibility requirements and under the expanded eligibility requirements, and shows where attrition occurs.
- **Chapter 5. Factors That Impact Program Outreach and Completion** provides detailed information about the participant characteristics that are most predictive of whether program outreach will be successful.
- **Chapter 6. Outcome Evaluation Results** provides findings from a regression discontinuity design regarding the effect the implementation of Project Reset had on case outcomes and rearrest rates.

- **Chapter 7. Participant and Program Staff Perceptions of Project Reset** provides perceptions of the program based on feedback from participants gained from interviews and surveys, as well as interviews with staff.
- **Chapter 8. Cost Analysis** provides the results from our fiscal cost analysis which compared the fiscal outlay of money spent for an individual to complete Project Reset versus the money that would have been spent if the individual had instead had their case adjudicated in court.
- **Chapter 9. Factors Affecting Program Implementation** discusses the facilitators and barriers to the implementation of the program.
- **Chapter 10. Takeaways and Recommendations** summarizes the key takeaways and recommendations from the evaluation.
- **Appendixes A–D** provide supplemental figures, tables, information on cost analysis, and outline of the interview protocols.

Evaluation Methods

In this chapter we review the qualitative and quantitative methods that formed this evaluation. The main goals of the qualitative portion of the evaluation were to describe the program implementation process and model strengths and challenges. Our quantitative analyses, which relied on program and administrative data, aimed to understand who was served by the program, as well as the effect the program had on case outcomes and rearrest rates. The cost analysis used the information learned from both the qualitative and quantitative analyses, along with information on program costs, to compare the fiscal costs spent on diversion programming versus adjudicating cases in the traditional manner via the courts. All components of the project were approved by RAND's Institutional Review Board (IRB). This IRB review included the approval of data sharing agreements and all data collection methods, including written consent for qualitative data collection.

Document Review

Because the evaluation started in late 2019 and program implementation had already begun a year earlier, it was critical to understand and review any historical documentation that could provide foundational knowledge about the program and the intended implementation and outcomes associated with each program. To that end, we conducted a document review to understand the basic operation and goals of the program. This included a past evaluation report of a youth pilot project of Project Reset,¹ program documentation, 41 quarterly grant reports, and provider applications for funding submitted to CJII.

Stakeholder Interviews

To better understand the implementation of Project Reset, in early 2020 we conducted preliminary informational discussions with DANY and the programs to introduce the evaluation. These interviews allowed us to speak to program leadership to get an understanding of the program, including how and why the program was created, which allowed us to finalize interview protocols. We also spoke to program leadership about data capabilities, which allowed us to begin drafting the necessary data use agreements. We also spoke to leadership regarding how the program was changing and adapting because of the coronavirus 2019 (COVID-19) pandemic.

Following this exploratory period, we conducted 14 semi-structured interviews with program staff across the three program provider organizations, including program leadership and staff members involved in recruitment, service provision, and administrative tasks. To recruit interviewees, we worked with leadership of each organization to identify key staff involved in the implementation of Project Reset and then reached out individually to these stakeholders via email to invite them to participate in an interview. Interviews took

¹ For more information about the youth pilot of Project Reset, see Dalve and Cadoff, 2019.

place from June to August 2020, and we conducted six interviews with staff of CCI; four with staff of the Osborne Association; and four with staff of YNY. Interviews typically included one or two staff members, and all staff provided verbal consent for participation in the interviews. The purpose of these 45–60-minute virtual interviews was to understand the way each organization implemented the Project Reset model (e.g., types of workshops offered), the flow of a typical participant through the program from recruitment to program completion, the nature of communication across the organizations that are involved with Project Reset, facilitators and barriers to implementation, and opportunities for improvement. For the full interview protocol, see Appendix D. Detailed notes were taken during the interviews, and they were also recorded to allow us to fill in gaps in the interview notes following the interview. To analyze the interview notes, we developed a qualitative codebook. Codes were largely developed deductively based on the interview guide. Two members of the evaluation team used the codebook to code an initial interview and then met to discuss discrepancies, after which they independently coded the remaining notes. The coders met regularly during coding to ensure consistent application of codes, and interview notes from two interviews were double-coded to ensure interrater reliability; they achieved greater than 93 percent agreement. We also drew illustrative quotes from interview notes. Because these are not transcriptions, there is some possibility that there are aspects of these quotes that are paraphrased, rather than a direct quote.

Participant Interviews

We conducted semi-structured interviews via telephone with Project Reset participants across the three program provider organizations. We partnered with the program provider organizations for recruitment, with each organization providing the names of individuals who had completed the program and agreed to be contacted. We requested that programs contribute names of clients from a range of backgrounds and program experiences and in total received names of 17 participants. Two participants declined to participate after being contacted by the evaluation team, and six could not be reached after two to three outreach attempts (including telephone, email, and text depending on available contact information). In total, we completed nine interviews (four with participants from CCI, two from the Osborne Association, and three from YNY) between October and December 2020. All participants provided verbal consent for participation and received a \$25 gift card for their participation. Interviews typically lasted 20–30 minutes and focused on the participants' experiences in the program, including how they learned about the program, perceptions of the program, nature of their interactions with key stakeholders, facilitators and barriers to engaging with the program, and opportunities for improvement. For the full interview protocol, see Appendix D. We took detailed notes during the interviews which were used for analysis.

To analyze data, we developed a qualitative codebook. Although we designed the participant and staff interview guides to have some comparable domains (e.g., how participants learned about the program, perceived benefits of the program), the different roles of these groups meant that some questions were distinct. Therefore, the codebook was designed to have some codes that were parallel to those in the staff semi-structured interview codebook and some that were unique to the content of the participant interviews. As with the staff semi-structured interviews, two members of the evaluation team used the codebook to code one interview and then met to discuss discrepancies, after which they independently coded the remaining notes. Application of codes was discussed regularly to ensure consistent application of codes, and interview notes from two interviews were double-coded, resulting in 95 percent agreement or higher. We also drew illustrative quotes from these interview notes. Because these are not transcriptions, there is some possibility that there are aspects of these quotes that are paraphrased, rather than a direct quote.

Review of Progress Reports

Each organization was required to submit quarterly progress reports to DANY to enhance DANY's understanding of the program's implementation and progress toward contractual goals. The progress report template included questions related to program successes and challenges; collaborations and partnerships leveraged to implement the program; and support or resources from DANY that would be helpful in program implementation. We developed a modified version of the qualitative codebook used to code the stakeholder interviews, focusing specifically on codes related to these aspects of implementation. Findings were integrated with the results of stakeholder interviews to identify themes that were consistent with those that arose through the interviews and determine if there were new themes related to program implementation.

Program Observations

To better understand how Project Reset has been implemented in practice and to assess fidelity to the Project Reset model, we conducted three program observations. We developed a structured observation form to examine key elements of program administration, including modality (i.e., via videoconference or teleconference), use of supplemental materials, key content areas, and participant engagement. Because these observations took place after the onset of the COVID-19 pandemic and services were being offered virtually, one member of the research team virtually joined the program session being offered. We were able to observe three workshops across two of the program provider organizations. We were unable to observe a session from the third organization because they raised privacy concerns, as they largely were only able to offer individual counseling sessions during COVID-19. Findings from the observations were used to inform our understanding of the format and content of the sessions and to assess fidelity to the few required elements of the program (e.g., program length).

Participant Survey

With the assistance of the three program providers, we surveyed 106 participants about their experience with Project Reset and their attitudes about the criminal legal system. From November 10, 2020, through March 19, 2021, the three program providers sent participants a link to the survey upon completion of their Project Reset program, which was hosted on Qualtrics. In addition, they sent information about the survey to prior participants via text and email. We do not have an exact number of current and former participants who the diversion programs contacted because they did not keep track, but we believe it to be several hundred based on their description of their efforts. In total, 106 people completed the survey. We have included all people who answered each question, so some slight fluctuation exists across the number of responses for each item. Participants completed a consent form before beginning the survey, and they received a \$10 incentive paid as either a Subway or Target gift card at the end of the survey. The survey was designed to assess key immediate outcomes identified in the logic model (e.g., procedural justice) and participant perceptions of their experience.

For most items, we collapsed responses across programs, because of the small number of responses from the Osborne Association and YNY participants, and because responses varied little across programs. For most items, participants read a statement and rated their level of agreement on a 5-point Likert scale (1 = strongly disagree, 2 = somewhat disagree, 3 = neither agree or disagree, 4 = somewhat agree, 5 = strongly agree). For some items, participants answered on a 4-point scale (1 = never, 2 = some of the time, 3 = most of the time, 4 = always), indicating how frequently the courts or police behaved in specific ways. The survey

also contained one free-response question that asked, “What do you feel was the most important thing you learned from participating in Project Reset?” A summary of those responses is in Chapter 7 along with the results from the other survey measures.

Analysis of Administrative Data

Description of Data Provided

We received administrative data from DANY, the three program providers, and Division of Criminal Justice Services (DCJS).² DANY provided information on all individuals who received a desk appearance ticket (DAT) between January 1, 2016, and January 31, 2021. Project Reset was first implemented on February 1, 2018, and DANY provided data for the two years prior to implementation to allow us to construct a historical comparison group. For each DAT, DANY provided information from their case management system on the individual’s demographic characteristics (age, gender, race and ethnicity), date and precinct of arrest, charges, and whether the individual was referred to Project Reset. For each referred individual, DANY provided additional data from a Salesforce platform that was used to communicate with program providers about eligible individuals. The Salesforce data included information on the diversion program and provider the individual was referred to, the date of referral, whether the individual was successfully contacted, whether they accepted the offer, and whether they completed programming.³ For all individuals in the sample who did not complete programming, DANY provided information on all court events associated with the adjudication of the case and the final disposition.

The data from the three program providers included additional information on individuals who completed programming, including the specific component program completed and more detailed information on their personal characteristics (such as their primary language spoken, education level, and employment status). One of the program providers also included information on referrals to outside services.

For each individual in our sample, DCJS provided data on all fingerprint arrests that occurred within the State of New York up through February 2022. For each arrest, information was included on the date of arrest, the offense date, the dominant charge, and the disposition of the incident (i.e., the charge the individual was convicted of, if any). This information allowed us to construct the criminal legal history (arrests and convictions) of the individual at the time of their *focal arrest*,⁴ which is the arrest incident that is included in the DANY data. The DCJS data also allowed us to construct rearrest rates, which measure whether the individual was rearrested within the first year after their focal arrest.

Data from the program providers, as well as DANY’s case management system and Salesforce platform, were merged using the arrest incident number that was present in each of these data sets. With respect to the DCJS data, we provided DCJS information on the name, date of birth, and demographics (gender and race and ethnicity) of everyone in our sample, and DCJS merged in the full arrest record for these individuals using a demographic matching algorithm.

² These data are provided by DCJS. The opinions, findings, and conclusions expressed in this publication are those of the authors and not those of DCJS. Neither New York State nor DCJS assumes liability for its contents or use thereof.

³ Salesforce is a cloud-based software that helps organizations streamline communication. DANY uses it to convey information to providers when there is an eligible person for the program, and providers then input information on whether the person engages, their program activity, and completion status.

⁴ The focal arrest is the arrest that was assessed for program eligibility.

Descriptive Analyses

We conducted several descriptive analyses using the sample of individuals that were referred to Project Reset, which includes 4,816 observations. To be referred, an individual had to be arrested after the program was implemented, and they had to meet eligibility requirements. In Chapter 3, we examine how the size of the referred population changed over time and document the characteristics of those who completed programming. In Chapter 4, we construct process maps that detail the outreach process for the diversion programs and the process by which individuals who do not complete diversion have their case adjudicated in court. Chapter 5 examines in-depth how the set of individuals that are referred to Project Reset compare with those that actually complete Project Reset.

Causal Analysis of Program Impact on Case Outcomes and Rearrest Rates

Construction of Analysis Sample

Because our outcome evaluation involves a historical comparison group, it required that we identify the set of individuals that would have been referred to Project Reset in 2016 and 2017 if the program had existed. Eligibility for the program is determined both by automated checks and detailed case-by-case checks. For DATs that occurred in 2016 and 2017, DANY was only able to run the automated portion of the process. Because our outcome evaluation requires that those in our sample who were arrested before program implementation are as similar as possible to those in our sample arrested after program implementation, DANY provided us with a flag that identified who passed the automated checks for the entire data sample (which included all DATs issued between January 1, 2016 and January 31, 2021). These automated checks ensure that all charges were on the eligible list and uncover any prior incidents that originated with an NYPD arrest. All our outcome analyses are estimated using various defined subsets of this flagged sample, which includes 39,074 observations.

It is important to note, however, that not all the flagged individuals who were arrested after program implementation were deemed eligible for the program, as some did not pass the subsequent individual case checks that occurred when the program was implemented. When presenting the results of our outcome analysis in Chapter 6, we refer to this fuller population of flagged individuals as those that *pass initial screening requirements*, while the individuals that are formally referred to Project Reset are termed the *referred population*. Note that while the individuals who pass initial screening requirements include those arrested both pre- and post-implementation, the referred population will naturally only include individuals whose focal arrest was post-implementation, as this is the only set of individuals who have a referral status.

Overview of Research Design

We analyzed the causal effect of Project Reset using a regression discontinuity design where the date of arrest is the running variable, referred to as a *regression discontinuity in time* research design (Hausman and Rapson, 2018). This research design involves examining how the outcome variables (case outcomes and rearrests) change for individuals that were arrested right before the program was implemented versus right after.⁵ Because the program expanded eligibility criteria over time, the exact date of program implementation will vary across individuals. We thus conducted the analysis separately for each group that fits under a different eligibility criterion. Within each group, all individuals have the same program implementation date.

The intuition behind the regression discontinuity in time design works as follows: suppose we compared the outcomes for individuals arrested on the day the program was implemented (defined as the threshold) with the outcomes for individuals who were arrested the day before the program was implemented. One

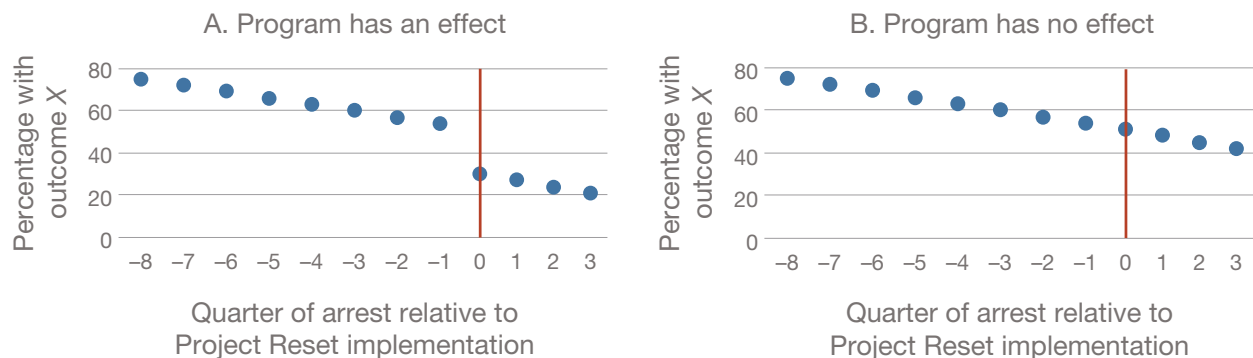
⁵ The regression discontinuity in time design is distinct from a pretest-posttest design; the difference between these two research designs will be explained at the end of this section.

would expect individuals on both sides of the threshold to be relatively similar, and we would also expect the general criminal legal environment for both groups to be the same. Thus, the only difference between these groups is that individuals arrested after the threshold can attend the program, and those arrested before the threshold cannot. This would mean that any difference in their outcomes would reflect the causal effect of the program. Realistically, though, we cannot estimate the program effect in this way because there are not enough individuals who were arrested right around the threshold to identify the effect with any precision. If we instead compared outcomes between individuals arrested in the quarter (i.e., three months) before implementation and the quarter after, we did have a large enough sample. However, because it is likely there are time trends in these outcome variables, we could not assume that the full difference in average outcomes between these quarters is the causal effect of the program. In other words, one might have expected the average outcome for those arrested in the quarter before implementation to be different than the average outcome for those arrested in the quarter after implementation even in the absence of the program (i.e., for differences to occur by chance alone). To address this issue, the regression discontinuity design requires adding in data on arrests from more quarters on either side of the threshold and examining whether a discontinuity in the time trend occurs at the threshold.

To better understand how the design works in practice, Figure 2.1 provides hypothetical examples of how an outcome might vary by individuals' quarter of arrest (i.e., the time trends). The x-axis corresponds to the quarter (i.e., three-month time period) someone was arrested in, and the y-axis presents the average outcome (with respect to either case outcome or rearrest rates) among the individuals arrested in that quarter. Quarter 0 on the x-axis corresponds to the set of arrests that occur in the first quarter after the program is implemented. Quarter 0 thus corresponds to the threshold of implementation. All other quarters are defined relative to quarter 0. For example, quarter -1 corresponds to the individuals who were arrested in the three-month period prior to the implementation of the program.

Both panels in Figure 2.1 present a situation in which outcome variable X is declining over time, as one can clearly see that those arrested in quarter -2 have a lower instance of the outcome than those arrested in quarter -8. However, if the program results in a causal decline of outcome X , one should observe a discontinuous shift down in the time trend at the threshold, as the program will cause a shift down in the outcome beyond the general time trend. This is shown in panel A. In contrast, panel B presents an example whereby the program has no effect on outcome X —here we see a continuation of the same time trend across the

FIGURE 2.1
Hypothetical Example Indicating How the Regression Discontinuity Design Identifies Causal Program Impacts



NOTE: Quarter 0 corresponds to the quarter in which Project Reset was implemented. All other quarters are numbered in reference to this quarter.

threshold that we saw for quarters -8 through -1 . While not shown, if the program increases outcome X , one would observe a discontinuous shift up in the time trend at the threshold. In this way, observing graphs in the same format as Figure 2.1 for each outcome of interest will reveal whether the program has a causal effect on each outcome. In Chapter 6, we present graphs similar to this.

Relatedly, it is also helpful to discuss why a traditional pretest-posttest research design—which is commonly used with historical comparison groups—is not likely to work well here, again using Figure 2.1 as an example. A traditional pretest-posttest design would involve, for example, comparing the average value for a given outcome for those arrested after the change (quarters 0 to 3) with the average outcomes for those arrested over a time period of similar length before the change (quarters -4 to -1). Because this research design will attribute the change in the average outcome measure to the effect of the program, it is important that nothing else is changing over this same time period. However, the examples in Figure 2.1 show there are clear downward time trends in the outcomes, and thus even if the program had no effect, the pretest-posttest design will identify the program as having a negative effect on outcome X . While the time trends presented in Figure 2.1 are just a hypothetical example, it is quite common for time trends to be present in case outcomes and rearrest rates, which are the outcomes we examined. These figures thus indicate that a traditional pretest-posttest design would not work well as it would misattribute the effect of natural fluctuations over time to the program.⁶ In contrast, a regression discontinuity design allows for time trends and we thus felt it was a superior method to identify the causal effects of the program.⁷

Requirements of the Research Design

For the regression discontinuity design to identify the causal effect of the program, three requirements must be satisfied: (1) There must be a discontinuity in program completion at the time of implementation. If there was no discontinuous increase in program completion, then even if the program did have an effect on outcomes, one would not expect to see a discontinuous change in outcomes. (2) Besides the program, there must be no other changes happening at the time of implementation. If there were, then a discontinuous change in outcomes could reflect the effect of those other changes. (3) The time trends in outcomes away from the time of implementation cannot be too noisy, as the noisiness makes it difficult to determine whether the time trends near the threshold are exhibiting a discontinuous change or just a continuation of a noisy trend. A more in-depth discussion of these requirements is discussed in Appendix A.

In order to satisfy the second requirement, it was necessary to make one key modification to our analysis sample. Because around the time of program implementation, DANY made the decision to no longer prosecute individuals for theft of services and possession of marijuana charges, it was necessary to drop all individuals who had these charges as their dominant arrest charge. If these individuals were not dropped, there would have been significant differences in arrest charges for those arrested pre- and post-implementation.

⁶ Other potential options to use with historical data include a difference-in-differences framework and an interrupted time series framework. We did not use a difference-in-differences framework because there was no natural control group that had similar time trends pre-implementation. We opted for a regression discontinuity design over an interrupted time series design because the latter requires estimating counterfactual time trends during the full post-implementation period, while the former only requires estimating this counterfactual at the time of implementation.

⁷ One criticism of using a regression discontinuity in time design to evaluate the effect of a program is that if the program takes a while to implement, this strategy will underestimate the effects (since it is only measuring the effect at the threshold). However, as will be shown in Chapter 6, this should not be an issue, as the program went into effect immediately on the implementation date and continued to operate in a similar way over time.

Presentation of Results

While we have so far discussed the regression discontinuity design entirely in a graphical framework, it is intended to be estimated with a regression framework that compliments the graphical results. The main value that regression estimation adds is that it is possible to identify if the discontinuous jump in the time trend at the threshold is statistically significant or not. To keep the explanations in the main text more accessible, all core outcome evaluation results presented in Chapter 6 are shown in graphs similar to Figure 2.1. In Appendix A, we describe the regression framework and present the regression results to show which outcome effects are statistically significant. We denote the statistical significance of these outcome effects on the graphs shown in Chapter 6 such that the graphs can be fully interpreted independent of the regression results.

Cost Analysis

The cost analysis compared the fiscal outlay of money spent for an individual to complete Project Reset versus the money that would have been spent if the individual had instead had their case adjudicated in court. The main information used to conduct this cost analysis was obtained from program invoices and interviews with relevant agencies about time spent on certain activities and staff wage rates. The key principles of the cost analysis are as follows:

- **The cost analysis compared the cost pathways for the time period between July 1, 2018 and June 30, 2019.** This time period was chosen primarily because the cost analysis required use of the results from our outcome evaluation (presented in Chapter 6), which was only able to identify the causal effect of the program up through June 30, 2019. Note that this date restriction means that the cost analysis did not examine the time period during which Project Reset expanded its eligibility requirements to include those who had prior convictions.
- **The cost analysis required identifying the various activities (or transactions) that are involved in providing programming and in adjudicating a case in court and pricing out each of those activities.** Crumpton, Carey, and Finigan (2004) provides an in-depth discussion of this approach. Identifying the activities on each pathway requires knowledge from the process maps we developed on each of these pathways in Chapter 4. Identifying the activities involved on the court side also required the results from our outcome evaluation presented in Chapter 6. In particular, the outcome evaluation identified what case disposition outcome program completers would have had in court if they did not complete Project Reset. The outcome evaluation also identified if the program had any effect on rearrest rates, which determined whether this cost needed to be accounted for.
- **The cost analysis focused on the marginal cost of program provision, and ignored the fixed costs associated with program startup.** While the start-up costs of the program—which included program curriculum development—were non-trivial, the question we tried to answer was the following: given the situation we are currently in, whereby the fixed costs have already been spent, how does the cost of continuing to run Project Reset compare to the program shutting down and potential participants instead having their case adjudicated in court? Our cost analysis thus only considered program cost elements that would need to continue to be spent to keep the program operational.
- **The cost analysis only accounted for fiscal costs and thus did not incorporate any realized benefits that participants might have received from completing Project Reset programming.** Fiscal costs included any monetary outlay spent to either process a case in court or to fund the diversion program, but did not include any costs that accrued to individuals themselves because these are difficult to monetize (e.g., avoiding interruptions to employment or other collateral consequences of criminal legal

involvement). This means that while individuals that go through programming might be able to avoid coming to court multiple times and instead participate in a programming session that might be more tailored to their underlying needs, we could not account for any of these other potential costs and benefits in our fiscal analysis. It will thus be important to keep in mind that there could have been additional unrealized benefits when we directly compared the fiscal cost components of the court and programming pathways. Put another way, our cost analyses arrived at conservative estimates of the advantages of the program relative to the traditional court approach.

The above framework lays out how to identify the components that need to be accounted for in the cost analysis. For components that involve the labor of individuals (which comprise the vast majority of all components that will need to be accounted for), there are two main ways to calculate these costs. For example, to calculate the cost of adjudicating a case in court one could either: (1) determine the staff positions involved in adjudicating the case in court and the time they spent on a typical case and then cost out these time commitments using the staff wage rates to determine the total cost for a given case; or (2) determine the staff positions involved in adjudicating a case in court and identify the annual salary paid to these individuals to handle these cases and then divide this total by the number of cases they handled in a given year. Generally, the second method is more accurate because it is not subject to recall issues that occur when staff are asked about how much time they typically spend on a case (as occurs with the first method). As we had program invoices but not court staff invoices, we needed to use the first method to cost out the court pathway, but the second method to cost out the programming pathway. The fact that the methods used to cost out each pathway are somewhat different were important to take into account when we presented the results of our cost comparisons.

The staff wage rates referred to in the above discussion should technically include the direct salary paid to the individual for that time amount, the fringe benefits (which are a percentage of the salary the employer pays on top of wages for the staff member's health benefits, time off, etc.), and the indirect cost rate (which reflects the money the organization spends to support staff members that are involved in the main tasks and can include the costs of supervising the staff member, providing them an administrative assistant, technology and resources, etc.) (Crumpton et al., 2004). However, because we were generally not able to obtain the indirect cost rate for staff involved in the court pathway, and because different agencies often include different factors in their indirect cost rates, we made the decision to price out staff costs as only the monetary outlay spent on direct salaries and fringe benefits.

Finally, while we would ideally like to compare the full marginal cost of continuing to run the program with adjudicating these cases in court, we were not able to directly account for all relevant costs on the court side. In particular, cost components of both running the program and adjudicating cases in court include the staff directly involved in handling cases, the staff that do not directly handle cases but are involved in more high-level decisions, and relevant building costs associated with case handling (i.e., the cost of using the courtroom and the cost of using building space to provide programming). Because we obtained invoices from each of the three program providers, we did directly observe this full set of costs for the programming pathway. However, to cost out the court pathway, we did not have access to invoices and were only able to obtain information on the costs related to direct case handling. To ensure balance in our cost comparisons, we thus only included the cost components on the programming side that were directly relevant to case handling (e.g., outreach, intake, and program delivery). We did not include costs on the programming side related to staff that were doing Reset activities outside of outreach, intake, and program delivery (such as supervising staff, participating in high-level coordination activities, data analysis, and budgeting), nor did we include funds that were spent on rent or general upkeep of the office.

Project Reset Eligibility, Screening, and Program Overview

In this chapter, we provide an overview of Project Reset, including eligibility criteria, screening and contact procedures, and a description of program activities. We also provide a case processing flow chart to show how the diversion outreach process works and present summary statistics of those that complete diversion programming.¹ For this chapter, we drew on data from qualitative interviews and administrative data. All quantitative analyses within this chapter are run on the set of individuals referred for programming, or a defined subset of this sample, and thus only use data on arrests that occurred after Project Reset was implemented.

Eligibility for Project Reset

When the Project Reset diversion program first rolled out on February 1, 2018, it had a strict set of eligibility criteria, which are shown in the first panel of Table 3.1. In particular, individuals had to be at least 18 years of age at the time of arrest, have no prior arrests (unless they were sealed), have received a DAT for an eligible charge, be arrested within selected areas of Manhattan, not fall within a public safety exception,² and be involved in a case that did not involve victim restitution. Table 3.2 presents the list of eligible offenses and their distribution among individuals referred for Reset. The offense categories in Table 3.2 are mutually exclusive as they correspond to an individual's dominant arrest charge.

In the two years following initial program implementation, program eligibility, prosecution practices, and criminal legal system policies within New York City shifted dramatically. In particular, the eligibility criteria for the program expanded, with the biggest change occurring in July 2019 when eligible participants were allowed to have prior convictions. Because of this significant change in eligibility criteria, throughout this document we refer to two groups of eligible participants: the *original population*, which includes everyone that was eligible under the initial criteria up through the changes that were made in July 2018 (see Table 3.1), and the *expanded population*, which includes those who were eligible under the new criteria that went into effect in July 2019. Therefore, the original population includes individuals who have no prior convictions (although they might have prior arrests), while the expanded population typically includes those with at least one prior conviction.³

¹ Note that if an arrest is handled through Project Reset, it is not docketed and thus does not technically become a case. However, for simplicity, we frequently refer to these arrests as cases throughout this document.

² Individuals fall within a public safety exception if there is either a gang alert or priority felony recidivist alert issued for them, they are on a city-wide multi-hit list, or there is other information from either NYPD or DANY that the individual poses a public safety concern.

³ Individuals in the expanded population receive different programming than those in the original population. DANY noted that not everyone in the expanded population has a prior conviction. Specifically, individuals that have already completed programming for the original population must do the programming for the expanded population if they participate in Reset

TABLE 3.1
Timeline of Events That Impacted Who Was Diversion Eligible

Initial Program Eligibility	
February 2018	Project Reset rolls out for individuals: <ul style="list-style-type: none"> • who are 18 years of age or older at time of arrest • who are issued a DAT at arrest • who are arrested in Upper Manhattan or Precincts 19, 20, 24, or 25 in Midtown Manhattan^a • who have an eligible charge listed in Table 3.2 • whose case does not involve victim restitution • for whom this is a first non-sealed arrest • who do not present a public safety concern.
Changes to Eligibility, Prosecution Practices, and Arrest Practices	
July 2018	Program rolls out to all of Manhattan <ul style="list-style-type: none"> • Eligibility expands to individuals receiving DATs in Downtown Manhattan and remaining precincts in Midtown Manhattan. Program eligibility expands <ul style="list-style-type: none"> • Eligibility expands to include individuals who have prior arrests (for either misdemeanor or felony charges), so long as they have no misdemeanor or felony convictions.
August 2018	DANY changes prosecution practices: <ul style="list-style-type: none"> • DANY no longer prosecutes marijuana possession cases.
July 2019	Program eligibility expands further: <ul style="list-style-type: none"> • Eligibility expands to individuals with prior convictions, as long as they had no prior sex crimes convictions and no prior violent felony convictions within ten years of the focal arrest. • Individuals who have previously taken part in Reset programming are allowed to participate in the program a maximum of three times.^b
January 2020	New York State passes criminal legal reforms that include: ^c <ul style="list-style-type: none"> • expanded use of DATs • shortened window between arrest date and first appearance date in court, requiring diversion programming to be scheduled in a shorter period of time.
March 2020	COVID-19 pandemic hits and the following occur: <ul style="list-style-type: none"> • arrests fall • court cases are delayed • diversion-eligible individuals are given more time to complete programming (due to case delays).

^a Midtown Manhattan consists of several precincts—only those arrested in Precincts 19, 20, 24, or 25 were diversion eligible between February 2018 and June 2018. From July 2018, all individuals arrested in precincts within Midtown Manhattan were diversion eligible.

^b Individuals were only allowed to participate in programming for the original population once and could then participate in the programming for the expanded population up to two times.

^c Key components of the statewide reforms include bail reform, which ruled out the use of bail for misdemeanor and nonviolent felony charges, and the increased use of DATs. Changes were also made to speedy trial statutes and policies regarding discovery.

All the key events that effected eligibility and caseloads are summarized in Table 3.1, including the state-wide criminal legal reforms passed in 2020 and the COVID-19 pandemic. To better visualize the effect these changes had on the number of eligible individuals, Figure 3.1 shows how the monthly counts of individuals referred to diversion has changed over time. We graphed the average monthly count for each quarter over this time period, with the exception of the first quarter of 2020. Because there were two big changes within this first quarter that hit at different times—state criminal legal reforms in January and the COVID-19 pandemic in March—we separated this quarter into two time periods. We generated separate counts for participants with original eligibility and those with expanded eligibility. While some of the fluctuations in the eligible

again. Further, if someone had a prior arrest outside of Manhattan, they will typically be recommended for the expanded population programming because DANY cannot always observe the disposition for that arrest (i.e., whether it ended in a dismissal or a conviction). Finally, DANY will also recommend individuals for the expanded programming option if they have other pending cases.

TABLE 3.2
List of Diversion Eligible Offenses

Offense	Percentage
Petit larceny	68.7
Trespassing	9.3
Possession of marijuana	4.3
Possession of a controlled substance	4.2
Graffiti	3.0
Possession of stolen property	2.7
Theft of services	2.2
Criminal mischief	1.7
Other offense ^a	3.8

^a The crime category "other" includes: criminal tampering, criminal use of drug paraphernalia, falsely reporting an incident, promoting gambling, reckless endangerment of property, unauthorized sale of certain transportation services, and unauthorized use of a vehicle.

population occur because of natural month-to-month variation in the number of arrests, the changing eligibility criteria and criminal legal environment also had a big effect, which we document in more detail below.

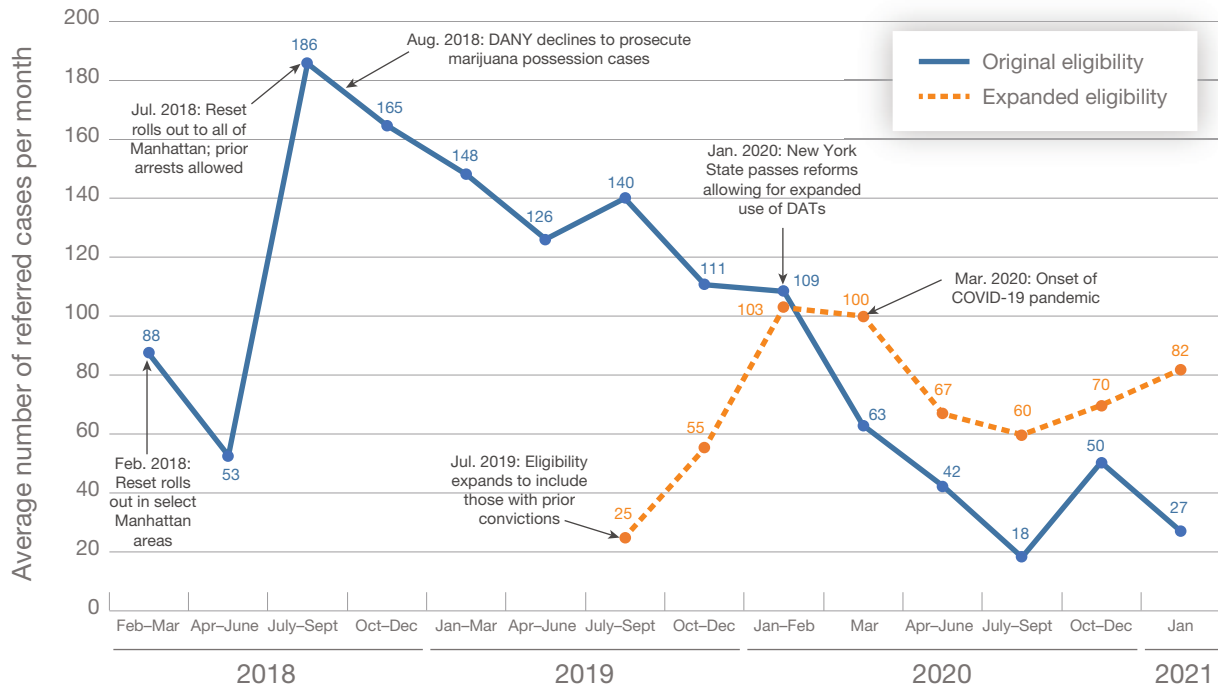
Figure 3.1 indicates that the number of individuals referred to diversion increased substantially in the third quarter of 2018. This was likely primarily because of the fact that the program expanded to all of Manhattan in July 2018 and expanded the eligibility criteria such that those with prior arrests were allowed. However, in August 2018, an offsetting change occurred when DANY began declining to prosecute marijuana possession cases, which previously made up 37 percent of diversion-eligible cases between February and July 2018. Because individuals were no longer arrested for this offense, this led to a decrease in eligible cases going forward.

In January 2020, New York State expanded the use of DATs such that police officers were required to issue a DAT for certain arrest charges. Previously, law enforcement had much more discretion as to whether they would issue a DAT versus use an in-custody arrest which required an individual to be jailed until arraignment. Figure 3.1 indicates this DAT policy change seemed to have a bigger effect on increasing the number of eligible participants for the expanded eligibility population, as eligible individuals in this grouping increased in January–February 2020 (compared with the previous quarter), whereas the number of individuals eligible under the original eligibility requirements continued to decline during this same time period. This is to be expected because the policy change regarding how DATs were issued likely had the biggest effect on those with prior convictions (i.e., after the change, those with prior convictions might have been much more likely to receive a DAT than before).

One eligibility change that is not included in Table 3.1 corresponds to the role of open cases. When eligibility was first expanded to allow prior arrests, DANY required that the individual have no open cases. Eventually, this requirement was relaxed such that the individual could not have three or more open cases. However, DANY is unsure when this criterion was relaxed, and we thus do not show it in the table.

In March 2020, the effects of the COVID-19 pandemic began to be realized in Manhattan, and arrests dropped substantially (Li, 2020), which meant the number of diversion-eligible individuals began to drop as well. Reasons for the drop in arrests include both a decline in enforcement of low-level offenses and a reduction in the commission of eligible crimes.

FIGURE 3.1
Average Number of Cases Referred to Project Reset Per Month, by Each Quarter of Program Implementation



Overall, the number of referred individuals was substantially lower than DANY anticipated. This occurred for two primary reasons. First, when the initial set of eligible offenses was developed, theft of services and marijuana possession comprised roughly half of eligible participants. However, around the same time as the initial rollout of the diversion program in February 2018, DANY stopped prosecuting turnstile jumping (which is the most common offense charged as theft of services); and in August 2018, DANY also stopped prosecuting marijuana possession cases. These two declination policies cut anticipated program referrals in half. The second reason eligibility was lower than initially envisioned was the COVID-19 pandemic, which reduced arrests substantially beginning in March 2020 (as discussed above).

Project Reset Screening

DANY’s Strategic Planning and Policy unit begins the daily screening process by conducting a first pass at those who are potentially eligible.⁴ In particular, they pull cases based on the following eligibility criteria: individual received a DAT, had eligible charges, and was age eighteen or above. The query also screens for the location and date of arrest since the diversion program was rolled out in different areas of Manhattan at different times.

Once the Strategic Planning and Policy unit conducts the initial query of diversion eligible cases, these cases are sent to DANY’s Quality of Life unit, which conducts more in-depth screening on a case-by-case

⁴ This is an accurate description of DANY departments and staff roles during the time period for which Project Reset was being evaluated in this report, although DANY’s office and Reset practices have changed somewhat since then.

basis. Each individual's official record of arrests and prosecutions (RAP) sheet is examined, which is a criminal history database controlled by the Federal Bureau of Investigations (FBI). The unit then examines whether the case involves victim restitution and whether the individual currently has an allowable number of open cases. The unit also checks for any public safety alerts regarding the individual, which would automatically disqualify them from referral to the program. This includes checking whether there is a gang alert or priority felony recidivist alert issued for the individual, or whether they are on a city-wide multi-hit list. Based on these checks, individuals eligible for the program are identified and entered into Salesforce, which DANY uses to communicate with the program providers about Reset cases. Based on the age of the individual and the location of the arrest, Salesforce notes which program provider is assigned the case. If the individual was arrested in Upper Manhattan, the case is referred to the Osborne Association; if the arrest occurred in Midtown Manhattan or if an individual age 21 or older is arrested in Downtown Manhattan, the case is referred to CCI; if the individual is between the ages of 18 and 20 and arrested in Downtown Manhattan, the case is referred to YNY. Salesforce also lists the individual's contact information (which draws directly from the arrest paperwork), details of the arrest, and information regarding the final date of program eligibility.

Project Reset Outreach and Case Processing Flow

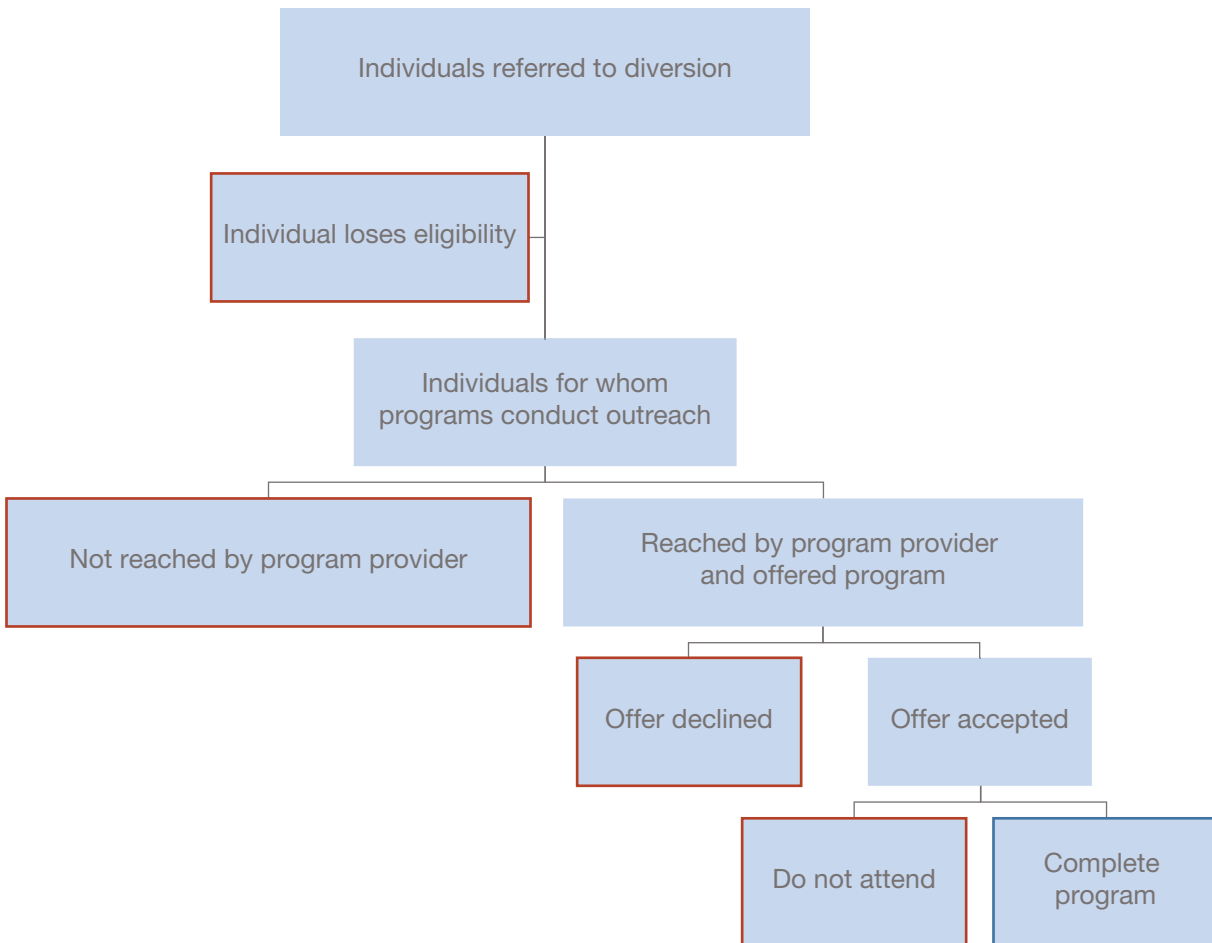
Figure 3.2 shows how the diversion outreach and engagement process works. Program providers are given until a specified date to contact the individual and engage them in programming. During this period, individuals can lose eligibility if they are arrested for a new charge. For those that remain eligible, the program provider will use various methods to contact these individuals. Program providers use the contact information from the arrest paperwork (which was entered into the Salesforce database) when conducting their outreach with individuals. The contact information could include either a phone number, an address, both, or occasionally an email address. Providers reported that they send outreach letters or postcards when an address is available and make telephone calls and send text messages when a phone number is available. Generally, the outreach that participants receive from the program is the first time that they learn about Project Reset.

If providers can reach the individual, they will offer them the opportunity to engage in diversion programming. Individuals offered the program can either accept or decline the offer.⁵ For those who accept, some will complete the program while others might not attend their scheduled programming session. The boxes in Figure 3.2 outlined in red show the various ways eligible individuals might not complete the diversion program—they can lose eligibility, not be reached by providers, lose contact with providers, decline the program offer, or not attend their scheduled programming session. All these scenarios result in a situation in which the providers close out the case in Salesforce and DANY begins prosecution. We discuss the process by which these cases are prosecuted in court in the next chapter. The box outlined in blue shows the only way in which an individual can complete the diversion program.

The specified date by which programming must be completed is determined by the individual's court appearance date listed on their DAT, as prosecutors need to start filing paperwork on the cases being prosecuted roughly two to three weeks before this court date. Between the inception of the diversion program and January 2020, the court appearance date was set about 8–9 weeks after the arrest date. During this time, individuals had roughly six weeks to complete programming from the time of the arrest. This arrangement

⁵ Program staff indicated that some individuals can be skeptical when they first learn of the program because it can seem "too good to be true." As one participant indicated, "I did think it was too good to be true. I had never heard anything about [the program] before. When people get arrested, there's not usually any sort of program that can help them drop their charges."

FIGURE 3.2
Map of Outreach Process



meant that if an individual did not complete programming in this time frame, prosecutors still had the requisite two to three weeks to file the necessary paperwork for cases proceeding in court. This also allowed prosecutors to circumvent all paperwork on cases for individuals who successfully completed diversion programming. In January 2020, the return window for a DAT was shortened to 20 days, and thus the previous procedure was no longer feasible. Instead, individuals had roughly two weeks to schedule their diversion session. Once it was scheduled, prosecutors would delay the court date for one month, and the individual would need to complete programming within that window. Individuals who did not get their programming scheduled within two weeks of arrest were referred for prosecution. Due to the short timeline between arrest and court appearance date, prosecutors were forced to begin working on the paperwork for the case before they knew whether the individual would complete diversion programming. In March 2020, the COVID-19 pandemic delayed court dates for all these cases, which then pushed the window between arrest and first appearance date to at least 120 days (although many cases were delayed further). DANY allowed individuals to have a much longer time frame to schedule and complete programming during this time.

Project Reset Intake Procedures

Each program has somewhat different intake procedures for those who agree to participate in the program. CCI collects demographic data from participants and information about how they learned about the program and their housing status. They also collect information that might determine whether the individual is better suited for a group or individual session (e.g., acute mental health or substance use concerns), as well as information that can be used to give appropriate service referrals. The Osborne Association collects basic demographic information and has participants complete a needs questionnaire which asks about domains such as medical concerns, substance use, mental health, housing needs, employment, education, and family service needs (e.g., childcare). If an individual reports any of these needs, they are offered the opportunity to discuss potential referrals with a program staff member. YNY does not have a formal structured intake; instead, when they reach a potential participant, they provide details about the program and its benefits. If the individual agrees to participate, they share information about the session but wait until the end of the program to collect information from participants (e.g., demographic data).

Program Activities

The core Project Reset program model was designed to be a one-time, two- to three-hour session. This is consistent with the principles of Risk, Need, Responsivity (RNR)—given that the program is designed to serve individuals who are at low risk for future criminal legal involvement, the intervention is brief. It should be noted that Project Reset organizations do not use a risk assessment instrument to assess eligibility among participants. Therefore, the designation of *low risk* largely refers to the fact that eligibility criteria require participants be charged with one of the eligible low-level crimes and have no prior convictions, until the expansion of the program that also allowed individuals with prior convictions.

Organizations had few specific requirements to fulfill when designing their programming, although the program RFP suggested that programs include elements such as community service, restorative justice, and educational programming. That said, the three organizations were allowed latitude to develop a program that met the needs of the population they serve. Each of the three organizations offering programming has developed unique programmatic options, which we learned about through a review of program materials, 14 stakeholder interviews that took place from June to August 2020, and program observations that took place in January 2021. We describe each organization's services in more detail below.

Center for Court Innovation

Prior to COVID-19 and during initial program implementation, CCI offered separate group workshops for participants ages 18 to 24 (the young adult group) and for participants over the age of 25. Most participants completed group workshops, which either took place on-site at CCI (for both age groups) or in partnership with the New Museum in Lower Manhattan (for the young adult group). Although there were some differences in the content for each age group, both groups had certain key similarities. The on-site CCI curriculum was based on principles of procedural justice and restorative justice. Programs generally started with an ice-breaker and review of group norms and expectations, followed by a discussion of the criminal legal system, including where the participants' cases fit into the larger system (e.g., discussing restorative justice and pre-arrest diversion). Participants participated in a discussion of the experiences that led to their arrest and then were given the opportunity to process their own experiences and discuss themes such as feelings of control and perceptions of being a victim versus offender. For the workshops taking place in partnership with the New Museum, a teaching artist co-facilitated the group with a CCI staff member. This workshop

began by asking participants to look at art in a gallery space and then use that experience as an entry point to discussion—for example, discussion of what they see in the art and learning about the artists and themes. This was then used as a segue to connect to discussions related to the criminal legal system, decisionmaking, or other relevant themes and was followed by an art-making exercise. Art and artmaking were used as ways to help people connect and process their experiences.

CCI also developed a curriculum specifically to expanded eligibility clients. It was similar to the original curriculum, beginning with a discussion of the criminal legal system and restorative justice. Rather than a discussion about the arrest experience, though, expanded eligibility participants then participate in a discussion of long-term values. One staff member noted that this is based on their experience that individuals who have prior convictions benefit from a discussion of the values that are important to them and what interferes with pursuing those values.

In addition to the group sessions, CCI offered individual counseling sessions to participants who might have difficulty engaging in a group setting, such as individuals with serious mental health concerns or individuals with a language barrier.

In response to the COVID-19 pandemic and associated restrictions, CCI transitioned to a virtual program model in March 2020. Although the organization has the capacity to offer virtual group programs, at the time we conducted the interviews, staff indicated that they had largely been conducting individual sessions to be responsive to privacy concerns and technology issues. They also had not been able to partner with the New Museum for programming at the time stakeholder interviews were conducted in 2020. These individual counseling sessions include some of the same key content as the group workshops (e.g., discussion of the criminal legal system, focus on principles of procedural justice and restorative justice), but can include more of an individualized focus on the circumstances of the individual. The individual sessions can be shorter than the typical two- to two-and-a-half-hour group sessions. A more recent update from CCI highlighted that virtual groups began to be offered again by spring 2021, which included virtual New Museum groups.

Osborne Association

The Osborne Association offers four workshop options. The first workshop is based on the Social Resilience Model. This workshop begins with biopsychosocial education about the effect of stress and trauma on the body, and then it focuses on teaching participants three skills to disrupt stress reactions: *tracking*, teaching individuals to identify the physiological sensations that accompany a stress response; *resourcing*, a strategy that disrupts the stress reaction by having an individual engage all their senses when thinking of something pleasant; and *grounding*, which teaches participants to bring attention to the body's contact to surfaces.

The second workshop is based on principles of restorative justice. This workshop focuses on nonviolent conflict resolution, including education on types of conflicts and how to use strategies such as consensus building and conflict resolution skills. This workshop is offered through a partnership with the Center for Justice at Columbia University.

The third workshop is a Narcan training during which participants learn about opioid use in New York City and how to administer Narcan if they are with someone who they think might be experiencing an overdose. Participants receive a Narcan kit from the Osborne Association that includes a standing, refillable prescription that can be filled by the Osborne Association or through a pharmacy.

The fourth option is to participate in Community Benefit Projects, which are community service events organized in partnership with organizations across Harlem and northern Manhattan.

Original eligibility participants select one of these programs, which are generally about two hours in length; expanded eligibility participants select two programs to complete.

Since the beginning of the pandemic to the time of the interviews, the Osborne Association also began offering a Narcan and Social Resilience Model workshop. Individuals who participated in the Narcan work-

shop could pick up their Narcan kit from the Osborne Association office or have the kit mailed to them. These workshops could take place via telephone or through a virtual meeting. A small number of participants also opted into individual Community Benefit Days (e.g., assisting a local food pantry).

Young New Yorkers

YNY initially offered an arts-focused, 3-hour workshop that took place at the Swiss Institute. The organization uses art and artmaking to guide young people through their criminal justice experiences, and workshops are led by practicing teaching artists. Programs begin with an icebreaker exercise during which participants share how they came to the program. The workshop then shifts to discussing portraiture and how people in different roles present to the world and then engages participants in taking pictures of one another reflecting different identities or occupations (e.g., president, music artist). After this exercise, teaching staff lead a discussion of the criminal legal system, policing, and collateral consequences of justice system involvement. This is followed by a pair of exercises focused on decisionmaking in which participants evaluate the choices they made that led to their arrest and then identify their short- and long-term goals. The session concludes with an artmaking exercise that uses the portraits taken earlier in the session. Interviewed staff members reported that the curriculum was designed as an alternative between more relaxed arts exercises and more intensive social justice and criminal legal-focused content, and throughout all sessions, participants are given an opportunity to reflect on their decisions. As a culminating event, YNY also has public art exhibitions, often in the courtroom, which they described as “a way to transform the ‘point of trauma’ for the young person and humanize them for legal professionals.”

When asked about programming for expanded eligibility participants, YNY staff noted that they receive few referrals for this group. This was also confirmed in the administrative data. However, YNY also serves a narrower population (i.e., individuals 18–20 years old).

YNY, like the Osborne Association, has also adapted programming over time. In response to COVID-19, sessions moved to a virtual format that takes place over 1.5–2 hours. As we learned at the time of the interviews, some content was adjusted (e.g., the icebreaker was adapted to a virtual format), and YNY now sends participants program materials in advance of the session. One staff member interviewed estimated that about 40 percent of participants have participated via video conference (e.g., Zoom) and the remaining 60 percent via telephone. In summer 2020, the organization received referrals of several participants who were arrested during the protests taking place in New York City in the wake of the murder of George Floyd. They tailored their curriculum to discuss the history of nonviolent protests and engage in a goal-setting exercise. YNY also hosted a virtual art exhibition with the Museum of Modern Art PS1 in 2020.

Completion of Program and Referrals to Outside Services

After participants complete Project Reset, program provider staff enter the completion information into Salesforce to notify DANY. Staff of the Osborne Association and CCI also provide participants with a completion letter from their program, which participants can use if they need to provide documentation to their employer or school. Participants can also obtain documentation of the decline to prosecute. CCI staff noted that DANY mailed all participants an official decline to prosecute letter and began to email the letters during the pandemic. However, it is unclear how consistently participants received this information, as some participants interviewed reported that they had not received official documentation that their arrest had been sealed. Program staff noted that they sometimes help participants navigate the request process for this documentation if a copy is needed. CCI staff indicated that they also provide *seal verification forms* to clients, which clients can use to contact DCJS to verify that their arrest has been sealed.

Program providers were encouraged to offer referrals to additional supports and services following program engagement, though such referrals are voluntary for participants. Each organization handled referrals somewhat differently. CCI initially discussed service needs during their intake process but found that participants did not always know what their needs were or did not feel comfortable sharing them with program staff. Now, they set aside time during program activities to discuss voluntary referrals to services. Most referrals are to off-site organizations and might relate to housing, education, or employment needs. A community liaison at CCI facilitates connections to these services. In addition, CCI offers participants voluntary short-term individual counseling (up to six sessions) facilitated by a staff social worker. For participants with more serious mental health needs, CCI provides a referral to outside services.

As described previously, the Osborne Association asks participants to complete a needs assessment during the intake. They offer voluntary referrals and case management to those with identified needs. The Osborne Association offers several different services from within the organization and is also able to make referrals to other nonprofit organizations located within the same building as the Osborne Association offices. One staff member noted that they keep cases open for a year and check in with past participants to see if they need additional services.

YNY offers a program specifically for Project Reset graduates, called Reset Ultra. Individuals can opt in to Reset Ultra, which offers monthly events and provides a stipend for those who participate. During the early stages of the COVID-19 pandemic, Ultra participants expressed interest in more frequent sessions, and they were taking place on a weekly basis for a period of time. Ultra sessions vary in nature and include presentations from community partners who present on paid opportunities (e.g., internships) or other community resources (e.g., free health care), arts-focused activities, and movement or exercise-focused activities. The organization also shares these opportunities with individuals who opt in to Reset Ultra, even if they do not attend a session. YNY also started a program for Project Reset graduates who had lost a family member or friend because of COVID-19. In addition, the organization occasionally discusses referrals to community-based services if a specific need comes up during the workshop. This includes referrals to services to address socioeconomic factors that might have contributed to their criminal justice involvement.

Summary Statistics of Program Participants

Table 3.3 presents summary statistics of the individuals who completed the diversion programs (who are a subset of the referred population), both overall and separately for each program. Program providers collect more detailed information on the individuals that complete programming, including their primary language spoken, education level, and employment status. However, the Osborne Association did not collect information on primary language spoken and only more recently started collecting data on employment status; as a result, a high percentage of participants had unknown values for these characteristics.

With the exception of the last category (referrals for outside services), all other categories in Table 3.3 are mutually exclusive such that an individual will only be classified in one of the options for that category. The results from the first column indicate that about half of those who complete diversion programming are female and about three-fourths are non-White. Most participants (where language was known) list English as their primary language, and almost one-third of participants had at least a college degree. As would be expected based on the assignment scheme, each program only draws from certain neighborhoods.⁶ Seventy percent of participants have no prior arrests, while only about 10 percent have a conviction record (and thus

⁶ There are a couple exceptions to this, where a program will include an individual that is not from their neighborhood catchment area; these are likely to be data errors regarding the neighborhood the individual is from.

TABLE 3.3
Characteristics of Individuals Completing Diversion

	Completed Programming with			
	Total (%)	CCI (%)	OA (%)	YNY (%)
Gender				
Female	50.7	53.3	44.3	50.7
Male	49.2	46.7	55.5	49.3
Race/Ethnicity				
White non-Hispanic	25.9	30.4	15.3	23.5
White-Hispanic	22.7	20.1	29.2	22.1
Black non-Hispanic	30.1	27.9	36.3	27.2
Black-Hispanic	6.0	4.1	11.3	2.9
Asian/Pacific Islander	13.3	15.1	6.5	22.8
Other/Unknown	2.0	2.4	1.4	1.5
Age Group				
18–20	20.2	12.5	20.6	99.3
21–24	17.4	19.5	16.0	0.3
25–29	17.3	17.6	20.4	0.0
30–39	18.1	19.8	18.2	0.0
40–49	11.0	12.3	10.3	0.0
50 and above	16.1	18.3	14.4	0.0
Primary Language Spoken				
English	51.2	69.4	0.0	79.4
Spanish	4.7	6.4	0.0	6.6
Other	5.1	6.9	0.0	8.1
Unknown	38.9	17.3	100.0	5.9
Education				
Less than high school	5.1	5.7	3.3	5.9
High school/General Education Diploma	37.6	36.5	43.6	24.3
Some college	21.2	16.4	23.2	62.5
College degree	24.7	30.5	16.2	0.0
More than college	8.2	10.8	4.0	0.0
Education unknown	3.2	0.1	9.8	7.4
Employment				
Currently employed	45.0	56.6	18.9	35.3
Not employed	37.3	43.4	17.2	58.8
Unknown	17.7	0.0	63.9	5.9

Table 3.3—Continued

	Completed Programming with			
	Total (%)	CCI (%)	OA (%)	YNY (%)
Neighborhood of Arrest				
Downtown	27.5	31.9	0.0	99.3
Midtown	45.6	68.1	0.7	0.7
Uptown	26.9	0.1	99.3	0.0
Criminal History				
No previous arrests	70.3	68.2	72.5	83.1
Misdemeanor arrest record	14.8	16.6	11.9	8.8
Felony arrest record	5.0	5.2	5.0	2.9
Conviction record	9.8	9.9	10.7	5.1
Dominant Offense Charge Type				
Petit larceny	69.7	78.5	47.4	72.1
Possession of controlled substances	5.1	1.1	15.8	0.7
Trespassing	7.7	5.9	11.5	10.3
Criminal mischief	2.0	2.5	0.5	3.7
Graffiti	3.2	3.6	0.9	8.8
Other eligible offenses	12.3	8.3	23.9	4.4
Specific Program Received				
Group Counseling	39.2	58.8	0.0	0.0
Individual Counseling	21.9	32.9	0.0	0.0
New Museum Workshop	5.5	8.2	0.0	0.0
Social Resilience Model Workshop	2.3	0.0	8.6	0.0
Restorative Justice Workshop	9.4	0.0	34.9	0.0
Narcan/Naloxone Treatment Training	15.3	0.0	56.4	0.0
Arts-Based Restorative Justice Intervention	6.3	0.0	0.0	100.0
Unknown	0.0	0.0	0.2	0.0
Referrals for Outside Services				
Referred to any service		14.9		
Referred to employment services		3.2		
Referred to financial services		2.8		
Referred to housing services		0.8		
Referred to immigration services		4.2		
Referred to legal services		2.1		
Referred to mental health services		2.1		

Table 3.3—Continued

	Completed Programming with			
	Total (%)	CCI (%)	OA (%)	YNY (%)
Referred to other services		3.1		
Observations	2,149	1,431	582	136

NOTES: OA = Osborne Association. Due to rounding within categories, sums are not always 100.

attend programming for the expanded eligibility population). While most participants were charged with petit larceny as their dominant offense, offense type does vary by program type (likely because offense type varies by neighborhood).

The program providers also record information on which specific program the individual completed. Note that these programs are all specific to the providers. As noted earlier in this chapter, CCI offers both group and individual counseling, as well as the New Museum program; group counseling was the more popular option among participants. The Osborne Association offers the Social Resilience Model Workshop, the Restorative Justice Workshop, and Narcan/Naloxone Treatment Training; the Narcan option was the program component most individuals completed. YNY only offers the Arts-Based Restorative Justice Intervention, and thus all of their participants completed this program.

While all program providers offered referrals to outside services, only CCI provided data on the frequency of these referrals. The first row of the last panel of Table 3.3 indicates CCI referred about 15 percent of participants to at least one outside service. The remaining rows indicate how often participants were referred to particular services; note that some individuals might have been referred to more than one service, so the sum of these values exceeds 15 percent. The most common referrals were to employment services and immigration services.⁷

Summary

This chapter described the Project Reset program model and eligibility criteria, including how these have changed over time. This chapter also described the overall screening process and case processing flow. Regarding program activities, each of the organizations offers distinct options for participants. Though many of the offerings explicitly incorporate elements of psychoeducation about the criminal legal system, principles of restorative justice, and arts-based activities, the programs have had flexibility to design workshops that they believe best address the needs of the populations they serve.

⁷ The types of services included in the *other* category include services for education, benefits, food, reentry, substance abuse, and the UpNext program.

Case Flow Through Project Reset and the Courts

In this chapter we present process maps of the outreach process for Project Reset under the original eligibility requirements and under the expanded eligibility requirements. For the individuals that do not complete programming, we present process maps of how these cases are adjudicated in court. As the court process changed substantially with the onset of the COVID-19 pandemic, we present separate process maps for arrests that occurred prior to the pandemic versus arrests that occurred after the onset of the pandemic. All the process maps in this chapter were constructed from the sample of individuals who were referred to Project Reset.

Project Reset Process Maps

Figures 4.1 and 4.2 present process maps of the outreach process for individuals who were referred to Project Reset under the original eligibility requirements (3,598 observations) and under the expanded eligibility requirements (1,218 observations), respectively. As was the case with Figure 3.2, the boxes outlined in red show the various ways individuals might ultimately not complete the diversion program, while the box outlined in blue shows the only way in which an individual can complete the diversion program.

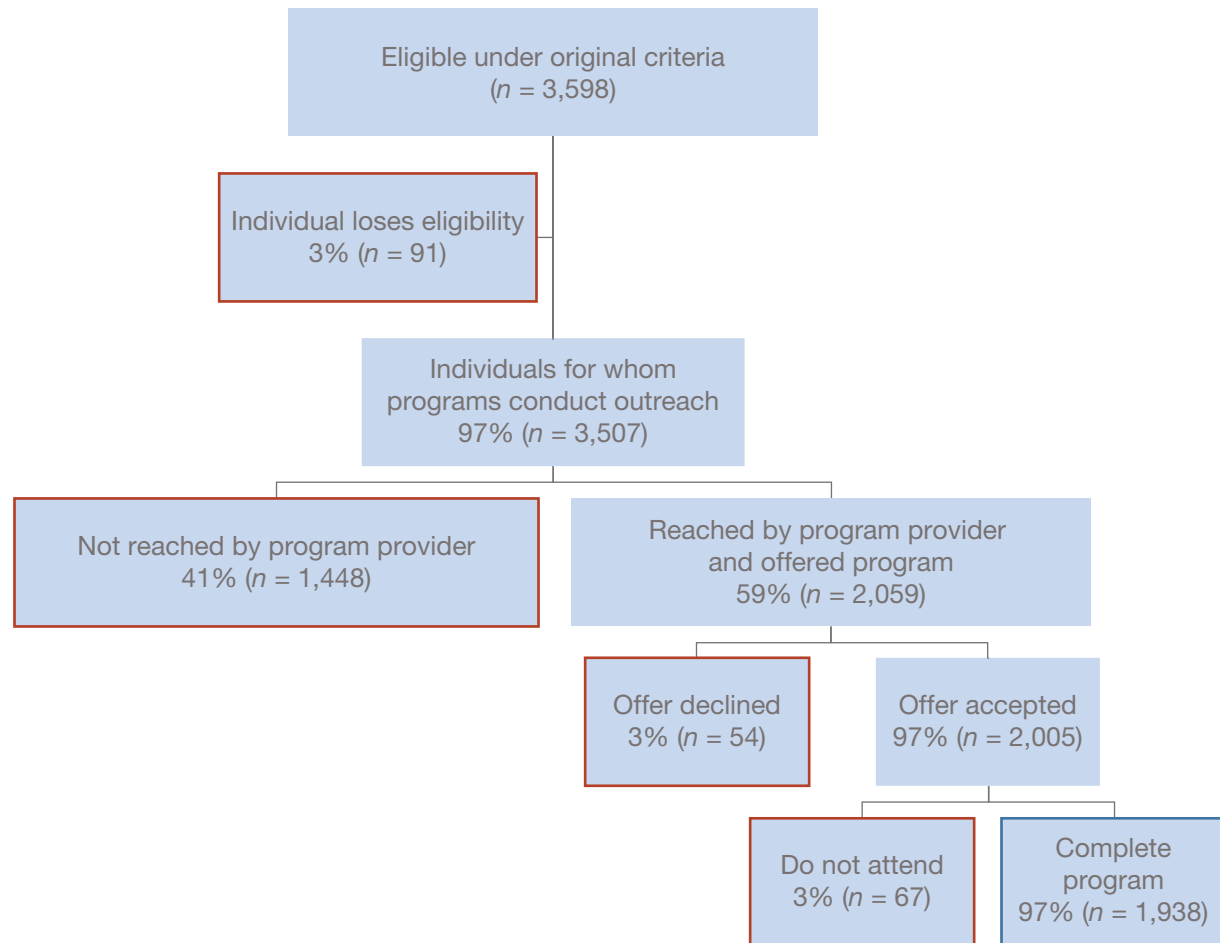
Figure 4.1 indicates that 3 percent of originally eligible individuals became ineligible after they were referred to the program because they were arrested again prior to completing programming. Among those who remained eligible and were referred to the program, only 59 percent could be reached by the program provider. Based on discussions with program providers, there were two key reasons why outreach was so difficult: (1) the contact information listed was invalid (either it was wrong or was incomplete and effectively non-actionable; this occurred, for example, when an address only included the street but not the building number), or (2) at least some of the information seemed correct but they could not get in contact with the individual (which was particularly prevalent in situations where a phone number was not listed even if an address was listed).¹ Providers noted that outreach tended to be more successful when they had an accurate telephone number. In Chapter 5, we provide more details on the characteristics of who could and could not be reached.

Of those reached by providers and offered the program, 97 percent of the individuals accepted the offer while 3 percent declined the offer. Of those who accepted the offer, 97 percent actually completed the program, while the remaining 3 percent did not attend their scheduled session. Of the total 3,598 individuals that were referred for diversion under the original eligibility criteria, 54 percent completed diversion and 46 percent had their cases referred for prosecution.

Figure 4.2 demonstrates the process for individuals that were referred to Project Reset under the expanded eligibility requirements. As compared with the group eligible under the original eligibility criteria (who did

¹ DANY noted that the address provided by the individual generally should be correct, because individuals typically need to provide a form of identification to law enforcement that verifies their address. However, many of these individuals might still not be reached because the outreach is conducted via letter; potential participants likely often do not open these letters. In other cases, for instance, the individual may live with other people who somehow interfere with the individual receiving the letter.

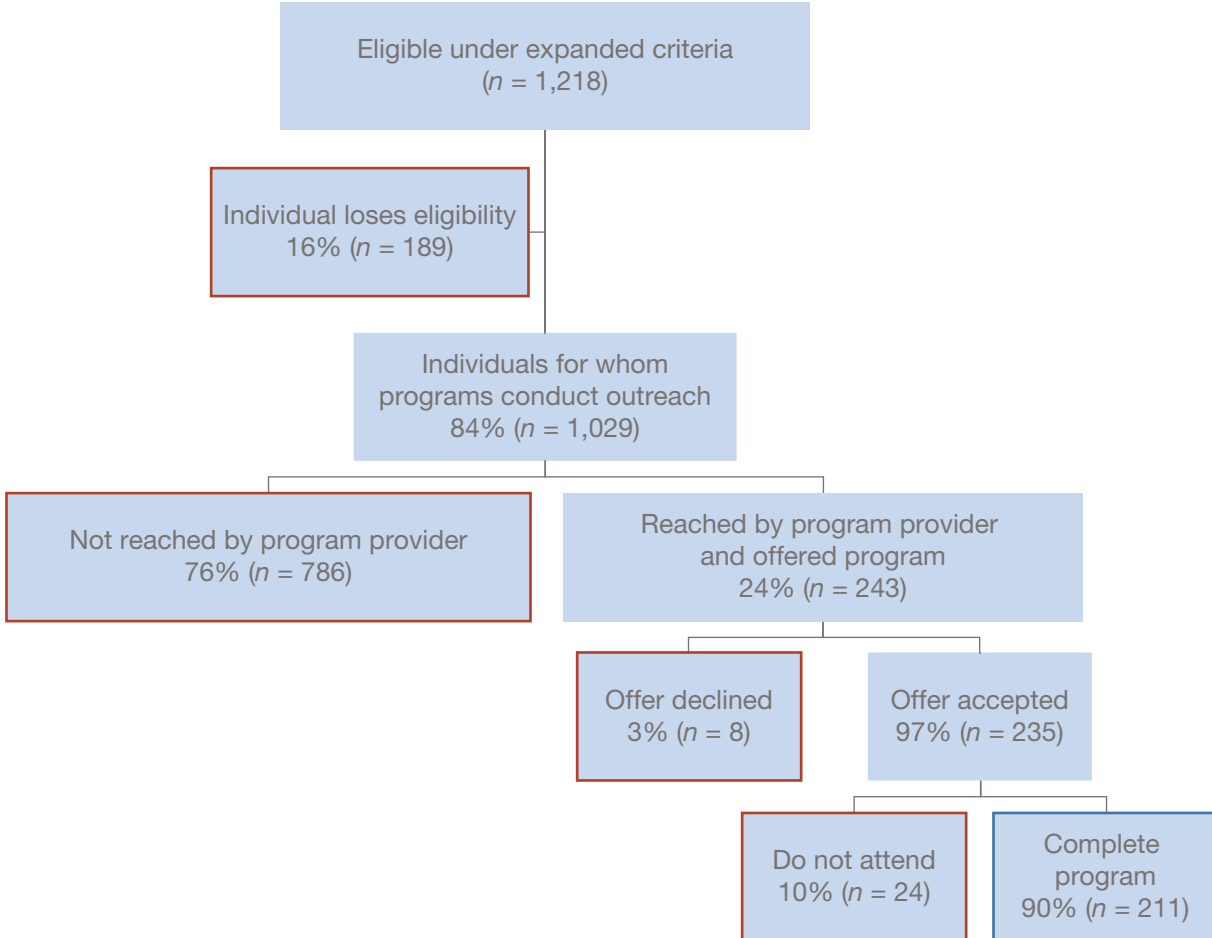
FIGURE 4.1
Map of Outreach Process for the Original Eligibility Population



not have prior convictions), there were a few key differences in attrition rates. First, a greater percentage of individuals lost eligibility after being referred (16 percent versus 3 percent of the original population). This is perhaps to be expected because potential participants typically lost eligibility as a result of a rearrest after referral, and the expanded eligibility group already had higher rates of prior arrest. Second, the proportion of eligible individuals that staff were unable to reach was much higher for the expanded eligibility group than for the original eligibility group (76 percent versus 41 percent). Finally, among those that accepted the offer to do the program, a larger proportion of individuals in the expanded eligibility group did not attend their scheduled session (10 percent versus 3 percent). These factors combined to make the rate of successful program completion much lower for individuals that were referred to the program under the expanded eligibility requirements: of the 1,218 individuals who were referred, only 17 percent completed programming (compared with 54 percent referred under the original eligibility criteria).

While thus far we have not mentioned NYPD's role in our discussion of diversion-eligible case processing, it is important to discuss their role in the overall diversion process. NYPD decisions play a large role in determining who is eligible, as they determine both what offenses to charge the individual with and whether they are going to issue them a DAT versus an in-custody arrest. In addition to this, Figures 4.1 and 4.2 indicate that many diversion-eligible individuals do not complete programming because providers cannot contact them via NYPD-procured contact information. The ability of NYPD to collect accurate and thorough

FIGURE 4.2
Map of Outreach Process for the Expanded Eligibility Population



contact information likely depends on the nature of the interaction between the officer and the individual arrested and the extent to which NYPD informs the individual that the contact information can be used to contact them regarding diversion programming. Our conversations with DANY indicated that the original vision was for NYPD to be the initial point of outreach for the program—the NYPD officer would provide the individual a flyer about the program and hopefully would discuss the purpose of the program, encouraging them to participate. However, based on what we learned in our qualitative interviews, it seemed that once eligibility requirements were relaxed (particularly with the onset of allowing those with prior convictions to participate), NYPD involvement in informing individuals about the program declined markedly.²

The Court Adjudication Process

In this section we discuss the process for cases that were referred to Project Reset but ultimately did not complete programming. These cases consist of individuals who did not complete programming for a variety

² Staff members noted that for the youth pilot of Project Reset, implemented by CCI, NYPD was more involved in the referral process and often told individuals about the program at the point of arrest (Dalve and Cadoff, 2019). This allowed potential participants to be familiar with the program when they received outreach from the program.

of reasons—either they lost eligibility, they were not reached, they declined the offer, or they did not attend. Thus, this analysis takes the samples used to construct Figures 4.1 and 4.2 and drops the set of individuals that complete programming. All these cases are then adjudicated by DANY. As will be discussed in the next section, the COVID-19 pandemic had a dramatic effect on how these cases were handled in court. Our analysis in this section thus focuses only on individuals who were arrested before January 1, 2020, so that their case progression should largely have been unaffected by the pandemic. These restrictions result in a sample of 1,495 individuals, including both those who were referred under the original criteria and the expanded criteria. For this sample we observed all case events that occurred through February 1, 2022.

The analyses presented in this section are intended to provide a more descriptive understanding of what the court process looks like but should not be interpreted as the causal effect of not completing diversion. Because of the way the outreach process works, those who completed diversion are likely systematically different than those who do not (as will be shown in more detail in Chapter 5), and thus we cannot assume that what happens to the individuals who went through the court process is exactly what would have happened for the individuals who completed diversion programming had they gone to court. In Chapter 6, we will examine in more detail what the likely court outcomes would have been for the individuals that complete diversion.

Figure 4.3 indicates that DANY declines to prosecute 4 percent of these cases. For the remaining 96 percent of cases where DANY proceeds with prosecution, individuals need to appear in court for an arraignment and additional court appearances in the future, if necessary. Among those that are prosecuted, there are five different outcomes that can occur, which are discussed in detail below. Most of these cases were resolved with an adjournment in contemplation of dismissal (ACD), which means the case was dismissed if the individual met certain conditions for a given time period (often six months).³ Some ACDs only require that the individual remain arrest-free over the supervision period, while others require that the individual also do community service or complete programming. When additional conditions such as these are attached, the individual usually has additional court appearances during the supervision period to ensure they are complying with the conditions of their ACD. Of the cases prosecuted, 57 percent received an ACD.⁴

Thirteen percent of prosecuted cases ended in a plea deal where the individual was convicted of a violation or infraction, while 5 percent of prosecuted cases ended in a plea deal where the individual was convicted of a criminal charge. During this time period, there were no convictions that occurred outside of a plea deal. Because all the individuals in this sample were originally charged with a misdemeanor offense, those that were convicted of a violation or infraction were given a deal that involved pleading guilty to a lesser charge. Violations and infractions are not considered a crime and thus do not generate a criminal record.

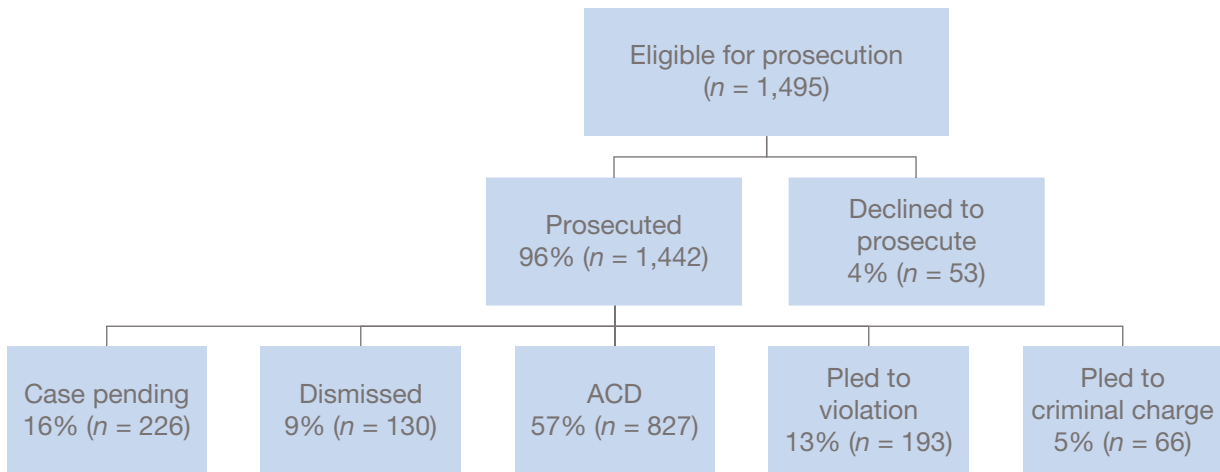
Nine percent of prosecuted cases ended in a dismissal that was not part of an ACD. Many of these dismissals involved the individual making multiple court appearances before their case was dismissed. Finally, 16 percent of cases prosecuted were still pending at the time of this study. Generally, almost all these pending cases occurred because of a failure to appear—either individuals never appeared for their arraignment, or they did not attend a future necessary court appearance. As a result, the case was stalled from moving forward.

³ ACDs for marijuana offenses have a supervision period of up to one year; ACDs for the remainder of offenses that are diversion eligible should have a supervision period of six months.

⁴ Declining to prosecute a case and dismissing a case have the same legal effect. The key difference is that with a declination to prosecute, the case is never docketed and thus does not generate a court record. However, the record of the arrest is sealed for both declinations and dismissals.

FIGURE 4.3

Process Map of Cases Eligible for Prosecution Prior to the COVID-19 Pandemic (Among Non-Participants Who Were Referred to Project Reset)



The Impact of COVID-19 on the Court Adjudication Process

The COVID-19 pandemic necessitated several changes in how cases were handled by the court system. We detail some of these changes in this section. While Figure 4.3 was constructed using cases adjudicated by DANY where the date of arrest was prior to January 1, 2020, Figure 4.4 instead uses the subset of cases adjudicated by DANY where the date of arrest was between March 14, 2020 and January 31, 2021.⁵ Figure 4.4 shows how these 853 cases progressed through the court system.

A comparison of Figures 4.3 and Figure 4.4 indicates that there are clear differences in how cases were handled in court after the onset of the pandemic. Generally, the figures indicate that because court appearances were difficult, DANY tried to close out as many of these cases as possible through either a declination of charges or by dismissing charges once they had been filed. For example, while only 9 percent of prosecuted cases pre-pandemic were dismissed, 58 percent of these cases were dismissed once the pandemic began. On the other hand, cases were significantly less likely to be disposed of with an ACD, likely because these dispositions often require several court appearances. In deciding whether to prosecute a case during the pandemic, DANY weighed many factors related to the court and jail system, public safety, and public health; they determined that many of the cases eligible for pre-arraignment diversion should be dismissed in the interest of justice. Collectively, these results indicate it will be important to account for the effects the pandemic had on the disposition of court cases when the outcome analysis is conducted.

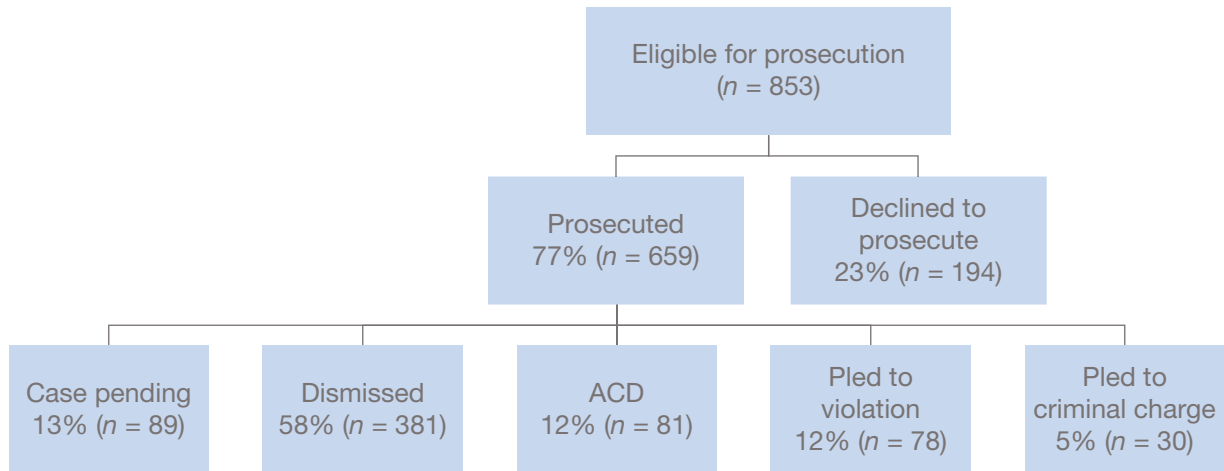
Summary

This chapter summarized the disposition process for individuals who were referred to Project Reset. The process maps highlighted a key area of attrition: a significant percentage of individuals referred to the program

⁵ These analyses are not looking at the set of cases where arrests occurred between January 1, 2020 and March 13, 2020, because the lifetime of those cases somewhat straddle the COVID-19 pandemic, making it difficult to cleanly examine the effect of the pandemic. Specifically, the early events for those cases would have been pre-pandemic, but later events would have occurred during the pandemic.

are not reached by program providers. This results in only 54 percent of individuals who are referred to the program under the original criteria completing programming. The attrition rate is even larger among those referred under the expanded criteria; only 17 percent of these individuals complete programming. Among individuals that have their case adjudicated in the courts, many received an ACD prior to the pandemic. However, once the pandemic began, these cases were more commonly either declined by DANY or dismissed.

FIGURE 4.4
Process Map of Cases Eligible for Prosecution During the COVID-19 Pandemic (Among Non-Participants Who Were Referred to Project Reset)



Factors That Impact Program Outreach and Completion

The process maps in Chapter 4 indicate that, for the sample of individuals referred to Project Reset, there is a large amount of attrition in terms of who is successfully contacted at the outreach stage. To better understand what types of individuals have less access to Project Reset, in this chapter we provide summary statistics of the referred individuals that do and do not complete programming and then use regression techniques to identify the characteristics that are predictive of being successfully contacted by program providers. The starting sample used for the analyses presented in this chapter is the same as in the previous chapter and includes the 4,816 individuals referred to Project Reset. Several of the analyses presented restricted this sample further.

Factors Associated with Successful Outreach and Program Completion

Table 5.1 examines what characteristics are correlated with completing programming. Column 1 presents summary statistics on the composition of the individuals that were referred to Project Reset. This set of individuals was then stratified into two mutually exclusive subsamples shown in Columns 2 and 3—those who completed programming and those who did not. The characteristics that are presented in the table were selected because these were factors we observed of everyone in the sample and because they potentially could effect whether a referred individual ended up completing programming. Note that characteristics such as language spoken, education level, and employment status were only collected for individuals who completed programming. Thus, while they could be used in Table 3.3, they cannot be used here.

A comparison of the demographic characteristics between Columns 1 and 3 demonstrates the effect of attrition (because of failures during the outreach process) on who completes programming. In particular, the results show that among those who are referred for programming, both female and younger individuals are more likely to complete the program. With respect to race and ethnicity, White Hispanic, Black non-Hispanic, and Black Hispanic individuals are all less likely to complete programming than White non-Hispanic and Asian eligible individuals. With respect to case characteristics, a comparison between Columns 1 and 3 indicates that criminal history is strongly correlated with whether a referred individual completes programming.

Because many of the factors examined in Table 5.1 are correlated with each other (including criminal history and demographic characteristics), these analyses cannot identify the extent to which each one of these variables is separately predictive of completing programming. To isolate the effect of a given characteristic, we regressed whether someone could be reached on all the characteristics presented in Table 5.1; these results are presented in Table 5.2. Note that while Table 5.1 focuses on who actually completes the program, Figures 4.1 and 4.2 indicate that the key area of attrition for referred individuals is in who is reached, as the vast majority of individuals that are reached will complete programming. We thus felt it was most useful in Table 5.2 to examine which referred individuals could be reached. Note that because we focused on who could be reached, we dropped individuals that lost eligibility during the outreach process.

TABLE 5.1
Summary Statistics by Program Completion Status

	All Individuals Referred to Reset Who		
	All Individuals Referred to Reset (%)	Do Not Complete Programming (%)	Complete Programming (%)
	(1)	(2)	(3)
Gender			
Female	40.1	31.6	50.7
Male	59.8	68.4	49.2
Race/Ethnicity			
White non-Hispanic	23.6	21.8	25.9
White Hispanic	23.8	24.7	22.7
Black non-Hispanic	34.0	37.1	30.1
Black Hispanic	7.4	8.6	6.0
Asian/Pacific Islander	9.4	6.3	13.3
Other/Unknown	1.7	1.4	2.0
Age			
18–20	15.9	12.4	20.2
21–24	15.4	13.8	17.4
25–29	17.0	16.7	17.3
30–39	21.8	24.8	18.1
40–49	13.1	14.7	11.0
50 and above	16.9	17.5	16.1
Criminal History			
No prior arrests	54.7	42.1	70.3
Prior misdemeanor arrest	13.9	13.2	14.8
Prior felony arrest	6.1	6.9	5.0
Prior conviction	25.3	37.8	9.8
Dominant Offense Charge Type			
Petit larceny	68.6	67.7	69.7
Possession of controlled substances	4.4	3.9	5.1
Trespassing	9.3	10.6	7.7
Criminal mischief	1.7	1.4	2.0
Graffiti	3.0	2.8	3.2
Other eligible offenses	13.0	13.6	12.3

Table 5.1—Continued

	All Individuals Referred to Reset Who		
	All Individuals Referred to Reset (%)	Do Not Complete Programming (%)	Complete Programming (%)
	(1)	(2)	(3)
Neighborhood of Arrest			
Downtown	27.5	27.5	27.5
Midtown	45.6	45.7	45.6
Uptown	26.9	26.8	26.9
Year of Arrest			
2018	28.7	24.3	34.2
2019	37.7	31.8	45.0
2020	31.3	40.6	19.7
2021	2.3	3.3	1.0
Observations	4,816	2,667	2,149

NOTE: As documented in Figures 4.1 and 4.2, among referred individuals that do not complete programming, 11 percent lose eligibility, 84 percent are not reached, 2 percent decline the offer, and 3 percent accept the offer but do not attend. Due to rounding within categories, sums are not always 100.

Table 5.2 presents the results from logit regressions for this decision point in an intuitive way. Each entry in the table corresponds to the predicted probability from a logit specification that an individual with that characteristic will be reached, holding constant the effects of the other characteristics included in the model. Note that while our model accounts for all variables shown in Table 5.1, we only present the results for a more limited subset in Table 5.2.

The first entry in Table 5.2 shows the predicted probability of being reached by program providers for someone who is male and average with respect to all other characteristics included in the model. The second entry shows the predicted probability of being reached for someone who is female and, again, average with respect to all other characteristics included in the model. In this way, by comparing the predicted probabilities within each variable grouping, we isolated the impact of that characteristic on the likelihood of being reached from other characteristics. We treated the first entry in each variable grouping as the reference group, and the notes indicate whether the predicted probability for the other values in the grouping are significantly different than for the reference group. For further ease of interpretation, Figure 5.1 presents a bar chart of these results.

Figure 5.1 indicates that the characteristic most predictive of whether outreach will be successful is criminal legal history. While those with no prior arrests have a 60 percent chance of being reached, the chance of being reached goes down by almost 20 percentage points for those with a prior felony arrest and by more than 30 percentage points for those with prior convictions. This results in those with a prior conviction only having a 27 percent chance of being reached by program providers.

It is important to discuss the role of race and ethnicity in whether individuals move forward at the outreach stage. In results not shown in the tables, we found that the rate of successful outreach for White non-Hispanic, Black non-Hispanic, White Hispanic, and Black Hispanic individuals are 54 percent, 47.2 percent, 48.4 percent, and 41.8 percent, respectively. All these differences between White non-Hispanic individuals and the other three race and ethnicity groups are statistically significant. Table 5.2 indicates the magnitude

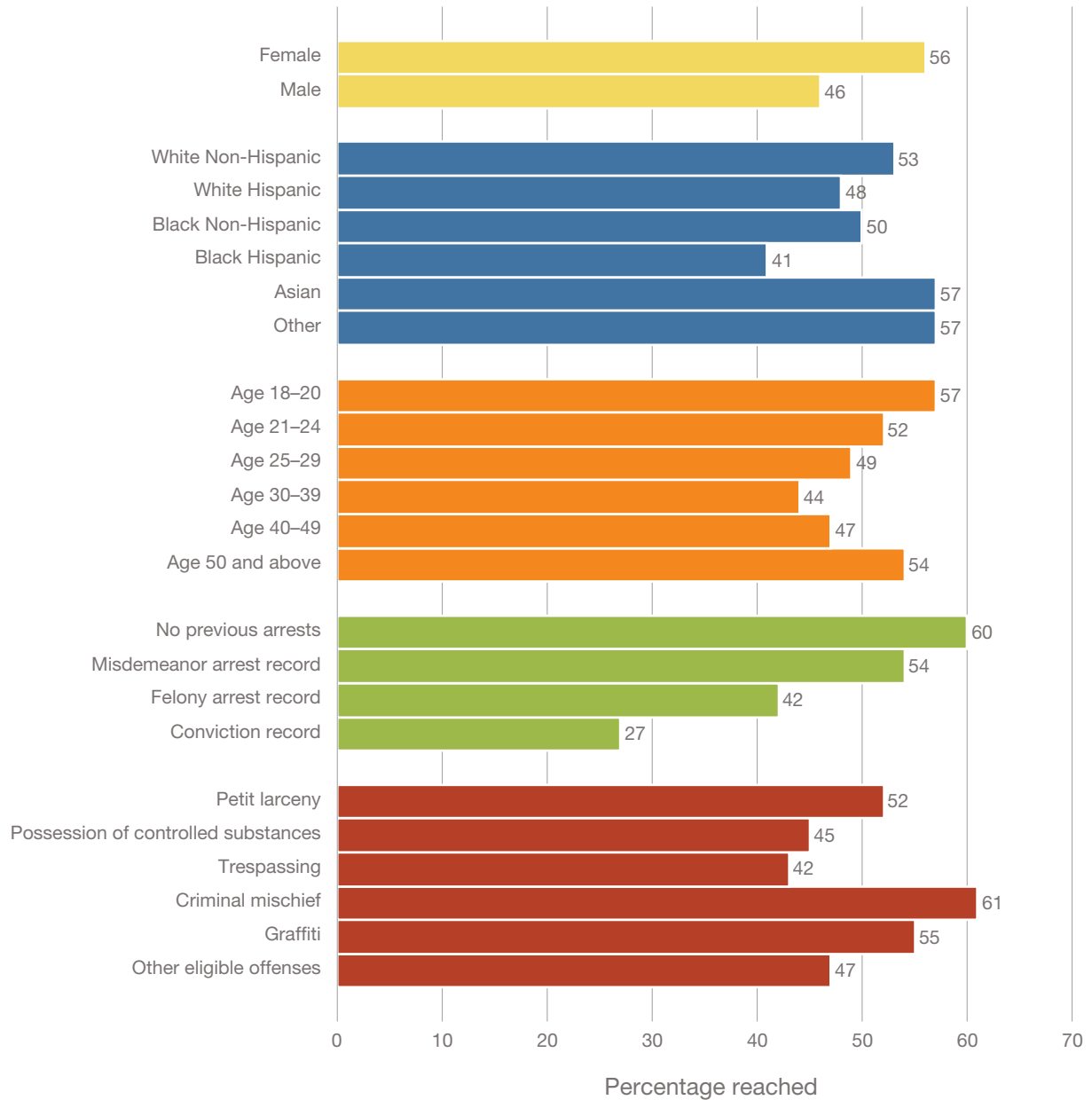
TABLE 5.2
Logit Regression-Adjusted Predicted Probabilities of Who Was Reached

Demographic	Reached
Overall	0.507
Gender	
Male	0.459
Female	0.564***
Race	
White non-Hispanic	0.532
White Hispanic	0.478**
Black non-Hispanic	0.496
Black Hispanic	0.408***
Asian	0.573
Other	0.572
Age	
18–20	0.566
21–24	0.521
25–29	0.491***
30–39	0.441***
40–49	0.468***
50 and above	0.540
Criminal History	
No previous arrests	0.602
Misdemeanor arrest record	0.535***
Felony arrest record	0.421***
Conviction record	0.270***
Offense	
Petit larceny	0.520
Possession of controlled substances	0.450*
Trespassing	0.424****
Criminal mischief	0.611
Graffiti	0.552
Other eligible offenses	0.461**
Observations	4,536

NOTE: These predicted probabilities were estimated from a logit specification. Within each grouping, the predicted probability presents the probability of being reached for someone who has that characteristic and has average values for all other characteristics. The full set of characteristics included in the model are all variables presented in the table, as well as year of arrest and neighborhood of arrest. The asterisks indicate whether a participant with that characteristic value has a probability of being reached that is statistically different from the reference group category, which is the first value for each grouping.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

FIGURE 5.1
Percent of Individuals Reached by Program Providers, by Selected Characteristics



NOTE: The data for this figure are from Table 5.2 and show the probability of being reached for specific groups while holding other variables at their mean values.

of the outreach disparity between Black non-Hispanic and White non-Hispanic individuals; the likelihood of successful outreach is essentially cut in half once other controls are accounted for. This likewise indicates that the key reason outreach is less successful for Black non-Hispanic individuals is that they are more likely to have some of the other characteristics included in Table 5.2 that are predictive of being hard to reach. However, this does not change the fact that, as a whole, Black non-Hispanic individuals are less likely to be reached than White non-Hispanic individuals. With respect to the disparities among White Hispanic and Black Hispanic individuals, the fact that the disparities between those groups and White non-Hispanic

individuals does not change much when the other factors are controlled for indicates that outreach to these groups is difficult for reasons other than what is included in the table. For example, language differences might have made it more difficult for NYPD to record contact information accurately and subsequently might have made it more difficult for providers to reach these individuals.

The takeaway of the race and ethnicity results discussed above is that White Hispanic, Black non-Hispanic, and Black Hispanic individuals make up 6.4 percentage points less of the population that completes programming than is referred for programming (based on Columns 1 and 3 of Table 5.1). Given the widespread racial disparities that are known to exist in the criminal justice system, it is important that diversion programs such as these do not exacerbate these disparities further. At a minimum, these groups must comprise the same percentage of program completers as they do the arrest population. The fact that race and ethnicity groups experience different levels of attrition at the outreach stage indicates this program might result in an increase in the level of observed racial and ethnic disparities in the legal system. To mitigate this issue will require solving the outreach issue. Note that it is also possible that the eligibility criteria result in racial and ethnic disparities in which arrested individuals are eligible for Project Reset; examining these potential disparities was outside the scope of the current project, but is an important topic for future research. Appendix Table B.10 suggests disparities are likely to be present at this earlier stage as well.

Summary

The analyses presented in this chapter indicate that criminal history is the strongest predictor of whether an individual referred to Project Reset can be successfully reached. The analyses also identified that the collective percentage of White Hispanic, Black non-Hispanic, and Black Hispanic individuals falls by 6.4 percentage points (from 65.2 percent to 58.8 percent) when comparing the set of individuals who are referred to the program to the set of individuals who complete programming. This indicates this program might exacerbate existing disparities in the criminal legal system, which highlights the need for the outreach issue to be resolved.

Outcome Evaluation Results

In this chapter we examine the effect Project Reset had on case outcomes and rearrest rates. Identifying the causal effect in this setting was challenging for several reasons, all of which were discussed in more detail in earlier chapters. First, within a given set of eligibility criteria, the program was not randomly assigned but rather rolled out to everyone that qualified all at once. Second, there were significant selection issues in terms of which individuals referred to the program actually completed it. Third, the eligibility requirements shifted considerably over time. And fourth, there were significant changes implemented shortly after rollout, both with respect to changes in criminal legal policy and with the onset of the COVID-19 pandemic.

To best identify the causal effect of the program within these constraints, we elected to use a regression discontinuity design where time was the running variable, and the threshold was defined as the date of program implementation.¹ As noted in detail in the methods section in Chapter 2, the regression discontinuity in time design relies on examining whether there is a discontinuity in outcome time trends at the date of program implementation. For this design to identify whether Project Reset had a causal effect on case outcomes and rearrest rates, three requirements must be satisfied: (1) There must be a discontinuity in program completion at the time of implementation. (2) Besides the program, there must be no other changes happening at the time of implementation. (3) The time trends in outcomes away from the time of implementation cannot be too noisy. As we present the results in this chapter, we will discuss the extent to which these requirements seem to be satisfied.²

All the results in this chapter examine the outcome effects in a graphical framework. The regression analysis that complements these graphs is presented in Appendix A. While the results are most intuitively understood in graphical format, one advantage of the regression results are that they identify the statistical significance of the program effect. For simplicity, we directly show the statistical significance of the results on the graphs presented in this chapter, although the methodology to determine this significance is discussed in Appendix A.

Below, we walk through the analysis sample we used for this outcome analysis—which was different than the sample of referred individuals we used in previous chapters—and then outline the specific groups for which we examined program effects.

¹ See Hausman and Rapson (2018) for a discussion of how the standard regression discontinuity model has been adapted to situations where time is the running variable.

² In September 2018, DANY implemented a new pre-arraignment diversion program (Manhattan Hope) that served individuals arrested on low-level drug charges. While there was some overlapping eligibility between Hope and Reset, Hope was substantially smaller than Reset, and we did not find any evidence that the implementation of Hope had any effect on our results. In particular, the results presented throughout this chapter do not indicate any discontinuities around the September 2018 time period that would be consistent with a significant take-up of Hope.

Data Construction and Structure of Analysis

Analysis Sample

All analyses in this chapter used an expanded data set that included individuals who received a DAT between January 1, 2016 and January 31, 2021 and who passed initial screening requirements. These were individuals that passed the automated checks for Project Reset although, as discussed in Chapter 2, not all these individuals would have passed the case-specific checks and thus only a subset of these individuals would actually have been eligible (and thus referred) to the program. Our research design required that the individuals included in our sample be similar pre- and post-program implementation. While ideally we would have only wanted to include individuals in our analysis who were eligible for Project Reset, DANY cannot identify who was eligible prior to program implementation (as they do not have the capacity to run case-specific checks on these individuals). We thus employed a second best solution in this scenario, which was to include all individuals who passed the initial screening requirements, as DANY can identify these individuals pre- and post-implementation. In our discussion of the results, we detail how this approach can still identify the effect of the program.

Just as Table 5.1 documented that the characteristics of referred individuals who do and do not complete programming are quite different, in Table B.10 we show that—for those in the post-program implementation period where both initial screening and referral status are observed—the characteristics of those that pass the initial screening requirements and those that are ultimately referred to Project Reset (because they pass the case-specific checks) are also quite different. The results also indicate that the case-specific checks screen out a substantial number of cases.

As will be detailed in the next section, all analyses in this chapter used defined subsets of this sample of individuals who passed the initial screening requirements.

Structure of Analysis

Because individuals with different characteristics became eligible at different times, we examined the results for each of four groups separately,³ as described in Table 6.1. Those in Group A are part of the original population, have no prior arrests, and were arrested in Uptown Manhattan or Precincts 19, 20, 24, or 25 in Midtown Manhattan. For these individuals, the program implementation date was February 1, 2018. Group B individuals are similar to those in Group A, except they were arrested in the remaining precincts of Midtown Manhattan or in Downtown Manhattan—for these individuals, the program implementation date was on July 1, 2018. Group C consists of those arrested anywhere in Manhattan but who had prior arrests but not prior convictions—these individuals also became eligible on July 1, 2018. Finally, Group D consists of those in the expanded population (those with prior criminal convictions)—these individuals became eligible on July 1, 2019. Note that while those in Group B and Group C had the same implementation date, and thus technically could be shown on the same graph, we concluded it would be more informative to keep the results separate as the group composition was quite different with respect to criminal history (thus, we were able to examine whether the effect of the program depends on criminal legal history).

The outcome results for each group will be presented graphically in the same format as in Figure 2.1. Thus, for each group, we used data on arrests that occurred two years prior to implementation and up to one-year post-implementation. As noted earlier, the research design required those arrested pre-implementation

³ The need to run the analyses separately for each group that has a different program implementation date can best be seen by examining Figure 2.1 in Chapter 2. Adding multiple different program implementation dates into the same figure would completely obscure whether time trends for a given group changed at the time of implementation, and thus these patterns must be examined separately.

TABLE 6.1
The Four Eligibility Groups

Group	Population	Eligibility Requirements	Implementation Date
A	Original	No prior arrests; arrested in Uptown Manhattan or Precincts 19, 20, 24, or 25 in Midtown Manhattan	February 1, 2018
B	Original	No prior arrests; arrested in Downtown Manhattan or remaining precincts in Midtown Manhattan	July 1, 2018
C	Original	Prior arrests but no prior convictions	July 1, 2018
D	Expanded	Prior convictions	July 1, 2019

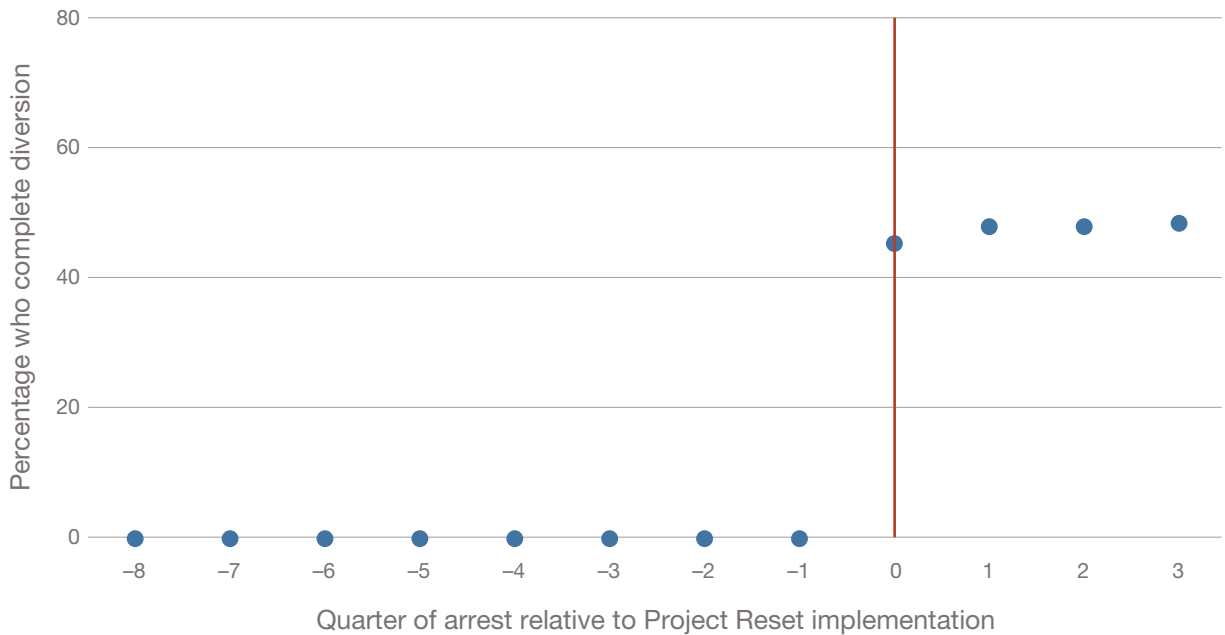
to be similar to those arrested post-implementation, which necessitated using only the individuals arrested within the time window who passed the initial screening requirements and who were not arrested for either a possession of marijuana or a theft of services charge. As discussed in Chapter 3, possession of marijuana and theft of services charges were eligible for Project Reset and made up a sizeable share of DATs until DANY decided to no longer prosecute individuals for these charges (which happened in February 2018 for theft of services and in August 2018 for possession of marijuana charges). Including these cases would have resulted in significant differences in arrest charges for those arrested pre- and post-implementation.

Results for Group A: Individuals Arrested in Uptown Manhattan or Select Precincts in Midtown Manhattan Who Had No Prior Arrests

For the research design to identify the effect of the program for individuals in Group A, it must be the case that program completion increased discontinuously at the time of implementation for these individuals. To examine this, Figure 6.1 shows how the proportion of individuals in Group A completing diversion changed over time. The x-axis represents quarter relative to Project Reset implementation, with 0 corresponding to those arrested in the beginning of implementation (i.e., all those arrested between February 1, 2018 and April 30, 2018). Quarter 1 corresponds to those arrested in the subsequent three months after Project Reset was implemented (i.e., those arrested between May 1, 2018 and July 31, 2018). Likewise, quarter -1 corresponds to those arrested one quarter prior to the implementation of Project Reset (i.e., those arrested between November 1, 2017 and January 31, 2018). In this way, the figure shows how the rate of program completion changed for Group A individuals arrested two years prior to implementation through one year post-implementation.

As would be expected, Figure 6.1 indicates that all Group A individuals arrested prior to program implementation did not complete programming. The moment the program was implemented (quarter 0), the rate of program completion immediately jumps to about 45 percent and stays at a similar level thereafter. As was discussed in detail earlier (e.g., see Tables 5.1 and B.10), not everyone who passed the initial screening requirements completed diversion programming—a significant portion were not referred because they did not pass the case-specific checks, and of those referred, many did not complete programming (largely because they could not be reached). This attrition explains why the rate of program completion did not jump from 0 to 100 percent when the program was implemented. However, with respect to the research design requirements, all that was necessary was that program participation jumps discontinuously at the

FIGURE 6.1
Percentage of Group A Completing Diversion by Quarter of Arrest



NOTE: Quarter 0 corresponds to the quarter in which Project Reset was implemented for Group A. All other quarters are numbered in reference to this quarter. For individuals arrested in a given quarter, the figure presents the percentage of those who pass initial screening requirements who complete diversion. This sample includes 1,933 observations (across the 12 quarters).

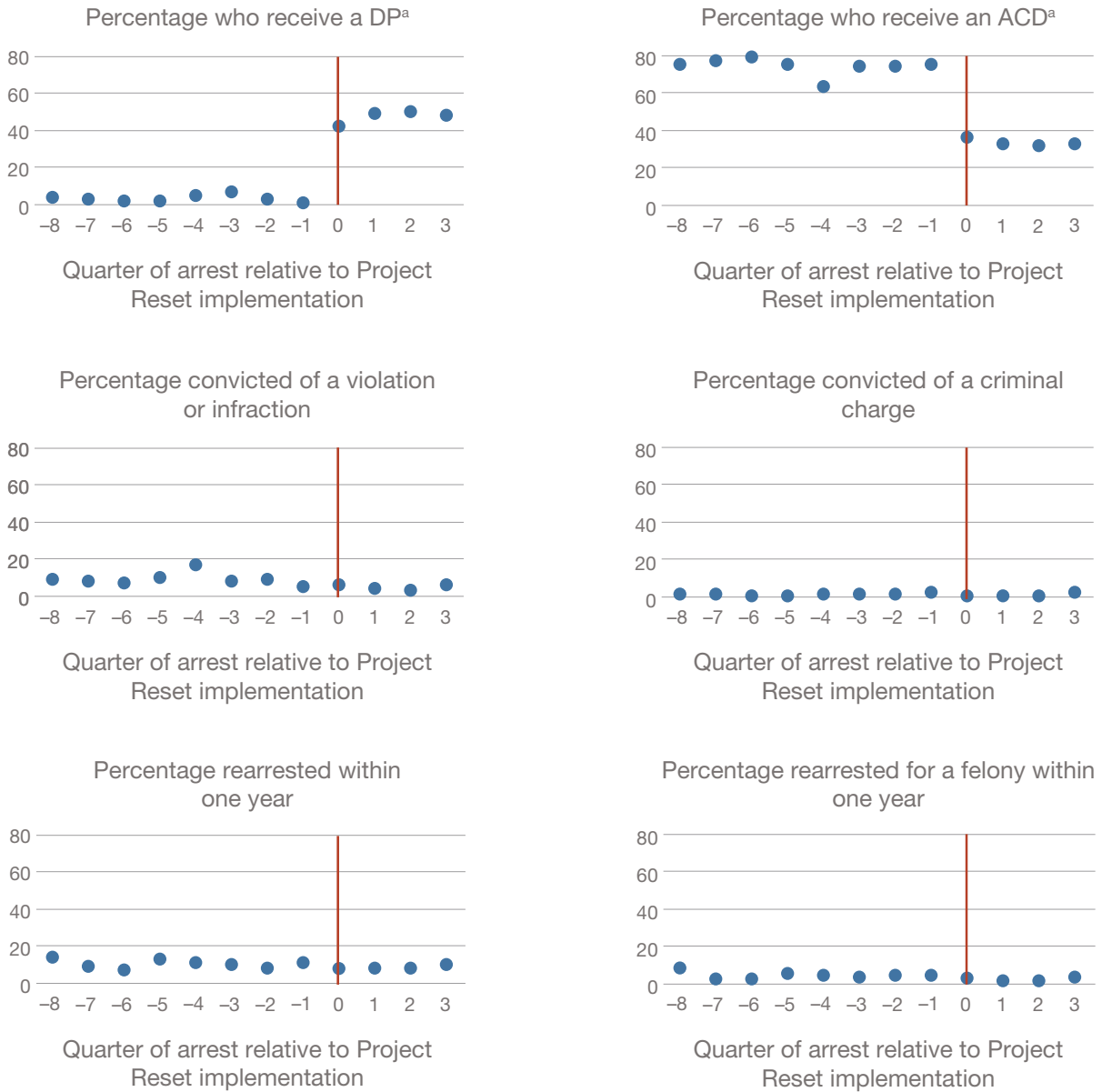
threshold—Figure 6.1 makes it clear this requirement was satisfied. The fact that not everyone completed programming post-implementation will be factored into how the outcome analyses were interpreted.

Figure 6.2 uses a similar set-up to Figure 6.1 to show how key outcomes changed for Group A individuals over this same time period. Note that throughout the outcome figures, the value for a given quarter was the average outcome among all those that were arrested in that quarter. The quarters are defined exactly as in Figure 6.1. Each of the six panels within Figure 6.2 shows the trends over time for a different outcome. Four of these six outcomes correspond to the case outcomes for those who were arrested in a given quarter, including: (1) the percentage who received a decline to prosecute (DP); (2) the percentage who received an ACD; (3) the percentage who were convicted of a violation or infraction; and (4) the percentage who were convicted of a criminal charge. The last two outcomes correspond to rearrest rates. The outcomes for the first rearrest figure represent the proportion of those arrested in that quarter who were rearrested at least once within one year of their focal arrest.⁴ Note that we did not count rearrests where the dominant charge was either a possession of marijuana charge or a theft of services charge because the enforcement of these rules changed drastically over this time period. Including these charges in our construction of the rearrest rate would likely have resulted in significant changes in rearrest rates around the time of program implementation that were independent of the program, which would invalidate the research design.

Figure 6.2 and all other outcome figures presented in this chapter control for gender, race and ethnicity, age, location of arrest (Uptown, Midtown, or Downtown), crime type, and criminal legal history. The reason we controlled for these factors in the outcome figures is that some of the noisiness in outcomes that occurs

⁴ We also examined whether Project Reset had an effect on the number of rearrests that occurred within one year of the focal arrest and did not find that any of the conclusions changed significantly.

FIGURE 6.2
Case and Rearrest Outcomes for Group A by Quarter of Arrest



NOTE: Quarter 0 corresponds to the quarter in which Project Reset was implemented for Group A. All other quarters are numbered in reference to this quarter. For individuals arrested in a given quarter, the figure presents the percent of those who passed initial screening requirements who had a given case or rearrest outcome. All figures were detrended for quarterly compositional changes in neighborhood of arrest, race, gender, age, and offense type. The sample includes 1,933 observations.

^a For this outcome, change at the threshold was statistically significant at the 1 percent level.

over time (i.e., in adjacent periods of arrest) are the result of natural compositional shifts of these factors over time. Removing this noise by controlling for these factors made it easier to identify whether there were discontinuous changes in the time trends at the time of program implementation—in other words, it made it easier to satisfy the second requirement of the research design.

The first panel in Figure 6.2 indicates the program led to a clear increase in DPs for arrests that occurred at the threshold of program implementation (versus earlier), and this higher level of case declinations continues through quarter 3. Specifically, for arrests that occurred in the two years leading up to the policy change, the share of DPs was typically 6 percent or lower, but jumps to 42 percent for arrests that occurred at the threshold (i.e., in the first three months after program implementation). It was also evident that the increase in case declinations occurring at the cutoff was essentially the same as the increase in those completing diversion (shown in Figure 8.1). This was exactly what we expected, as those who completed diversion programming had their case declined by DANY.

While the effect of the program on case outcomes was clearly to increase case declinations, it is important to understand what outcomes these individuals would have received if diversion were not in place. To identify this, we needed to examine what other case outcomes shifted significantly at the threshold. The figures clearly show that the increase in DPs came from ACDs—graphically, the decline in ACDs at the threshold was essentially the same as the increase in DPs. In contrast, the results for convictions did not show any real change at the threshold, indicating the level of convictions was unaffected by the implementation of Project Reset. However, it is important to note that the general time trends for convictions were noisier than for ACDs and DPs, which made it more difficult to determine whether any underlying shift had happened. While any large change in conviction likelihoods that occurred as a result of the program should still have been apparent in the figures, we would not be able to identify whether relatively small shifts in these conviction likelihoods occurred.

The results for rearrests that occurred within one year of the focal arrest did not indicate any substantial shift in the outcome trend at the threshold. However, because of the relative noisiness of time trends for these outcomes, it is unlikely that we would be able to detect small changes in rearrest rates. In particular, while rearrests for felony charges did seem to decline slightly in quarters 1 through 3 post-implementation, their level was not much different than in quarters –6 and –7. Considering there was no discontinuous change that happened at the cutoff, this decline in quarters 1 through 3 could simply reflect the cyclical pattern of rearrest rates. Thus, there did not seem to be any evidence that the program had any sizeable effect on rearrest rates for Group A individuals.

Collectively, the figures indicate that the key effect of Project Reset for Group A individuals was that some (but not all) of the individuals who would traditionally have gotten ACDs in court ended up completing diversion programming and receiving a DP. There was no evidence that this programming affected the rearrest rates for these individuals. Individuals who traditionally received a conviction in court seemed to be unaffected by the program—these individuals did not complete programming either because they were not formally referred (i.e., they did not pass the case-specific checks) or because they did not complete programming conditional on being referred. As a result, these individuals continued to receive convictions in court, and we thus saw no change in conviction rates at the threshold.

Results for Group B: Individuals Arrested in Downtown Manhattan or Select Precincts in Midtown Manhattan Who Had No Prior Arrests

For Group B individuals, the date of program implementation occurred on July 1, 2018. For all figures presented in this section, we kept the same numbering scheme for the quarters of arrest, where quarter 0 corresponds to those in this group who were arrested in the quarter when the program was implemented, and the other quarters are numbered by how close they are to this threshold quarter. However, the calendar dates that each quarter represents will naturally be different between Group B and Group A because the implementation date of the program was different.

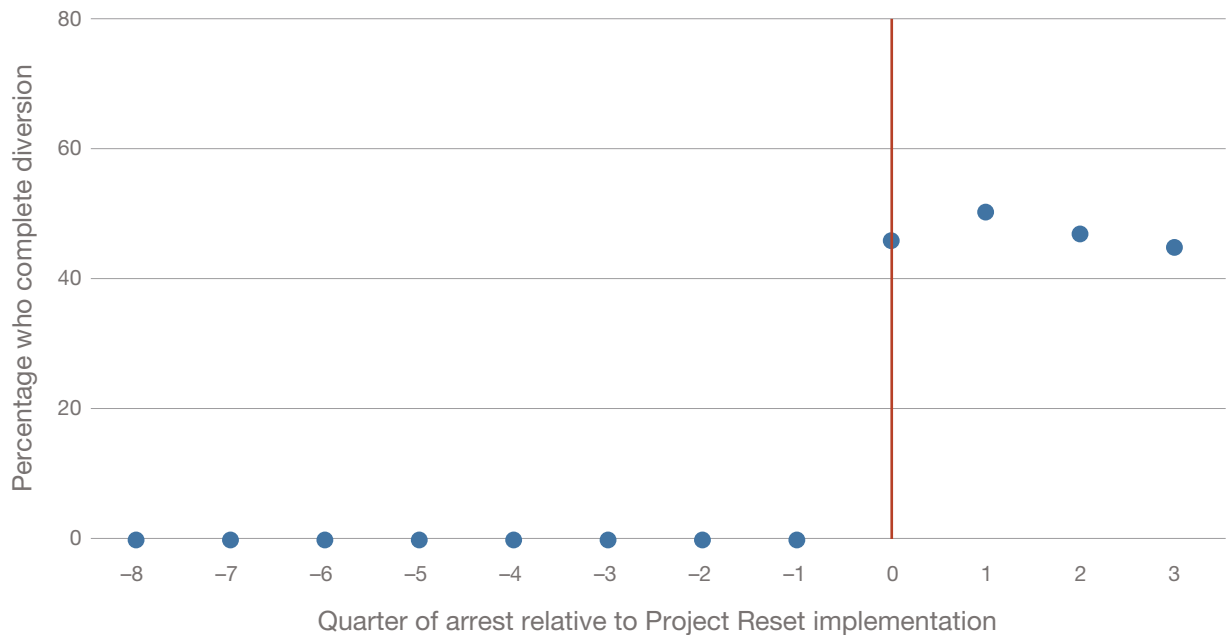
Figure 6.3 indicates that, prior to program implementation, no one in Group B completed programming. Once the program was implemented, the completion rate was about 46 percent. These results are very similar to what was seen for Group A individuals in Figure 6.1, which was expected as those in Group B were similar to Group A in that they also had no prior arrests; the only difference between these groups was that they were arrested in different areas of Manhattan.

With respect to outcomes, the post-implementation period used to estimate the effects for Group B individuals included all those whose focal arrests were before June 30, 2019. Those arrested in quarter 3 (from April 1, 2019 through June 30, 2019) likely had their one-year rearrest rates affected by the COVID-19 pandemic, as generally arrests tended to fall in the immediate aftermath of the onset of the pandemic (March 2020). This is important to keep in mind when examining the results for rearrests (specifically for quarter 3 individuals), although the results for case outcomes themselves should have been largely unaffected by the pandemic.

Figure 6.4 indicates the main effect the program had on case outcomes was to increase DPs and reduce ACDs. As was the case with Group A, the threshold percentage point increase in DPs and the percentage point decrease in ACDs closely coincides with the percentage point increase in program completion at the threshold, which indicates that those who completed the program would have gotten an ACD but now get a DP. Note that while the figure for convictions for violations and infractions indicates that the likelihood of this outcome was lower in quarters 0 to 3 than it was for arrests in the two years prior to implementation, this pattern fits a general declining time trend, rather than a discontinuous change at the threshold that was a result of the program. In particular, the conviction rates were very similar for arrests that occurred in quarters -1, 0, and 1, indicating the program did not appear to independently affect conviction rates.

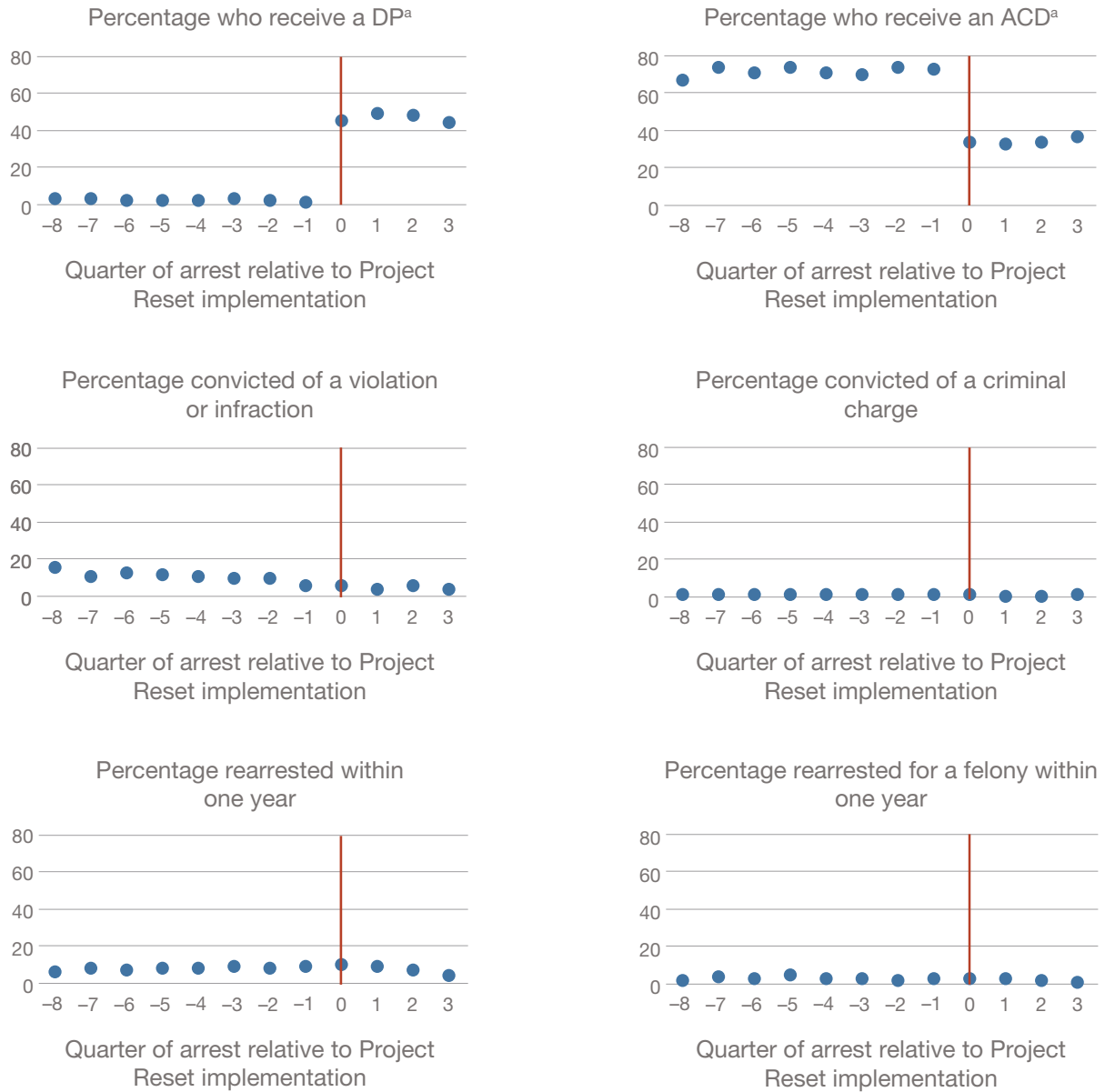
With respect to one-year rearrest rates, the trends were remarkably flat over time, indicating the program had no effect on rearrests. The main exception to this relatively flat time trend occurred in quarter 3, when re-

FIGURE 6.3
Percentage of Group B Completing Diversion by Quarter of Arrest



NOTE: Quarter 0 corresponds to the quarter in which Project Reset was implemented for Group B. All other quarters are numbered in reference to this quarter. For individuals arrested in a given quarter, the figure presents the percent of those who pass initial screening requirements who complete diversion. The sample includes 4,386 observations.

FIGURE 6.4
Case and Rearrest Outcomes for Group B by Quarter of Arrest



NOTE: Quarter 0 corresponds to the quarter in which Project Reset was implemented for Group B. All other quarters are numbered in reference to this quarter. For individuals arrested in a given quarter, the figure presents the percent of those who passed initial screening requirements who had a given case or rearrest outcome. All figures were detrended for quarterly compositional changes in neighborhood of arrest, race, gender, age, and offense type. The sample includes 4,386 observations.

^aThe change in the outcome at the threshold was statistically significant at the 1 percent level.

arrests rates fell slightly; as noted earlier, this likely occurred because the one-year time window for measuring rearrests for this group included the first few months of the pandemic (when arrest rates were generally low).

Thus, the results for Group B were essentially the same as we found for Group A. Namely, the main effect of the program was that some of those who traditionally would have received an ACD in court completed programming and received a DP instead.

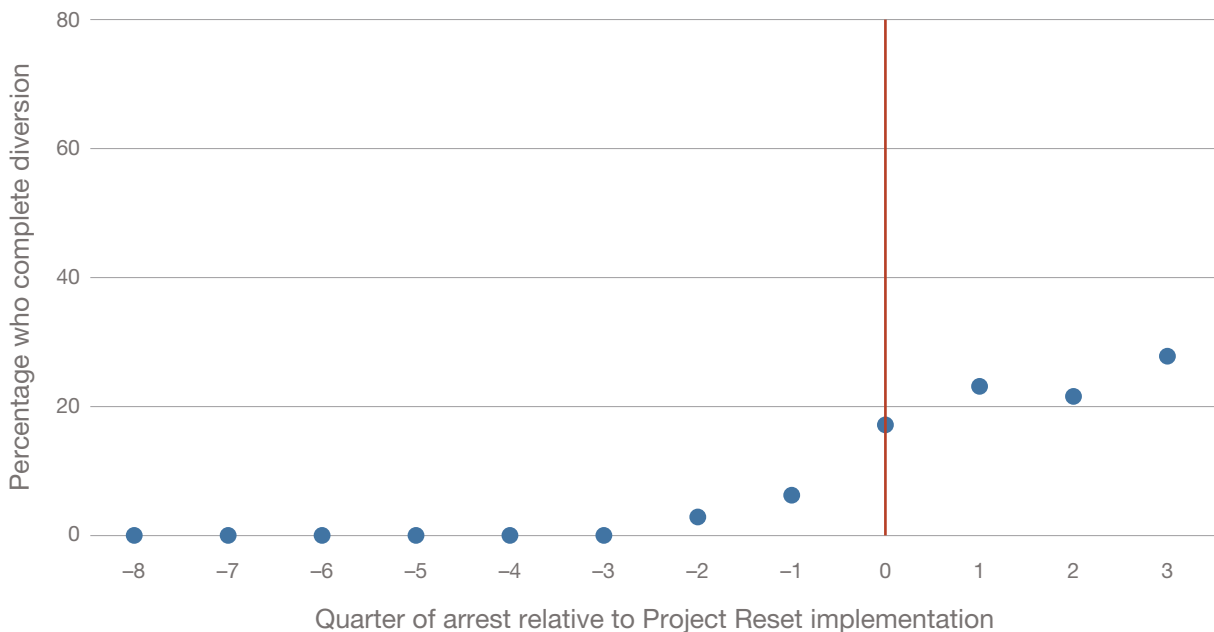
Results for Group C: Individuals Who Had Prior Arrests (but No Prior Convictions)

For Group C individuals who had prior arrests but no prior convictions, the date of program implementation occurred on July 1, 2018. Thus, as for Group B, those arrested in quarter 3 (from April 1, 2019, through June 30, 2019) likely had their one-year rearrest rates affected by the COVID-19 pandemic.

Figure 6.5 shows how the percentage of Group C individuals completing programming varied by quarter of arrest. The pattern was quite different than that seen for Groups A and B. While the rate of program completion was 0 for those arrested from quarter -8 to quarter -3, from quarter -2 to quarter 1 there was an increase in the completion rate and again between quarters -1 and 0. While one would have expected that the rate of program completion would be zero prior to implementation, one issue we had in the data was that the flag created by DANY only identified whether individuals passed the initial screening requirements under the original criteria or under the expanded criteria (i.e., whether they should be placed in Groups A, B, or C versus Group D.) We thus had to classify the former set of individuals into either Group A, B, or C, which we did based on the arrest data provided by DCJS. It is possible that some of the individuals we classified as being in Group C were thought to be in Group A by DANY and thus offered programming at an earlier date.

Another result of note is that the overall completion rate among this group was significantly lower than what was seen for Groups A and B. The results presented in Chapter 5 and Table B.10 help explain this finding, as they indicated that an arrest history predicted flagged individuals (i.e., those that passed initial screening requirements) would be less likely to be referred to Reset and less likely to complete it if referred. Although the completion pattern in Figure 6.5 was clearly not as clean as shown in Figures 6.1 and 6.3, what was most important for the validity of the research design was that there was a discontinuous increase in the

FIGURE 6.5
Percentage of Group C Completing Diversion by Quarter of Arrest



NOTE: Quarter 0 corresponds to the quarter in which Project Reset was implemented for Group C. All other quarters are numbered in reference to this quarter. For individuals arrested in a given quarter, the figure presents the percent of those who passed initial screening requirements who completed diversion. The sample includes 3,583 observations.

completion rate at the threshold, which was evident from the figure (since this was where the largest rate shift between adjacent quarters occurred). However, because the general uptake of programming for this group was relatively small, if the program did have an effect, we would have expected to see relatively smaller discontinuous changes at the threshold for the case outcomes examined in Figure 6.6.

Figure 6.6 indicates that, prior to program implementation, the general outcomes for individuals in Group C were somewhat different than for Group A and B individuals. In particular, the ACD rate was lower, the conviction rate was higher, and the subsequent rearrest rate was higher. These patterns were to be expected as Group C differed significantly from Groups A and B in that they did have prior arrests. In terms of the effect Reset had on case outcomes, once again we did see that DPs increased at the threshold and ACDs decreased. The results for convictions for a violation or infraction were decidedly noisier, as while there was an increase at the implementation cutoff (that was statistically significant), this seemed to be part of a general time trend that occurred on either side of the cutoff. Further, intuitively, it would not make sense for the program to result in an increase in conviction rates. Because some individuals in Group C completed programming, the DP rate necessarily needed to increase (since hardly anyone was receiving a DP beforehand). This increase in the DP rate needed to be offset by a decline in other case outcomes—the specific outcomes that show a decline reasonably indicate the outcomes the diversion completers would have otherwise experienced. Given that it would not make sense for the program to increase conviction rates, combined with the fact that the trends for this outcome were very noisy in general (which could invalidate the research design), we concluded the program did not have an effect on conviction rates.⁵ Thus, once again we concluded that the effect of the program was that the individuals in Group C who completed programming likely would have received an ACD otherwise.

The results for one-year rearrest rates for any criminal offense indicate that there was a relatively constant level of rearrests except for those individuals with their focal arrest in quarter -1 and those who had their focal arrest in quarter 3. The low rate for the latter group was likely to have occurred because of the COVID-19 pandemic. It was unclear why the rate for those with a focal arrest in quarter -1 was different, although it should be independent of the program since these individuals were ineligible. Because there was no time trend in arrests that seemed significantly different post-implementation versus pre-implementation, we concluded that Project Reset did not seem to have an effect on rearrest rates.

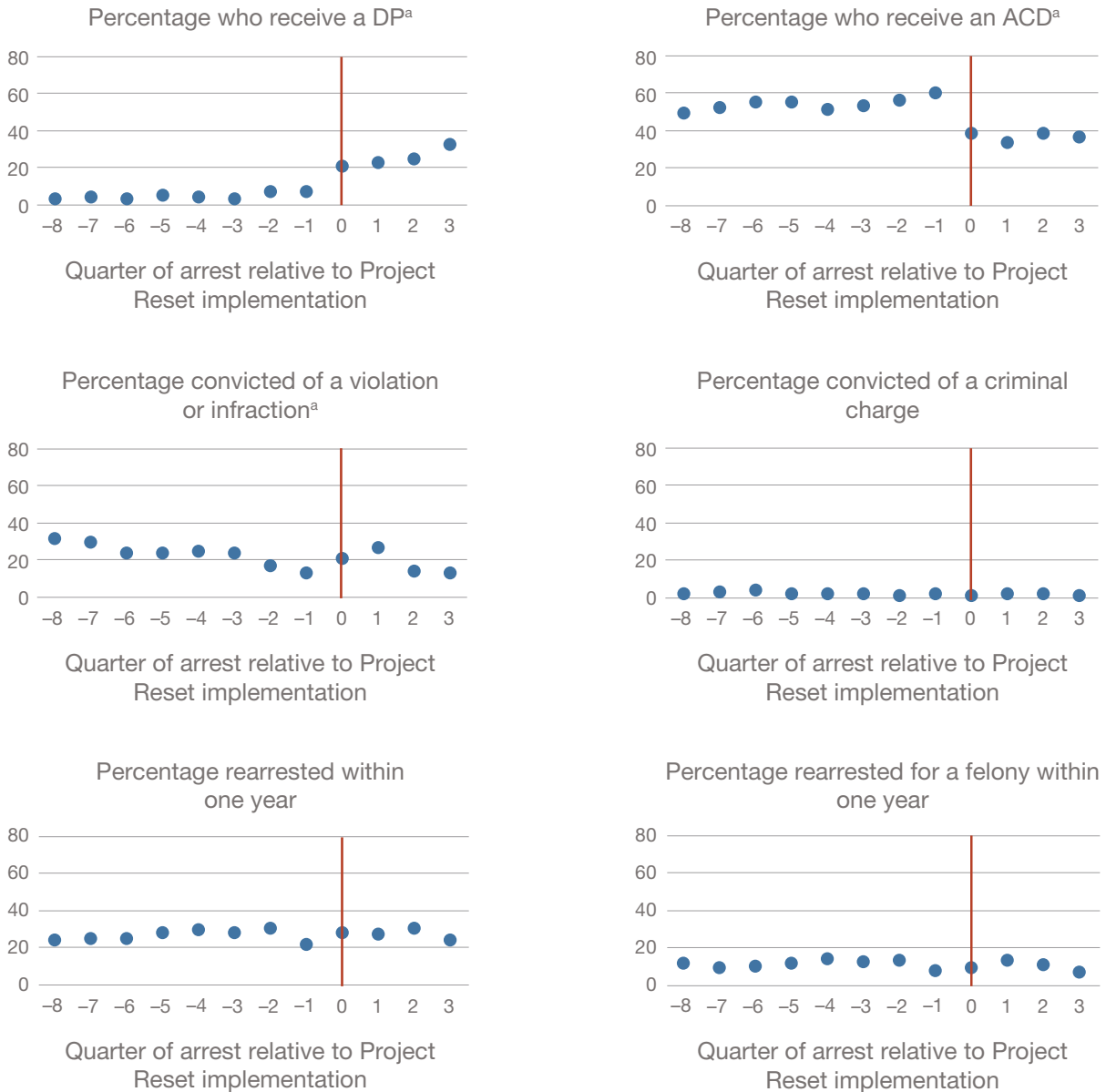
Results for Group D: Individuals Who Had Prior Convictions

For Group D, the program implementation date occurred on July 1, 2019, and thus the four-quarter post-implementation sample included individuals who were arrested from the implementation date up through June 30, 2020. Figure 6.7 shows how the portion of Group D individuals who completed diversion varied by quarter of arrest. By far, the program take-up among this group was significantly smaller than what we saw for other groups—in quarter 0, only 8 percent of individuals who passed initial screening requirements completed programming.

While groups A, B, and C generally met most of the research design's required assumptions to identify the causal effect of the program, this did not hold for Group D. In particular, the second requirement of the research design was that, besides the program, there must be no other changes occurring around the same time. Several significant changes occurred during this time period. First, on January 1, 2020, more lenient policies were adopted as part of a statewide reform regarding issuing DATs, which implies that those receiv-

⁵ This argument implicitly assumes that DANY was not penalizing individuals for not completing diversion—i.e., they were not more likely to give out convictions to individuals solely because they did not do diversion. Based on our conversations with DANY, there was no indication they were doing this.

FIGURE 6.6
Case and Rearrest Outcomes for Group C by Quarter of Arrest

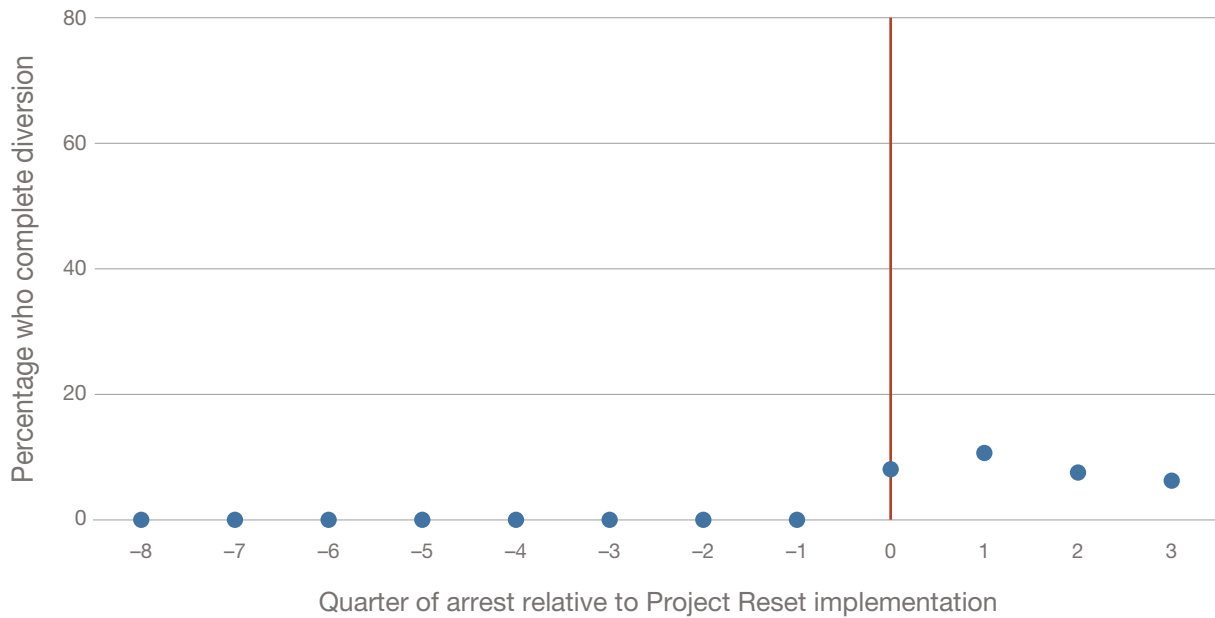


NOTE: Quarter 0 corresponds to the quarter in which Project Reset was implemented for Group C. All other quarters are numbered in reference to this quarter. For individuals arrested in a given quarter, the figure presents the percent of those who passed initial screening requirements who had a given case or rearrest outcome. All figures were detrended for quarterly compositional changes in neighborhood of arrest, race, gender, age, and offense type. The sample includes 3,583 observations.

^aThe change in the outcome at the threshold was statistically significant at the 1 percent level.

ing DATs after this time period were likely to be different than those receiving DATs prior to this date. Specifically, the population of those receiving DATs would be expected to both increase and include individuals that had more involvement with the criminal legal system. To examine these potential changes empirically, in Table A.1 we compared the characteristics of those arrested in the four quarters prior to implementation with those arrested in the four quarters after implementation. The results indicated that 46 percent of those arrested in Group D post-implementation had a prior felony conviction (versus a prior misdemeanor con-

FIGURE 6.7
Percentage of Group D Completing Diversion by Quarter of Arrest



NOTE: Quarter 0 corresponds to the quarter in which Project Reset was implemented for Group D. All other quarters are numbered in reference to this quarter. For individuals arrested in a given quarter, the figure presents the percent of those who passed initial screening requirements who completed diversion. The sample includes 5,090 observations.

viction), while 39 percent of those arrested in this group pre-implementation had a prior felony conviction. Figure A.1 shows that in quarter 2, the number of individuals that passed initial screening requirements for Project Reset was essentially double what it was in quarter 0, which also implies a somewhat different population was present post-implementation versus pre-implementation.

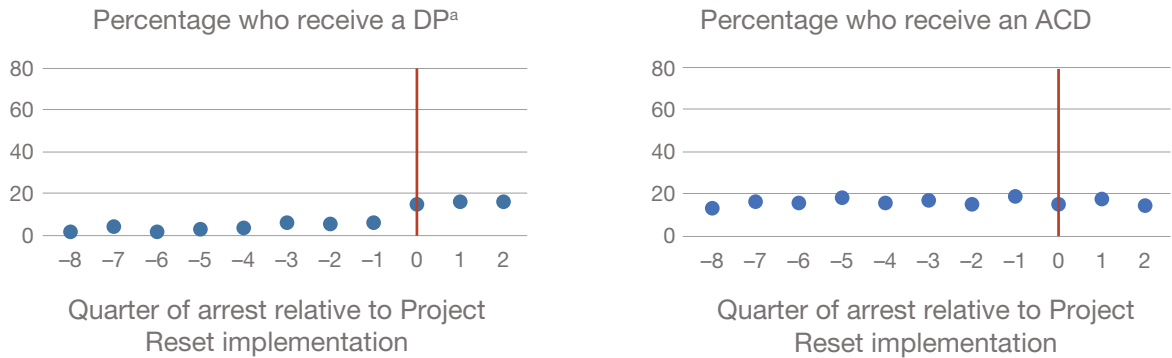
Another change that occurred for those arrested in quarter 2 and beyond was that DANY's Manhattan Justice Opportunities (MJO) program was launched in February 2020. The program allowed judges, prosecutors, and defense attorneys alternative sentencing options, which included various programming options.⁶ The data indicated that the MJO program was an option for those arrested January 1, 2020, or later, which also allowed for case outcomes to change during the post-program time period for reasons other than the program.

A third factor that likely had a significant effect on outcomes for those arrested after the program was implemented was the onset of the COVID-19 pandemic. As the program was implemented on July 1, 2019, for this group, everyone arrested quarter -1 and later had their one-year rearrest rates affected by the COVID-19 pandemic. Further, in Chapter 4, we noted that the onset of the pandemic affected how cases were adjudicated in the court. Due to the time it takes to adjudicate cases in court, it is likely that at least some subset of those arrested in January 1, 2020, or later had their case outcomes affected by the pandemic.

Due to the issues outlined above, it was not possible to identify the effect the program had on Group D individuals. However, for informative purposes, we do show a subset of the figures included in earlier sections (i.e., corresponding to Groups A, B, and C). Figure 6.8 presents the results for DPs and ACDs, with results for

⁶ Individuals identified as MJO participants in the data primarily had a disposition of either an ACD or a conviction for a violation or infraction.

FIGURE 6.8
Case and Rearrest Outcomes for Group D by Quarter of Arrest



NOTE: Quarter 0 corresponds to the quarter in which Project Reset was implemented for Group D. All other quarters are numbered in reference to this quarter. For individuals arrested in a given quarter, the figure presents the percent of those who passed initial screening requirements who have a given case or rearrest outcome. All figures were detrended for quarterly compositional changes in neighborhood of arrest, race, gender, age, offense type, and conviction history. The sample includes 5,090 observations.

^aThe change in the outcome at the threshold was statistically significant at the 1 percent level.

those arrested in quarter 3 dropped from analysis because of their case outcomes being significantly affected by the pandemic. Note that the low program uptake rate among this group means it would have been difficult to identify the effect of the program even if there had been no other changes, because a small uptake rate will necessarily mean that outcome changes at the threshold will also be proportionately small. While the figures do seem to indicate that the program resulted in an increase in the DP rate, it is not clear what the outcomes of these diversion completers would have been in absence of the program. In particular, the results for ACDs indicate that the rate of ACDs did not change across the threshold, but other confounding factors could have masked this effect. We were not able to assess whether convictions declined discontinuously at the threshold because the patterns were too noisy to examine.⁷ For these reasons we were unable to determine fully what the case outcomes would have been for these Group D individuals if they had not completed Project Reset.

Summary

Collectively, the results indicate that for those individuals in the original population (i.e., Groups A, B, or C), the main effect of Project Reset was that it resulted in a subset of those who would traditionally have received ACDs in court participating in the diversion program. There was no evidence this program had any effect on the one-year rearrest rates for these individuals.⁸ Due to the myriad of changes that occurred when the pro-

⁷ We elected to show the results for DPs and ACDs (versus other case outcomes) because they seemed less noisy, indicating that the other changes discussed in this section might not have affected these groups as much. In contrast, the results for conviction rates were extremely noisy, indicating they might have been more affected by the other changes discussed.

⁸ Throughout this chapter, our rearrest results focused on whether the program had an impact on rearrest rates, and thus we focused on whether there was a change in the rearrest level at the threshold, rather than discuss what the level of rearrest rates were among those that completed programming. However, for completeness, this footnote documents the average rearrest rates among program completers. Among those in Group A that completed programming, 5.7 percent were rearrested within one year of arrest, and 2.4 percent were rearrested for a felony within one year of arrest. Among those in Group B that completed programming, 4.9 percent were rearrested within one year of arrest, and 1.5 percent were rearrested for a felony within one year of arrest. Among those in Group C that completed programming, 10.6 percent were rearrested within one

gram was implemented for the expanded population (Group D), we cannot determine the effect the program had on outcomes for this group.

It is important to note that the program effects we found for the original population (Groups A, B, and C) might have been different if the outreach process had been more successful and (presumably) completion rates had been higher. Chapter 5 indicates that only a select group of individuals who were referred completed programming, and the results in this chapter indicate that these were likely the individuals who would have the best outcomes in court anyway, reflecting that they were likely to be a lower-risk group. If the program completion rates (conditional on referral) had been higher, that would mean that somewhat riskier individuals would also be participating (i.e., individuals who traditionally would be receiving convictions in court), and the effects of the programming might be different for them. Thus, because treatment effects might be heterogeneous (whereby different groups might respond differently to completing the program), we could not make any conclusions about what the causal effects of this program on rearrest rates would have been if more referred individuals had completed Reset.

Finally, given that both an ACD and a DP effectively result in the same end outcome for an individual (i.e., both outcomes result in a non-record of the offense), it is important to consider how the experiences within these two pathways differ for individuals, since moving some individuals from an ACD to a DP is the main effect of the program. As discussed in Chapter 4, an ACD can be a much more onerous process as it involves multiple court appearances and often requires the individual to complete some form of programming or community service. If individuals are unable to make these court appearances this can expose them to additional consequences. Further, the primary requirement of an ACD is that individuals must remain arrest free for either a six-month or one-year period. During this supervisory time period, the case will be open and visible in a criminal history check. In contrast, a DP effectively closes the case right away and does not place any other conditions on an individual. Thus, even if the effects of completing diversion do not seem to be reflected in conviction rates or on rearrest rates, there can still be many other beneficial effects of the program beyond what can be measured. In Chapter 7, we present survey results on participants' perceptions of program benefits. In Chapter 8, we present a cost analysis that compared the fiscal outlay of money spent on the diversion pathway versus the ACD pathway to determine which pathway is most cost-effective.

year of arrest, and 4.1 percent were rearrested for a felony within one year of arrest. Among those in Group D that completed programming, 20.3 percent were rearrested within one year of arrest, and 10.1 percent were rearrested for a felony within one year of arrest.

Participant and Program Staff Perceptions of Project Reset

To paint a picture of perceptions about and the effect of Project Reset, we drew from participant interviews, interviews with staff, and a survey of program participants. We conducted 20–30 minute interviews with nine Project Reset participants from each of the three programs (four with participants from CCI, two from the Osborne Association, and three from YNY). These interviews focused on a range of topics, including how participants learned about the program, what guided their decision to participate, the benefits of the program, and opportunities for improvement. We also learned about their interactions with various program stakeholders. In addition, we conducted 14 interviews with program staff across the three program providers. Our goal was to learn about the ways the organizations implemented the Project Reset model, the flow of a typical participant through the program, the communication across the organizations involved with Project Reset, facilitators and barriers to implementation, and opportunities for improvement. As with the participant interviews, the survey focused on topics such as factors that contributed to the decision to participate in diversion, satisfaction with program services, ways that program services have or have not addressed participant needs, and barriers to access. Diversion participants completed the survey from November 10, 2020, through March 19, 2021, about their experiences with Project Reset and perceptions of the criminal legal system. The survey respondents overwhelmingly participated in CCI programming, including the original eligibility program (61.3 percent) and expanded eligibility program (20.8 percent) (Table 7.1).

Looking at program participation by age group, the respondents were fairly equally spread across the age groupings (Table 7.2). Participation was evenly split between cisgender men and women with an additional seven people who identified as transgender or nonbinary. By race and ethnicity, 26.8 percent identified as White-only, 24.7 percent as Black/African American-only, 2.1 percent as American Indian/Alaska Native-only, 20.6 percent as Asian-only, 2.1 percent as Native Hawaiian/Pacific Islander-only, 2.1 percent as Other-only, and 21.6 percent as Hispanic/Latinx along with one of the racial categories. Those who indi-

TABLE 7.1
Survey Completion

Program Provider	Frequency	Percent
CCI original eligibility	65	61.3
CCI expanded eligibility	22	20.8
Osborne original eligibility	16	15.1
YNY original eligibility	3	2.8
All programs	106	100.0

cated Other added in free responses: Dominican (1), Latino/Hispanic (4), and mixed race (2). The gender and racial and ethnic composition of the survey sample was similar to those who completed the diversion program in our sample.¹ Our survey recruitment strategy to reach back to participants from prior years is reflected in the time of Project Reset participation.

TABLE 7.2
Survey Participant Demographics

Characteristic	Number (Percentage)
18–20	15 (15.2%)
21–24	16 (16.2%)
25–29	17 (17.2%)
30–39	20 (20.2%)
40–49	17 (17.2%)
50 and over	14 (14.1%)
Male	44 (44.4%)
Female	48 (48.5%)
Transgender	3 (3.0%)
Nonbinary	4 (4.0%)
White	26 (26.8%)
White Hispanic/Latinx	8 (8.2%)
Black or African American	24 (24.7%)
Black Hispanic/Latinx	5 (5.2%)
American Indian or Alaska Native	2 (2.1%)
Asian	20 (20.6%)
Asian Hispanic/Latinx	2 (2.1%)
Native Hawaiian or Pacific Islander	2 (2.1%)
Other	2 (2.1%)
Other Hispanic/Latinx	6 (6.2%)
Any Race Hispanic/Latinx	21 (21.6%)
2018	7 (7.4%)
2019	38 (40.4%)
2020	35 (37.2%)
2021	14 (14.9%)

¹ Note that the race and ethnicity and gender categories are slightly different in Table 7.2 than they are in previous tables presented in Chapters 3 and 5. The previous tables used administrative data and thus required using the categories that DANY defined. In the survey we asked about race and ethnicity in a slightly more detailed manner.

Entry into the Diversion Program

The most common way that people learned about Project Reset was from a program provider (see Table 7.3). The police were the second most frequent source, and defense attorneys were third. Examining by year reveals a change in how people learned about the program. Among individuals who participated in 2019, people learned about Project Reset the most from the police (26 percent) followed by the program provider (24 percent) and other (21 percent). However, that changed among individuals who participated in 2020, as participants learned less from the police (17 percent) than from program providers (51 percent) and other sources (20 percent). That trend continued among the 14 participants who participated in 2021 as they learned about the program less from the police (7 percent) than from program providers (50 percent). This change from program recruitment through the police to the program providers is consistent with the information we learned from program staff. Program staff and participants did not indicate in interviews that a prosecutor or judge had helped or should help recruit participation, so it is not clear why a few participants selected this response.

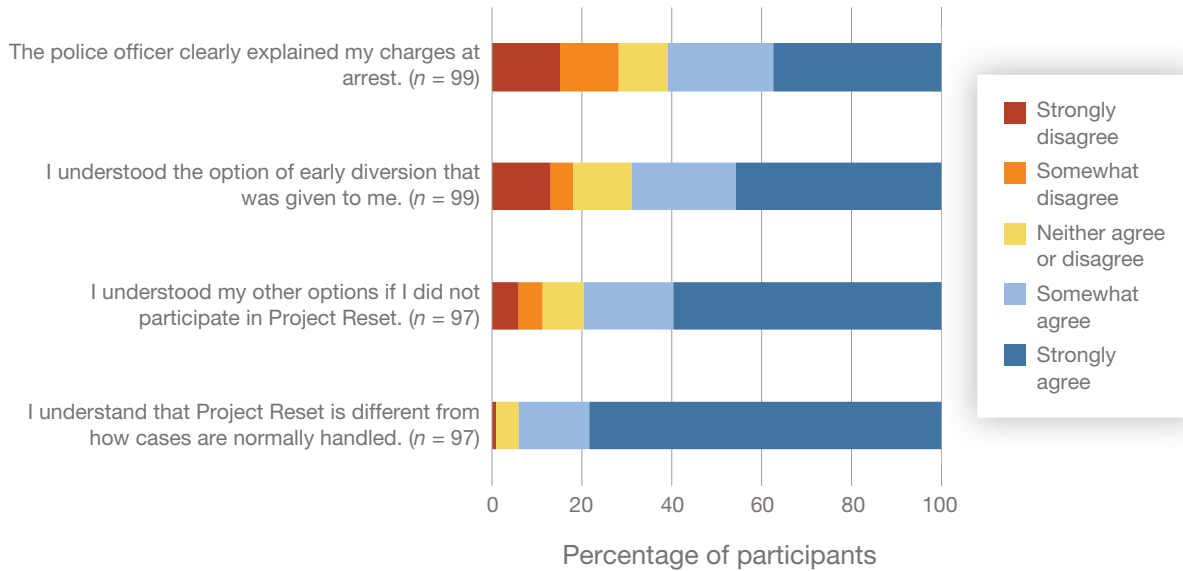
In interviews, participants also largely reported entering the program through outreach efforts by staff. Of the nine participants interviewed, none reported that their arresting officer told them about Project Reset. Once they found out about the program through phone calls and letters by staff, many participants reported thinking that it was “too good to be true” and doing their own internet searches to make sure the opportunity was legitimate. Participants found the program easy to participate in; they appreciated the provision of MetroCards for transportation to the program, the flexibility in scheduling, and the ability to complete the program over Zoom (for those who enrolled after the onset of COVID-19). Participants did not report any specific challenges to their participation in the program. They also stated that their decision to participate hinged on the fact that, after completion, their record would be sealed with no fees, fines, or jail time. Entering the program meant they did not have to interact with the court system. Participation also allowed participants to avoid key collateral consequences, such as losing their jobs and immigration-related issues (e.g., deportation).

We asked participants about what they understood of their case, including what they were told by the arresting officer and what they were told about diversion as an option. Participants had a variety of experiences with police at their arrest (Figure 7.1; Table B.1). About 60 percent somewhat or strongly agreed with the statement: “The police officer clearly explained my charges at arrest.” However, slightly more than 25 percent somewhat or strongly disagreed. Regarding their level of understanding about diversion, about 66 percent of

TABLE 7.3
Source of Knowledge About Option to Participate in Project Reset

Source	Frequency	Percent
Program provider	37	37
Police	21	21
Defense attorney	14	14
Prosecutor	7	7
Judge	2	2
Other	19	19
Total	100	100

FIGURE 7.1
Participant Understanding of Case and Diversion Option



respondents somewhat or strongly agreed that they “understood the option of early diversion that was given to me” and they “understood my other options if I did not participate in Project Reset.” Nearly all understood “Project Reset is different from how cases are normally handled.”

Satisfaction with Project Reset Experience

When responding to the statement on the survey “I am satisfied with Project Reset overall,” 95 people (89.6 percent) strongly agreed while only 2 (1.9 percent) strongly disagreed and no one somewhat disagreed (Figure 7.2; Table B.2). As one participant noted in an interview, “The program is very good, and I hope that other people will get a chance to experience it. I hope it will be around for a long time. It’s much needed, especially in our neighborhoods.” In the free-response question of the survey where participants stated what they thought was the most important thing they learned from Project Reset, some participants praised the program, including one person who answered: “The staff really care about the people they serve.”

Participants completed six items that measured the effect of program experience. They answered on a 5-point Likert scale indicating their agreement for each item (1 = strongly disagree, 5 = strongly agree). Because responses were similar across programs, we collapsed the results across programs (Figure 7.3; Table B.3). For four of the items, 89–92 percent of respondents strongly agreed with the statement. For the statement, “I learned useful information about the legal system from the program,” 85.8 percent of people somewhat or strongly agreed. The only item that did not garner overwhelming agreement was, “The program is only helpful for getting out of other punishment,” a statement that minimizes the value of Project Reset.

We also calculated the mean score for the six items on a scale with a minimum of 1 (most negative) and maximum of 5 (most positive). We reverse coded the second statement because disagreement indicates a positive view of Project Reset. This mean score represents a rating of program experience. In total, participants rated their experience of the programs very similarly and very positively (Table 7.4). Moreover, these very positive survey ratings again echo participant sentiments from the interviews.

FIGURE 7.2
Program Satisfaction

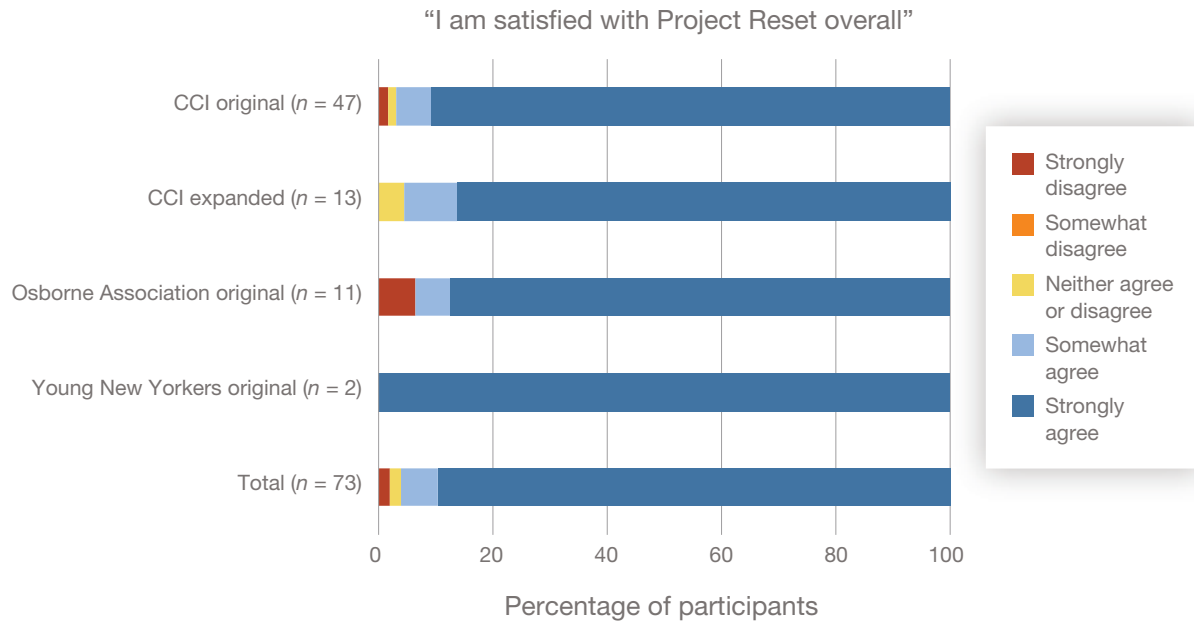
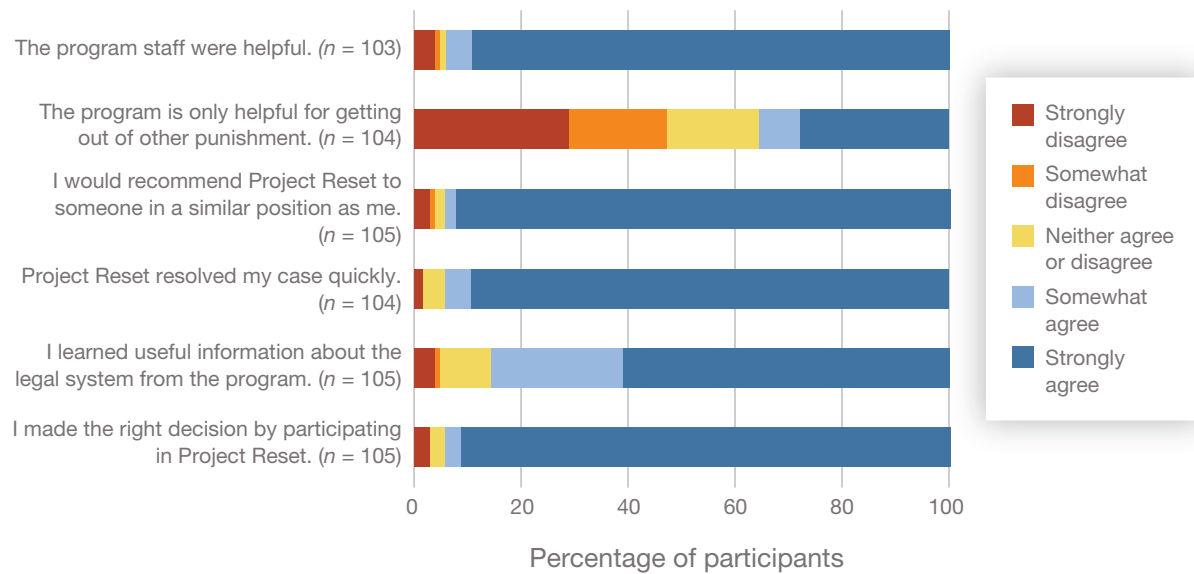


FIGURE 7.3
Program Satisfaction Sub-Items



The staff interviewed at the three Project Reset organizations held a very positive view of their work. They exhibited strong belief in the power of early diversion programs, with one staff member remarking that the program represents an outcome that “is proportionate to the offense and rethinks accountability in a way that does not hang the coercive threat of jail or conviction over people’s heads.” In sum, perceptions of the Project Reset programs across all programs and data sources were very positive.

TABLE 7.4
Participant Rating of Program Experience

Program	Mean Score	SD	Min	Max	Frequency
CCI (Original)	4.47	0.53	1.67	5.00	63
CCI (Expanded)	4.40	0.52	3.17	5.00	20
Osborne (Original)	4.54	0.42	3.67	5.00	14
YNY (Original)	4.72	0.35	4.33	5.00	3
All programs	4.47	0.51	1.67	5.00	100

NOTE: SD = standard deviation.

TABLE 7.5
Frequency of Participants Receiving Referrals for Additional Services

Program	Yes	No	Unsure/ Do Not Know	Frequency
CCI (Original)	32 (50.8%)	18 (28.6%)	13 (20.6%)	63
CCI (Expanded)	8 (42.1%)	5 (26.3%)	6 (31.6%)	19
Osborne (Original)	10 (66.7%)	4 (26.7%)	1 (6.7%)	15
YNY (Original)	1 (33.3%)	0 (0%)	2 (66.7%)	3
All programs	51 (51%)	27 (27%)	22 (22%)	100

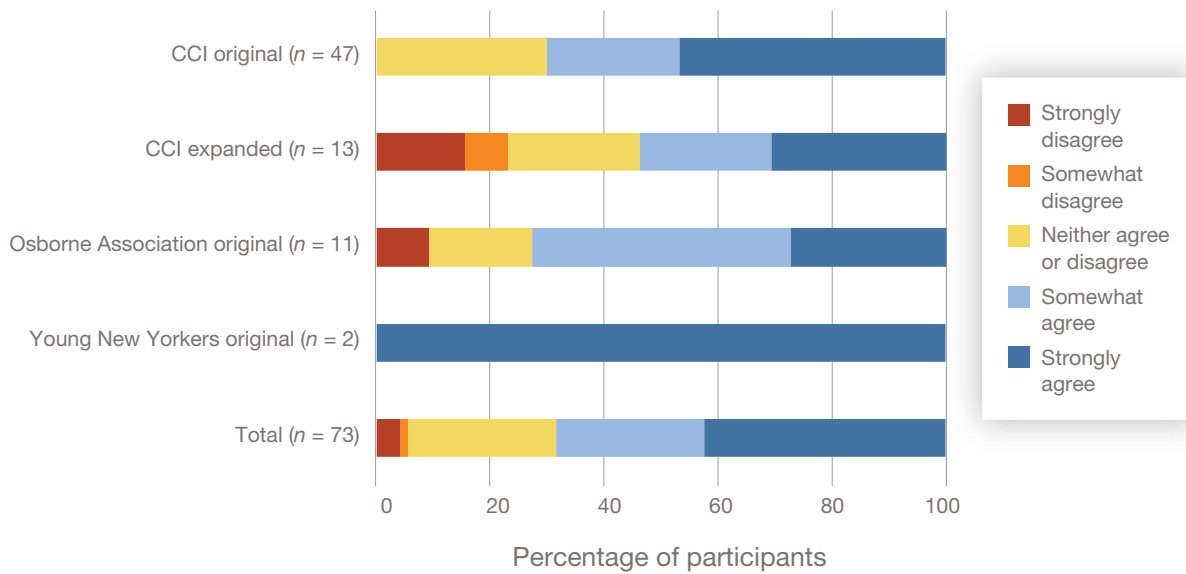
Wraparound Support

Organizations involved with Project Reset were encouraged to offer referrals for additional supports and services after completing the program (as discussed in Chapter 5). Based on the survey results, about half of all participants received referrals for additional services from their program (Table 7.5). Note that these numbers are higher than the percentages based on the administrative data, but that could reflect the convenience sample participating in the survey (e.g., it is possible that those who were more engaged with the program and post-program referrals were more likely to respond to the survey) or perhaps a limitation of the administrative data.

Among those that received referrals, 50 out of the 73 somewhat or strongly agreed with the statement that the referrals were “helpful” while only 4 somewhat or strongly disagreed; 19 expressed neither agreement nor disagreement (Figure 7.4; Table B.4).

Participants reported the referrals they received to mental health services and counselors as an added benefit. For example, participants who participated in the optional short-term counseling provided by CCI described the service in extremely positive terms. As one participant noted: “I had a great experience, there was a therapist . . . she offered free therapy for 3 months, she and I connected, [it’s the] best thing I’ve ever done.” On the free-response section of the survey, one respondent stated: “It was a great learning experience, and I was able to get connected to services that would help me overcome my mental health issues that led me to my arrest in the first place. I am so grateful for the program!” Other than mental health services and counselors, participants reported receiving referrals to other services that they needed, such as pro bono lawyers. In interviews, some others reported turning down referrals or that they were not offered any additional

FIGURE 7.4
Participant Assessment of Whether Referrals for Additional Services Were Helpful



services. Nevertheless, most surveyed participants received referrals, and both participants and staff agreed that they were beneficial.

Views on the Criminal Legal System

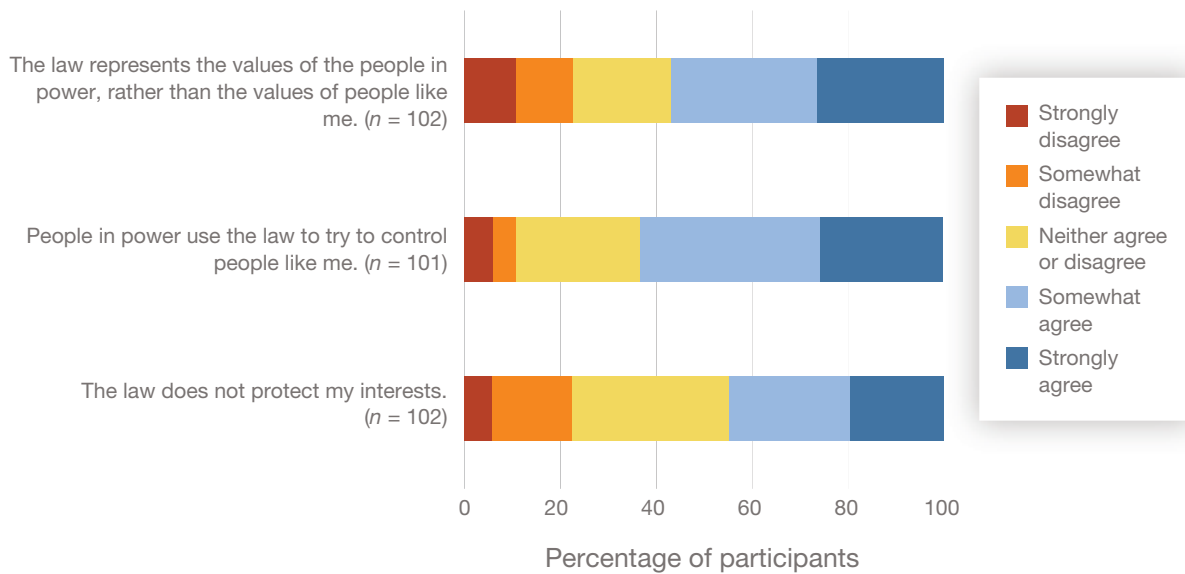
Overall

Participants responded to statements about the criminal legal system after being instructed the following: “The following statements are to understand your thoughts and feelings about the justice system in general. Please indicate how strongly you agree or disagree with each of the following statements.” Across the three items, about half the people somewhat or strongly agreed with the statements that reflected the belief that the justice system lacks legitimacy (Figure 7.5; Table B.5). Thus, respondents tended to have a cynical view of the criminal legal system.

However, on the survey participants also had the opportunity to complete a free-response question that asked, “What do you feel was the most important thing you learned from participating in Project Reset?” While some voiced discontent with the criminal legal system, many expressed that Project Reset gave them a more favorable outlook on it. Furthermore, many expressed that Project Reset had changed their lives by giving them a second chance and by helping them better understand the causes of their prior behavior, helping them make different decisions going forward. The following are a few sample quotations about what participants learned from the program, which related to perceptions of the justice system:

- “There are better ways to settle crimes such as shoplifting and other nonviolent crimes.”
- “That incidents can be resolved without wasting taxpayer money.”
- “The main thing is I didn’t commit a crime. So, Project Reset saved both State and myself aggravation and cost of a court case.”

FIGURE 7.5
Overall Thoughts and Feelings About the Justice System



- “Knowing that young people, especially young folks of color, can have access to a program that will help them be in the best position to take care of themselves, i.e., not having a record or going through the legal system gives me a great deal of comfort. I will absolutely speak highly of this program.”
- “I feel that it was most important knowing that someone was there to understand your situation and was willing to talk about it, without necessarily judging you. With that it teaches that “stupid” mistakes shouldn’t have to have a negative impact on someone’s rest of their livelihood.”
- “I found Project Reset helped me understand the legal system and its nuances. I gained clarity about my case and also options to reset my life.”
- “That the justice system is more helpful than I thought.”

Similarly, when asked in an interview about the long-term benefits of a program like Project Reset, one participant’s connection to fairness in the criminal legal system speaks to the effect the program had on such perceptions:

[Project Reset is] a more fair and just response to these low-level crimes that tackles the cause of it. A more holistic approach to meet the needs of the criminal legal system—to prevent future crime . . . People commit these crimes out of circumstances or need—for counseling, financial help, food stamps. It’s not fair to punish someone because they have a need. So, rather than punishing someone and not meeting that need and creating this vicious cycle of recidivism, really addressing the root cause, I think that’s what Project Reset does. Just having that support, given that everyone knows there is stigma—once you’re arrested, you’re labeled as a criminal. [With Project Reset, however, [n]o, you’re just a person who needs help processing what happened . . . That’s real criminal legal reform, that’s real social justice. [Project Reset] responds fairly, effectively, proportionately . . . My life was saved. My future was protected, and I was given a second chance.

In total, based on the surveys and interviews, participants voiced a negative outlook on the criminal legal system in general, but many also took the time to express that Project Reset had given them a more favorable impression of it.

Participant Perceptions of the Courts

The survey also contained more specific items about the court system and judges within it. Participants responded on a 5-point Likert scale to the statement: “As a result of participating in Project Reset, I have a more favorable opinion of the court system.” Across all programs, roughly 66 percent (70 out of 105) stated they either somewhat or strongly agreed, while 11.5 percent (12 people) somewhat or strongly disagreed.

Participants also responded to statements after being asked: “The following statements are to understand your thoughts and feelings about the justice system in general. Please indicate how strongly you agree or disagree with each of the following statements.” For the items “Judges put people in jail for no good reason” and “Judges make decisions based upon their prejudices or personal opinions” (43–46 percent of participants somewhat or strongly agreed, while 23–33 percent somewhat or strongly disagreed) (Figure 7.6; Table B.6). This indicates that people still held a suspicious view of judges’ decisionmaking, but a notable proportion did not share that perspective.

This sentiment continued when participants were asked: “The following questions are about your perceptions of the courts. Please indicate how frequently you think the courts behave in the following ways.” They responded to five specific statements with one of four choices: never, some of the time, most of the time, and always. About half of respondents agreed with “some of the time” for the five statements, and another half endorsed “most of the time” or “always,” whereas very few agreed with “never” (Figure 7.7; Table B.7). The largest segment of respondents believed in the fairness and decency of the courts only some of the time.

Participant Perceptions of the Police

Participants rated their agreement with the statement: “As a result of participating in Project Reset, I have a more favorable opinion of the police.” Slightly more than half of respondents (59 out of 104) either somewhat or strongly agreed, but 20.2 percent (21 people) somewhat or strongly disagreed, which was about double the frequency to the same item regarding Project Reset’s effect on views about the court system. Thus, it appears that Project Reset had a greater effect on views of the court. This might reflect the social issues of the time and not a feature of the program as the national conversation about racial equity and the justice system

FIGURE 7.6
Thoughts and Feelings About the Courts

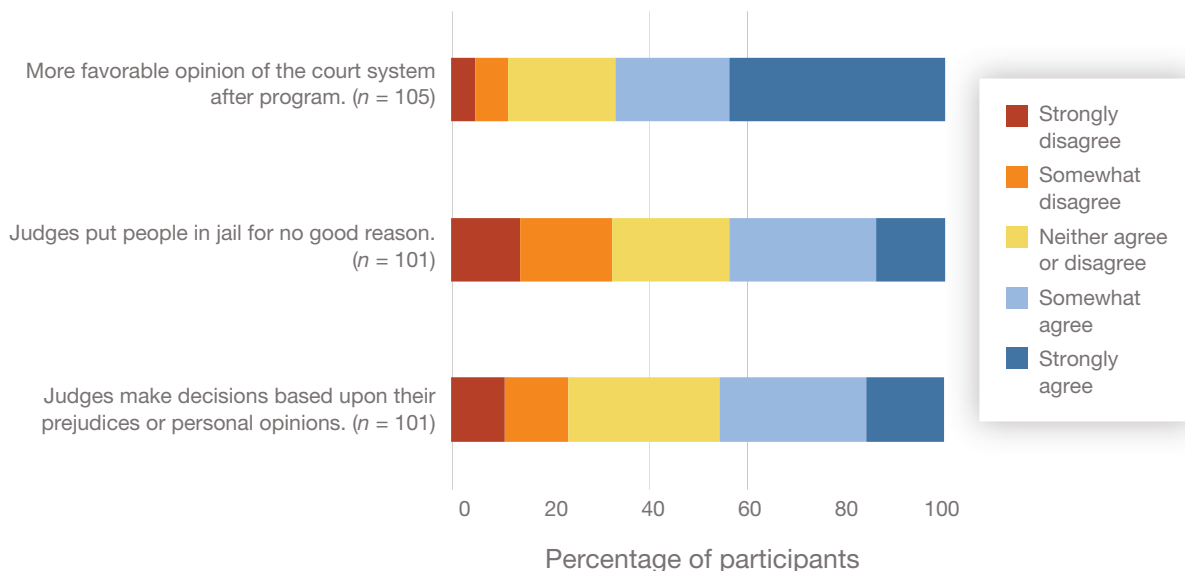
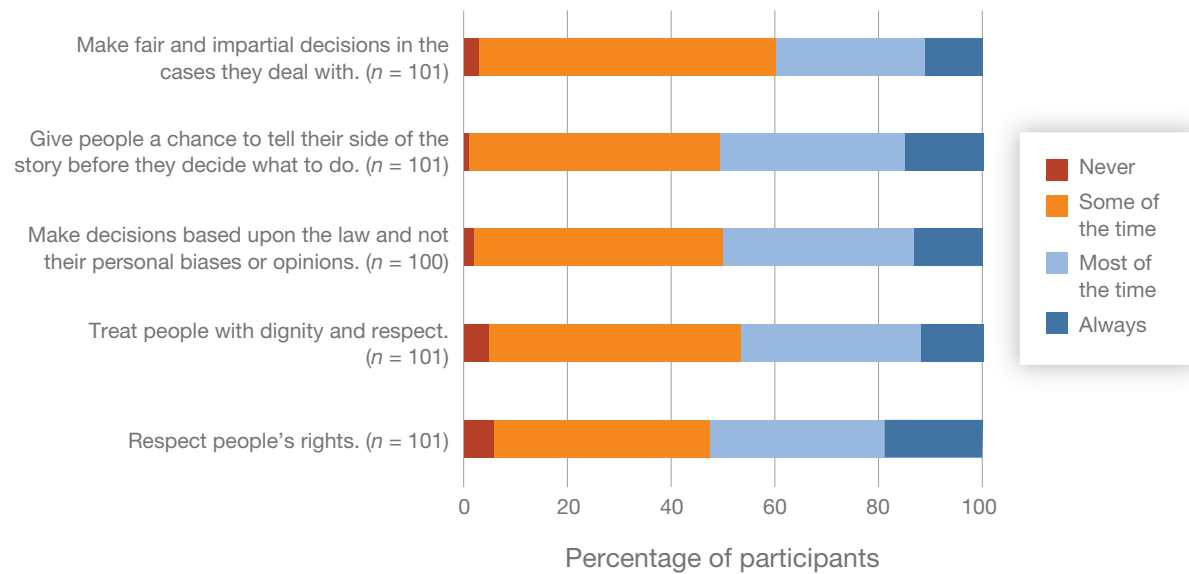


FIGURE 7.7
Specific Perceptions of the Court System



focuses more on the police than the courts. It might also reflect their feelings about their arrest specifically, or that this is viewed as a court-related intervention and not a program that involves the police.

Three items explored participants' specific views on the police. For the statement "The police only care about the views of some of the people in my community," nearly half somewhat or strongly agreed and slightly less than a quarter somewhat or strongly disagreed. About one-third somewhat or strongly agreed with the statements "I generally support how the police act in my community" or "When the police deal with people they almost always behave according to the law," but 40–46 percent disagreed (Figure 7.8; Table B.8). These responses indicate suspicion of the police remains despite the positive effect of Project Reset on participant opinions of the police. This was similar to the opinions about judges.

Participants were also asked: "The following questions are about your perceptions of the police. Please indicate how frequently you think the police behave in the following ways." As with the items about the courts, they then responded to five specific statements with one of four choices: never, some of the time, most of the time, and always. For all five statements, more than half of participants thought the police rarely behaved appropriately ("never" or only "some of the time"), and only around one-third agreed that police behaved appropriately "most of the time" or "always." On average, around 10 percent said police "never" behave in the ways described in the questions (Figure 7.9; Table B.9).

Benefits of Participation

When asked during the interviews about the content of the program, participants described the benefits of learning about the legal system, having a space to think about the effect of their arrest on their mental health, discussing the circumstances surrounding the arrest, connecting with other individuals who had been through a similar experience, and learning to be accountable for their decisions. Participants noted that the program helped them to gain a support system that they can continue to draw on even after completing the program. Participants found the experience enriching and appreciated the goal setting and art components (of YNY's program, specifically).

FIGURE 7.8
Thoughts and Feelings About the Police

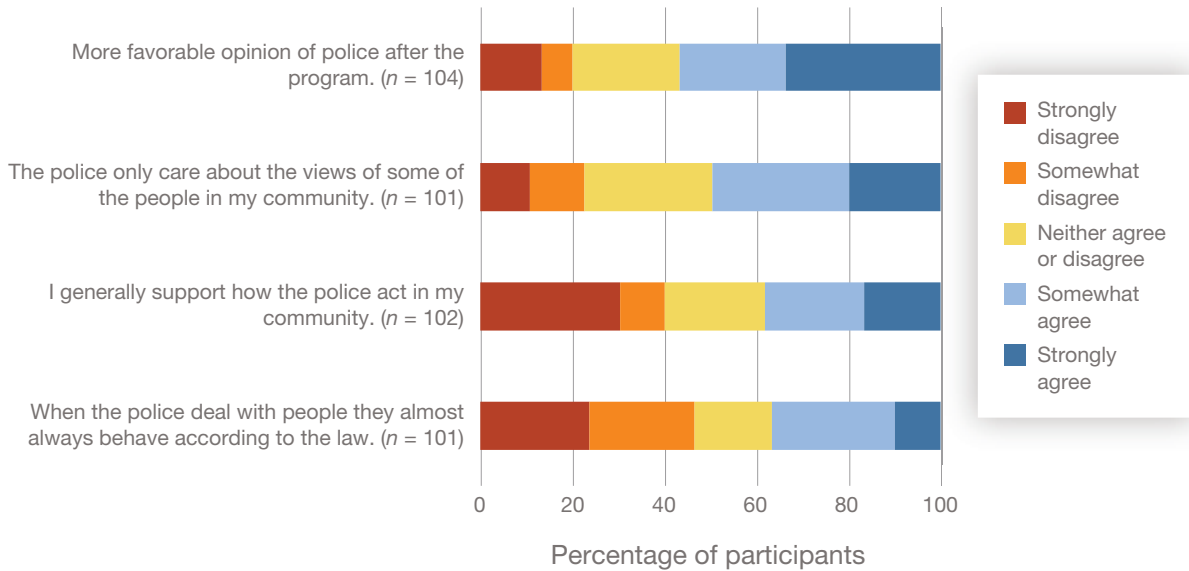
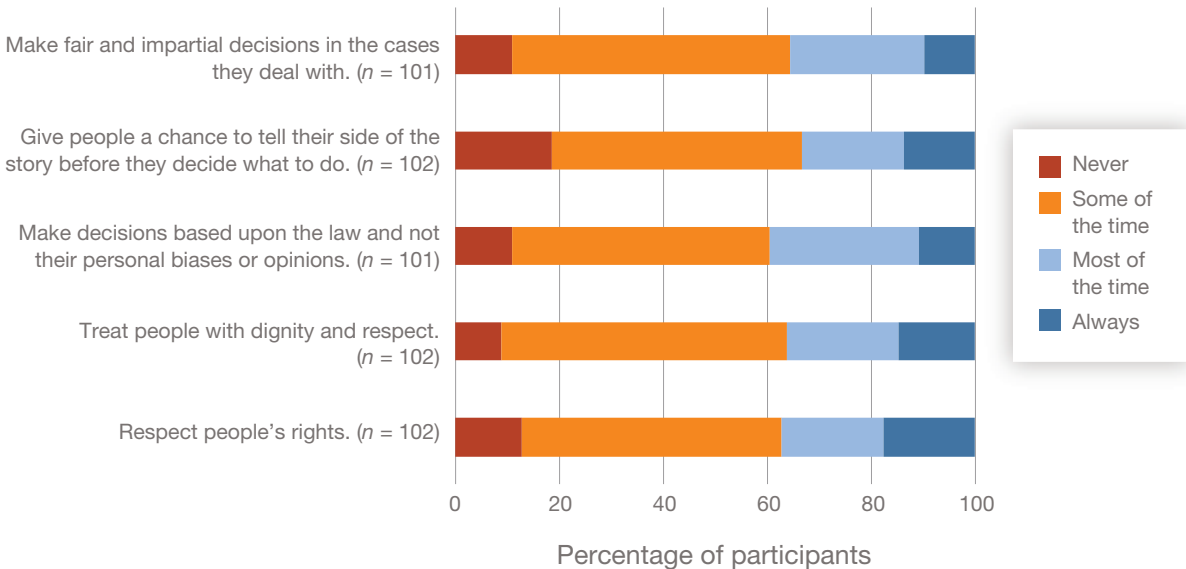


FIGURE 7.9
Perceptions of the Police



Other answers to the survey free-response question “What do you feel was the most important thing you learned from participating in Project Reset” corroborated the interviews. In addition to themes related to perceptions of the justice system described earlier, many participants described the benefits of self-reflection afforded by Project Reset:

- “I learned the consequences of my actions.”
- “I was given a second chance, and I turned my life around.”
- “Learning about other ways to live my life.”
- “How to recognize personal issues that led me to make the decisions I made.”

Similarly, staff members expressed that as a team they are helping the participants grow and learn from their experiences by reflecting on their behaviors and by linking them with other resources that can help them in other arenas of their lives. As one staff member stated:

I think when it comes to diversion programs, there is a lot of focus on what they allow people to avoid—going to court, dismissal or ACD that extends case. That is great and has huge benefits, but it's also been helpful to us to think about the ways that the program is additive. . . . I think one of the real benefits, besides the very concrete outcome of the [decline to prosecute] . . . is our amazing staff and going through the curriculum that, I think, empowers people to think meaningfully about their experience and what that means for them . . . whether it's a reflection on their own behavior, [or] how their lives are impacted by systems that are larger than themselves.

Another key benefit identified by staff members is that participants avoid interacting with the court system and the negative effects of missing work for court dates. This was also described as a benefit to the courts, as the program eliminates cases from the docket.

Suggestions for Improvement

When asked about desired changes to the program in the interviews, participant responses centered on getting more services; for example, making the program several sessions instead of simply one session. Participants also wanted more communication documenting that they had fulfilled program requirements, as some said little information was provided about the status of their case after completing the workshop. In general, though, participants were very satisfied with their experience.

Overall, staff were also satisfied with how the program is run. When asked about any suggestions for improving the program, they desired to expand eligibility so people with a broader range of charges had the opportunities offered by Project Reset and for more resources to follow up with former participants (e.g., by offering optional follow-up programming). One staff member also remarked on the lack of representativeness of participants relative to the eligible population and suggested that more efforts could be made to recruit a more diverse and representative participant base.

Summary

The participant interviews, staff interviews, and survey results indicate that Project Reset participation led to a more favorable outlook on the justice system, but many respondents still maintained a negative outlook on the police specifically and the justice system as a whole. Participants voiced very positive reviews of Project Reset, its staff, and the content, and they stated they would recommend it to others. In particular, free-response question respondents strongly stated that Project Reset had improved their lives by helping them better understand their actions and creating a second chance to move forward without a criminal record. Thus, the program itself seems to have been beneficial and rewarding to participants, including helping them to see the criminal legal system differently. However, the program's effect on beliefs and perceptions of the police was not as great. Overall, the survey results indicate that Project Reset was seen as worthwhile by those who experienced it.

Cost Analysis

In this chapter we present the results from a cost analysis that compared the fiscal outlay of money spent for an individual to complete Project Reset versus the money that would have been spent if the individual had instead had their case adjudicated in court. The outcome evaluation results presented in Chapter 6 indicate that if program completers had not completed Reset, they likely would have received an ACD in court. Note that because the outcome evaluation did not provide any evidence that the program had an effect on rearrest rates, the costs of rearrests did not need to be accounted for in the cost analysis. Thus, our cost analysis directly compared the cost of providing Reset programming to an individual with the cost of adjudicating an ACD in court. The cost analysis only included costs that occurred between July 1, 2018 and June 30, 2019, as our outcome evaluation was not able to determine the causal effect of the program beyond that date.

The next two sections present the costs for the programming pathway and the court pathway, respectively. The third section then directly compares these costs and discusses the implications regarding the usefulness of the program. The discussion presented in this chapter provides an overview of how all relevant costs were obtained. An in-depth discussion of the underlying methodology used to conduct this cost analysis is presented in Chapter 2, while Appendix C shows how all the cost estimates in this chapter were obtained from the source material.

Programming Pathway Costs

In this section we discuss how we determined the fiscal outlay spent for one individual to complete diversion, with the key results presented in Table 8.1. The per-person cost was calculated by identifying the total relevant amount spent on the diversion program between July 2018 and June 2019, and then dividing by the total number of people who completed diversion programming during that year (which was 1,001 individuals).

To determine the total amount spent on the program over the course of a year, there were two key components that needed to be included: (1) the amount spent by DANY to screen individuals and determine who is eligible for Project Reset and (2) the amount programs spent on delivering programming to these individuals. To determine the amount spent by DANY on screening individuals, we obtained information from DANY on the specific staff members involved in this activity, the amount of time they spent per week on this task, and their annual salaries. Based on this information, the total cost DANY spent on screening individuals between July 2018 and June 2019 was \$37,455.08. Note that DANY staff naturally will spend time screening activities for those who complete programming and for those who do not complete programming (either because they are ultimately not eligible or because they are eligible but do not complete programming) at the same time. Because we divided this total cost by the number of completers, we are inherently assigning the resources spent on non-completers to the completers. This was necessary because, even though this cost is technically not all being spent on completers, it needed to be accounted for as it will continue to be a cost present as long as the program is in operation.

TABLE 8.1
Project Reset Cost-Per-Person

Cost Component	Total Cost for July 2018–June 2019 (\$)
DANY screening	37,455.08
CCI program costs	312,997.50
YNY program costs	103,346.06
Osborne Association program costs	415,937.37
Total cost for all 1,001 completers	869,736.01
Cost per each completer	868.87

The other component of programming costs we accounted for was the amount the programs spend on outreach, intake, and program delivery. We obtained information on these costs from program invoices. Specifically, we included the amount spent on staff salaries for the staff members who were specifically involved in outreach, intake, and program delivery; we also included the money spent on program supplies, which included MetroCards, food, and program supplies (e.g., art supplies for YNY programming). Table 8.1 presents information on the total amount spent on these activities by each of the three program providers.

As the last two rows of Table 8.1 indicate, a total of \$869,736.01 was spent on Project Reset between July 2018 and June 2019, which resulted in a cost-per-completer of \$868.87.

How Programming Costs Vary by Provider

While Table 8.1 presents the total relevant amount spent by each of the three providers of Project Reset, Table 8.2 presents information on how the total cost-per-participant varied by provider. The provider cost-per-participant was obtained by dividing the total cost for each provider by the number of participants they served. The results indicate that the cost-per-participant for CCI is substantially lower than observed for both YNY and the Osborne Association.

There are a couple potential reasons why costs for CCI were relatively less expensive than for YNY and the Osborne Association. First, for reasons discussed in Chapter 3 and 4, all providers received a caseload significantly smaller than what they had been expecting. For example, the Osborne Association noted that the number of individuals they were serving was about half of what they had been expecting. However, even though the caseload was smaller than anticipated for all three providers, as Table 8.2 indicates, CCI still served substantially more individuals than either YNY or the Osborne Association. Because program providers generally onboarded new full-time staff for this program, they could likely only titrate down the number of staff so far, as they had to maintain staffing above a certain threshold to meet fluctuations in demand during any given period. Thus, even though each provider had only a few full-time staff to handle program delivery, CCI's larger case numbers indicate that they likely operated much closer to capacity than either the Osborne Association or YNY did.

It is also possible that providers varied in how they handled lower than expected caseloads. For example, CCI might have used a level of staff commensurate with the caseload they were receiving, whereas the Osborne Association and YNY might have continued to staff the program at a level based on what they were expecting to receive; smaller organizations (e.g., like YNY) often are less able to proportionally staff projects,

TABLE 8.2
Variation of Programming Cost-Per-Person Across Providers

Program	Cost (\$)	Participants	Cost/Participant (\$)
CCI	312,997.50	697	449.06
YNY	103,346.06	68	1,519.80
Osborne	415,937.37	236	1,762.45

because they might have fewer projects in general. Table C.2 indicates that the Osborne Association had more staff members than CCI did, despite the fact that CCI served about three times as many people.¹

An additional reason why the per-participant cost for CCI might have been less—which ties into the different staffing levels discussed above—is that the providers offered different programming options, and these inherently might have carried different optimal staff-to-participant ratios. For CCI, most programming was conducted in group sessions whereby one social worker could serve up to eight participants. In contrast, the staff-to-student ratio at YNY’s arts-based programming was much higher, as they tended to have multiple staff members at each session. Further, the Osborne Association had a subcontractor (Columbia University) run one of their programs, which potentially created inefficiencies if both agencies employed a certain number of full-time staff that were ultimately underutilized because the capacity was lower than planned.

While the cost-per-participant clearly differ across the programs, one important factor to note is that each provider offered different programming options and thus the benefits that individuals might have received from the different providers (which we cannot account for in this fiscal analysis) might have been different. Thus, it is possible that while the cost of some of the programming was clearly higher, the benefits accrued to individuals for that programming might have been higher as well.

Court Pathway Costs

Identifying the court pathway costs required determining how much it would cost to adjudicate an ACD in court, as our outcome evaluation indicated this would have been the court disposition for program completers. The first step in identifying the cost of an ACD was to determine the components of the ACD that needed to be accounted for. As noted in Chapter 4, when an individual receives an ACD there are often several different court appearances involved, and the ACD can also require that the individual meet certain conditions before the case is dismissed. As both the conditions and the number of hearings vary across ACDs issued, we worked to identify the specifics of the ACDs that likely would have been issued to the individuals that completed diversion programming using a matching method, whereby the set of individuals that completed diversion programming between July 2018 and June 2019 were matched with similar individuals that received an ACD in court the previous year (when Project Reset was unavailable).² Individuals were matched based on gender, age group, dominant offense, indicator variables for whether they had prior misdemeanor and felony arrests, and the location of their court hearing.³

¹ Note that the Osborne Association’s subcontract to Columbia University was to fund additional staff positions that provided programming. This is why the net total of Osborne Association staff exceeds the staffing of CCI.

² The control sample that was used for matching consisted only of individuals that had passed initial screening requirements and thus would have been eligible for Project Reset had it been around.

³ We did not match on race and ethnicity because we wanted to maximize the likelihood of finding a match, and the proportions of individuals in some race and ethnicity groups were relatively small. If a program completer had multiple matches, we

The court outcomes for the above matched sample were used to approximate what the experience would have been for the program completers had they gone through the court process. Specifically, we found that the matched sample had an average of three hearings, which included an initial arraignment hearing where the ACD was offered and conditions were set, a second hearing which likely served as a check-in on whether conditions were being satisfied, and a third hearing which served to ensure necessary conditions were satisfied and issue a dismissal.⁴ We found that 0.5 percent of the matched sample was required to complete programming, and 42.4 percent of the matched sample was required to complete community service. The remaining individuals were not required to complete any other conditions (outside the standard requirement for ACDs that no new arrests occur).

Collectively, the above discussion indicates that to determine the cost of the court pathway for an individual requires identifying the cost of an ACD that consists of three hearings and ends in a dismissal, as well as the cost of programming and the cost of community service. Per-person cost estimates of each of these components are provided in Table 8.3, and we outline below how each of these components were estimated, with more specific details in Appendix C.

To determine the per-person costs associated with adjudicating the ACD in court, we identified the staff positions that were involved in handling these cases, determined the time they spent on relevant activities, and used staff wage rates to determine the cost of the time spent. Interviews with DANY staff indicated that groups that spent time on these cases were prosecutors, public defenders, and court staff (including judges, clerks, and bailiffs), and thus it was necessary to get cost estimates for each of these three groups. DANY provided estimates of the time prosecutors spent on these cases, as well as staff wage rates. A public defender from Legal Aid (one of the public defender agencies that handle these cases) provided time estimates and wage rates for public defender involvement. We were not able to connect with anyone in the courts to determine the relevant time costs spent by court staff, judges, and bailiffs, so instead approximated this using time estimates from a previous diversion cost analysis where this information was obtained (Rempel et al., 2018) and calculated the value of this time using wage rates for court staff that were publicly available

TABLE 8.3
Court Cost-Per-Person

Cost Component	Cost-Per-Person (\$)
Cost to DANY to adjudicate case	199.59
Cost to public defenders to adjudicate case	188.62
Cost to court staff to adjudicate case	173.89
Cost to provide court-sanctioned programming	4.16
Cost to coordinate community service sentence	10.50
Total cost-per-person of court pathway	576.76

selected the match whereby the month of arrest for the control group was closest to the month of arrest for the program completer. For example, if a program completer was arrested in October 2018, and there were multiple potential matches from the treatment group, we selected the arrest that was closest to October 2017. We could not identify matches for 14 of the program completers; these individuals were dropped from the sample.

⁴ Note that while not every ACD outcome from the matched sample resulted in a dismissal, the vast majority did. Specifically, 3.6 percent of ACD cases were still pending (likely indicating a failure to appear), and 0.3 percent resulted in a plea deal. Because the remaining 96.1 percent ended in a dismissal we made the decision to just cost out an ACD that had an end stage of dismissal.

We proxied for the cost of required court programming using the average cost of Project Reset. Note that this amount (\$831.45) was multiplied by the proportion of individuals required to complete programming (0.005) to determine the amount spent on the average individual (\$4.16). We were not able to find relevant estimates for community service, so we used what we knew about the program to create credible estimates of the typical community service sentence, the time staff spend ensuring the community service is completed, and staff wage rates. Based on these estimates, we estimated the per-person cost of community service to be \$25.00. As only 42 percent of individuals receiving an ACD are required to complete community service, the average cost-per-person of community service is \$10.50 (as $0.42 * \$25.00 = \10.50).

The sum of all the cost components discussed above is shown in the last row of Table 8.3 and indicates that the cost of the court pathway for an individual is \$576.76.

Comparison of Project Reset and Court Pathways Costs

Tables 8.1 and 8.3 indicate that if an individual were to complete Project Reset, the average cost would be \$868.87, and if they were to have their case adjudicated in court the average cost would be \$576.76. While this cost analysis indicates that the court pathway has a lower cost than the Reset pathway, there are several caveats to this analysis which are noted below.

First, as was discussed when we presented the methodology for the cost analysis in Chapter 2, we identified the cost of the Reset and court pathways differently because we did not have invoices for the court pathway. The Reset pathway was priced out using exact information on annual expenditures and dividing by the number of completers, while the court pathway was priced out by multiplying time estimates on a given case with staff wage rates. If staff spend all their work time on cases, then both methods should give similar cost estimates. However, if staff members spend their work time on other activities—which could occur if the employer requires them to participate in general work activities (such as group meetings) or if caseloads were not high enough—then the first method will inherently result in a higher cost-per-person estimate as it will include the time paid to staff when they are not working on case activities. Note that the first method is inherently more accurate because the true costs of providing these services is the total amount that is spent on staff that provide these services, and these staff members will naturally not be able to spend all their time only on case activities. Thus, while we expected to get a relatively accurate cost estimate of the Reset pathway, we likely underestimated the cost of the court pathway since we implicitly assumed these staff spend all their time on case work. Given that staff members that serve the courts likely spend most of their work time on their cases (because of the high volume of cases that come through the courts each week) our underestimate of the cost might not be too significant, but it is important to keep this issue in mind when comparing the costs of the two pathways.⁵

A second important factor that affects how the pathway costs should be interpreted is that the program costs going forward could potentially decline. The fact that the cost-per-participant varied so widely across providers indicates that it should be possible to decrease program costs, although that will require first understanding why CCI's program provision costs were so much lower than the costs for YNY and the Osborne Association. If the Osborne Association and YNY are employing more staff than required for their existing caseload, then one option would be for them to either reduce staff, or ensure their staff have flexibility to work on other tasks that would not be charged to Reset funding. Another potential way to reduce costs would

⁵ Note that using the second method to cost out the Reset pathway was not a viable option as our interviews with providers indicated that caseloads were significantly less than expected and that existing staff easily could have taken on more cases. The second costing method would completely ignore these documented inefficiencies, and thus we did not feel it was a viable comparison to the court pathway cost estimate.

be to increase program uptake, as these programs were staffed to handle many more cases than they eventually provided programming for (see Chapters 3 and 4). This increased program uptake could occur either through improved outreach or expanded referrals or eligibility,

Finally, a third factor to note is that this fiscal analysis was not able to account for any of the benefits that accrue to individuals, as it is difficult to monetize these. In particular, individuals that participate in Reset avoid making multiple appearances in court (and the collateral consequences associated with this) and instead are able to attend programming in a supportive atmosphere that might be more tailored to their underlying needs. The survey results from Chapter 7 indicate these benefits are likely non-trivial as the vast majority of participants were satisfied with the program and felt it was useful, and many had a negative outlook on the legal system. Another potential benefit not accounted for in this analysis is that the diversion program can reduce the caseloads for the courts, which might be helpful to judges, public defenders, prosecutors, and other defendants that have cases adjudicated in court.

Table 8.4 shows how the cost of programming per participant would change under two alternative scenarios: (1) if all providers had the same cost-per-participant as CCI did and (2) if providers kept their existing cost structure but provided programming to the 734 individuals (during the analyzed time period) who were referred but unreachable. The results indicate that the fiscal cost-per-person of providing programming under both alternative scenarios is less than the cost of the court pathway. This indicates that, going forward, there should be an effort to have providers operate at a more efficient level—either reducing staff or finding ways to increase program participation. Potential ways to increase program participation include making a coordinated effort to solve the outreach issue or further relaxing program eligibility requirements.⁶

Determining whether the value of the program outweighs its costs will require that stakeholders take into account both the results from fiscal cost analysis and consider the benefits that might accrue to individuals. Because the potential benefits that accrue to individuals cannot naturally be converted to dollars without a significantly expanded scope of analysis, stakeholders will need to directly consider whether these potential benefits are worth the additional amount per person that might be required. Under the existing program cost structure, where the program can cost up to \$292.11 more per person than the court pathway, this will depend on how stakeholders weigh the potential benefits to individuals (e.g., better employment prospects, less disruption to relationships and family)—and to whom (and which agencies) those benefits accrue. However, under scenarios like those in Table 8.4, where program costs decline such that they fall below the cost of the court pathway, all stakeholders—regardless of the weight they place on the value of benefits to individuals—should be in favor of continuing the program.

TABLE 8.4
Programming Cost-Per-Person Under Alternative Scenarios

Alternative Program Cost	Cost-per-Person (\$)
Total cost-per-person of Reset Pathway if CCI costs used	486.48
Total cost-per-person if all outreach was successful	501.29

⁶ Note that our evaluation only examined the effect of providing programming for those who completed it. To the extent that program participation increases, the effects the program has on case and rearrest outcomes might be different.

Summary

The results of the cost analysis indicated that under the existing program cost structure, the per-participant cost of the program was greater than the per-participant cost of court adjudication—as much as \$292.11 on average. One reason why programming costs are relatively high is that providers are likely employing more staff than required for their existing caseload, which was significantly less than it was expected to be. Under credible alternative scenarios where provider efficiency was increased—either by lowering staff levels or increasing caseloads—the fiscal cost of providing programming falls below the cost of the court pathway, indicating the program should continue even if the benefits that might accrue to individuals were not taken into account. Collectively, the results suggest that even if stakeholders value individual benefits enough that they would find continuing the program under the existing cost structure to be beneficial, it would likely be worthwhile to pursue changes that would decrease programming costs.

Factors Affecting Program Implementation

Barriers and Facilitators to Implementation

During our 14 interviews with program staff, we asked about barriers and facilitators to the implementation of Project Reset. In this section, we review the six barriers and six facilitators that staff members across organizations raised, integrating additional information from program observations and our review of quarterly progress reports.

Barriers

Lack of Accurate Contact Information

The main barrier to program implementation reported by program staff was that contact information for potential participants is often incorrect or missing. As one provider stated, “The number one difficulty is getting to people who are eligible.” Providers indicated that if they can reach potential participants, those individuals are generally interested in attending the program. The lack of contact information was also one of the most common implementation challenges described in progress reports, and this finding was further supported by the quantitative data reported in Figure 4.1, which showed that 94 percent of potential participants who were reached went on to complete the program. However, the program providers are unable to reach a significant proportion of eligible individuals. One staff member also raised equity concerns about the lack of contact information, noting that people of color or those from low socioeconomic backgrounds appear to be more likely to have inaccurate contact information. This supports the findings that Black Hispanic individuals were less likely to be successfully contacted. There are many potential explanations for this—for example, these individuals might be more likely to have their phone turned off, use a temporary cell phone, or have unstable living situations. However, this might also reflect the nature of their interaction with police, such as a lack of trust in the police and unwillingness to provide accurate contact information, or errors made on the part of the police when recording contact information. A progress report from 2020 noted that this challenge became even more significant after the onset of the COVID-19 pandemic:

Contacting new referrals has become even more challenging during these months, and subsequently, successful contact rates are lower than usual. During weekly calls with DANY and the partner providers, it was noted that this is a consistent issue all three organizations are dealing with. There are a number of possible factors related to this decline in contact, including: the population of arrestees during the pandemic represent a higher number of people struggling with housing stability who lack communication means; and/or those arrested in conjunction with protest-related activities may be untrusting of the system and purposefully give bad [incorrect] contact information to the NYPD.

In their progress reports, the organizations also noted that they had a particularly difficult time contacting the expanded eligibility population.

Low Referral Volume

In progress reports, program staff often noted that they were receiving fewer referrals than expected. Combined with the lack of contact information, this resulted in far fewer enrollments than the organizations anticipated. Some organizations noted that this was driven in part by changes to program eligibility. For example, a progress report from 2018 noted:

The original estimated number of participants to be served by the [organization] during Implementation Year One was 1,566. However, in Quarter 1, [organization] only received a total of 101 referrals to Project Reset, which is not on track to meet this service target. The service target included “turnstile jumping” arrests. Once the highest volume of misdemeanors in criminal court, [this charge] was removed from program eligibility early in Quarter 1.

Another organization noted that the “number of referrals dwindled because of changes on how DANY prosecutes or approaches certain charges.” Concerns regarding referrals and enrollments were reported in progress reports across all implementation years (2018 to 2021), and the lack of referrals appeared to be exacerbated by the pandemic. For example, in 2020, one organization noted that “[s]taff noticed the referral volume dropped from around 35–50 per week prior to COVID-19 to 5–15 per week during this quarter.”

Challenges Related to Stakeholder Engagement

NYPD. When discussing the challenges of obtaining contact information from potential Project Reset participants, some interviewees perceived the lack of involvement by NYPD as a related barrier. As one interviewee indicated, more proactive police involvement might yield better contact information from individuals who are arrested. When participants have not heard about the program from the police, they might be more hesitant about participating out of fear that it is a scam. In this way, the lack of robust police partnership also results in more effort for the outreach workers to engage the potential participants. Our discussions with staff indicated that, over time, NYPD has taken less of an active role in the program. Program staff indicated that in the early stages, they presented at police precincts to explain the program and the role that police officers could play in educating potential participants about it. However, one staff member noted that over time, NYPD preferred to take a more centralized approach to their involvement with the program—for example, rather than having informal relationships between programs and individual precincts, there would be a consistent approach for precincts across the city (e.g., all precincts would have the same process for informing potential participants about the program at the point of arrest). Rather than resulting in an increased role for NYPD in informing potential participants about the program, this appeared to instead result in a reduced role for the NYPD. In addition, as described in Chapter 3, both DANY and program staff reported that the expansion in eligibility for Project Reset also seemed to result in NYPD taking a less active role with the program.

Defense organizations. Though the defense organizations—agencies that provide public defense services—were part of the design of the youth Project Reset pilot, one staff member stated that the defense organizations were not part of the early conversations establishing Project Reset and that it took some effort to engage with them. Some of the participants who were interviewed reached out to defense attorneys; although one participant worked with a public defender who helped to “handle the details” and explain the charges, two others reported that they attempted to contact a defense attorney and were not able to get in touch with them. There have been some opportunities for defense organization to provide key input; for example, CCI staff reported working with defense agencies to put together a “defense one-pager” that gets included with their outreach letters. They also indicated that when participants have questions about participation, they

refer them to the defense agencies to answer those questions. However, staff members suggested that a more formalized role for the defense organizations could be beneficial for participants.

A separate issue raised in progress reports concerned clients who might learn of Project Reset but were not formally referred to the program. For example, one program raised a concern about private attorneys who contacted program staff about clients who thought they might be eligible for Project Reset. They noted that their staff was initially unprepared to speak with these attorneys but developed a script they could use to facilitate communication with them.

Limited Community Knowledge of the Program

As described, some participants were skeptical when they first learned about the program and expressed concern that it might be a “scam.” In interviews, the program provider organizations described some efforts to increase perceptions of program legitimacy as part of their outreach efforts. For example, changes were made to the mailed flyers so that they looked less like junk mail or solicitations to participants. However, staff members noted that it would be beneficial to have more official information about Project Reset, which could be a useful resource for individuals who are contacted about participating. This could include posters or pamphlets available at NYPD precincts or distributed by NYPD. Greater visibility of the program in the community might improve uptake and might also increase the willingness of potential participants to provide accurate contact information to police.

Timeframe for Completing Diversion Programming

Another challenge identified during stakeholder interviews was the implementation of new legislation that went into effect in January 2020 which reduced the time between the arrest date and first appearance date in court.¹ As a result of this new policy, the amount of time allotted for programs to reach potential participants and schedule them to participate in Project Reset decreased. A staff member from CCI reported that the organization was able to pilot the briefer timeframe with DANY before the legislation went into effect, which helped to address this challenge, but there were still some issues to work out (e.g., cases accidentally being docketed and receiving a dismissal instead of a DP). However, this became less of an issue after the onset of COVID-19; in fact, staff from two organizations noted that potential participants had been less motivated to schedule their workshop because court dates were delayed, and perhaps felt less urgency as a result.

Challenges Related to COVID-19

The interviews that we conducted took place from June to August 2020, and the effect of COVID-19 was an important theme in those discussions. In interviews, many staff members described COVID-19–related challenges to providing services, including the remote platforms and an increased difficulty in creating rapport with potential participants in a virtual setting. They expressed concern that some participants did not have access to the appropriate technology to engage remotely and noted that it can be difficult to engage participants in the content when delivering the material via phone instead of video conference. We observed this challenge firsthand during program observations (which took place when programming was still being offered virtually) during which staff and participants sometimes had difficulty connecting to the meeting. In progress reports, organizations reported that there had been a drop in enrollment as a result of the pandemic. Another challenge raised in the interviews and progress reports related to the court system itself. For example, staff members noted during interviews that with virtual court, cases were being called and adjourned with little communication with the clients themselves. In progress reports, one organization also described the uncertainties regarding case status: “There were uncertainties around outcomes for specific cases and

¹ The legislation was included in New York State Senate Bill S1509C and New York State Assembly Bill A2009C, both in 2019.

whether they would be docketed, adjourned, or dismissed.” This organization noted that these uncertainties necessitated changes to programmatic timelines and processes.

Facilitators

Strong Relationship Among Providers and DANY

During interviews, staff members described a largely positive relationship between their organizations and DANY. Staff described a standing biweekly (pre-COVID-19) or weekly (post the onset of COVID-19) call with DANY and representatives from each provider organization. These served as an opportunity for DANY to provide updates and for programs to troubleshoot issues as they arose and were described by one provider as helpful to making sure the program ran smoothly. Staff also described informal, day-to-day communication with DANY about specific cases. In general, communication with DANY was described in positive terms by staff members; DANY was described as “very available and responsive,” and one staff member indicated that “communication lines are really open.”

Staff members reported that DANY is responsive to their concerns, and they feel like they share a common goal. One staff member commented: “When a partner has a stake in the program, they are more invested in the success of the program. They want to see this succeed.” One staff member also expressed appreciation that DANY has been willing to expand criteria for the program over time. They appreciated the broadness of DANY’s eligibility criteria for the program, though some felt it could be broader. Staff members noted that the partnership between DANY and the provider organizations has grown stronger and more collaborative over time. They described open and frequent communication as an important part of this relationship.

In progress reports, staff members also discussed the benefits of communication with one another. For example, this included the ability to meet with court partners and the other organizations to “solve any current issues, mitigate future problems, and increase efficacy of programs.” One organization also expressed the benefit of being able to meet with these stakeholders to “share our experiences and learn from each other.”

Staff Dedication and Capabilities

During interviews, provider organizations reported that they have built strong teams where individuals feel like they can rely on their peers. Staff members trust each other’s grasp of the content and feel the warmth that they extend to participants as they establish rapport. As one staff member noted: “I have an amazing team that works well with participants and goes above and beyond with our participants.” Staff dedication was also described as important to outreach efforts; one staff member described their team as “tenacious” with outreach. Participants highlighted that the program staff created a non-judgmental environment where they felt comfortable sharing about their experiences. In addition, during our program observations, we saw firsthand the effort that staff made to engage all participants in the material and discussions and to bring material to life with examples relevant to the lives of the participants.

Program Flexibility

In progress reports, two of the program provider organizations noted that their ability to offer the program in a flexible manner helped them to serve a larger number of clients. Prior to the pandemic, examples of this flexibility included scheduling participants at times that were convenient for their schedules, offering certain programs more often because they could be facilitated by a Spanish-speaking staff member, and a willingness to reschedule individuals who did not attend their scheduled appointment. After the onset of the COVID-19 pandemic, organizations noted that their shift to virtual workshops provided clients with additional flexibility. For example, one organization noted: “We’ve had participants take workshops on their lunch break or in the car.” Another organization noted that they offer their virtual program via both Zoom and telephone to accommodate the needs of a broader group of clients.

Salesforce Case Tracking System

The integrated case tracking system between DANY and program providers was identified as a key facilitator to program implementation. In interviews, staff members described how the Salesforce platform streamlined the referral process from DANY to providers and allows all stakeholders to know the status of each case at a given moment. In interviews and progress reports, staff reported that Salesforce also made it easy for staff to capture data and run reports on the number of referrals and program completions, which is useful for internal performance tracking purposes and fulfilling reporting requirements under Project Reset. The system also enabled staff to auto-populate outreach and completion letters with participant information rather than managing these functions manually. One interviewee stated:

Before Salesforce, the system we were using [to] hear of referrals was via email. Imagine 20 emails coming through back-to-back-to-back about eligibility, when the person is scheduled, when they completed, etc. It was overwhelming and exhausting. Salesforce made it easier for everyone, including [DANY], to see participant information, when someone's scheduled and completed, so it became easier for everyone to manage case profiles and details.

In progress reports, organizations also said that they were able to develop customized reports and forms to further support their internal processes, such as reports that indicated open cases that had not been scheduled and fields to indicate when they lost contact with a client.

Inviting Nature of Program Locations

Staff members described the spaces where they hold program activities as another key facilitator. They described the offices and community-based locations of the activities as welcoming spaces that provide snacks and drinks. A YNY staff member highlighted the difference between attending a program with the probation department, which might be a more typical pathway for individuals who have been arrested, and attending Project Reset at the Swiss Institute, a “beautiful location” where participants are surrounded by art. Interviewees highlighted that the program staff and physical locations create a space where participants can speak and not be judged.

Relevant and Responsive Programming

In interviews and progress reports, staff described certain facilitators related to the program content. One of these was the fact that the provider organizations have been willing to modify program content over time and in response to specific needs. For example, organizations reported developing protest-related content in response to protest-related arrests during the summer of 2020. In addition, multiple organizations made efforts to use data to inform program improvements. For example, CCI worked with a consulting group who interviewed participants and staff, observed groups, and then assisted CCI with creating a more consistent version of their curriculum. At least one organization made efforts to build their staff's capacity to evaluate and improve upon their program. In progress reports, YNY noted that their program coordinator attended workshops on topics related to evaluation and outcomes-based planning; by building these capabilities, they anticipated that their coordinator would be better able to improve their program's design “to ensure that programming engenders desired outcomes for participants.” Program staff also described using pre- and post-program data (e.g., intake data and feedback collected via surveys or feedback forms) to tailor the curriculum to the needs and interests of their clients. A small number of staff members also highlighted their ability to refer participants to additional services and the partnerships they have formed with other agencies as an important facilitator.

Summary

Our findings identified some of the key barriers and facilitators to Project Reset implementation. Stakeholder communication is a key element of the program, and the provider organizations described a strong and collaborative relationship with DANY. They noted that relationships with NYPD and the defense organizations have been less formal but that stronger relationships with these groups would benefit the program. Regarding other barriers to implementation, the lack of accurate contact information was named as a key issue, as have changes to the timeframe in which the program must be completed. Facilitators to implementation included the team cohesion and rapport among staff members of the provider organizations and the Salesforce case management system. Staff largely reported positive perceptions of the program and its effectiveness.

Takeaways and Recommendations

The final chapter of this report summarizes the key takeaways and recommendations from the evaluation, as well as the study limitations. This report provides DANY and program partners the opportunity to reflect on the implementation of the program and learn more about the participants who are being served and the services that are being provided, as well as the effect the program had and how the program could be made stronger in the future.

Takeaways

Program Implementation

The program provided participants a range of possible benefits. Project Reset participants had their arrest sealed with no fees, fines, or jail time, and it eliminated interacting with the court system. Participation also removed key collateral consequences, such as job loss and immigration-related issues. Moreover, the programming was designed to help address the factors that might lead to arrests in the first place, such as behavioral health concerns, decisionmaking, or economic assistance, through the core programming and the referrals made to other services. This was particularly salient given the needs that participants presented with, including high rates of unemployment.

A lack of reliable contact information and community knowledge of the program are barriers to recruitment. Obtaining reliable and accurate contact information is a key issue as a sizeable proportion of eligible participants cannot be reached by the provider—41 percent of individuals eligible under the original criteria and 76 percent of individuals eligible under the expanded criteria. Without accurate contact information, many eligible individuals are not provided with this diversion opportunity. In addition, even when potential participants are reached, some individuals express concern that the outreach they receive from programs is a “scam.” This is especially the case if they were not previously made aware of the program (e.g., at the point of arrest).

Inequities in program completion might compound existing systemic disparities. Given the widespread racial disparities that are known to exist in the criminal justice system, it is important that diversion programs do not exacerbate these disparities further. Unfortunately, the outreach issue noted above results in White Hispanic, Black non-Hispanic, and Black Hispanic individuals composing 65.2 percent of the referred population, but only 58.8 percent of program completers. This represents a drop in representation of 6.4 percentage points and indicates that this program might result in the level of observed racial and ethnic disparities in the legal system increasing at subsequent system points.

Stakeholder engagement is important to program success. Program providers described the importance of their relationship with DANY in implementing the program. In particular, they cited DANY’s support, availability, and willingness to hear the perspective of the providers. The Salesforce data tracking system was also an important tool for facilitating stakeholder communication, particularly with respect to making referrals and communicating the status of eligible potential participants. It appears that additional communication with other relevant stakeholders—such as NYPD or the defense organizations—could strengthen the program.

Program staff are key to program success. Both program staff and Project Reset participants emphasized the importance of program staff in making Project Reset successful, including staff members' dedication and their nonjudgmental approach to working with participants. Staff willingness to evaluate and modify the curriculum in response to feedback was also noted as important. Similarly, participants seemed to benefit from the emotional and mental health support provided by Project Reset staff, and both they and staff felt that connection. As such, it is clearly important to maintain the program providers' emphasis on rapport-building as the program continues in the future, and it serves as a reminder to hire, support, and keep quality staff.

Participants appreciated the wraparound support provided by Project Reset. The variety of services provided by Project Reset contributes to its success and therefore should be maintained or further developed. Participants valued referrals for outside services that had the potential to be helpful for them. In particular, they mentioned mental health treatment, legal services, and financial aid. Project Reset staff should strive to maintain up-to-date contact information and resources for participants to further facilitate such referrals. Referrals likely extended the effect of Project Reset beyond the initial light touch of the one-day intervention.

Participants would benefit from additional follow-up about their case disposition. Some participants noted that they would have appreciated clearer follow-up after completion so that they were sure their arrest had been sealed. As noted earlier in the report, some providers did attempt to follow up after completion but because of inaccurate or outdated contact information and resource strain, these attempts were often unsuccessful. Programs could also provide clear guidance to participants as to how they can check the status of the record; at least one of the provider organizations currently does this.

In-person services in community-based settings promote engagement. The location of services in the offices of nonprofits— and even sometimes in local museums and other community settings— rather than courtroom-type settings helped to create a welcoming environment for participants. Though programs were able to transition services to a virtual format because of the COVID-19 pandemic, it does appear that there are benefits of offering the program in person when it is possible.

COVID-19 had a substantial effect on program implementation. As expected, beginning in early 2020, COVID-19 had a great effect on the number of cases referred to Project Reset. However, the number of cases was steadily declining pre-COVID-19 since the peak of enrollment in mid-2018. COVID-19 also altered the ability of programs to deliver services, and each program had to pivot dramatically to provide the necessary programs to individuals.

Program Outcomes and Cost Analysis

Participants have positive perceptions of the program and its effectiveness. Overall, participants voiced very positive perceptions of Project Reset, its staff, and the content, and they stated they would recommend it to others. In particular, in the survey free-response section, respondents strongly stated that Project Reset had improved their lives by helping them better understand their actions and creating a second chance to move forward without a criminal record.

Program participation might be associated with improved views of the justice system. Participants reported that Project Reset participation led to a more favorable outlook on the justice system.¹ Most participants indicated that Project Reset improved their opinions of the courts and the police, and many volunteered

¹ While we were unable to administer surveys before and after participation in Project Reset, respondents completed two items about the effect of the program on their opinions of the justice system. Roughly 66 percent (70 out of 105) stated they either somewhat or strongly agreed with the statement, "As a result of participating in Project Reset, I have a more favorable opinion of the court system." Only 11.5 percent (12 people) somewhat or strongly disagreed. When asked, "As a result of participating in Project Reset, I have a more favorable opinion of the police," slightly more than half of respondents (59 out of 104) either somewhat or strongly agreed, but 20.2 percent (21 people) somewhat or strongly disagreed.

that outlook as answers to free-response questions on the survey and in interviews, including as a long-term benefit. However, their views of both the courts and police were mixed at best, suggesting that the effect of Project Reset on these types of outcomes might be more limited. In part, this might reflect the fact that program participants had little interaction with either the police or the courts as a result of enrolling in Project Reset—that is, they might see their experiences with Project Reset as fairly separate from law enforcement or court. Even though we cannot generalize or draw firm conclusions on the effect that Project Reset had on participant views on the justice system because of the limitation of not being able to survey participants before program participation and not being able to gain the perspective of those participants (and eligible non-participants) who did not complete the survey, the qualitative interviews and survey responses suggest that the program had a positive effect. This program goal, which was important for DANY, appears to be successful.

The individuals that participated in the program likely would have received an ACD if their case had been adjudicated in the courts. While both program participation (which results in a DP) and an ACD effectively result in the same non-conviction outcome for the individual, the experience of the diversion pathway might be significantly better for the individual. ACDs require the individual to remain arrest-free and make multiple court appearances over a six-month to one-year period, and many are required to complete either programming or community service as a condition of their ACD. During this supervisory time period, the case will be visible in a criminal history check. In contrast, Project Reset allowed individuals to attend programming in a supportive atmosphere that tried to tailor services to their underlying needs, and the arrest was sealed shortly after program completion.

The program had no observable effect on the one-year rearrest rates for individuals that participated in the program. It is important to note that the program effects we are finding might have been different if the referral and completion rates had been different. In particular, if the program completion rates had been higher, that would mean that somewhat riskier individuals would now be participating, and the effects of the programming on both case outcomes and rearrest rates might be different for them.

Under the existing program cost structure, the program costs were greater than the costs of traditional court adjudication. The cost analysis indicated that providing programming to an individual can cost up to \$292.11 more per person than it would cost for the individual to have their case adjudicated in court. However, the cost analysis only includes money spent by agencies and does not include the benefits that individuals might receive by avoiding traditional adjudication and receiving more tailored, supportive services instead. Stakeholders will thus need to determine whether the higher fiscal cost is worth the perceived benefit of programming to individuals.

If program provision costs were to fall, the program would be considered cost-effective even without taking into account the benefits of avoiding adjudication. Program costs were relatively high as providers were likely overstuffed because caseloads were significantly less than expected. Under credible alternative scenarios whereby provider efficiency was increased—either by lowering staff levels or increasing caseloads—the fiscal cost of providing programming falls below the cost of the court pathway.

Recommendations

Based on these findings, we identified a set of recommendations for the program, which are detailed below.

- **Increase community awareness of the program and highlight benefits to improve recruitment.** Efforts to raise the external profile of the program might help to strengthen the perception of program legitimacy, which was a concern raised by participants. This could include developing clear communication about benefits of the program, including the ways that it helps participants avoid collateral consequences of justice system involvement.

- **Strengthen the outreach process.** Efforts to gather reliable and accurate contact information at the stage of arrest would allow a much greater population of individuals the diversion opportunity. Individuals might be more willing to share their contact information if they were informed at the point of arrest about the program; moreover, if there were more general awareness about the program in the community, it could also increase the likelihood that eligible individuals would be willing to respond when they do receive outreach materials. As these outreach issues contributed to racial disparities in completion rates, resolving this issue could also potentially result in more equitable outcomes.
- **Ensure the referral process uses eligibility criteria that are justifiable, transparent, and consistently applied.** The fact that a significant proportion of individuals are ultimately not referred after passing initial screening checks highlights the importance of ensuring that there is a clear rationale behind the criteria used, especially because existing criteria disproportionately screens out minority individuals.
- **Examine whether existing eligibility criteria result in racial or ethnic inequities in referral rates.** The current screening process relies on both automated checks and detailed case-specific checks that must be conducted by hand. If an individual passes the automated checks but is not referred to the program because they do not pass the case-specific checks, the existing data does not record the specific reason they were determined to be not eligible. In order to facilitate analysis that will identify the impact existing eligibility criteria has on racial or ethnic inequities in referral rates, the screening process either needs to be fully automated so it can easily be determined why an individual was deemed not eligible, or the precise reason the individual was screened out needs to be recorded in the data. Fully automating the screening process has additional advantages as well, as it would be more efficient and it would increase transparency into the screening process.
- **Sustain existing relationships among stakeholders and deepen relationships with other relevant stakeholders.** The strong relationships between program providers and DANY were an important program facilitator. At the same time, providers indicated that additional communication with other relevant stakeholders—such as NYPD or the defense organizations—could strengthen the program.
- **Support staff autonomy and program flexibility.** Staff dedication to delivering this program was an important facilitator, as was the ability of each program provider to draw on their existing strengths as an organization. Providing encouragement and support to staff to ensure they can continue to fulfill the mission of the program is key.
- **Continue to offer programs in in-person, community-based formats.** Prior to the pandemic, services were offered in community-based locations and group formats, which were described as strengths of the program model. Though the pandemic might create some ongoing obstacles to providing services in this format, it is worthwhile to continue offering services outside the justice system and, when possible, provide programming in person.
- **Connect participants with needed services after completion.** Participants described the value of the referrals they received through the program, and it is important that the providers continue to maintain strong referral relationships to high-quality mental health, legal, and financial services. This might include referrals provided immediately after participants complete the program, but could also include ongoing contact with program participants, such as through an alumni program. These services can help address risk factors for future justice system involvement. Post-program services should also include better education on how participants can request documentation that their arrest has been sealed, which could be easily incorporated into the final parts of a workshop.
- **Reduce program provision costs.** The primary reason program costs were relatively high was that providers seemed to be staffed to serve a higher caseload than they received. To reduce program costs, providers could either reduce the number of staff or ensure that staff members are able to work on other projects when caseloads are low. This highlights the need for program providers to have the ability to

be flexible in staffing levels. Alternatively, providers could continue with their existing staff, and there could be a coordinated effort to increase program caseloads either by mitigating outreach difficulties or relaxing eligibility requirements further.

- **If participation in the program increases, further evaluation work on program effects might be necessary.** Several of the recommendations made above touch on increasing the number of participants in the program. The outcome evaluation and cost analysis conducted in this report are only able to determine the effects and cost-effectiveness of the program for the set of participants between July 2018 and June 2019. To the extent that efforts to increase the number of participants are successful, either by improving the outreach process or by relaxing eligibility requirements, the effects of the program might be different.

Study Limitations

This study had a few limitations. First, as detailed in Chapter 6, the regression discontinuity design implemented was not able to detect small changes in outcome variables. Thus, the program might have had small effects on case outcomes or rearrest rates that our research design was not able to identify. Second, because of the fact that many other changes occurred at the same time that the expanded population gained eligibility (i.e., COVID-19 and other state criminal justice system changes), this evaluation was not able to determine the effect Project Reset had for the expanded population. Third, because of the relatively small samples that would have occurred if the results were stratified by provider, it was not possible to determine the effects the program had separately by provider. This is unfortunate given that providers had different programming options, and it thus would have been useful to examine whether certain programming options were more effective than others in improving participant outcomes (such as rearrest rates). Relatedly, programs were allowed flexibility in the type of diversion programming they designed, including promising, evidence-based practices. However, it was beyond the scope of this study to assess fidelity to the program models, and it was also possible that certain components of programming had a stronger evidence base than others. This also could have contributed to participant outcomes, both within and across programs. Fourth, we were unable to recruit a representative sample of participants for the client survey. We collaborated with staff from each agency to provide clients with the survey link and share information about participation, but we do not know the total number of people that each organization reached out to, and there might have been variations across organizations with respect to the number of follow-up messages they sent to their clients. Fifth, with respect to the participant interviews, the individuals who agreed to be contacted and agreed to participate might have been systematically different from those who did not participate. Though we requested that organizations reach out to a range of participants and not select on certain characteristics (e.g., individuals who had a positive experience with the program), there still might have been a selection effect in who decided to participate. Sixth, it is important to keep in mind that this intervention was specifically designed to serve a low-risk population, and that might also explain the lack of significant effects of the program. Finally, the scope of our evaluation was only able to determine the effect on case outcomes and rearrest rates and was not able to determine the effect the program had on factors such as individuals' perceptions of the justice system or outcomes besides rearrest rates that are associated with the collateral consequences of criminal justice system involvement (e.g., job loss).

Regression Discontinuity Design for Outcome Evaluation

The outcome evaluation results presented in Chapter 6 present the regression discontinuity results in graphical form. Estimating the formal regression models of these outcome figures is a helpful complement because it both indicates whether the outcome results are statistically significant and can provide more precise estimates of the magnitude of the effects. However, because the core takeaway results are visible from the graphs alone, for simplicity we only showed the graphs in the main text and presented the regression results in this appendix. Below we outline the specific requirements of the regression discontinuity design and the regression specifications estimated, provide more support for the requirements of the regression discontinuity design, and then present the regression results.

Requirements of the Research Design

For this research design to identify a causal effect, there were three implicit requirements that needed to be met. We describe each of these below and discuss the modifications we made to help satisfy some of these requirements.

Requirement 1: There needed to be a discontinuity in program completion at the time of implementation. For the program to have a discontinuous effect on outcomes at the threshold, it needed to be the case that program completion jumped discontinuously at the threshold as well. Put another way, if the rate of program completion increased at a steady rate over time and there was a program effect, we would observe a steady change in outcomes over time (as opposed to a discontinuous change at the threshold). The graphs constructed in Chapter 6 verify this requirement was satisfied.

Requirement 2: Besides the program, there must be no other changes occurring at the time of implementation. In other words, there were no other policies being implemented around the same time as Project Reset, nor were there any other systematic differences between the individuals arrested on either side of the threshold. If other changes occurred simultaneously, there would be no way to determine if the presence of a discontinuous change in time trend at the threshold was because of Project Reset or because of other changes that occurred simultaneously. The main policy changes we were aware of that occurred during this time were that DANY stopped prosecuting possession of marijuana and theft of service charges. To account for this, our outcome analyses dropped anyone charged with these offenses (as their dominant charge) throughout this entire period. If these charges had been included there would have been a significant shift in the composition of those who were arrested before and after the threshold. If this composition shift resulted in a change in outcomes (which would occur if outcomes were correlated with crime type), a discontinuous jump in the time trend at the threshold would pick up both the effect of the program and the effect of the compositional shift.

Note that the need for similar composition of groups arrested pre- and post-implementation is precisely why we used a measure (i.e., the flag provided by DANY in the data that identified who passed the initial screening requirements) that used the same criteria to identify individuals on both sides of the threshold. Alternatively, if our post-implementation sample included only the set of individuals that had completed diversion programming, while our pre-implementation sample included the individuals flagged as meeting some initial screening requirements, there would have been substantial compositional differences in these groups (as is shown in Chapter 5 and Table B.10). This compositional shift would result in there being a discontinuous jump in the time trend at the threshold for reasons outside of program effect.

Requirement 3: The time trends in outcomes, defined as the fluctuations that occur in outcomes over time, cannot be too noisy away from the threshold. Specifically, if prior to the program implementation there were a lot of discontinuous changes in the outcome variable (i.e., a pattern that goes up and down frequently), it would be difficult to identify whether a discontinuous change at the threshold was reflecting the fact that the time trends were noisy, or whether the program was actually having an effect. This issue is best seen by examining Figure 2.1—the time trend illustrated slopes downward in a continuous manner and thus it is easy to identify a discontinuous change in the time trend at the threshold. For this reason, our figures included quarters of arrest up until two years before the implementation date so that we could better understand how noisy the time trends were. Note that our figures only include quarters of arrest up through the first year after implementation in order to highlight only the relevant data. Further, as described in more detail in Chapter 3, many other changes (including the pandemic and significant state-level policy changes in the criminal legal system) were implemented after the threshold, indicating that including data beyond the one-year time period would not be helpful in understanding the program effects.

Regression Specifications

To understand the effect Project Reset had on case outcomes and rearrest rates we estimated both a reduced form specification and an instrumental variables specification. As will be discussed in more detail below, the reduced form specification identifies the effect of the program on the set of individuals that passed the initial screening requirements (where many of these did not complete the program), whereas the instrumental variables specification identifies the effect of the program for just the set of individuals that completed the program. Note that while these models are estimating two (related) effects, these were both covered in the discussion of results in Chapter 6. In particular, an examination of the outcome figures by themselves corresponds to the reduced form specification. When we related the discontinuities in the outcomes at the threshold to the discontinuity in program completion rate at the threshold, that was inherently identifying what the effect of the program was for those who actually complete the program (i.e., the instrumental variables results).

Reduced Form Specification

The reduced form regression specification estimated the outcome figures presented in Chapter 6, where the key component of interest was on estimating the size of the jump in the outcome variable at the threshold. As noted above, the reduced form specification estimated the effect of program implementation for the set of individuals that passed the initial screening requirements. This effect will naturally be smaller than the effect the program has on those who complete diversion, because the reduced form effect averages the effect of the program on the completers with the zero effect the program will have on the set of individuals that pass the initial screening requirements but do not complete the program.

To identify the jump at the threshold accurately, it is important for the regression model to estimate the time trends well. Put another way, if the model does not account for time trends well, then what it might identify as a discontinuous jump at the threshold could just reflect time trends that were not properly accounted for. To account for time trends as accurately as possible, the regression discontinuity literature recommends using as narrow a band around the cutoff as possible, and using a flexible function (i.e., non-linear) to estimate the time trends (Hausman and Rapson, 2018). We thus only used data within a year of the cutoff on either side and estimated monthly time trends. Thus, to estimate the effect of the program for a given group, we estimated the following model using all individuals in that group that were arrested from one year prior to implementation to one year after implementation:

$$Outcome_i = \beta_0 + \beta_1 * Post_program_i + \beta_2 * Month_i + \beta_3 * Month_i^2 + \beta_4 * Month_i^3 + \beta_5 * X_i + \varepsilon_i \quad [1].$$

Outcome corresponds to the six outcome variables shown in Figures 6.2, 6.4, and 6.6, which were all defined as indicator variables. *Post_program* is an indicator for whether the individual was arrested after the program was implemented. The vector *X* includes controls for the individual's neighborhood of arrest (Uptown, Midtown, or Downtown), their race and ethnicity, gender, age, and their dominant offense charge type. Our analysis sample included individuals arrested over 24 months—the variable *Month* numbers these months in order from 1–24. We included a linear, quadratic, and cubic measure of this month variable to allow the model to estimate a flexible time trends function.

Specification [1] estimates the outcome figures presented in Chapter 6 (the figures examined outcome variation by quarter, but specification [1] uses monthly variation; the regression results do not change much if quarterly variation is used instead). The fact that the outcome figures in Chapter 6 controlled for the characteristics in *X* implies that the β_1 coefficient should measure the jump in outcomes at the threshold for these figures. Thus, β_1 identifies the reduced form effect of the program. When we attributed statistical significance to certain outcome results in Chapter 6, it was based off the statistical significance of the β_1 coefficient.

To estimate the effect of the program for only those that completed it, we used an *instrumental variables estimator*. Specification [1] estimates the effect of implementing the program across all individuals who passed the initial screening requirements, including those who did not complete the program (and thus contribute an effect size of zero). To estimate the effect of the program for only those that complete it (i.e., the treatment-on-the-treated effect) we estimated a slightly modified version of the earlier specification using an instrumental variables (IV) estimator:¹

$$Outcome_i = \alpha_0 + \alpha_1 * Diversion_i + \alpha_2 * Month_i + \alpha_3 * Month_i^2 + \alpha_4 * Month_i^3 + \alpha_5 * X_i + \varepsilon_i \quad [2].$$

The variable *Diversion* is an indicator for whether the individual completed diversion programming; this variable is instrumented by the indicator *Post_program* (defined earlier). All other variables are the same as described before. With this specification, α_1 identifies the causal effect of the program for those who complete the program. The IV estimator essentially takes the estimate of β_1 from [1] and divides it by the increase in the proportion of individuals at the threshold that complete the program. Intuitively, if the three requirements outlined in Chapter 2 hold, the only reason the outcome should change at the cutoff (i.e., $\beta_1 \neq 0$) is because some of these individuals completed diversion. To determine how much the program affects the average person that completes it, we divided the change in the outcome at the cutoff by the change in the completion of diversion programming at the cutoff. This is essentially what the IV estimator does in estimating α_1 .

¹ See Angrist and Pischke (2009) for more details on the instrumental variables method.

Additional Support for Research Design Requirements

In both Chapters 2 and 6, we highlighted that one important requirement of the regression discontinuity design is that there must be no other changes occurring simultaneously at the threshold. To verify the extent to which this requirement held, it was helpful to examine how the characteristics of those arrested in the year prior to implementation compared to those arrested in the year post-implementation. If no other changes besides the program occurred, one would expect the composition of the pre- and post- groups to look relatively similar with respect to the characteristics included in X . Table A.1 presents the results for each group separately. The results for Group A, B and C indicate the pre- and post-groups are remarkably similar.² In contrast, the pre- and post-groups looked somewhat different for Group D, indicating the research design likely did not work well for this group.

Another common consistency check run with regression discontinuity designs is to examine how the number of observations changes around the threshold. Specifically, if the number of observations changes discontinuously around the threshold (termed “bunching”) this indicates that another factor is changing discontinuously around the threshold. Figure A.1 presents figures examining how the number of observations changes over time for each group (where these observations correspond to the individuals that passed the initial screening requirements). The figures for Groups A, B, and C indicate the number of observations is relatively smooth around the threshold.

Regression Results

Table A.2 presents the results from both the reduced form specification and the instrumental variables specification for Groups A, B, C, and D. Note that, as was the case with the figures presented in Chapter 6, we did not present the full set of outcomes for Group D because the time trends were too noisy (likely because of the multitude of other changes occurring in the post-implementation period for Group D). Column 1 presents the results from estimating the reduced form specification—specification [1]—where each entry in the column corresponds to the β_1 coefficient for a given outcome. Thus, each entry in the table was generated from a separate regression. Analogously, Column 2 presents the estimates of α_1 from the instrumental variables specification—specification [2]—for each outcome.

As expected, the regression results confirmed the results presented in Chapter 6. For example, examining the IV results for Group B, the results indicate that for those that completed programming, the likelihood of receiving a DP increases by 96 percentage points, and their likelihood of receiving an ACD declines by 92 percentage points. This essentially represents that the effect of completing the program is a participant receiving an ACD to a participant receiving a DP—these same conclusions were presented in Chapter 6.

² One exception to this is that the results indicate petit larceny offenses are more prevalent in the post-period, which might have occurred if offense patterns or enforcement procedures changed over time. Regardless, we included a control for offense type in our regression model which adjusted for this effect.

TABLE A.1
Comparison of Group Characteristics Pre- and Post-Program Implementation

	Group A (%)		Group B (%)		Group C (%)		Group D (%)	
	Pre	Post	Pre	Post	Pre	Post	Pre	Post
Female	43.6	45.6	47.8	49.9	38.8	39.9	20.7	16.6
Male	56.4	54.4	52.2	50.1	61.2	60.0	79.3	83.4
White non-Hispanic	28.6	24.9	30.9	30.8	22.0	21.3	19.0	15.4
White-Hispanic	27.5	29.9	22.6	20.3	25.9	25.3	25.3	26.4
Black non-Hispanic	24.1	25.1	24.1	24.0	35.8	37.9	45.0	47.1
Black-Hispanic	10.4	9.8	4.5	4.5	8.9	8.1	7.9	8.8
Asian/Pacific Islander	7.7	7.5	14.9	17.1	5.2	5.9	2.1	1.4
Other/Unknown	1.7	2.9	3.0	3.3	2.2	1.5	0.6	1.0
Age 18–20	21.8	21.5	24.2	19.2	13.7	10.2	1.7	2.8
Age 21–24	25.8	20.3	22.5	19.2	18.3	17.2	4.9	5.4
Age 25–29	17.7	17.8	16.7	19.2	20.7	20.2	13.4	11.5
Age 30–39	13.5	15.7	18.7	19.0	23.0	25.0	28.8	27.8
Age 40–49	8.6	10.2	9.2	10.4	11.6	13.3	21.6	21.9
Age over 50	12.6	14.6	8.7	12.9	12.7	14.2	29.6	30.7
Arrested Downtown	0.0	0.0	48.3	51.1	32.3	29.1	27.1	20.1
Arrested Midtown	32.7	29.5	51.7	48.9	41.8	39.2	38.2	36.2
Arrested Uptown	67.3	70.5	0.0	0.0	25.9	31.7	34.8	43.7
Petit larceny	41.9	49.0	61.5	70.8	52.4	57.1	43.5	48.1
Possession of controlled substances	24.0	23.9	9.1	3.9	13.5	12.6	33.7	33.4
Trespassing	15.4	10.3	15.6	10.2	14.5	10.6	9.4	7.5
Criminal mischief	5.2	6.5	5.1	5.5	6.6	8.2	5.9	3.2
Graffiti	2.3	0.4	4.8	5.1	4.9	3.4	1.6	1.6
Other eligible offenses	11.2	9.8	3.9	4.4	8.0	8.0	5.9	6.3
Prior felony arrest					26.3	24.8		
Prior felony conviction							38.8	46.4
Observations	651	522	1,528	1,124	1,183	973	1,401	1,848

FIGURE A.1
Number of Individuals in Each Group Who Passed Initial Screening Requirements by Quarter of Arrest



NOTE: For each figure, quarter 0 corresponds to the quarter in which Project Reset was implemented for that group. All other quarters are numbered in reference to the implementation quarter. For individuals arrested in a given quarter, the figure presents the number of individuals who passed initial screening requirements.

TABLE A.2
Regression Discontinuity Estimates for Groups A, B, C, and D

		Reduced Form Results Specification (1)	Instrumental Variables Results Specification (2)
Group A (1,173 Observations)	ACD	-.354 (.076)***	-.777 (.149)***
	DP	.420 (.061)***	.922 (.079)***
	Any conviction	-.008 (.049)	-.017 (.106)
	Conviction of criminal offense	-.018 (.015)	-.040 (.034)
	Any rearrest in 12 months	.0001 (.051)	.0003 (.111)
	Any felony rearrest in 12 months	-.007 (.033)	-.016 (.071)
Group B (2,652 Observations)	ACD	-.442 (.048)***	-.915 (.090)***
	DP	.466 (.036)***	.963 (.032)***
	Conviction of violation/infraction	-.012 (.029)	-.024 (.059)
	Conviction of criminal offense	-.003 (.009)	-.007 (.018)
	Any rearrest in 12 months	-.011 (.029)	-.023 (.059)
	Any felony rearrest in 12 months	.008 (.017)	.017 (.036)
Group C (2,151 Observations)	ACD	-.235 (.060)***	-2.89 (1.28)**
	DP	.117 (.041)***	1.44 (.361)***
	Conviction of violation/infraction	.151 (.049)***	1.85 (1.11)*
	Conviction of criminal offense	-.020 (.016)	-.244 (.222)
	Any rearrest in 12 months	.089 (.055)	1.09 (.883)
	Any felony rearrest in 12 months	.040 (.039)	.487 (.538)
Group D (3,246 Observations)	ACD	-.040 (.034)	-.478 (.404)
	DP	.101 (.035)***	1.19 (.375)***

NOTE: Each estimate in Column 1 corresponds to the β_1 coefficient from specification [1], where a separate Ordinary Least Square (OLS) regression was run for each outcome. Each estimate in Column 2 corresponds to the α_1 coefficient from specification [2], where a separate two-stage least squares regression was run for each outcome. The F-stat from the first-stage regression for Column 2 for Group A is 67; for Group B it is 206; for Group C it is 4.9, and for Group D it is 15.6. Asterisks indicate whether the β_1 or α_1 coefficient was statistically different than 0 in specification 1 and 2, respectively. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Supplemental Figures and Tables

TABLE B.1
Participant Understanding of Case and Diversion Option

	Strongly Disagree	Somewhat Disagree	Neither Agree or Disagree	Somewhat Agree	Strongly Agree	Total
The police officer clearly explained my charges at arrest.	15 (15.2%)	13 (13.1%)	11 (11.1%)	23 (23.2%)	37 (37.4%)	99
I understood the option of early diversion that was given to me.	13 (13.1%)	5 (5.1%)	13 (13.1%)	23 (23.2%)	45 (45.5%)	99
I understood my other options if I did not participate in Project Reset.	6 (6.2%)	5 (5.2%)	9 (9.3%)	19 (19.6%)	58 (59.8%)	97
I understand that Project Reset is different from how cases are normally handled.	1 (1.0%)	0 (0.0%)	5 (5.2%)	15 (15.5%)	76 (78.4%)	97

TABLE B.2
Overall Program Satisfaction

	Strongly Disagree	Somewhat Disagree	Neither Agree or Disagree	Somewhat Agree	Strongly Agree	Total
CCI (Original)	1 (1.5%)	0 (0%)	1 (1.5%)	4 (6.2%)	59 (90.8%)	65
CCI (Expanded)	0 (0%)	0 (0%)	1 (4.5%)	2 (9.1%)	19 (86.4%)	22
Osborne Association (Original)	1 (6.3%)	0 (0%)	0 (0%)	1 (6.3%)	14 (87.5%)	16
YNY (Original)	0 (0%)	0 (0%)	0 (0%)	0 (0%)	3 (100%)	3
Total	2 (1.9%)	0 (0%)	2 (1.9%)	7 (6.6%)	95 (89.6%)	106

TABLE B.3
Program Satisfaction Sub-Items

	Strongly Disagree	Somewhat Disagree	Neither Agree or Disagree	Somewhat Agree	Strongly Agree	Total
The program staff were helpful.	4 (3.9%)	1 (1.0%)	1 (1.0%)	5 (4.9%)	92 (89.3%)	103
The program is only helpful for getting out of other punishment.	30 (28.8%)	19 (18.3%)	18 (17.3%)	8 (7.7%)	29 (27.9%)	104
I would recommend Project Reset to someone in a similar position as me.	3 (2.9%)	1 (1.0%)	2 (1.9%)	2 (1.9%)	97 (92.4%)	105
Project Reset resolved my case quickly.	2 (1.9%)	0 (0.0%)	4 (3.8%)	5 (4.8%)	93 (89.4%)	104
I learned useful information about the legal system from the program.	4 (3.8%)	1 (1.0%)	10 (9.5%)	26 (24.8%)	64 (61%)	105
I made the right decision by participating in Project Reset.	3 (2.9%)	0 (0.0%)	3 (2.9%)	3 (2.9%)	96 (91.4%)	105

TABLE B.4
Participant Assessment of Whether Referrals for Additional Services Were Helpful

	Strongly Disagree	Somewhat Disagree	Neither Agree or Disagree	Somewhat Agree	Strongly Agree	Total
CCI (Original Eligibility)	0 (0.0%)	0 (0.0%)	14 (29.8%)	11 (23.4%)	22 (46.8%)	47
CCI (Expanded Eligibility)	2 (15.4%)	1 (7.7%)	3 (23.1%)	3 (23.1%)	4 (30.8%)	13
Osborne Association (Original Eligibility)	1 (9.1%)	0 (0.0%)	2 (18.2%)	5 (45.5%)	3 (27.3%)	11
YNY (Original Eligibility)	0 (0.0%)	0 (0.0%)	0 (0.0%)	0 (0.0%)	2 (100.0%)	2
Total	3 (4.1%)	1 (1.4%)	19 (26.0%)	19 (26.0%)	31 (42.5%)	73

TABLE B.5
Overall Thoughts and Feelings About the Justice System

	Strongly Disagree	Somewhat Disagree	Neither Agree or Disagree	Somewhat Agree	Strongly Agree	Total
The law represents the values of the people in power, rather than the values of people like me.	11 (10.8%)	12 (11.8%)	21 (20.6%)	31 (30.4%)	27 (26.5%)	102
People in power use the law to try to control people like me.	6 (5.9%)	5 (5.0%)	26 (25.7%)	38 (37.6%)	26 (25.7%)	101
The law does not protect my interests.	6 (5.9%)	17 (16.7%)	33 (32.4%)	26 (25.5%)	20 (19.6%)	102

TABLE B.6
Thoughts and Feelings About the Courts

	Strongly Disagree	Somewhat Disagree	Neither Agree or Disagree	Somewhat Agree	Strongly Agree	Total
More favorable opinion of the Court System after program	5 (4.8%)	7 (6.7%)	23 (21.9%)	24 (22.9%)	46 (43.8%)	105
Judges put people in jail for no good reason.	14 (13.9%)	19 (18.8%)	24 (23.8%)	30 (29.7%)	14 (13.9%)	101
Judges make decisions based upon their prejudices or personal opinions.	11 (10.9%)	13 (12.9%)	31 (30.7%)	30 (29.7%)	16 (15.8%)	101

TABLE B.7
Specific Perceptions of the Court System

	Never	Some of the Time	Most of the Time	Always	Total
Make fair and impartial decisions in the cases they deal with.	3 (3.0%)	58 (57.4%)	29 (28.7%)	11 (10.4%)	101
Give people a chance to tell their side of the story before they decide what to do.	1 (1.0%)	49 (48.5%)	36 (35.6%)	15 (14.9%)	101
Make decisions based upon the law and not their personal biases or opinions.	2 (20%)	48 (48.0%)	37 (37.0%)	13 (13.0%)	100
Treat people with dignity and respect.	5 (5.0%)	49 (48.5%)	35 (34.7%)	12 (11.9%)	101

Respect people's rights. 6 (5.9%) 42 (41.6%) 34 (34.7%) 19 (18.8%) 101

TABLE B.8
Thoughts and Feelings About the Police

	Strongly Disagree	Somewhat Disagree	Neither Agree or Disagree	Somewhat Agree	Strongly Agree	Total
More favorable opinion of police after the program	14 (13.5%)	7 (6.7%)	24 (23.1%)	24 (23.1%)	35 (33.7%)	104
The police only care about the views of some of the people in my community.	11 (10.9%)	12 (11.9%)	28 (27.7%)	30 (29.7%)	20 (19.8%)	101
I generally support how the police act in my community.	31 (30.4%)	10 (9.8%)	22 (21.6%)	22 (21.6%)	17 (16.7%)	102
When the police deal with people they almost always behave according to the law.	24 (23.8%)	23 (22.8%)	17 (16.8%)	27 (26.7%)	10 (9.9%)	101

TABLE B.9
Perceptions of the Police

	Never	Some of the Time	Most of the Time	Always	Total
Make fair and impartial decisions in the cases they deal with.	11 (10.9%)	54 (53.5%)	26 (25.7%)	10 (9.9%)	101
Give people a chance to tell their side of the story before they decide what to do.	19 (18.6%)	49 (48%)	20 (19.6%)	14 (13.7%)	102
Make decisions based upon the law and not their personal biases or opinions.	11 (10.9%)	50 (49.5%)	29 (28.7%)	11 (10.9%)	101
Treat people with dignity and respect.	9 (8.8%)	56 (54.9%)	22 (21.6%)	15 (14.7%)	102
Respect people's rights.	13 (12.7%)	51 (50%)	20 (19.6%)	18 (17.6%)	102

TABLE B.10
Comparison of Individuals Who Passed the Initial Screening Requirements by Program Referral Status

	All Individuals that Passed Initial Screening Requirements (%)	All Individuals that Passed Initial Screening Requirements Whose:	
		Cases Not Referred to Reset (%)	Cases Referred to Reset (%)
	(1)	(2)	(3)
Gender			
Female	32.7	23.2	40.1
Male	67.3	76.8	59.8
Race/Ethnicity			
White non-Hispanic	21.8	19.5	23.6
White Hispanic	25.1	26.8	23.8
Black non-Hispanic	36.5	39.7	34.0

Table B.10—Continued

	All Individuals that Passed Initial Screening Requirements (%)	All Individuals that Passed Initial Screening Requirements Whose:	
		Cases Not Referred to Reset (%)	Cases Referred to Reset (%)
	(1)	(2)	(3)
Black Hispanic	8.2	9.1	7.4
Asian/Pacific Islander	6.8	3.5	9.4
Other/Unknown	1.5	1.3	1.7
Age			
18–20	13.1	9.5	15.9
21–24	14.3	12.9	15.4
25–29	16.3	15.4	17.0
30–39	23.3	25.3	21.8
40–49	14.6	16.6	13.1
50 and above	18.4	20.3	16.9
Criminal History			
No Prior Arrests	42.9	27.8	54.7
Prior Misdemeanor Arrest	14.8	16.0	13.9
Prior Felony Arrest	9.2	13.2	6.1
Prior Conviction	33.0	42.9	25.3
Dominant Offense Charge Type			
Petit larceny	48.2	22.0	68.6
Possession of controlled substances	19.4	38.6	4.4
Trespassing	10.2	11.3	9.3
Criminal mischief	4.5	8.2	1.7
Graffiti	2.5	1.9	3.0
Other eligible offenses	15.2	18.0	13.0
Neighborhood of Arrest			
Downtown	24.4	20.5	27.5
Midtown	39.0	30.4	45.6
Uptown	36.6	49.1	26.9
Year of Arrest			
2018	22.1	13.5	28.7
2019	40.1	43.2	37.7
2020	35.0	39.7	31.3
2021	2.8	3.6	2.3
Observations	8,556	3,740	4,816

NOTE: All analyses were run using individuals who passed the initial screening requirements and were arrested after Project Reset had been implemented for individuals with their eligibility criteria. For this sample of individuals, we observed who was actually referred for programming and thus we could compare who passed the initial screening criteria with who was ultimately referred.

Supplemental Information for Cost Analysis

This appendix provides information on how the numbers presented in Tables 8.1 through 8.4 were obtained from the source material (which included program invoices, staff interviews, and prior cost analysis literature of diversion programming). With the exception of the information obtained from prior literature, all other information obtained corresponded to the period between July 2018 and June 2019.

Many of the cost calculations presented in this appendix required information on the hourly wage rates of individuals. As we were typically provided annual wage rates, all hourly wages were calculated as follows: (1) we summed the direct annual wage paid to a staff member with the fringe benefits associated with that salary (which was a percentage of the salary), and (2) we divided that total amount by 52 to get the weekly wage rate and then divided that amount by 40 to obtain the hourly wage rate.

Program Costs

DANY Screening Costs

In Table 8.1, we noted the annual amount of money DANY spent on screening cases for eligibility for Project Reset was \$37,455.08. Table C.1 below provides more detail on how that estimate was determined. DANY provided information on the staff positions that screened cases, the amount of time these individuals spent per week, and the annual wage rate of these individuals. Table C.1 shows how that information was used to determine the total amount spent per year on screening cases.

Program Delivery Costs

In Tables 8.1 and 8.2 we provided line items of the costs of program delivery. These costs were obtained from the invoices providers filed. However, because the costs we included were only a subset of the costs providers invoiced, Table C.2 documents the specific costs listed on the invoices that were included for each of the three providers.

As noted in the cost analysis framework presented in Chapter 8, the only program costs we accounted for are the costs involved in program delivery. We relied on our interview notes and some follow-up conversations with providers to determine the specific costs listed on the invoice that would be involved in program delivery. This included labor costs for staff members who conducted outreach or intake or were involved in

TABLE C.1
DANY Screening Costs

Staff Position	Hours per Week	Hourly Wage Rate (Including Fringe)	Weekly Cost	Annual Cost
Program Coordinator	18.96	\$37.99	\$720.29	\$37,455.08

TABLE C.2
Included Provider Costs

Provider	Labor Costs	Other Costs
CCI	Intake Specialist (~2.5 FTE)	Transit (Metro cards)
	Social Worker (1 FTE)	Food
	Manager of Diversion Programs (1 FTE)	
	Fringe (32% of salaries)	
Osborne Association	Diversion Specialist (~3.5 FTE)	Columbia University subcontract (for program delivery)
	Administrative Assistant (1 FTE)	Transit (Metro cards)
	Fringe (35% of salaries)	Food Program supplies
Young New Yorkers	Program Coordinator/Lead Teacher (~0.4 FTE)	Transit (Metro cards)
	Program Assistant/Peer Mentor (~0.7 FTE)	Food
	Program Assistant/Peer Mentor (contract position)	Art supplies and equipment
	Program Coordinator/Lead Teacher (contract position)	
	Teaching Artist and co-Teacher (contract position)	
	Credible Messenger and co-Teacher (contract position)	
	Fringe (33% of salaries for non-contract staff)	

NOTE: FTE = full-time-equivalent.

providing programming. It also included funds spent on food, metro cards, and program supplies. We did not include labor costs for staff members who were involved in developing curriculum or the Salesforce software, staff that supervised those providing programming, staff that were responsible for high-level coordination (such as with DANY), or staff that were research associates. We also did not include funds spent on rent or on general office supplies. All of these excluded costs were either involved in the program setup or were factors not observed for the court pathway. Note that different providers labeled positions differently and thus the included staff positions vary across providers.

Relevant costs were obtained from five invoices from each of the three providers. The time period covered by these invoices was from June 1, 2018, through August 31, 2019, a period of 15 months. To ensure we only included costs that occurred between July 1, 2018 and June 30, 2019, we summed up the relevant totals across all five invoices (for a given provider) and then multiplied by 12/15 to get an estimate of the amount that would have been spent during the focal year we are examining.

Program Costs Under Alternative Scenarios

Table 8.4 presents estimates of the per-person cost for programming under alternative scenarios. The total cost-per-participant presented for CCI in Table 8.2 shows what the cost-per-person would be if CCI costs were used.

To identify the cost-per-person of programming if all outreach had been successful, we identified the number of individuals over this time period who had been referred for programming but could not be reached by the providers—this was an additional 734 individuals. To determine what the cost-per-person would have been if those 734 individuals had been serviced with existing staff, we divided the total cost outlay identified in Table 8.1 by this expanded number of participants: $\$869,736.01 / (1,001 + 734) = \501.29 .

Court Pathway Costs

Prosecution Costs

Table 8.3 states the cost for DANY's time spent on prosecuting a case is \$199.59. Table C.3 shows how this estimate was calculated. DANY provided information on all staff positions involved in adjudicating a three-hearing ACD, the time each staff position spent on a case, and the hourly wage rate for each of these staff positions. Table C.3 shows how this information was used to generate the total amount DANY spends on adjudicating a case.

Public Defender Costs

Table 8.3 states the cost for public defense services for a case is \$188.62. To determine this estimate we spoke with an attorney that handles these cases at Legal Aid, which is one of several public defense organizations that services these cases. The attorney provided information on the typical number of hours spent on a three-hearing ACD case and their hourly wage rate. Information on the fringe benefit rate was not provided, so we used an estimate of 42 percent. We chose this estimate because it was about midway between the fringe rates used by providers (around 35 percent) and the fringe rates used by DANY (49 percent). Table C.4 shows how this information was used to arrive at the final per-case amount spent by public defenders.

TABLE C.3
Amount Spent by DANY per Case

Staff Position	Hours-per-Case	Hourly Wage Rate (Including Fringe) (\$)	Total Amount Spent on a Case (\$)
ECAB DAT associate	0.06	30.77	1.85
Complaint room staff	0.05	28.18	1.41
Expeditors	0.10	42.69	4.27
Court part specialist	0.04	26.84	1.07
ECAB paralegal	0.75	31.90	23.93
Scanning unit	0.03	29.08	0.73
Assistant district attorney	2.00	83.17	166.34
Combined			\$199.59

ECAB = Early Case Assessment Bureau.

TABLE C.4
Amount Spent by Public Defender per Case

Staff Position	Hours-per-Case	Hourly Wage Rate	Total Amount Spent on a Case (\$)
Public Defender	2.25	\$83.83	\$188.62

Court Costs

Table 8.3 states the cost for court staff for a case to be \$173.89. We were not able to speak with anyone at the court that handles these cases and thus had to determine estimates of these costs from prior cost analyses that have been conducted on diversion programming. Rempel et al. (2018) identifies the court costs for a similar population and use the same methodology we do in identifying costs. Namely, they identified the staff positions involved in adjudicating these cases and the time each of these positions spent on a given case. These estimates are presented in Table C.5. We determined the hourly wage rate for each of the three positions using publicly available information on court staff wages. As we did not observe the fringe benefit rate, we again used our estimated rate of 42 percent. Table C.5 shows how this information was used to arrive at the final per-case amount spent by court staff.

Programming Costs

Table 8.3 states the cost of providing programming to individuals required to complete it as part of their ACD is \$4.16. The data indicate that only 0.5 percent of those who were in our matched control sample (described in Chapter 8) were required to complete programming as part of their ACD, and the programs required were either Program Stoplift or Program Youthgroup. As we were not able to obtain information on how much these programs cost, we proxied for this using the average cost-per-person of Project Reset. The average cost-per-person of Project Reset was determined by adding the amount spent by all three providers (shown in Table 8.1) and then dividing by the 1,001 participants served—this resulted in a per-person cost of \$831.45. Because only 0.5 percent of individuals that receive an ACD in the court process are required to complete this programming, the cost for programming for the average person is $0.005 * \$831.45 = \4.16 .

Community Service Costs

We were not able to speak with anyone involved in community service provision, nor were we able to find credible estimates of the cost of this service in the literature. We thus made credible assumptions based on what we knew about the program, which is that it requires individuals to do park cleanup. We assumed that the average individual assigned community service was required to complete 10 hours of park clean-up, and that staff spent 10 minutes coordinating these sessions with individuals and 0.67 hours supervising these individuals. The latter was obtained by assuming that one staff member could supervise 15 people at a time; thus, the staff members' 10 hours of supervision would be split across 15 people, which resulted in effectively 0.67 hours spent on each individual. These estimates indicate that a staff member spends a total of 0.83 hours on an individual which, assuming a wage rate of \$30 per hour, would mean the per-person cost of community service is \$25. Note that because only 42 percent of individuals with an ACD are assigned community service, the cost of community service for the average individual with an ACD is $0.42 * \$25 = \10.50 .

TABLE C.5
Amount Spent by Court per Case

Staff Position	Hours-per-Case	Hourly Wage Rate	Total Amount Spent on a Case (\$)
Judge	0.33	\$124.00	\$40.92
Court clerk/reporter	2.00	\$65.80	\$131.60
Bailiff	0.07	\$19.61	\$1.37
Combined			\$173.89

Qualitative Interview Protocols

Staff Semi-Structured Interview Guide

Estimated length: 45–50 minutes. Note: Interview guide is designed to be used with staff from all stakeholder agencies. Specific questions may be omitted if determined to be not relevant for a given party. Note that because a given party will not be asked all of these sets of questions, the sum of the projected time for each of these sets of questions exceeds the estimated interview time length of 45-50 minutes for a given party.

Background (3 Minutes)

1. To start us out, can you tell me about your role in Project Reset?

Nature of the Program (25 Minutes)

Next, I have some questions to better understand how participants move through the program and the types of services they receive.

1. What are the eligibility criteria for the program?
 - a. Can you discuss the set of individuals you start with (i.e. all individuals with a DAT) and walk us through the specific screening criteria (e.g., with a first-time arrest for a minor crime) that are used to determine who is diversion-eligible, for both the original and the expanded populations? Is all of the information necessary to determine whether someone is diversion-eligible housed with DANY, or are data from another agency necessary?
 - b. Who is involved in the process of identifying potential participants? (Potential probes: NYPD, DANY, agencies)
 - c. Does DANY ever dismiss a case before making the decision on whether the case is diversion-eligible?
 - d. How is it determined if an individual is low risk?
 - e. Have there been any changes in these criteria over time – and if so, what? (PROBE on expansion)
 - f. During COVID, how have changes in the legal system (e.g., amount of police citations, court room activity) affected eligibility?
 - g. Have there been changes over time in how Desk Appearance Tickets (DAT) have been handed out, or in the enforcement of low-level crimes that are diversion-eligible? (PROBE broader law/policy changes)
 - h. How is it decided whether an individual will receive a DAT for their arrest, and what criteria do officers typically consider?

2. We're interested in learning how participants flow through the program. We can begin with identification and outreach.
 - a. When do you learn about potential participants?
 - b. What information does DANY include in the Salesforce database (which is how DANY communicates with the agencies about who is eligible)?
 - c. Who conducts outreach to those potential participants? How is outreach conducted?
 - d. What agency first tells the participant about the diversion program?
 - i. Is this typically successful? Why/why not?
 - e. How has the coronavirus impacted recruitment for the program?
 - i. If you offer a remote or online option, how commonly are potential program participants interested in doing so? How commonly do limitations (e.g., having reliable online access) prevent such participation?
 - f. What is the timeline between when you learn of a potential participant and when you contact them? What is the timeline for when the individual has to agree to the program, and complete the program, and how does this relate to their scheduled court date?
3. What types of intake procedures do you have for participants who decide to participate in the program? How long does the intake typically last?
 - a. Potential probes: Types of assessments used, what specific information is collected at intake and whether this is recorded in their data.
 - b. Approximately how long between acceptance/enrollment and the program date?
 - c. How do you do intake currently due to the coronavirus?
4. Once an individual is enrolled, what types of services are provided?
 - a. Potential probes: specific courses/services offered, length of program, location of program and whether they record the specific program attended.
 - b. How has the coronavirus altered service delivery?
 - i. How have you changed the program requirements?
 - ii. For remote/online service delivery, how commonly can participants successfully do the program this way? How common is technological failure? How common are home distractions? How comparable is the remote version to the in-person version?
 - c. Probe: differences between YA and adult groups
 - d. Who developed the original vision/requirements for the program?

For internal reference, these are the components listed in the original material we received about the programs:

- CCI: Early diversion workshop; Individual counseling session, New Museum workshop, Reset Plus workshop
 - Osborne Association: Social Resilience Model Workshop or Restorative Justice Works Leadership Training or Narcan/Naloxone Treatment Training or Community Benefit Projects
 - Young New Yorkers: Arts-Based Restorative Justice Intervention
5. How many individuals typically participate in a given program session? During COVID, how many currently participate in a given session?
 6. Are all program elements you described required? If not, how are the specific elements decided? (e.g., by the program, by the participant, some combination)

7. Do participants receive referrals for other services? If so, what types of services?
 - a. Are referral services mandatory? Do you track participation in referred services? How often are referrals given? How often are they followed up on? How often are those services offered by the APR provider vs. another partner organization?
8. How much contact do they have with the court (and related actors) throughout this process? (PROBE: DANY, Public Defenders, Courthouse Staff, Others?)
9. What happens when someone completes the program services? (Potential probes: Future contact with participant; status of legal charges)
10. In what types of circumstances do individuals not complete the program (e.g., enroll but do not attend services, leave partway through a program)? What happens when a participant does not complete the program (e.g., to legal case)? Is there a formal/informal process to make-up a workshop? How has the coronavirus impacted program completion?
11. Are individuals charged a fee to participate in the program?
12. What information does your agency record in the Salesforce database (for both individuals that complete the program, as well as those that could not be contacted)? How can the Salesforce data be matched to DANY data—would one need to match on name, date of birth, etc.?
13. What other ways has the coronavirus affected your ability to participate in Project Reset?

Communication (5 Minutes)

I want to talk briefly about the other organizations you may be in contact with as part of delivering this program.

1. What different agencies do participants or potential participants interact with as part of the program?
2. What stakeholders do you communicate with to implement this program (i.e., district attorneys, public defenders)?
 - a. What type of communication do you have with these agencies/organizations? (Potential probes: discussing specific cases, submitting data, obtaining procedural/policy information about the program)
 - b. How often do you communicate with them?

Perceptions of the Program (15 minutes)

1. What factors seem to contribute to whether an eligible defendant is successfully contacted by diversion program staff? How has the coronavirus altered this process?
2. While we understand that the vast majority of successfully contacted defendants agree to participate, what factors do you think shape someone's decision to not participate if they have been contacted? What factors do you think shape someone's agreement to participate? What impact do you think the coronavirus has had?
3. What are the benefits of participating in this program?
 - a. How well do you think the program meets the needs of the participants you serve?
 - b. Do you think that this program is effective, and in what way?
4. What has facilitated the implementation of this program? (Potential probes: Collaborations with the other agencies; training; other resources)

5. What challenges have been experienced in implementing this program? (Potential probes: Identifying and engaging potential participants; resources; staff needs; coronavirus)
6. Are participants consistently matched with their preferred workshop? Why/why not?
7. Given the chance, what changes would you make to the program?
8. Are there additional resources that you need to implement the program?

The Court Process (15 Minutes)

Next, I have some questions to better understand the process for participants that have their case resolved in court.

1. How many hearings are typically involved to resolve a case like this, presuming an individual shows up at all required court hearings? How long after arrest will the first hearing typically take place? What is the typical resolution time for cases like these?
2. Is a public defender provided to the defendant at their first hearing? What Public Defender agencies are assigned to these cases? How are they selected? Are some more effective than others (e.g., at contacting folks)?
3. What happens if the individual does not show up for court?
4. How has the coronavirus's changes to court functioning impacted diversion to Project Reset?
5. Will individuals that were diversion-eligible but could not be contacted about the program ever know that it existed? (i.e. would they be told about it at the court hearing and given the opportunity to participate?)
6. How much time are prosecutors spending on a given case before the first court hearing, and what are they deciding on?
7. Are prosecutors typically offering a plea deal at the first court hearing? Will defendants typically accept the offer?
8. If you complete the diversion program for your first arrest and then get arrested again, how is your first diversion-eligible arrest handled? Will that automatically count as a prior conviction?
9. What are the typical fines and fees associated with the possible case dispositions?
10. What is the set of all cases that prosecutors that handle these diversion-eligible cases take? (i.e., will they handle all misdemeanor cases, do they handle felonies, etc.)

Program Cost (15 Minutes)

1. What were the initial set-up costs your agency encountered and what was this money spent on? (These would be one-time costs that cannot be recovered—i.e. Salesforce database, etc.)
2. For a given year of program provision, break down all the costs of program provision, and how much each activity costs.
 - a. What part of these costs are fixed? (i.e. regardless of the number of people participating, they must spend this money) What is the time length for these fixed costs? (i.e. if they rent building space, do they do this month-to-month or annually? Are employees hired on annual contracts?)
 - b. What components of the cost are marginal? (i.e. they depend on the number of people participating.)
 - c. Has coronavirus affected costs?
3. With the fixed costs already spent, how many more individuals could your program serve in a given year?

Court/Prosecutor/Defense Cost (15 Minutes)

1. How much total time does the prosecutor spend on a given case, including time in court?
2. What is the wage rate and overhead rate for the average prosecutor that handles these cases?
3. What court officers spend time on the processing of these cases? What tasks are involved, how much time is spent, and what are the wage and overhead rates for the average individuals in these respective positions?
4. How much time do public defenders spend on these cases, and what percentage of their caseload is this? What are the wage and overhead rates for the average public defender handling these cases?
5. How much does it cost to incarcerate someone in jail for one day? How much does it cost to supervise them on probation for one day?

DANY Evaluation—Project Reset Participant Interview Draft

Estimated length: 45 minutes

Experience with Program Outreach (10 Minutes)

1. When did you complete Project Reset?
2. How did you find out about the program? (Potential probes: Program staff, NYPD, attorney)
 - a. (For those who learned of it from the program staff) Did you have any questions as to whether this was a legitimate option for resolving your case?
3. What are some of the reasons why you decided to participate in this program? (Potential probes: More favorable case outcome, more timely case resolution, interested in the services)
 - a. What were you hoping to get out of the program?
4. Did you have any questions about your participation in this program and if it was a good option? Who did you talk to about those questions?
5. Did you have a choice as to the type of workshop you wanted to complete? (If yes) Why did you choose the option you did?

Experience with Program Participation (15 Minutes)

I'd like to ask a few questions about the workshop you completed Project Reset.

1. What did you find helpful about the workshop you completed? (Potential probes: Program material, connection with an organization that offers other services, connection with other participants)
2. Were there parts of the workshop you found less helpful? Why?
3. If you had the chance to redesign the program, what would you change?
 - a. Is there anything missing from the current program, and if so, what?
4. Did you receive any referrals for other services as part of your participation? (If yes) Have you used those services? Why or why not?
5. What happened after you completed the program? (Potential probes: Additional communication with the program, receiving the decline to prosecute)

Experience with Program Stakeholders (10 Minutes)

Next, we want to learn more about your experience working with the different organizations involved in the program.

1. What was your experience like with the officer who arrested you? (Probes: Respectful; explained the program; trusted to provide contact information)
2. Have you been in contact with the District Attorney's office? What has your experience been like with them?
3. Have you been in contact with a defense attorney? What was your experience like with them?
4. What was your experience like working with the staff of [organization delivering program] – from outreach through to completing the program?

Barriers, Facilitators, and Impact

1. Were there any challenges to participating? What were they? (Potential probes: Location, timing)
2. What factors made it easier to complete this program? (Potential probes: Location, timing, length, did not interfere with work)
3. What are the overall benefits of this program? (Potential probes: Immediate benefits, such as useful information learned; longer-term benefits, such as dismissal of case)

Abbreviations

ACD	adjournment in contemplation of dismissal
CCI	Center for Court Innovation
CJII	Criminal Justice Investment Initiative
COVID-19	coronavirus 2019
CUNY	City University of New York
DANY	Manhattan District Attorney's Office
DAT	desk appearance ticket
DCJS	Division of Criminal Justice Services
DP	Decline to Prosecute
FBI	Federal Bureau of Investigations
FTE	full-time equivalent
IRB	Institutional Review Board
ISLG	Institute for State and Local Governance
IV	instrumental variables
MJO	Manhattan Justice Opportunities
NYPD	New York Police Department
OLS	Ordinary Least Square
RAP	record of arrests and prosecutions
RFP	request for proposals
RNR	Risk, Need, Responsivity
SD	standard deviation
YNY	Young New Yorkers

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