PURSUING PRETRIAL JUSTICE THROUGH AN ALTERNATIVE TO BAIL

Findings from an Evaluation of New York City's Supervised Release Program

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BUILDING KNOWLEDGE TO IMPROVE SOCIAL POLICY

This report presents the findings of MDRC's implementation and impact evaluation of New York City's Pretrial Supervised Release Program (the "Program"), which was designed to provide judges with the option of releasing some defendants to their communities under supervision instead of setting bail. Using a combination of regular check-ins with case managers and referrals to various services calibrated to the needs of the clients, the Program substantially decreased the use of money bail and pretrial detention while at the same time ensuring high court appearance rates. Although Supervised Release was not designed as a program to reduce re-arrest — because New York State's bail law largely forbids judges from considering public safety in setting conditions of release — the evaluation shows that the Program also resulted in low re-arrest rates for defendants.

The City worked with the NYC Criminal Justice Agency (CJA) in 2009 to pilot the Supervised Release Program in Queens to provide judges in the borough with an alternative to releasing defendants charged with non-violent felonies on their own recognizance, setting bail, or remanding them. The Program released defendants under the supervision of case managers at CJA, with whom defendants were required to check-in on a regular basis to ensure they appeared in court pending the resolution of their cases. The City expanded the Program citywide in 2016 following the success of the initial Queens pilot — along with similar successes in additional pilots in Manhattan[1] and Brooklyn — and in consultation with national experts and court stakeholders. The Mayor's Office of Criminal Justice (MOCJ) oversees the citywide Program operations. Client services in each borough are provided by three non-profit organizations that specialize in social services and alternatives to detention and incarceration. The Program initially served low- to medium-high risk defendants charged with non-violent felonies and misdemeanors. Supervised Release later became an option for all defendants with pending cases in the City, a change that was made following the historic bail reform legislation implemented by New York State in January 2020.

In 2016, the City engaged MDRC to conduct an evaluation of the Program to assess program implementation citywide and measure program impact on appearance rates, arrest rates, and overall case outcomes for its clients. To answer these questions, MDRC implemented a mixed-method approach that involved the review of program documents and interviews with prosecutors, defenders, judges, defendants, and providers as well as observation of courtroom proceedings. In addition, the MDRC researchers collected and analyzed administrative and programmatic data from New York City and State agencies as well as from Supervised Release providers. The results show that the Program:

- Substantially reduced the use of money bail and pretrial detention for those who were Program eligible
- Maintained high court appearance rates, even though those clients spent nearly twice as long in their communities pretrial as those in the comparison group
- Maintained low re-arrest rates for Program clients as compared to similar defendants

Overall, these findings suggest that the Supervised Release Program is a promising strategy for reducing the City's use of pretrial detention, while ensuring the maintenance of public safety and attendance in court.

The evaluation also found that the Program was successful in enrolling its target population: defendants who likely would have received bail pretrial in the absence of the Program.

The evaluation also identified some important challenges around Program implementation. These included case managers with heavy caseloads, issues with case managers balancing their social work and case-monitoring responsibilities, and issues dealing with difficult client cases. However, interviews with clients suggest clients were very satisfied with the Program, especially since it gave them a chance to avoid money bail and allowed them to spend their pretrial period living in their communities. In addition, judges approved of the Program's focus on clients' social service needs, which Supervise Release emphasized relative to other pretrial supervision programs across the country. These findings were helpful as MOCJ developed an expanded program in 2019, in which some of these issues were addressed, including adding staff to lower caseloads and hiring specialized staff for harder-to-reach populations.

These findings are particularly encouraging for MOCJ, demonstrating the success of one part of the City's strategy to improve public safety and promote fairness while reducing unnecessary arrests and incarceration. In addition to the importance of achieving these goals as a matter of fundamental fairness and decency, the City has also committed to replacing the dilapidated jails on Rikers Island with smaller, safer, and modern borough-based facilities. This plan anticipates that programs like Supervised Release and other approaches to reducing both crime and incarceration will ensure that the City continues to shrink the footprint of the criminal justice system in the lives of New Yorkers.

As always, the City faces challenges ahead. The COVID-19 pandemic and the brutal killing of George Floyd have distilled those issues for us and strengthened our resolve to build a safer and fairer city. We have worked to ignite a virtuous cycle in which we look beyond the criminal justice system apparatus to ensure that New Yorkers are able to find the path towards a productive life, continuing an iterative shrinking of the touch of enforcement as the mechanism that keeps each of us safe. The Supervised Release Program is an important part of this effort to increase the well-being of all New Yorkers. We hope it can provide some ideas in other places in New York State and across the nation as they, like us, search for ways to promote fairness in the pretrial process.

Liz Glazer, Director
Mayor's Office of Criminal Justice

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[1] Solomon, Freda F., and Russell F. Ferri. 2017. "Reducing Unnecessary Pretrial Detention: CJA's Manhattan Supervised Release Program." Research Brief series, no. 42. New York: New York City Criminal Justice Agency, Inc.

Pursuing Pretrial Justice Through an Alternative to Bail:

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MDRC — along with subcontractor the Vera Institute of Justice (a partner on the implementation study) — conducted the evaluation of New York City's Pretrial Supervised Release Program with funding provided by the District Attorney of New York and administered through the Mayor's Office for Economic Opportunity (NYC Opportunity) and the Mayor's Office of Criminal Justice (MOCJ).

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Overview

On any given day in the United States, nearly half a million people are detained in jail while awaiting the resolution of their criminal cases, many because they cannot afford to pay bail. Bail is meant to ensure that defendants appear for court dates and are not arrested for new charges while they wait for their cases to be resolved. However, research has shown that setting bail as a condition of release can lead to unequal treatment and worse outcomes for defendants who do not have the ability to pay, regardless of the risk they pose. Additionally, systemic racial inequities throughout the criminal justice system mean that communities of color are disproportionately affected by cash bail and pretrial detention.

In 2016, New York City rolled out a citywide program known as Supervised Release (SR). SR offers judges the option of releasing defendants under supervision in lieu of setting bail. Defendants released to SR are required to report to program staff members regularly and are offered reminders of their court dates, case management support services, and voluntary connections to social services. The city developed the SR program to reduce the number of defendants detained in jail because they could not afford to pay bail, while at the same time maintaining court appearance rates and public safety. The findings presented in this report offer strong evidence that SR achieved these overarching goals.

The vast majority of defendants in New York City were not considered for SR during the time of this study because the program targeted and screened for eligibility only those defendants facing misdemeanors or nonviolent felony charges who were likely to have bail set and for whom it was believed a judge would be willing to grant SR. The directly measurable effects of SR described in this report therefore apply only to the relatively small proportion of citywide defendants enrolled in the SR program. These findings include:

- Presenting judges the option of SR substantially reduced money bail and pretrial detention.
- SR produced comparable reductions in releases without conditions.
- SR enrollees were subject to court rules that are applied to defendants with open cases for significantly longer time periods. Nevertheless, they were not significantly more likely to have a bench warrant issued for failing to appear for a court date.
- SR did not increase arrests for new crimes during the nine months following case initiation.
- SR enrollees were less likely to be convicted and more likely to have their cases dismissed.
- SR's effects on money bail, pretrial detention, bench warrants, and new felony arrests did not differ meaningfully among defendants of different races/ethnicities or ages.
- When SR was presented as an option, judges assigned more than half of defendants to it.
- SR largely succeeded at enrolling its intended target population of moderate-risk defendants.
- SR focused more on clients' social service needs than many other pretrial supervision programs — an aspect that made the program appealing to some judges.

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We hope this report will prove worthy of the time and energy contributed by so many to its production by providing research evidence that policymakers and practitioners can use in their efforts to build a more effective, equitable, and just pretrial system.

The Authors

Executive Summary

This report presents results from the first independent evaluation of the implementation and impacts of New York City's pretrial Supervised Release program. The program is designed to address a major injustice: On any given day in the United States, nearly half a million people are detained in jail while awaiting the resolution of their criminal cases, despite the presumption of innocence.[1] Many of these individuals are held because they cannot afford to pay the bail that was set as a condition of their release. Bail is meant to ensure that defendants appear for court dates and are not arrested for new charges while they wait for their cases to be resolved. However, setting cash bail as a condition of release leads to unequal treatment and worse outcomes for defendants who do not have the ability to pay.[2] Furthermore, systemic racial inequities throughout the criminal justice system mean that communities of color are disproportionately affected by the setting of money bail and its harmful consequences.[3] To address these concerns, jurisdictions are seeking alternatives such as pretrial supervision that will allow them to release more defendants safely.

In 2016, New York City rolled out a citywide program known as Supervised Release (SR). SR offers judges the option of releasing appropriate defendants under specific supervisory conditions in lieu of setting bail. Defendants released to SR are required to report to program case managers regularly and are offered reminders of their court dates, case management support services, and voluntary connections to social services. The city developed the SR program to reduce the number of defendants detained in jail because they could not afford to pay bail, while at the same time maintaining court appearance rates and public safety.

Pretrial supervision programs have existed since the 1970s and their use is increasing, but there is little research about their effectiveness.[4] Thus, lessons from this evaluation will help criminal justice policymakers nationally and in New York State, where monetary bail was recently eliminated for many cases involving misdemeanor and nonviolent felony charges, leading to a vast expansion of the city's SR program. This study evaluates the effects of New York City's SR program as it was implemented from 2017 to 2019, before statewide bail reform took effect in January 2020.

The overarching research questions for the evaluation are:

1. How was the SR program implemented?

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[1]Pretrial Justice Institute, "Why We Need Pretrial Reform," (website: www.pretrial.org/get-involved/learn-more/why-we-need-pretrial-reform, 2018).

[2]Pretrial Justice Institute (2018).

[3]The Sentencing Project, "Report of the Sentencing Project to the United Nations Special Rapporteur on Contemporary Forms of Racism, Racial Discrimination, Xenophobia, and Related Intolerance" (website: www.sentencingproject.org/publications/un-report-on-racial-disparities, 2018).

[4]Kristin Bechtel, Alexander Holsinger, Christopher Lowenkamp, and Madeline Warren, "A Meta-Analytic Review of Pretrial Research: Risk Assessment, Bond Type, and Interventions" (http://dx.doi.org/10.2139/ssrn.2741635, 2016).

2. What were the effects of the SR program on pretrial release conditions, pretrial detention, bench warrants for missed court appearances, new arrests, and case outcomes?

To address the latter research question, the evaluation uses a regression discontinuity design. The design compares the outcomes of defendants just above and just below an SR eligibility cutoff that was based on their scores on a risk assessment. Because these two groups of defendants were comparable at the outset but differed in their potential access to the SR program, any differences in their outcomes can be attributed to the SR program with a high degree of confidence.

Findings

The findings presented in this report offer strong evidence that SR achieved its overarching goals of reducing the use of money bail and pretrial detention while maintaining high court appearance rates and preserving public safety. These findings include:

- **SR was presented to judges as a release option at arraignment for only a small proportion of defendants in the system.**

At the time of the evaluation, the vast majority of defendants in New York City were not considered for SR because the program targeted and screened for full eligibility only the narrow group of defendants who were facing eligible charges (misdemeanors and nonviolent felonies that did not involve domestic violence allegations), who were likely to have bail set, and for whom it was believed judges would be willing to consent to SR. Defense attorneys acted as gatekeepers to the SR program: Defendants were screened only at their attorneys' request or with their permission. As a result, SR was a release option for fewer than 10 percent of all defendants arraigned on SR-eligible charges during the study time frame.[5] *The directly measurable effects of SR described in this report apply only to the relatively small proportion of citywide defendants enrolled in the SR program*.

- **The option of SR substantially reduced money bail and pretrial detention.**

When SR was presented as an option at arraignment hearings, it produced a sharp reduction in the use of money bail. Consequently, there was a similarly large reduction in the proportion of defendants detained in jail after their arraignment hearings.

- **The option of SR produced comparable reductions in release without conditions (ROR).**

At the same time, SR produced a large reduction in release without conditions. This finding means that some defendants who would have otherwise been released without conditions had additional conditions imposed as a result of SR. This circumstance, referred to as "net widening," was not widespread because most defendants were never considered for SR, largely

[5]This figure is among custodial arrests only — that is, arrests where the defendants were taken into custody. It does not include desk appearance ticket-based arrests, in which defendants were given tickets and told to appear for arraignment later.

because defense attorneys served as gatekeepers to the program. The vast majority of New York City defendants received ROR during the study time frame, as was true before SR was implemented. However, bail reform has changed New York City's pretrial process, and as of 2020 defense attorneys no longer serve in this gatekeeping role and all defendants can be considered for SR. Thus, it is important for the city to implement strategies to protect against widespread net widening, given the increased conditions and risks it places on individuals awaiting trial.[6]

- **SR enrollees were subject to court rules that are applied to defendants with open cases for significantly longer time periods.**

There are two reasons why SR enrollees were exposed longer to the potential for breaking a court rule, for example by missing a court hearing. First, SR enrollees had longer times to case resolution, and therefore probably had more required hearings. Second, they spent more days in the community (and not detained in jail). Taken together, these factors doubled the time that SR defendants were exposed to court rules. This issue is critical when measuring the effects of SR on outcomes such as bench warrants issued for failing to appear at court hearings.[7] Because SR enrollees were exposed longer to pretrial court rules, the evaluation sought to disentangle the effects of the SR program on failures to abide those rules from the effects of having to abide them for additional time.

- **Despite being subject to court rules for twice as much time, SR enrollees were no more likely to have bench warrants issued for failing to appear in court.**

The study found no statistically significant increase in the likelihood of receiving a bench warrant for failure to appear among SR enrollees, even though these defendants were at risk for twice as much time.

- **SR did not increase arrests for new crimes during the nine months following case initiation.**

The approach to isolating the impact of SR on new arrests was more straightforward than for bench warrants. Rather than focus on the pretrial period — which is subject to wide variation across individual defendants, and was dramatically affected by SR — the analysis assessed the effect on new arrests for a nine-month follow-up period common to all defendants. Enrollment in SR did not produce a substantial or statistically significant increase in new arrests overall or by type of charge.

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[6]In tandem with bail reform, the New York City Criminal Justice Agency — which administers release assessments to nearly every individual arrested and held for arraignment in New York City — began using an updated assessment that has greatly increased the proportion of cases recommended for ROR. The new assessment was developed based on a strategy of recommending as many individuals for release as possible while maintaining the city's high court appearance rate. The use of this new assessment may help counter the expanded potential for net widening.

[7]A bench warrant is issued by a judge, typically because a defendant has failed to appear for a mandated court hearing. It gives the police the authority to arrest the defendant.

- **Defendants enrolled in SR experienced lower rates of conviction and higher rates of case dismissals.**

Defendants who are detained awaiting trial will often plead guilty to their charges without extensive negotiation because they receive immediate or quicker release if they do.[8] Because SR reduced pretrial detention, it also reduced the incentive for defendants to plead guilty quickly. This circumstance probably made it more difficult for prosecutors to obtain guilty pleas for cases, requiring them to conduct more investigation and build substantial evidence to support their prosecution, simultaneously leading to longer times to case resolution. (Speedy trial requirements that apply to detained defendants, but not released defendants, may have also led to SR enrollees having longer case-processing times.) As a result of this combination of factors, SR ultimately reduced convictions and increased rates of case dismissal, meaning the prosecution dismissed charges or a judge determined there was not enough evidence for the case to proceed.

- **SR's effects on money bail, pretrial detention, bench warrants, and new felony arrests did not differ meaningfully among defendants of different races/ethnicities or ages.**

However, SR did have stronger effects on reducing the use of bail and on pretrial detention for felonies than misdemeanors, and had stronger effects on these outcomes in Manhattan than in the Bronx, Brooklyn, or Queens. There was no variation in effects on bench warrants or new felony arrests by charge class or borough.

- **When SR was presented as an option at arraignment, judges assigned more than half of defendants to it.**

When SR was rolled out citywide in 2016 it was a new option for most arraignment judges, since previously existing pilot programs had operated on a relatively small scale. It was not certain that judges would know enough about the program to feel comfortable using it in lieu of bail. Implementation study results show, however, that many arraignment judges did make use of SR: More than half of defendants were assigned SR when it was available as an option.

- **SR largely succeeded at enrolling its intended target population of moderate-risk defendants.**

Compared with defendants whose charges made them eligible for SR but who were not considered for the program, SR enrollees were more likely to be facing felony charges, were at a higher risk of being arrested for new felonies while awaiting trial, and were less likely to be recommended for ROR based on their likelihood of returning to court if released. At the same time, the SR eligibility criteria during the study excluded defendants at high risk of incurring new felony arrests while their cases were pending, as well as those facing violent felony charges.

[8]Will Dobbie, Jacob Goldin, and Crystal S. Yang, "The Effects of Pretrial Detention on Conviction, Future Crime, and Employment: Evidence from Randomly Assigned Judges," *American Economic Review* 108, 2 (2018): 201-240 (https://doi.org/10.1257/aer.20161503).

- **SR focused more on clients' social service needs than many other pretrial supervision programs — an aspect that made the program appealing to some judges.**

New York City's approach to supervised release differs from that of many other jurisdictions because the program is operated by community-based providers; is staffed by trained social workers, clinicians, peer mentors, and others; and includes a strong emphasis on counseling, case management, and connections to services, all to address clients' underlying needs. (Many other jurisdictions that operate supervised release programs house them in an office of the courts or probation. Such programs focus more on monitoring and compliance than on case management.)

Looking Ahead

Once New York State's bail reform legislation took effect in January 2020, the vast majority of defendants were no longer eligible for bail (except those arrested for most violent felony offenses), and instead had to be released without monetary conditions. These changes effectively limited judges' options to ROR or SR. *All defendants became eligible for SR at arraignment, with no exclusions based on charge or risk*. These shifts led to a significant expansion of the New York City SR program: SR began serving both a larger number of defendants and defendants with different characteristics and types of cases than in the past (until the COVID-19 pandemic temporarily disrupted SR enrollment beginning in March 2020). Rollbacks to portions of the original bail reform legislation went into effect in July 2020 and those may result in further changes to SR, though the program will probably continue to serve a larger, more varied caseload than it did before bail reform. Although this study cannot directly speak to the impact SR will have moving forward in light of its expansion and ongoing changes to New York's pretrial rules, the results presented in this report remain highly relevant as policymakers consider tools to support the goals of bail reform: to maximize pretrial release rates while maintaining defendants' court appearance rates and the safety of communities.

Chapter 1

Introduction

Background and Policy Context

On any given day in the United States, nearly half a million people are detained in jail while awaiting the resolution of their criminal cases.[1] Many of these individuals are charged with low-level, nonviolent offenses and do not meet the legal criteria for pretrial detention — that is, they are unlikely to miss court dates and they do not present a significant risk to public safety — yet they are held because they cannot afford to pay the bail amount set by a judge.[2] This pretrial detention can take a significant toll on the lives of affected individuals by putting them at increased risk of losing their jobs, housing, and child custody.[3] Those who are detained are more likely to be found guilty of their current charges and receive harsher sentences for those charges than comparable defendants who are granted release while their charges are pending.[4] Low-income defendants are disproportionately affected by the setting of bail, as many cannot pay even small sums and are thus detained.[5] Furthermore, due to racial inequities in pretrial practices similar to those found across the criminal justice system, Black and Latino people receive higher bail amounts and are more likely to be detained while awaiting trial than White people with similar charges and criminal histories.[6] Communities spend about $14 billion each year to incarcerate people who have not been convicted of crimes.[7]

To address concerns with the use of monetary bail and its accompanying human and financial costs, many jurisdictions across the United States are reforming their criminal justice systems. The aims are to limit the use of monetary bail and reduce unnecessary incarceration while maintaining public safety and high court appearance rates. New York City is among these jurisdictions: In 2016, New York City rolled out a citywide program known as Supervised Release (SR). SR offers judges the option of releasing appropriate defendants under specific supervisory conditions in lieu of setting bail. While reforms like these are springing up across the country, little is known about their effects.[8]

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[1]Pretrial Justice Institute (2018).

[2]Legal criteria for bail and pretrial detention vary by jurisdiction. The New York State bail statute indicates that a judge can only legally consider risk of flight when determining a defendant's release conditions.

[3]Pretrial Justice Institute (2018).

[4]Leslie and Pope (2017); Lowenkamp, VanNostrand, and Holsinger (2013).

[5]Council of Economic Advisers (2015).

[6]Pretrial Justice Institute (2018); Gelbach and Bushway (2011); Sentencing Project (2018). The United States Census defines Latino (masculine) or Latina (feminine) as any person of "Cuban, Mexican, Puerto Rican, South or Central American, or other Spanish culture or origin." In recent years, some research literature and other publications have started using "Latinx" as a broader, gender-neutral reference to this population. See Carnevale and Fasules (2017); Nichols (2017).

[7]Pretrial Justice Institute (2018).

[8]Bechtel, Holsinger, Lowenkamp, and Warren (2016).

In 2016, the New York City Mayor's Office for Economic Opportunity (NYC Opportunity) and the Mayor's Office of Criminal Justice engaged MDRC to conduct an implementation and impact evaluation of SR; the Vera Institute of Justice partnered with MDRC on the implementation portion. This report presents the results of the evaluation. It describes the citywide SR program, its implementation, and its effects on pretrial release conditions, pretrial detention, bench warrants issued for missed court appearances,[9] new arrests, and case outcomes. The findings from this study are based on the SR program as it existed before the bail reform in New York State that went into effect in January 2020.[10] There is a pressing need for effective alternatives to bail and a limited evidence base for such alternatives. This study can therefore offer important insights to New York as it implements statewide changes to its pretrial system, as well as to other jurisdictions and to the broader criminal justice field.

The Origins of New York City's Pretrial Supervised Release Program

In 2009, New York City engaged the New York City Criminal Justice Agency, Inc., the city's principal pretrial services agency, to pilot test a supervised release program in Queens.[11] The pilot program allowed judges to release defendants charged with nonviolent felony offenses under the supervision of Criminal Justice Agency case managers. Program participants were required to undergo needs assessments and to check in with those case managers regularly until their cases were resolved, and were provided with voluntary referrals to community-based service providers. This pilot program offered judges a new option in addition to releasing defendants on their own recognizance (ROR) — that is, without conditions — setting monetary bail, or remanding defendants (detaining them without the option of bail). Building on the Queens pilot test, in 2013 the city joined the Criminal Justice Agency to launch a similar program in Manhattan.[12] Separately, in 2013 the Center for Court Innovation pilot tested a misdemeanor-only supervised release program in Brooklyn. Early evaluations of these programs pointed to successful outcomes.[13]

In 2014 Mayor Bill de Blasio convened a Task Force on Behavioral Health and the Criminal Justice System. This task force was charged with diverting people with mental health and substance use disorders from the criminal justice system as part of a broader effort to reduce the growing number of people in jail with behavioral health needs.[14] One working group formed under the task force focused on strategies to reduce unnecessary incarceration. Upon close examination of the issue, the task force recommended an expansion of the supervised release pilot

[9]A bench warrant can be issued by a judge and triggers the authority of the police to arrest the defendant.

[10]Changes to the initial bail reform legislation were passed in April 2020 and went into effect in July 2020.

[11]Curbelo, McElroy, and Phillips (2013).

[12]Solomon (2014).

[13]Solomon (2013); Solomon and Ferri (2016, 2017); Hahn (2016, 2017).

[14]Individuals with mental illness represent 38 percent of the New York City jail population. See the task force's action plan: City of New York (2014).

programs to all five boroughs of New York City, as well as the use of a validated risk-assessment tool to identify appropriate candidates for the program.[15]

In response to this recommendation, the Mayor's Office of Criminal Justice established a steering committee composed of court stakeholders including the citywide supervising judge for arraignments, leaders from district attorney's offices and public defender agencies in each borough, court administrators, community-based providers, and Office of Criminal Justice staff members. In collaboration with the steering committee, the Mayor's Office of Criminal Justice led the development of a citywide program, informed by existing research and best practices. The SR program's goal was to provide an alternative to bail and potential pretrial detention for individuals charged with eligible misdemeanors or nonviolent felonies who were not at high risk of being arrested for new felonies while awaiting trial (according to a risk assessment) but were likely to have bail set. At the same time, SR aimed to maintain court appearance rates and public safety. SR presented judges with an additional release option for defendants believed to require more structure and support than is offered through ROR, the least restrictive release option, but without the financial burden that bail poses or the potential pretrial detention.

During the study time frame, the SR program was operated by community-based organizations in each borough that were responsible for screening and enrolling eligible defendants, providing supervision and case management services, and reporting noncompliance. Individuals enrolled in the SR program were supervised in the community by trained social workers through mandated phone and in-person check-ins, received reminders of court dates, and were provided with referrals to voluntary services such as drug treatment, counseling, education, and workforce development programs while their cases were pending. Following New York State's 2020 bail reform, all defendants became eligible for SR, eliminating the screening process; otherwise, this description of the SR program remains accurate.

As part of SR's implementation, the steering committee meets regularly to assist with SR's continuing development, receive program updates, and provide comments on SR's operations.

The Supervised Release Evaluation

In June 2016, the city engaged MDRC and the Vera Institute of Justice to conduct an independent evaluation of the SR program. The SR evaluation includes both an implementation study and an impact study. This report presents the results of both studies, focusing on the following research questions:

[15]City of New York (2015). Pretrial risk-assessment tools are controversial because of concerns that they perpetuate racial bias. Proponents of the tools argue that they are more objective than individual decisions made by judicial officers relying primarily on professional judgment. Critics point to the fact that most tools incorporate information — such as a defendant's criminal history — that is itself biased, given racial inequities that exist throughout the criminal justice system. The concern is that basing risk assessments on that biased information serves to reinforce and extend these disparities. For a diversity of perspectives on this issue, see Pretrial Justice Institute (2020); Arnold Ventures (n.d.); and Picard, Watkins, Rempel, and Kerodal (2019).

1. What was the process for identifying and screening cases for SR eligibility during the time frame of the evaluation?
2. How many defendants were released to SR? What were the characteristics of those defendants and their cases?
3. How was the SR program implemented?
4. What were the effects of SR on pretrial release conditions, pretrial detention, bench warrants issued for missed court appearances, new arrests, and case outcomes (that is, case dispositions — whether or not the defendant was found guilty)?
5. Did the effects of SR vary by age, race/ethnicity, charge class, or borough? If so, how?

Data Sources

The research team investigated these research questions using qualitative and quantitative data from the following sources:

- **Documents.** To gain familiarity with the genesis and continuing development of the citywide SR program, the team reviewed documents on past pilot programs, current policies and procedures, stakeholder training, data reporting, and program updates.
- **Courtroom observations.** The research team made two visits to each borough's criminal court to observe arraignment hearings and the SR eligibility and screening practices and procedures.
- **In-depth interviews.** Researchers interviewed 3 judges, 6 prosecutors, 10 defense attorneys, and 13 court liaisons (SR provider staff members who screen defendants for eligibility). Researchers also visited provider offices in each borough and interviewed SR provider managers, 12 case managers, and 23 SR participants to understand SR supervision and monitoring practices. Phone interviews were later conducted with 3 clinical supervisors (to understand this newly added role) and 6 additional case managers.
- **Judge survey.** A web-based survey was sent to 108 criminal court judges on the bench as of the summer of 2019, yielding 27 completed surveys (an overall response rate of 25 percent). The survey asked how often judges used SR, the types of cases they considered suitable for SR, and their suggestions for SR program improvements.
- **Focus groups.** Three separate, one-time focus groups were conducted with (1) judges/court administrators, to learn more about the origins of SR and the process for ongoing program development; (2) leaders from public defender agencies, to provide context regarding broader court practices and the culture

affecting the pretrial process; and (3) provider managers, to collect updates on provider operations as SR matured and programmatic changes were implemented. Focus groups were conducted several months after the in-depth interviews mentioned above.

- **Administrative data.** Quantitative data were obtained from the following sources:

  a. **SR providers.** These data include information regarding screening results, defendant and case characteristics, enrollment, compliance with supervision, and service referrals from March 2016 (program launch) through January 2019. Provider data are used in this report to identify defendants who were screened for SR and those who ultimately enrolled in the program; to describe the characteristics of SR clients, their participation, and their compliance with program requirements; and to identify the risk scores of screened defendants according to the SR risk assessment tool.

  b. **The New York State Office of Court Administration.** These data include information about all criminal cases in New York City from May 2017 through January 2019.[16] Court data are used in this report to provide contextual information on criminal cases in New York City and their characteristics (to help illuminate SR screening, eligibility, and enrollment) and as the source for information regarding pretrial release conditions, bench warrants for missed court appearances, new arrests, and case outcomes.

  c. **The New York City Department of Correction.** These data include information about all New York City jail admissions and discharges from May 2017 through March 2019. These data were used to create pretrial detention outcome measures, and to establish how long defendants spent in the community during their pretrial periods.

  d. **The New York City Criminal Justice Agency.** These data include information from Criminal Justice Agency prearraignment interviews (described in Chapter 2) for all interviewed cases from May 2017 through January 2019. The data are used in this report to provide information about defendant characteristics. Additionally, they were used in the research team's efforts to calculate SR risk scores based on administrative data

[16]Due to limitations in the Office of Court Administration data available to MDRC, reliable data are only available beginning in May 2017, whereas longer histories are available for other data sources. For this reason, analyses requiring Office of Court Administration data include a smaller sample than analyses that do not require those data.

(rather than provider-calculated risk scores), to conduct various sensitivity checks for the impact analysis.[17]

e. **The New York State Division of Criminal Justice Services.** These data include unsealed criminal histories (arrest and conviction information) for all individuals with unsealed arrests between May 2017 and April 2019.[18] These data, like the data from the Criminal Justice Agency, were used to calculate SR risk scores based on administrative data, to conduct sensitivity checks for the impact analysis.

Methodology

Data collection for the implementation study began with a document review in the summer of 2016. Observations and interviews were largely conducted in the fall of 2016, with follow-up focus groups occurring in the spring of 2017 and phone interviews with clinical supervisors and additional case managers occurring in the summer of 2017. Review of additional documents occurred in the fall of 2019 to capture program updates that occurred after qualitative data collection concluded. Administrative data were collected at various intervals throughout the life of the project. The judge survey was administered in the summer of 2019. All qualitative data were compiled into prestructured write-up templates by topic, then analyzed within and across boroughs to identify themes.

Quantitative data were cleaned, matched, processed into relevant descriptive and impact measures, and analyzed. Where relevant, descriptive quantitative measures were integrated with qualitative findings for a mixed-methods approach to addressing the implementation study's research questions.

The impact study, which measures how SR affected defendant and case outcomes, employs a quasi-experimental design known as regression discontinuity analysis. Regression discontinuity analysis can be used in situations in which access to an intervention is based on whether an individual falls above or below a certain point on a continuous rating variable. In simplified terms, by comparing the outcomes of those just above and just below that point, one can estimate the effects caused by an intervention. More detailed information about regression discontinuity analysis and its application in the SR evaluation is provided in the impact chapter of this report and in Appendix B.

Roadmap to This Report

The remainder of this report is divided into five chapters. Chapter 2 describes New York City's SR program as it existed during the study time frame in more detail, including its providers, structure, staffing, approach to supervision, and eligibility and screening procedures. Chapter 3 presents findings from the implementation study regarding eligibility and screening outcomes.

[17] See Appendix B for more information about these sensitivity checks.

[18] Aside from rare exceptions, arrests that do not result in convictions are sealed.

Chapter 4 describes the characteristics of SR clients and their cases and presents findings related to SR program implementation and case management practices; additionally, it offers an overview of SR program updates since data collection occurred. Chapter 5 presents findings from the impact study, including a description of the impact sample, an overview of the evaluation design, and a discussion of the SR program's effects (both overall and by subgroup). The report concludes in Chapter 6 with a summary of implementation and impact findings and a discussion of these findings' implications for policy and practice.

Chapter 2

The Supervised Release Program

This chapter provides an overview of the Supervised Release (SR) program, including a description of the community-based organizations that operate it, its structure, its staffing, and its philosophical approach to supervision. The chapter also explains how New York City's arrest-to-arraignment process worked during the study time frame, and describes SR's eligibility and screening procedures at the time and how they fit into the broader arrest-to-arraignment flow.

Provider Organizations

In 2015, the Mayor's Office of Criminal Justice put out a request for proposals for community-based service providers to operate the SR program in each borough, and ultimately engaged three nonprofit organizations:

- **The Center for Alternative Sentencing and Employment Services (CASES) in Manhattan.** In addition to its role in the courts as an SR provider, CASES operates several other alternative-to-detention and alternative-to-incarceration programs in the Bronx, Brooklyn, Manhattan, and Queens.[1]
- **The Center for Court Innovation in the Bronx, Brooklyn, and Staten Island.** Like CASES, the Center for Court Innovation also plays a broader role in New York City's courts, operating a variety of alternative-to-incarceration and other court-based programs intended to reduce the use of unnecessary incarceration in all five boroughs.[2]
- **The New York City Criminal Justice Agency in Queens**. The Criminal Justice Agency is New York City's main pretrial services agency. In addition to its role as an SR provider in Queens, it plays a central role in the pretrial process citywide: It interviews nearly every individual arrested and held for arraignment, offers the court a pretrial release recommendation for each interviewed individual, and notifies defendants of their upcoming court dates.[3]

The Mayor's Office of Criminal Justice oversees the performance of these three providers by holding regular check-in meetings to discuss program operations and challenges. In addition, providers are required to report data on screening, enrollment, client participation, and client compliance to the Mayor's Office of Criminal Justice monthly.[4] The Mayor's Office of

[1]Center for Alternative Sentencing and Employment Services (2018).

[2]Center for Court Innovation (2020).

[3]New York City Criminal Justice Agency (2020).

[4]Screening is no longer applicable following the elimination of SR eligibility criteria in 2020.

Criminal Justice uses these data to monitor performance and disseminates program metrics to stakeholders and the public.

The Structure of the Program

During the study time frame, SR provider responsibilities could be sorted into two broad categories: (1) screening defendants for SR eligibility in the arraignment parts of criminal court and (2) providing supervision, case management, and compliance reporting for enrolled clients, typically done from an office setting. SR provider staff members were present at every arraignment shift in the main criminal courthouse of each borough to screen potentially eligible defendants for the program. (These main courthouses were in operation from nine a.m. through one a.m. each day in all boroughs except for Staten Island, where arraignment shifts were scheduled during traditional nine-a.m.-to-five-p.m. business hours and between nine-thirty a.m. and one p.m. on weekends.) They coordinated their efforts with defense attorneys to confirm defendants' eligibility based on their criminal charges (violent felonies, cases involving domestic violence, and a handful of other charges were not eligible for SR), their SR risk-assessment scores (their scores could not be in the high-risk range), and, until June 2019, their community ties (each defendant had to have a friend, family member, or other contact whose contact information could be confirmed by the provider).[5] For eligible defendants, SR could be presented as an option to judges at arraignment.

Office-based case managers started working with defendants granted SR shortly after their arraignments. They conducted a needs assessment for each defendant, determined the defendant's supervision level, and set a schedule for mandatory phone and in-person check-ins. Over the course of their supervision of each client, case managers provided reminders of court dates, offered counseling and other support during check-ins, and made referrals to various community-based services, including employment services, shelters and housing programs, and mental health and substance abuse treatment, among others. These services were voluntary and referrals were made based on clients' needs and interest. Additionally, case managers were responsible for keeping the court up to date on their clients' compliance with the conditions of SR.

Additional information regarding the operation of the SR program during the study time frame is presented in Chapters 3 and 4, which discuss implementation findings.

Staff Roles

The SR programs in all boroughs had:

- **Directors,** who oversaw all program operations

[5] As of June 2019, this last eligibility requirement was eliminated. In issuing this policy change, the Mayor's Office of Criminal Justice noted that community ties that could not be verified before arraignment were often verified later, and that the verification of community ties had not been strongly correlated with success in the SR program.

- **Court liaisons,** who screened defendants for SR eligibility at arraignment and subsequent court hearings
- **Clinical supervisors,** who oversaw the work of case managers, provided clinical support, and supervised clients with severe behavioral and mental health problems
- **Case managers,** who completed initial client needs assessments, conducted mandatory phone and in-person check-ins, provided counseling, made referrals for voluntary services, and generated letters to the court reporting on defendants' compliance
- **Peer specialists** with relevant lived experience, who provided additional support to aid SR participants in following up on service referrals

The Therapeutic Approach to Pretrial Supervision

Across the country, many pretrial supervision programs are operated by pretrial services agencies within the criminal court system or by departments of probation. The SR program in New York City takes a different approach in that it is operated by community-based nonprofit organizations. Supervision and case management are provided by social workers and counselors, many of whom are licensed and clinically trained, rather than by court staff members or officers. By design, the SR program takes a more therapeutic, service-oriented approach to pretrial supervision than some of its peer programs that are more focused on enforcement and compliance.

New York City's Arrest-to-Arraignment Case Process

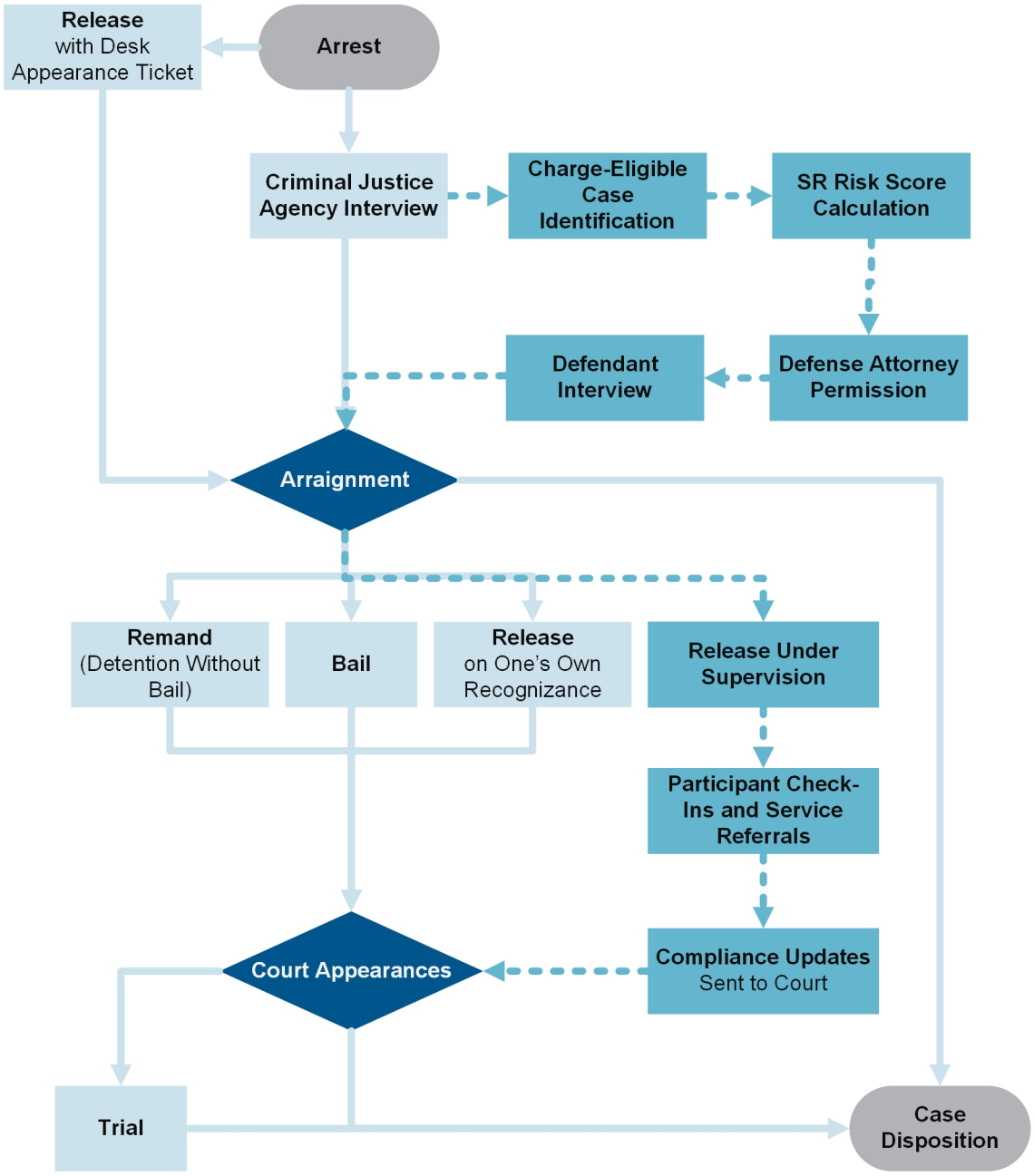
At the time of the study, the SR program was only available to a subset of defendants arraigned in New York City's criminal courts and entry into the program was determined in the early stages of the case process, shortly after arrest.[6] Therefore, the SR program is best understood in the context of the larger arraignment and pretrial case process. Figure 2.1 illustrates in simplified form the flow of criminal cases, beginning with arrests and ending with dispositions. This flow represents the arraignment process during the study time frame and may not reflect the process after legislative changes affecting bail in New York State were implemented in 2020.

As shown in the figure, after an arrest a case was processed in one of two ways: (1) A person could be released from the police precinct with a notice to appear in court at a later date (referred to as a desk appearance ticket), or (2) the individual could be held in custody pending an arraignment hearing where — if the case was not resolved with a guilty plea or a dismissal — a judge decided whether the defendant would be released while awaiting trial, and if so, under

[6]There were some exceptions wherein defendants entered the SR program later in the course of their criminal cases, after the arraignment hearing. These later entries were relatively rare.

Figure 2.1

Path of a Criminal Case in New York City During the Supervised Release Evaluation



NOTE: This figure depicts the New York City pretrial process during the study time frame and may not accurately reflect the process following the implementation of bail reform legislation in 2020.

what conditions.[7] By law, an arraignment hearing for a defendant was supposed to take place within 24 hours of the arrest. Since individuals released with desk appearance tickets generally faced lower-level charges that were unlikely to result in bail, they very rarely entered the SR program. The remainder of this report focuses only on those cases where defendants were held for arraignment hearings.

After an arrest and before the arraignment hearing, a defendant was brought to central booking in each borough's criminal court for processing.[8] Charges were filed in a criminal complaint by the district attorney's office. A report of the individual's criminal history (known as a "rap sheet") was generated. The defendant was interviewed by a staff member from the Criminal Justice Agency (in that organization's citywide pretrial services role) to determine that defendant's employment, school enrollment, and family or community ties.[9] The defendant met with his or her attorney. In addition, using data from the interview and a risk-assessment tool designed to predict a defendant's risk of failing to appear in court, the Criminal Justice Agency produced a risk score and recommendation for conditions of release. (This assessment differs from the SR eligibility risk assessment, which was only completed for defendants if they were to be considered for SR and which assessed their risk of incurring new felony charges while awaiting trial.)[10] The criminal complaint, rap sheet, and Criminal Justice Agency interview report (including the score and recommendation) were made available to judges, prosecutors, and defense attorneys, who often referred to these materials to inform their arguments and decisions during the arraignment hearing.

At the arraignment hearing, the judge formally advised the defendant of the charges and the prosecutor and defense attorney made arguments about bail and release conditions for cases that would continue past arraignment (that is, those that did not conclude at arraignment through a guilty plea or dismissal). Before SR was introduced, the judge would then decide whether to (1) release the defendant on his or her own recognizance (ROR) without any financial conditions, (2) set monetary bail as a financial assurance that the defendant would return to court, or (3) remand the defendant pending trial (detain the defendant without the option of bail), a relatively uncommon outcome reserved for the most serious and violent charges. Defendants who received ROR left court and could remain in the community while their cases were pending, with no monetary or supervisory conditions. If bail was set, defendants had to either pay bail or be detained in jail while their charges were pending or until they could post bail.

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[7]In New York City, "custodial arraignments" refer to arraignment hearings for defendants held in custody after their arrests (that is, not released with desk appearance tickets).

[8]Initial processing, including fingerprinting, usually began at local police precincts before continuing in central booking. Some central booking facilities are outside of the five main criminal courthouses.

[9]This role played by the Criminal Justice Agency citywide was not related to the agency's operation of the SR program in Queens. Defendant interviews took place in an area of the criminal courthouse known as the "pens," a location separate from central booking.

[10]The possible recommendation options on the Criminal Justice Agency risk-assessment tool were "recommended for ROR," "moderate risk for ROR," and "not recommended for ROR." However, "not recommended for ROR" included defendants whose risk assessment scores put them in the high-risk range as well as defendants who did not receive point scores, but who were not recommended for ROR because of a policy exclusion (for example, because they had active bench warrants).

Starting in March 2016, supervised release was added as a fourth possible release option. The darker blue shaded boxes in Figure 2.1 leading up to arraignment illustrate how the process of screening defendants for SR eligibility was incorporated into the standard criminal case process.[11]

The arraignment hearing can determine whether an individual remains in the community while his or her case is adjudicated or is detained in jail. Defendants and their attorneys therefore often consider it to be one of the most critical moments in a criminal case. In 2015, the year before SR was introduced as a release option, 70 percent of all cases in New York City that went through this arraignment process received ROR, 29 percent had bail set, and the remaining 1 percent were remanded.[12] For cases in which bail was set, only 11 percent of defendants citywide were able to post bail at arraignment. Over 40 percent of those with bail set were not able to post bail at any point. They remained in jail until their cases were resolved.[13]

SR Eligibility and Screening Procedures

During the study time frame, individuals were eligible for assignment to SR if they met the following criteria when they were arraigned:

- They were arraigned on criminal charges in New York City.
- Their arraignment charges did not include violent felonies or any other ineligible charges.[14]
- They did not have pending cases that included violent felony charges.[15]
- Their cases did not involve domestic violence allegations.
- They were not at high risk of being arrested for new felonies during the pretrial period according to a risk-assessment tool that was developed specifically for SR (distinct from the Criminal Justice Agency risk assessment used for all defendants in New York City).[16] The SR risk assessment considered eight factors, including a defendant's age, number and type of prior arrests and convictions, warrants, open cases, and full-time activity (for example, employment

[11]SR rolled out citywide in March 2016, with some variation in the exact start date for each arraignment shift depending on when providers began staffing that shift. All arraignment shifts were staffed by the summer of 2016.

[12]New York City Criminal Justice Agency (2016, 2018).

[13]New York City Criminal Justice Agency (2016, 2018).

[14]A small number of nonviolent felony charges were ineligible for SR.

[15]This criterion was not part of the program's original eligibility requirements but was added in August 2016.

[16]The SR risk assessment measured a defendant's risk of being arrested for a new felony while awaiting trial, and the Criminal Justice Agency risk assessment measured a defendant's risk of failing to appear for future court dates. The Criminal Justice Agency risk assessment was used to inform bail decisions, as New York State bail statute requires judges to consider only a defendant's risk of failing to appear (not risk of new criminal charges) in making bail determinations. The SR risk assessment was used to determine eligibility for the SR program.

or school). Appendix Table A.1 shows the risk factors and scoring criteria for the SR risk tool. SR risk scores were categorized as follows: low = -16 through -10, medium-low = -9 through -5, medium = -4 through 0, medium-high = 1 through 4, and high (and ineligible) = 5 through 18. The risk score eligibility cutoff of 4 is essential to the regression discontinuity method used to estimate SR's impacts in Chapter 5. (Note that other than being informed of whether a person was eligible for SR, judges generally did not receive any information about a person's score on this risk assessment.)

- They had verified community ties (that is, they could identify a family member, friend, case manager, or someone else in the community whom providers were able to reach). This criterion was assessed during an interview with the SR court liaison before arraignment. *This criterion was eliminated in June 2019*.

SR was designed as an alternative for defendants who would have had monetary bail set. That is, it was not intended for defendants who were appropriate for ROR. However, it was not always clear in advance of the arraignment hearing which defendants would have bail set and should therefore be considered or "screened" for SR. Therefore, screening procedures for SR were critical in determining the use or "take-up" of the program and the types of defendants who ultimately had access to it.

If a defendant's charges did not make him or her ineligible, a defense attorney could ask the SR court liaison on duty to score the defendant on the SR-specific risk assessment. If the SR assessment did not determine the defendant to be at a high risk of being arrested for a new felony, the court liaison then interviewed the defendant to verify a community tie (which at the time of the evaluation was the final requirement for SR eligibility) and to gauge the defendant's interest in the SR program. It is important to note that a judge could only consider SR as an option at the arraignment if the SR court liaison had screened the defendant and determined him or her to be fully eligible. And the SR liaison would only conduct that screening at the request or with the permission of a defense attorney (though occasionally judges would request that defendants be screened for SR). *Thus, defense attorneys acted as gatekeepers to SR*. In cases in which defense attorneys anticipated that judges would grant their clients ROR, they often would not request that their clients be assessed for SR eligibility, since they did not want judges to assign SR instead of ROR.

Chapter 3

Supervised Release Screening and Enrollment Outcomes

This chapter describes the patterns of screening and enrollment or "take-up" of the Supervised Release (SR) program in New York City during the study time frame. Findings show that SR was presented as an option at arraignment for a small fraction of eligible defendants, and that that happened because few defendants were screened for the program. The chapter explains the factors that were considered by court staff members when deciding whether to screen a defendant and the implications of those decisions on SR enrollment rates. Next, the chapter explains the types of challenges faced by court and provider staff members when incorporating screening for SR into the case process before arraignment. Finally, it describes how judges and defense attorneys viewed the SR eligibility and screening policies.

Supervised Release Enrollment Rates

Table 3.1 presents measures of eligibility and screening among all 287,297 cases whose defendants were arraigned following custodial arrests in New York City between May 2017 and January 2019. (For a description of the various samples used throughout this report, see Box 3.1.) The top panel of Table 3.1 shows that nearly three-fourths (72 percent) of cases arraigned included charges that made the defendant eligible for SR. Of those "charge-eligible" cases, fewer than 10 percent were screened for SR (9.7 percent). Screening for full SR eligibility among charge-eligible cases required several steps, including administering SR-specific risk assessments and conducting interviews with the defendants (described further below).

- **SR was a release option at arraignment for a small proportion of all charge-eligible defendants because most defendants were not screened for eligibility before their arraignment hearings. The main explanation for this pattern is that defense attorneys opted to have their clients screened for SR only when they believed them to be at risk of having bail set, otherwise preferring to preserve their chances for being released on their own recognizance (ROR).**

Most cases in New York City had charges that made their defendants eligible for the SR program, but the defendants were not screened for the program before their arraignment hearings. In essence, the defendants were never considered for the program. One reason why is that some court stakeholders — defense attorneys in particular — were concerned that judges given the option of SR may have assigned it for defendants they would otherwise have granted ROR. This choice would place those defendants at greater risk for technical violations and pretrial failures because they would have to comply with additional conditions (specifically, SR reporting requirements), even though their backgrounds and case characteristics suggested that they did not require

Table 3.1

SR Eligibility, Screening, and Arraignment Outcomes of Custodial Arrests

Outcome (%)	Total
Charge-eligible for SR	72.2
Screened for SR, among those charge-eligible	9.7
Sample size	287,297
Among those screened for SR	
Eligible based on the SR risk assessment	82.8
Interviewed	73.4
Fully eligible for SR	66.1
Sample size	20,073
Among those fully eligible for SR	
Arraignment outcome	
ROR	19.6
SR	57.5
Bail set	20.1
Case resolved at arraignment	2.2
Unknown/other	0.6
Sample size	13,271

SOURCES: MDRC calculations based on data from the New York State Office of Court Administration and SR providers.

NOTE: Sample includes all New York City arraignments for custodial arrests between May 2017 and January 2019.

additional support to ensure they returned to court. This situation — where defendants who otherwise would have been granted ROR are instead placed under supervision — is often referred to as "net widening." Thus, defense attorneys elected to have their clients screened for SR only when they believed them to be at risk of having bail set. Further limiting screening numbers, many cases were resolved at arraignment through dismissals or plea deals, meaning pretrial release decisions were not a consideration. Additionally, there were some defendants who may have been charge-eligible and likely to have bail set, but for whom the facts of their cases meant judges would be highly unlikely to consent to SR even if the clients were fully eligible. In such cases, defense attorneys would usually not have their clients screened, both to avoid giving the clients false hope of release and to avoid spending the extra time it would cost without hope of improved outcomes for the clients. Defense attorneys had access to their clients' Criminal Justice Agency risk scores and recommendations, criminal histories, and other case information, all of which informed these decisions.

Box 3.1

Samples Analyzed in This Report

Due to both variation in data availability and the differing goals of data analyses conducted throughout this report, several different samples are used for various purposes. This box provides a description of these different samples.

Custodial arraignment sample (287,297 criminal cases). This sample includes all arraignments for custodial arrests in New York City between May 1, 2017 and January 31, 2019. It is used to assess eligibility, screening, and enrollment outcomes for the broadest sample of relevant cases available. It is used as the starting sample for analyses in Tables 3.1 and 3.2.

Charge-eligible custodial arraignment sample (77,100 defendants). This sample includes all defendants with custodial arrests (arrests where the defendants were taken into custody) between May 1, 2017 and April 30, 2018 who had misdemeanor or felony charges that made them eligible for SR and who had not been enrolled in SR previously. It is used to put the impact sample into the context of the broader group of criminal defendants in New York City. If a defendant had multiple qualifying arrests within this time frame, only the first case is included. This sample is the overall sample for Table 5.1, which is subdivided for comparative purposes into columns for those not screened for SR (66,753 defendants), those screened but not enrolled (6,226 defendants), and those enrolled (4,121 defendants).

Supervised Release enrollee sample (11,004 defendants). This sample includes all defendants enrolled in SR from March 1, 2016 (when the citywide SR program launched) through January 31, 2019. It is used to describe the characteristics of SR enrollees and their cases and assess their SR intake outcomes, participation, and compliance. Tables 4.1 through 4.5 use this sample, as do Appendix Tables A.1 through A.3. If an individual was enrolled in SR more than once for different cases, only the first enrollment is included.

Impact sample (10,347 defendants). This sample includes all defendants with custodial arrests between May 1, 2017 and April 30, 2018 who had misdemeanor or felony charges that made them eligible for SR, who were screened for SR, and who had not previously been enrolled in SR. It is used to estimate the impacts of SR for those screened and those enrolled (through statistical adjustments), allowing for nine months of minimum follow-up for some outcomes (for example, new arrests) that are not specific to the defendant's pretrial period. If a defendant had multiple qualifying arrests within this time frame, only the first case is included. This sample is used in Tables 5.2 through 5.4 and 5.7 through 5.9. Tables 5.5 and 5.6 use a slightly smaller subset of this sample: only those defendants whose cases had reached resolutions as of January 31, 2019.

The lower half of Table 3.1 shows that among those defendants who were screened for SR, most (83 percent) were determined to be eligible based on the SR risk assessment, and most of those risk-eligible defendants were interviewed for eligibility by an SR court liaison (after both the defense attorney and the defendant consented to the interview). Some of those interviewed did not meet the eligibility criteria for the program based on information uncovered during the interview (for example, they could not provide verifiable contact information for community ties or did not agree to comply with program requirements). Ultimately, about two-thirds (66 percent) of those screened were determined to be fully eligible for SR.

If a defendant had been screened and deemed eligible, the defense attorney could raise SR as an option for the judge during the arraignment hearing. (In addition, whether or not a defendant had been screened before the arraignment, a judge could always inquire about the possibility of SR, in which case the defense attorney would ask the SR court liaison to screen the client if the liaison had not already done so.) In most cases, judges were made aware of a defendant's eligibility for SR, but not the details of the SR risk assessment score.[1] Ultimately, the judge decided whether to assign the defendant to SR or choose a different pretrial release condition (either to set monetary bail or to grant ROR).

- **When SR was presented as an option at arraignment, judges assigned more than half of defendants to this release condition. The remainder were equally likely to have bail set and to be released without conditions.**

The bottom portion of Table 3.1 shows the percentage of defendants assigned to each release condition at arraignment, among those where SR was presented as an option for the defendant (that is, they were screened using the SR risk assessment, interviewed, and determined to be fully eligible for SR). More than half were assigned to SR (58 percent). The remainder were equally likely to receive ROR and have bail set, at approximately 20 percent each. A small percentage of cases were resolved at arraignment.

Supervised Release Enrollment Rates by Borough

Across boroughs, the New York City criminal courts operated under the same policies and protocols related to the arrest-to-arraignment process and SR screening procedures during the time of the evaluation. Overall, despite borough-specific contexts and differences in personnel and culture, screening approaches for SR were implemented quite similarly across boroughs.

Nonetheless, there were some logistical differences that affected how often SR providers could screen for eligibility. For example, the Queens and Brooklyn providers had access to E-arraignments, a system that gives provider staff members direct access to criminal case files. This system allowed SR court liaisons to screen potential clients without having to wait for defense attorneys to ask them, because they had access already to the information required to calculate SR risk scores. The Queens and Brooklyn providers often took advantage of this capability — particularly for felony cases, because bail was a heightened possibility for these more serious cases — which allowed them to approach defense attorneys to ask permission to interview their risk-eligible clients. SR court representatives in the other boroughs (the Bronx, Manhattan, and Staten Island) did not have access to E-arraignments for all or most of the study period. In April 2018, Manhattan court liaisons gained access to E-arraignments.

[1] On occasion, a judge may have requested a defendant's risk level after learning the defendant was eligible for the program. In such instances, the SR provider liaison would give the judge this information. However, SR providers did not offer information on risk unsolicited.

Beyond the logistical difference of access to E-arraignments, the research team observed some other minor variations across boroughs. First, three of the boroughs had SR pilot programs before the citywide rollout. (The Center for Court Innovation previously operated a misdemeanor-only supervised release program in Brooklyn, while the Criminal Justice Agency operated felony-only programs in Manhattan and Queens.) These boroughs enjoyed the benefit of established relationships and perhaps increased awareness of SR when the citywide program rolled out. However, they also had to educate system actors about the policies of the new citywide program — including eligibility criteria that were different from the criteria in the pilot programs. These differences may have resulted in missed referral opportunities and confusion and frustration from various stakeholders.

- **There were only small differences across boroughs in the frequency with which charge-eligible defendants were considered for SR. The proportion screened averaged between 6 percent and 13 percent in each borough other than Staten Island, which was an outlier at 22 percent.**

Ultimately, even with some variation in SR screening practices, there were only small differences in the proportion of defendants screened and considered for SR across boroughs. Table 3.2 presents information regarding eligibility, screening, and arraignment outcomes by borough. The top panel of the table shows that Queens had only a slightly higher rate of screening (13 percent) than the other boroughs (approximately 6 percent to 10 percent), with the exception of Staten Island (22 percent), a smaller borough with a lower criminal caseload.

- **When SR was presented as an option at arraignment, judges assigned it most frequently in Manhattan (74 percent of eligible defendants) and least frequently in Staten Island (40 percent of eligible defendants). Judges in the other boroughs assigned SR to about three out of five defendants, on average.**

The middle panel of Table 3.2 shows the proportion of screened defendants who were ultimately interviewed and determined to be eligible for SR (meaning SR could be presented as an option at the arraignment hearing), by borough. Overall, most defendants screened in all boroughs were deemed eligible for SR, ranging from a low of 50 percent in Queens to a high of 75 percent in Staten Island. Differences in SR eligibility rates across boroughs could occur for several reasons, including different types of defendants and cases, or differences in screening practices, court contexts, and personnel. There was some variation across boroughs in the proportion of clients assigned to SR at arraignment, among those deemed eligible for SR, with rates ranging from 40 percent in Staten Island to 74 percent in Manhattan.

Table 3.2

SR Eligibility, Screening, and Arraignment Outcomes of Custodial Arrests, by Borough

Outcome (%)	Citywide	Brooklyn	Bronx	Manhattan	Queens	Staten Island
Charge-eligible for SR	72.2	70.6	71.1	77.7	69.6	64.7
Screened for SR, among those charge-eligible	9.7	7.5	9.5	6.2	12.7	22.4
Sample size	287,297	84,928	56,173	72,492	60,429	12,041
Among those screened for SR						
Eligible based on the SR risk assessment	82.7	80.5	78.3	79.6	90.9	83.8
Interviewed	71.8	75.6	78.0	77.4	61.2	83.1
Fully eligible for SR	64.3	73.6	73.7	65.2	50.0	74.6
Sample size	20,073	5,265	3,991	3,794	5,122	1,901
Among those eligible for SR						
Arraignment outcome						
ROR	19.6	17.6	33.2	6.6	17.2	24.2
SR	57.5	60.7	47.8	74.3	58.0	39.5
Bail set	20.1	19.2	15.6	17.6	23.2	30.1
Case resolved at arraignment	2.2	2.1	3.0	0.5	1.7	4.7
Unknown/other	0.6	0.5	0.3	1.0	0.0	1.4
Sample size	13,593	4,039	2,987	2,490	2,600	1,477

SOURCES: MDRC calculations based on data from the New York State Office of Court Administration and SR providers.

NOTES: Sample includes all New York City arraignments for custodial arrests between May 2017 and January 2019. Among the sample in the top panel, 1,234 records are missing borough information, therefore the individual borough sample sizes do not sum to the total sample size in the citywide column.

Challenges and Stakeholder Perspectives

Challenges with SR Screening and Enrollment

When SR launched in 2016, an additional 2,300 program slots were added across all five boroughs to the 1,100 existing slots available through the pilot programs in three boroughs. Early staffing levels suggest that the program expected to enroll a relatively small proportion of eligible defendants. However, within several months of the program's launch, caseloads grew beyond initial expectations and additional staff hiring was funded in all boroughs to accommodate the

growth in program enrollment. Notably, no one raised concerns to the research team that SR enrollment was limited by the number of program slots available. In addition, there were no meaningful differences in screening and enrollment rates over the course of the study period, suggesting that the SR program was fully rolled out and had reached a steady state of enrollment during the time frame of the impact analysis.[2]

Procedural factors such as time constraints at arraignment created screening challenges. Several public defenders the research team interviewed suggested that it was often difficult to explore SR fully as an option for their clients, even if they would have liked to, because of heavy caseloads and time pressures to prepare for arraignment hearings. In most boroughs, SR court liaisons could not help defense attorneys identify appropriate clients for SR because they did not have access to the system (that is, E-arraignments) that would provide them with the necessary information to assess SR eligibility. In fact, in these boroughs, defense attorneys had to lend SR court liaisons their case files for screening, which could make it harder for attorneys to prepare for their clients' arraignments. However, access to information does not appear to have led to substantially higher rates of screening in Queens and Brooklyn, where the E-arraignments system was available during the time of the study (as shown in Table 3.2).

While SR was considered for a relatively small proportion of all defendants, it is not clear that it was expected to serve a larger proportion, or that serving a small proportion affected the success of the program. SR was in fact designed to serve only a specific subset of defendants: those at moderate risk of incurring new felony arrests or failing to appear for court dates while awaiting trial. An analysis presented in Chapter 5 of this report suggests that defendants who were screened for SR were at higher risk of new felony arrests than those who were not screened — an indication that, *on average*, those screened for SR were more appropriate for the program and aligned with the target population than those who were not screened (a group whose lower-risk status made them more assured of receiving ROR). At the same time, among the group of charge-eligible cases where defendants received bail,[3] in fewer than one-fifth (about 17 percent) were the defendants screened for SR. While this low rate suggests some potential "false negatives," or defendants who could have been assigned to SR but were overlooked for screening, as discussed earlier in this chapter, defense attorneys reported that they avoided SR screening for higher-risk clients when they believed judges would almost certainly set bail even if given the option of SR. This approach would explain at least some of these "false negatives."

Stakeholder Perspectives on Eligibility and Screening

Many court stakeholders who were interviewed by the research team, including public defenders and judges, generally supported the SR program and its goals. Judges particularly liked that defendants assigned to SR could be connected to services to address their underlying needs. In fact, most defense attorneys and some judges expressed a preference for looser SR eligibility

[2]The program launched in 2016 but the impact study includes cases initiated more than one year into operations — between May 2017 and April 2018.

[3]Among all custodial arrests between May 2017 and January 2019 that were charge-eligible for SR, about 16 percent resulted in bail.

criteria that would allow them greater discretion regarding which defendants to consider for the program. For example, nearly 60 percent of judge survey respondents indicated that they would like the discretion to consider cases involving domestic violence. At the same time, over half of judge respondents felt that defense attorneys referred clients to SR too infrequently. During one court visit, the research team observed a judge request an SR screening for a defendant who had not been screened before arraignment. SR court representatives noted that judges do make such requests occasionally, a fact corroborated by judge survey respondents, the vast majority of whom reported having requested SR screening occasionally, if not frequently.

In general, defense attorneys reported that they faced a difficult challenge in trying to identify the clients who were likely to have bail set by their judges (and in turn to have them screened for SR), while not compromising their clients' chances of ROR. Some defense attorneys wished for more guidelines or standardization in the SR screening process. One suggestion was to allow recommendations for all release conditions to be generated by the risk assessment. Notably, this is the case in other jurisdictions that use risk-assessment tools and frameworks to develop guidelines for release conditions based on defendant risk. With New York's 2020 bail reform, it seems likely that SR will become a more standard option for defendants previously eligible for bail and thus will be incorporated into the arraignment process more consistently.

Some court workers — largely SR provider staff members — wished for the opportunity to work with more young people (defendants ages 16 to 24).[4] Research has shown that youth is often associated with greater risk of arrests for new crimes.[5] As a result, risk tools tend to assess young people as being at elevated risk.[6] The SR risk assessment does so, and as a result often made young people ineligible for SR. For those ages 16 to 19 in particular, relatively few were eligible for the SR program since their age automatically added six points to their risk scores.[7] While provider staff members generally believed young people to be more difficult to supervise and more likely to violate release conditions than older clients, their prevailing opinion was that young people were also more likely to benefit from their services, potentially meaning SR could be most effective with that group.[8] Interestingly, one of the prosecutors whom the research team interviewed also seemed sensitive to this way of thinking, noting that he would be more willing to consent to SR for young people than adults. In response to these ideas, the Mayor's Office of Criminal Justice pilot tested and then expanded a Youth Engagement Track. Additional information about this track is provided in the SR Program Updates section of Chapter 4.

[4]New York's Raise the Age legislation increased the age at which a child can be prosecuted as an adult — to 17 as of October 1, 2018 and to 18 as of October 1, 2019. These changes removed 16- and 17-year-olds from the adult criminal court.

[5]Alper, Durose, and Markman (2018).

[6]Robinson, Sassaman, and Stevenson (2018).

[7]See Appendix Table A.1 for detailed information regarding risk-score computation.

[8]Staff members in Staten Island felt differently. In interviews they said they found young people easier to engage because they were less cynical about court-mandated programs than older clients who had often cycled through a number of similar programs and could be "programmed out."

Some stakeholders expressed concerns about the ineligibility of defendants who were homeless or who had severe mental health or substance abuse problems. While SR did not technically exclude these individuals, many of them were unable to provide verifiable contact information (this criterion was later eliminated) or fully comprehend and agree to the terms of the program at the time of their arrest. As one defense attorney noted, "[I] understand that [the program] is not meant to be full service and that they don't have unlimited resources, but there's a bit of a gap there." To bridge this gap, program administrators at the Mayor's Office of Criminal Justice funded a clinical supervisor position in each borough beginning in the summer of 2017 to improve providers' ability to work with clients with more severe issues.

Chapter 4

Supervised Release Program Implementation and Case Management Practices

This chapter describes the characteristics of Supervised Release (SR) enrollees and their cases and discusses how the SR program was implemented after clients enrolled during the time period of this evaluation. It covers the intake process, the determination of supervision levels, the provision of supervision and case management, referrals to outside services, and the handling of noncompliance. Overall, SR program services were implemented quite consistently across boroughs, with a high degree of fidelity to the intended model. Where minor variations among boroughs existed, they are discussed below.

Characteristics of Defendants Enrolled in Supervised Release

Table 4.1 shows the demographic and case characteristics of defendants who were enrolled in SR between March 2016 (when the program launched) and January 2019, the latest available data for this report.[1] In total, 11,004 defendants were enrolled in the program during this time frame.

The average age of defendants enrolled in SR was 36. The vast majority are Black or Hispanic (81 percent) and most are male (83 percent). These demographics are representative of the broader criminal caseload in New York City, though not of the city's overall demographics due to historical and systemic inequities — including the disparate policing of communities of color — that result in the overrepresentation of Black and Hispanic men throughout the U.S. criminal justice system.[2]

About 1 in 10 SR clients reported living in unstable housing, such as in shelters or transitional living residences, or living on the streets. About 41 percent were engaged full time in employment, school, training, or caretaking at the time of their arrests.

Fifty-nine percent of defendants faced felony charges on the cases that brought them to SR, while 41 percent faced misdemeanor charges. Looking across both felonies and misdemeanors, charges for drug and property crimes were most common, which is consistent with the caseload for New York City as a whole.[3] Most defendants were assessed by the SR risk-assessment tool as being at medium (42 percent) or medium-high (33 percent) risk of new felony arrests while

[1]To include the most comprehensive group of SR enrollees possible, this sample includes a longer time frame than Tables 3.1 and 3.2, which are based on a more limited sample due to the available court data. For more information regarding the different samples used throughout this report, see Box 3.1.

[2]New York City Criminal Justice Agency (2018, 2019); U.S. Census Bureau (2020); Sentencing Project (2018).

[3]New York State Division of Criminal Justice Services (2018).

Table 4.1

SR Client Characteristics at Program Entry

Characteristic	Total
Age	35.5
Gender (%)	
Male	83.3
Female	16.1
Other	0.6
Race/ethnicity (%)	
Hispanic of any race	35.8
Black, non-Hispanic	45.2
White, non-hispanic	8.9
Other	10.1
Housing status (%)	
Private or market-rate housing	56.8
Affordable housing	23.0
Shelter or transitional living	9.0
Street homeless	1.4
Unknown or other	9.8
Engaged in full-time activity[a] (%)	40.7
Charge class and type (%)	
Felony	58.6
Drug	25.9
Property	25.3
Public order	5.3
Other	2.0
Misdemeanor	41.4
Drug	6.5
Property	12.6
Public order	10.7
Violent	11.6
SR risk level (%)	
Low	9.8
Medium-low	15.9
Medium	41.5
Medium-high	32.6
High	0.1

(continued)

Table 4.1 (continued)

Characteristic	Total
Criminal Justice Agency recommendation (%)	
Recommended for ROR	20.2
Moderate risk for ROR	13.3
Not recommended for ROR	66.6
More than one case on SR (%)	4.3
Sample size	11,004

SOURCES: MDRC calculations based on SR provider data.

NOTES: Sample includes SR enrollees from March 2016 through January 2019. If a defendant was enrolled in SR more than once for different cases during this time frame, data from the first enrollment are used.
Due to rounding, categories may not sum to 100 percent for some measures.
[a]Full-time activity is based on clients' own reports and includes work, school, training, and caretaking, and combinations thereof.

awaiting trial. Defendants assessed as high risk are not eligible for SR, so it is not surprising that a negligible number of defendants in the enrolled sample are at that level.[4]

Virtually all defendants held in custody before arraignment were assessed for risk of failure to appear for future court hearings by the Criminal Justice Agency.[5] According to data from defendants' Criminal Justice Agency risk assessments, shown at the end of Table 4.1, two-thirds of SR enrollees were not recommended for release on their own recognizance (ROR) because they were at elevated risk of failing to appear according to their risk scores, or because they had active bench warrants.[6] (As mentioned in previous chapters, a bench warrant can be issued by a judge and triggers the authority of the police to arrest the defendant.) While none of the eligibility criteria for SR were formally based on the results of the Criminal Justice Agency risk assessment, those risk recommendations may have influenced defense attorneys' decisions about screening for SR and judges' decisions about release conditions at arraignment.

Four percent of defendants enrolled in SR were being supervised on more than one case during the study time frame.

Characteristics by Borough

There was some variation by borough in terms of clients' demographic characteristics and their presenting cases, primarily corresponding to demographic differences and differences

[4]In a very small number of instances, judges have required SR providers to accept defendants whose SR risk scores were in the high end of the range.

[5]New York City Criminal Justice Agency (2020).

[6]A small portion of those not recommended for ROR may have been placed in this category because they were facing bail-jumping charges or because of conflicting residence information.

in the larger criminal caseloads across boroughs. (For example, there were higher proportions of Hispanic defendants in the Bronx and higher proportions of white defendants in Staten Island.)[7] Characteristics of SR clients and their cases are presented by borough in Appendix Table A.2.

Interestingly, Brooklyn enrolled a much greater proportion of defendants with misdemeanor cases (63 percent) than all the other boroughs except for Staten Island (70 percent). This difference may relate to the borough's previous supervised release pilot program, which served only misdemeanor cases. On the opposite end of the spectrum, about 81 percent of Queens enrollees had felony cases, which is consistent with the Criminal Justice Agency's supervised release pilot program in that borough and its focus on serving defendants with felony charges. The Bronx served a notably high percentage of drug cases, which may reflect the heroin epidemic with which the borough was grappling.[8]

Manhattan and Queens served the highest proportions of cases with property crime charges (49 percent and 48 percent, respectively), while Brooklyn's SR program served the largest proportion of defendants charged with violent crimes (22 percent, all misdemeanors as per the SR eligibility criteria during the study time frame, which excluded violent felonies). The SR risk level of enrollees across boroughs was similar, although the caseload in Queens was assessed as being at somewhat lower risk of new felony arrests than the other boroughs, with over one-third of the Queens SR caseload assessed as being at low or low-medium risk. Queens SR enrollees were also at somewhat lower risk of failing to appear for future court hearings, as evidenced by the fact that 36 percent of clients there were recommended for ROR by the Criminal Justice Agency (compared with a citywide average of 20 percent).

Intake and Setting Supervision Levels

Once a defendant was released to the SR program, he or she was required to report to the program office for intake as soon as possible after arraignment.[9] The SR program office was located in the borough's criminal courthouse itself in Brooklyn and the Bronx, within a few city blocks in Staten Island, and a subway ride away in Manhattan and Queens.[10] During the initial intake meeting, a case manager interviewed the participant, conducted a needs assessment, and determined the participant's supervision level. Table 4.2 presents information on client intake outcomes based on provider data. It shows that about 4 percent of those assigned to SR citywide did not report for

[7]New York City Criminal Justice Agency (2019).

[8]Correal (2019); City of New York (2018).

[9]If the defendant's arraignment occurred during a daytime shift, he or she had to go to the program office immediately after the hearing. If the arraignment occurred during a night or weekend shift, when SR program offices were closed, the defendant was required to report the following day or on Monday, respectively.

[10]The Manhattan SR case management office was in Harlem. The Queens SR case management office was in Queens Criminal Court in Kew Gardens until October 2016, when it moved to Long Island City. Both offices were close to public transit options.

Table 4.2

SR Client Intake Outcomes

Outcome (%)	Total
Did not report for the intake appointment	4.4
Supervision level	
Level 1	28.5
Level 2	43.3
Level 3	25.6
Level 4	2.6
Sample size	11,004

SOURCE: MDRC calculations based on SR provider data.

NOTES: Sample includes SR enrollees from March 2016 to January 2019. If a defendant was enrolled in SR more than once for different cases during this time frame, data from the first enrollment are used.

their initial intake appointments.[11] Some SR case managers attribute these failures to report to clients not fully understanding the program when they agreed to it at arraignment.

The framework in Figure 4.1 provides an overview of how supervision levels were determined, along with the contact requirements for each supervision level. Case managers set each client's supervision level based on the client' risk category as determined by the SR risk assessment,[12] the charge class (misdemeanor or felony), and any aggravating factors, such as unstable housing or serious mental health or substance abuse problems.[13] Participants set supervision level 1 received relatively little supervision (one in-person meeting per month), while those set level 4 had more stringent requirements (one phone call and one in-person meeting per week), with supervision levels 2 and 3 falling in the middle (a combination of two telephone and one to two in-person check-ins each month). As shown in Table 4.2, about 29 percent of SR clients were supervised at level 1, over two-thirds were supervised at levels 2 and 3, and 3 percent were supervised at level 4. (As shown in the framework in Figure 4.1, assignment to level 4 requires the presence of aggravating factors, which may explain why few defendants were supervised at this level.)

[11]Missed intake appointments were handled like any missed SR appointments: Defendants had 48 hours to make contact with the provider, after which the provider had to report the noncompliance to the court. A judge then decided whether to revoke SR and set bail.

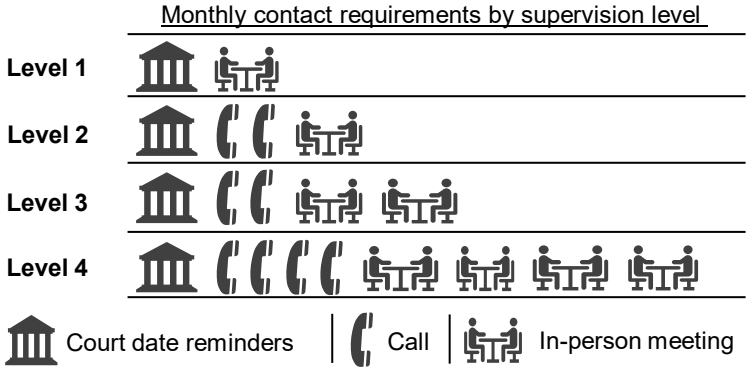
[12]As described in Chapter 2, SR risk scores were categorized as follows: low = -16 through -10, medium-low = -9 through –5, medium = -4 through 0, medium-high = 1 through 4, high = 5 through 18.

[13]SR providers generally had discretion over supervision levels. However, on occasion a judge required a provider to supervise a defendant at a higher level than indicated by the supervision-level framework, as a condition of that defendant's release.

Figure 4.1
Supervision-Level Determination and Contact Requirements

Supervision-level guidelines

Supervised Release (SR) Risk Category	Supervision Level: Misdemeanor	Misdemeanor with Aggravating Factors[a]	Felony	Felony with Aggravating Factors[a]
Low	1	2	1	2
Medium-low	1	2	1	2
Medium	2	3	2	3
Medium-high	2	4	3	4
High				Ineligible



SOURCE: New York City Mayor's Office of Criminal Justice.

NOTE: [a]Aggravating factors include defendant characteristics such as having an out-of-state record, unstable housing, or serious mental health or substance abuse problems.

- **SR case managers set clients' supervision levels in alignment with the level recommended by the SR framework over 90 percent of the time.**

Table 4.3 shows how often defendants' actual supervision levels aligned with the recommendations of the framework. Overall, case managers followed the framework recommendations closely, setting supervision levels consistent with the recommendations over 90 percent of the time. When they did deviate from the recommendations, it was usually to increase supervision to a higher level. Provider data indicate increased supervision was given mainly to defendants who had both severe mental health and substance use problems. Notably, and somewhat intuitively, most of the defendants who were given more supervision than recommended by the framework ended up assigned to the highest supervision levels (3 and 4) at intake.

These findings sync with reports from some case managers that they occasionally used their discretion to "bump up" and, to a lesser extent, "bump down" clients' supervision levels, based on an assessment of their needs. This practice was generally undertaken only with the

Table 4.3

Alignment Between the Framework Recommendation for Supervision and the Actual Supervision Level at Intake, by Supervision Level at Intake

Supervision-Level Alignment (%)	Level 1	Level 2	Level 3	Level 4	Total
Match	98.3	95.4	82.7	52.1	91.8
Nonmatch					
Lower supervision level assigned at intake than recommended by the framework	1.7	1.5	0.3	0.0	1.2
Higher supervision level assigned at intake than recommended by the framework	0.0	3.2	17.1	47.9	7.0
Sample size	3,134	4,755	2,808	289	10,986

SOURCE: MDRC calculations based on SR provider data.

NOTES: Sample includes SR enrollees from March 2016 through January 2019. If a defendant was enrolled in SR more than once for different cases during this time frame, data from the first enrollment are used. There are 18 enrollees for whom supervision-level information is missing, who are therefore not included in this table.

approval of a manager after a discussion of the defendant and the case. Appendix Table A.3, which presents information on SR intake outcomes, participation, and compliance by borough, shows that "bumping up" supervision levels at intake was uncommon in all boroughs, but occurred most frequently in Queens (16 percent) and Staten Island (8 percent) compared with the other three boroughs (ranging from 2 percent to 6 percent). Case managers noted that they also sometimes adjusted defendants' supervision levels after intake based on how they were doing in the program. For example, if a defendant was rearrested, the case manager might increase the defendant's supervision level to provide additional support. Alternatively, the case manager might reduce supervision in response to ongoing program compliance and demonstrated stability. SR instituted a formal, graduated response policy in March 2019 to guide adjustments to supervision levels; further detail regarding this policy is provided in the SR Program Updates section at the end of this chapter.

Meetings, Check-Ins, and Reminders

Once the initial needs assessment was complete and a supervision level determined, the case manager established a schedule for required in-person meetings and phone check-ins with the client. Generally, this schedule involved a set day of the week and time for each check-in. Case managers reported doing their best to make these schedules as convenient for clients as possible, a point confirmed by SR participants interviewed by the research team. Table 4.4, which presents information on SR clients' participation in services, shows that overall, SR clients averaged two phone and two in-person check-ins per month enrolled. The average length of stay in the program was just shy of four months, roughly one and a half months less than the average time it took for a case to be resolved. (The difference is explained by defendants who left the program before their cases were resolved, whether due to noncompliance or for other reasons.)

Table 4.4

SR Client Participation

Outcome	Total
Months enrolled, among those who have left the program	3.8
Number of contacts per month	
In-person	2.0
Phone	2.1
Ever referred to services (%)	26.0
Employment/vocational	10.5
Education	3.3
Substance abuse treatment	5.0
Housing/shelter	3.0
Mental health	4.0
Other	10.1
Sample size	11,004

SOURCE: MDRC calculations based on SR provider data.

NOTES: Sample includes SR enrollees from March 2016 to January 2019. If a defendant was enrolled in SR more than once for different cases during this time frame, data from the first enrollment are used.

- **SR clients' average number of in-person and phone contacts exceeded the requirements for each supervision level other than Level 4, for which contacts fell short.**

Appendix Table A.4 presents SR intake outcomes, participation, and compliance by client supervision level. It shows that on average, SR clients had more in-person and phone contacts per month than required for each type of contact at each supervision level other than level 4, where averages fell shy of the four in-person and four phone contacts required. As noted above, in addition to their higher assessed risk of new felony arrests, many clients supervised at level 4 faced struggles with housing instability, mental health issues, or substance abuse; these combined challenges probably contributed to this group's lower average numbers of contacts. Overall, however, looking across supervision levels, this finding suggests that clients were highly engaged with SR case management. In fact, at some levels clients' average contacts exceeded requirements, which indicates that some clients voluntarily interacted with their case managers more often than required.

Across boroughs, case managers described the content of in-person meetings as client-specific, responding to each client's level of engagement and need. Case managers said that for some clients — generally those who were stable, employed, in compliance, and not in need of services — in-person meetings were perfunctory, meaning the client showed up, the case manager asked how things were going, and within a few minutes the client departed. For clients with

housing or employment needs, or those in some type of crisis, check-ins could last for 45 minutes to an hour while case managers provided counseling or helped clients set goals and take steps toward achieving them. For example, case managers described assisting clients with job applications or discussing potential referrals for other service needs. Some in-person meetings also fell between these two ends of the spectrum. Phone check-ins tended to be brief and focused on simply maintaining contact with clients.

When asked about their strategies for keeping clients engaged and in compliance, case managers in multiple boroughs said that it was important to build trust and strong relationships. In addition to in-person meetings and phone check-ins, case managers also texted or called clients with reminders of upcoming court dates to help them stay in compliance. Beyond reminders, case managers noted that they often helped their clients navigate the complex bureaucracy of the court system.

Referrals

The approach to SR in New York City during the evaluation time frame differed from pretrial services in many other jurisdictions in that it focused strongly on case management, counseling, and referrals to need-based services. Service referrals were voluntary — clients were not required to engage in services as part of the terms of their release.

- **About one-fourth of SR enrollees were referred to a new supportive service by their case managers; referrals to employment/vocational programs were most common.**

Table 4.4 shows that just over one-fourth of SR clients were referred to services. Employment/vocational-focused services were the most frequent subjects of referrals (11 percent), with substance abuse treatment, mental health services, educational programs, and housing programs/shelters following (all at 5 percent or lower). While one-fourth of SR clients may seem like relatively few in light of interview data that suggests that a significant portion of the SR caseload faced mental health or substance use concerns (described below), many clients may have been engaged in services before their SR enrollment. In some instances, these previous service connections had lapsed and SR case managers reconnected their clients. However, these reconnections would not have been recorded in the data and therefore are not reflected in the 26 percent referral rate. At the same time, some case managers struggled with imperfect service options to offer their clients, particularly those facing housing instability. This lack of available services also may have contributed to the referral rate being lower than expected given the service needs of the SR caseload.

A few interesting variations in referrals emerge when examining the data by borough and by supervision level. Appendix Table A.3 shows that referral rates were highest in Manhattan and Staten Island, at 34 percent and 31 percent, compared with other boroughs' referral rates in the low to middle 20s. This pattern may be partially explained by referral logistics in these two boroughs. In Manhattan, a licensed mental health program shares a building with the provider's case

management office, and in Staten Island most community-based programs are within walking distance of the courts and the SR case management office. These circumstances may appeal to clients and increase both their interest in receiving referrals and their ability to follow through on them. Appendix Table A.4 shows that referral rates increased as supervision level increased. This pattern may reflect the greater service needs of higher-risk clients, or alternatively, it may reflect case managers' additional attention to clients at higher supervision levels.

Judges expressed favorable opinions regarding the referral component of the SR program. They said they appreciated the possibility of engaging defendants in supportive services, particularly those defendants whose repeated contact with the criminal justice system appeared to be the result of behavioral health problems.

Compliance Reporting and Managing Noncompliance

Case managers were also responsible for updating the court as to their clients' compliance with SR program requirements at each court appearance. Case managers usually provided this information in letters; court liaisons then delivered the letters to the courts for distribution to judges, prosecutors, and defense attorneys.

To meet their obligations fully while on SR, clients had to attend all court dates, make their required phone and in-person check-ins, and avoid new arrests. If a client did not meet any one of these requirements, that client could be at risk of having bail set or having a bench warrant issued.

- **About two-fifths of SR enrollees failed to comply with a condition of SR at some point during their time in the program, with missed SR check-ins being the most common form of noncompliance and missed court dates the rarest.**

Table 4.5 presents information on client compliance based on SR provider data. It shows that about two-fifths of SR enrollees violated a condition of the program, with some clients violating SR conditions in more than one way: One-fourth of clients missed a phone or in-person SR check-in, 19 percent were rearrested, and 11 percent failed to appear for a court date.

As shown in Appendix Table A.3, noncompliance rates varied somewhat by borough, with Manhattan and Queens showing lower overall rates. Those boroughs — particularly Queens — may have had lower noncompliance rates because they had somewhat lower-risk caseloads to begin with (see Appendix Table A.2). Appendix Table A.4 shows each form of noncompliance — missed appointments, new arrests, and failures to appear for court — becomes more prevalent as supervision level increases, an unsurprising finding given that the SR risk assessment, though designed to predict the likelihood of new felony arrests, was also shown to be predictive of both new arrests overall and failures to appear.[14]

[14]Healy (2015).

Table 4.5

SR Client Compliance

Outcome (%)	Total
Ever noncompliant while in the program	40.9
Ever missed a phone call or in-person check-in	24.4
Ever rearrested	19.4
Ever failed to appear for court	10.9
Sample size	11,004

SOURCE: MDRC calculations based on SR provider data.

NOTES: Sample includes SR enrollees from March 2016 to January 2019. If a defendant was enrolled in SR more than once for different cases during this time frame, data from the first enrollment are used.

If an SR client missed an appointment without prior notification or was rearrested, the SR provider was required to report the event to the court within 48 hours. (Missed court appearances were known to the court.) In the event of a missed appointment, a case manager attempted to get in touch with a client using all available contact information, including the client's community contacts. If the case manager was not able to reach the client within 24 hours, he or she would make contact with the client's defense attorney and enlist the attorney in efforts to reengage the client. If after 48 hours the client still had not been reached, the SR provider reported the noncompliance to the court in a formal letter.

SR policy required that a noncompliant SR case be "advanced," meaning a hearing was put on the calendar for that day. The SR provider then attempted to notify the client's attorney so that the attorney could attend. At the hearing, the judge had discretion over how to respond to the missed appointment. The judge generally decided between waiting until the next scheduled court date to see if the defendant returned and issuing a bench warrant. The judge could also choose to revoke SR. At the time of the research team's court observations in the fall of 2016, the policy of advancing noncompliant cases was inconsistently enforced: It rarely occurred in most boroughs apart from Brooklyn, where it was regularly applied.

SR policy also required that new arrests be reported to the courts, prosecution, and defense. In Queens the SR provider — the Criminal Justice Agency — knew about rearrests through its larger pretrial-services role in the city, but in other boroughs the providers did not have a consistent way to know when clients were rearrested, making it challenging for them to meet this requirement. The SR program's policy was that clients rearrested for violent felonies were no longer eligible for SR on their original cases and had to leave the program. When rearrests occurred for crimes other than violent felonies, judges had various responses. Many judges were willing to keep clients on SR if providers were willing to continue to supervise them, while others might revoke SR and set bail.

Implementation Challenges

Overall, SR supervision and case management was delivered successfully and with a high degree of fidelity to the intended program model across boroughs. Nonetheless, there were challenges. At the time of the research team's visits to SR case management offices in late 2016, caseloads were high in some boroughs due to cases remaining open longer than expected (as also shown in administrative data, described in the next chapter) and enrollments occurring at a faster pace than originally predicted. Case managers in Brooklyn, for example, had caseloads of about 70 clients each, whereas expectations for caseload sizes had been closer to a range of 40 to 50. To address this challenge, the Mayor's Office of Criminal Justice funded the hiring of additional case managers in the summer of 2017.

- **SR providers reported challenges including heavy caseloads, difficulties balancing a social work-oriented approach with their supervision/ monitoring responsibilities, high-need clients, and discontent regarding the SR noncompliance reporting policies.**

Case managers identified a number of logistical challenges that could interfere with client check-ins, including client transportation problems, child care and work conflicts, inconsistent phone service, and scheduling mishaps (for example, when a meeting with a client in crisis ran long and other clients were waiting). Case managers reported doing their best to "make it work" when these types of problems occurred, employing strategies such as offering clients MetroCards (to pay for public transportation) and being as flexible as possible when clients needed to reschedule appointments.

Case managers also reported some inherent tensions with approaching SR — which is, at its core, a pretrial supervision/monitoring program — from a therapeutic standpoint, with an emphasis on services and intensive relationship building. This tension was further complicated by the proportion of SR clients suffering from serious mental illness and their high level of service needs. (In the spring and summer of 2017, clinical supervisors were added to the SR staff in each borough to provide additional support and guidance to case managers, including clinical advice and assistance serving higher-need clients.) Case managers in all boroughs said they wished they had fewer cases on their caseloads and more opportunity to do clinical work. Although case managers explained that they provided crisis management, counseling, and referrals to longer-term therapy during check-ins, it was difficult for them to do deeper clinical and emotional work, given the limited opportunities for interaction and the narrow scope of the program's mandate.

Finally, many SR provider staff members (and defense attorneys) objected to the 48-hour noncompliance-notification policy for missed appointments, believing 48 hours to be an insufficient amount of time to determine whether a client was willfully refusing to comply with SR requirements or was experiencing a temporary crisis (for example, hospitalization or eviction). Staff members offered multiple suggestions for alternative noncompliance reporting policies, such as extending the 48-hour period or allowing case managers more discretion based on their experiences working with each client.

Participant Perspectives

As noted in Chapter 1, the research team conducted interviews with 23 SR clients across all five boroughs to learn about their perspectives on the program. These interviewees are not representative of the overall SR caseload, and probably represent a more engaged subset of enrollees with less serious cases (in that they were present in SR program offices on the day interviews were conducted and agreed to participate, and their defense attorneys consented to their participation). In deference to defense attorneys' concerns that if clients responded to questions about their SR experiences they might unintentionally reveal information about the open criminal charges against them, these interviews were quite narrow in scope. These limitations notwithstanding, interviewed SR clients generally expressed positive views of the SR program. They particularly appreciated the opportunity SR gave them to be released while their cases were adjudicated. As one SR participant in Manhattan said:

> It's either this or prison, and this beats that hands down any day of the week. I could be sitting in jail waiting for my court date or working and doing positive and spending time with my family and doing everything that I should have been doing since day one. Took me getting arrested to get there, but hey, I'd pick this regardless of if I had to come five days a week.

A Brooklyn SR client similarly acknowledged the benefit of the SR program as an alternative to bail and pretrial detention, and also cited the support his case manager provided:

> You could've been in jail — doing that every day. You only have to do this once or twice per month. I'm cool with my social worker. I am glad I got [her]. She is cool though. She actually helps me.

From a practical standpoint, another SR participant in Brooklyn appreciated his case manager sending court reminders:

> We talk about how I am doing. She updates me on my court stuff, when my court dates are, reminds me the day before and morning of. She usually texts it to me. Checking in with her is helpful because I would never remember my court date. If I lost my paper then I'd come here to ask about court dates.

Based on interviews with case managers, some lower-risk clients may have found the SR reporting requirements frustrating due to the additional burdens they pose for these more stable defendants, who probably would have made their court appearances without additional oversight. Overall, however, most interviewed clients appreciated the support and structure offered by their case managers, despite the program's requirements. Additionally, several expressed gratitude for their case managers' assistance in connecting them with needed services.

SR Program Updates

Much of the implementation study's data collection took place from the fall of 2016 through the summer of 2017, when the citywide SR program had been in operation for a relatively short time. While the basic structure of the program remained in place until larger changes were implemented

in response to New York State's bail reform in 2020, some adjustments and augmentations were made through 2019 to address issues raised by stakeholders, and to support the city's larger criminal justice reform efforts. An overview of these program updates is presented chronologically below:

- **Client incentives.** As of July 2017, the Mayor's Office of Criminal Justice began providing additional funding to each borough's SR provider. Providers generally used the funds to provide clients with clothing for court appearances and help getting food, and to offer incentives (for example, food and movie tickets) for attending check-ins and other meetings.

- **Standardized judicial action forms for noncompliance reporting.** As of October 2017, the Mayor's Office of Criminal Justice created a standardized form used citywide to report noncompliance to judges. The form includes a section with possible responses for the judge to select, including addressing the noncompliance at the next scheduled court date, advancing the case, issuing a bench warrant or "staying" it (that is, holding the warrant until a later date), as well as an "other" option open to the judge's discretion. This form was developed in response to concerns raised by SR court liasons that judges wanted more guidance regarding their options when responding to SR noncompliance.

- **Youth Engagement Track.**[15] In March 2018, to serve more young people, the Brooklyn SR program launched the Pretrial Youth Engagement Program. This pilot program served 16- to 19-year-old defendants charged with assault in the second degree or robbery in the second degree (both violent felony offenses), as well as people in this age group whose SR risk assessments put them in the high-risk range. In October 2018, the Bronx, Manhattan, Queens, and Staten Island SR programs initiated the Youth Engagement Track, which served 16- and 17-year-olds meeting the same charge and risk eligibility categories as the Brooklyn pilot program. In June 2019, the Youth Engagement Track began operating in all five boroughs, serving 16- to 19-year-olds and expanded to include six eligible charges: assault, burglary, and robbery in both the first and second degrees.[16] It uses a positive youth development model and offers expanded services that include cognitive behavioral therapy, family engagement, and educational and vocational assistance.

[15]New York's Raise the Age legislation changed the age at which a child can be prosecuted as an adult to 17 as of October 1, 2018, and 18 as of October 1, 2019.

[16]Young people between 16 and 19 years of age who have open cases involving one of the eligible charges can also participate in the Youth Engagement Track.

- **Graduated response policy.** Effective March 2019, the Mayor's Office of Criminal Justice issued recommendations for graduated responses to client behavior, including both positive and negative responses:
    - **Positive responses.** With supervisor approval, case managers may offer incentives including food/coffee, a request for a verbal commendation from the judge, a $10 gift card, a reduction in supervision level, and a request to the judge to end supervision and grant ROR. These incentives are to be offered on a case-by-case basis to clients who have been attending regular court appearances, have achieved goals set out at intake with their case managers, have attended treatment or educational/vocational programs, or have gotten back on track after falling out of compliance. More specific eligibility criteria govern changes in supervision.
    - **Negative responses.** In coordination with supervisors, in response to a missed court appearance, missed check-in appointment, or rearrest, a case manager may require an additional reminder phone call, request a verbal admonishment or ultimatum from the judge, increase required phone calls by one per week until the client's next court date, or increase the client's supervision level.
- **Expanded risk eligibility.** Effective June 2019, the SR risk tool was recalibrated. "High-risk" was redefined as 9 risk points or higher (instead of 5 risk points or higher, as had been the case previously), shrinking the pool of ineligible defendants.
- **Community contact verification.** Also as of June 2019, verifying a community contact before arraignment was removed as a requirement for SR eligibility. In issuing this policy change, the Mayor's Office of Criminal Justice noted that community ties that could not be verified before arraignment were often verified later, and that the verification of community ties had not been strongly correlated with success in the SR program.

As will be described in further detail in the next chapter, the sample identified for the impact analysis includes defendants screened for SR between May 2017 and April 2018, with their follow-up period extending through January 2019. Given this fact, only two broader program changes were made during the time frame when the impact sample was enrolled: the addition of client incentives (in June 2017) and standardized judicial action forms (in October 2017). It is unclear how changes made after the sample time frame may have altered the program's effects, if at all. Program changes were geared toward improving client compliance through positive and negative reinforcement and expanding the program (to additional young people, slightly higher-risk defendants, and those for whom a contact could not be immediately verified). While the changes may have boosted compliance for later SR clients, the population served by SR after these latter changes went into effect may have resulted in a somewhat higher-risk population, potentially counteracting compliance improvements.

Chapter 5

Impact Findings

This chapter presents the estimated impacts, or effects, of Supervised Release (SR) on pretrial release conditions, pretrial detention, bench warrants for missed court appearances, new arrests, and case dispositions. The chapter begins with a description of the sample used for the impact analysis, followed by a brief overview of the regression discontinuity method used to conduct the analysis. Finally, it discusses the impacts of SR.

The results of the impact analysis show that among those enrolled:

- SR sharply reduced the use of bail and pretrial detention, but also led to a comparable reduction in release on one's own recognizance (ROR) at arraignment.
- SR had little observable effect on bench warrants for failures to appear at court hearings, particularly when considering clients' increased exposure to court conditions through longer pretrial periods and decreased pretrial detention.
- SR had no observable effect on new arrests during a nine-month follow-up period.
- SR increased case dismissals and, consequently, reduced convictions.
- SR's effects on money bail, pretrial detention, bench warrants, and new felony arrests did not differ meaningfully among defendants of different ages or races/ethnicities. However, the effects of SR on bail and detention at arraignment were stronger for felonies than for misdemeanors and stronger in Manhattan than in other boroughs.

The Impact Analysis Sample

The impact analysis focuses on a subset of 10,347 defendants with custodial arraignment hearings for cases that were charge-eligible and screened for SR between May 1, 2017 and April 30, 2018. Data are available through January 2019, allowing for the measurement of outcomes within nine months after arrest for each case.[1] The method and sample used in this analysis allow for a valid estimate of the impacts of SR for defendants who were screened and enrolled in SR during the study time frame. The impact sample consists of a relatively higher-risk population that was more likely to be facing felony charges than the larger population of defendants in New York City during the same period. Therefore, although these findings represent the experience of SR enrollees during the time frame of the present analysis, they may not represent what the impacts of SR would be for a broader population of criminal defendants in New York City.

[1]For more information about the samples analyzed in this report and how they compare, see Box 3.1.

When interpreting the present results, it is helpful to consider how the impact sample compares with other criminal defendants and cases in New York City. First, as described in Chapter 3, the proportion of cases screened for SR was only a small fraction of all charge-eligible cases generated from custodial arrests during the study time frame. Therefore, the impact analysis, which examines screened cases, focuses on a small subset of all criminal cases in New York City (although it applies to a large fraction of all SR enrollees during the study time frame). Second, most of the present impact findings are analyzed using a treatment-on-treated statistical approach.[2] This approach means that the results presented later in this chapter are for defendants who enrolled in SR, which includes about half of those screened. The treatment-on-treated approach takes impacts estimated using the full screened sample, then effectively concentrates them among the enrolled sample based on the assumption that any impacts of the program are due to enrollment rather than screening alone and should therefore be attributed to enrollees. Note that statistical significance testing under the treatment-on-treated approach produces identical results to those obtained using the full screened sample, as described in detail in Appendix B. It is only the size of the impact estimates that are altered by concentrating effects among those enrolled, not their statistical significance.

Finally, owing to the method used for this analysis, the impact estimates themselves are, strictly speaking, applicable to defendants with a score of four on the SR risk assessment. Notably, however, there is evidence that the results of the analysis apply to a meaningfully wider range of risk scores (described further below and in Appendix B).

To help readers assess the generalizability of the impact results to the larger criminal caseload in New York City, Table 5.1 compares the personal and case characteristics of all defendants with custodial arraignments — those who were held at the time of their arrest — whose charges were eligible for SR during the impact study time frame, separated into four categories: (1) all charge-eligible arraigned cases, (2) unscreened cases, (3) screened but unenrolled cases, and (4) screened cases enrolled in SR.

Note first that defendants with screened cases (the impact sample) differed from those who were not screened for SR in several important ways. For example, screened defendants tended to be assessed as being at higher risk of new felony arrests while awaiting trial (based on their SR risk scores and risk levels) and as less likely to return to court if released (based on the higher proportion not recommended for ROR by the Criminal Justice Agency), and their cases included more serious charges (71 percent felonies — not shown in the table because it is calculated among enrolled and unenrolled defendants) compared with unscreened cases (41 percent felonies). Most screened cases were for felony drug and property charges, while most unscreened cases were for misdemeanor public order charges (such as disorderly conduct).[3] In sum, the impact sample (screened defendants) comprises a higher-risk group of defendants who

[2]For a detailed explanation of the treatment-on-treated statistical approach, see Appendix B.

[3]Misdemeanor public order charges are, not coincidentally, the types of cases most likely to be resolved at arraignment.

Table 5.1

Defendant and Case Characteristics by SR Screening and Enrollment Status, Among Cases Charge-Eligible for SR

Measure	All Custodial Arraignment Cases	Not Screened for SR	Screened for SR, Not Enrolled	Screened for SR, Enrolled
Defendant characteristics				
Age (%)				
16 to 19	7.8	7.9	7.4	7.4
20 to 29	35.7	36.3	32.3	30.5
30 to 39	25.4	25.5	26.0	23.0
40 or above	30.7	30.3	31.2	35.9
Unknown	0.4	0	3.1	3.1
Male (%)	82.9	82.6	84.9	83.9
Race/ethnicity (%)				
Hispanic of any race	34.7	34.7	34.1	34.6
Black, non-Hispanic	47.3	47.1	47.5	51.4
White, non-Hispanic	11.5	11.6	12.0	10.3
Other	6.5	6.6	6.4	3.8
Mean SR risk score	-5.6	-6.1	-1.9	-3.0
SR risk level (%)				
Low	30.8	33.5	15.1	10.7
Medium-low	22.2	23.2	14.5	18.5
Medium	29.2	28.4	29.7	41.4
Medium-high	11.7	10.3	18.5	24.1
High	5.6	4.6	19.2	2.1
Not recommended for ROR by the Criminal Justice Agency (%)	44.1	40.9	64.7	66.8
Case characteristics				
Charge class (%)				
Felony	45.3	41.3	69.5	73.1
Misdemeanor	51.4	55.9	23.0	21.6
Charge type (%)				
Drug	20.9	19.5	28.0	32.1
Property	22.8	20.7	37.9	34.3
Public order	38.1	41.3	17.2	16.6
Violent	15.0	15.8	9.4	11.6
Unknown	3.2	2.6	7.4	5.4

(continued)

Table 5.1 (continued)

Measure	All Custodial Arraignment Cases	Not Screened for SR	Screened for SR, Not Enrolled	Screened for SR, Enrolled
Case outcomes				
Arraignment outcome (%)				
ROR	59.1	64.1	44.5	0
SR	5.3	0	0	100.0
Bail set	14.5	12.0	50.3	0
Dismissed	12.4	14.2	0.4	0
Pled guilty	6.9	7.7	3.4	0
Other/unknown	1.8	2.0	1.4	0
Pretrial detention				
Detained at arraignment (%)	12.5	10.4	41.9	1.7
Average days detained	4.4	3.7	14.4	0.3
Case disposition (%)				
Convicted/pled guilty	56.7	55.3	68.4	61.5
Case dismissed	33.4	35.6	17.3	22.2
Pending disposition	9.4	8.7	13.3	15.1
Bench warrant issued (%)	5.5	4.6	8.5	15.8
Arrested for any new charge within 9 months (%)	25.9	23.7	39.8	41.0
Sample size	77,100	66,753	6,226	4,121

SOURCES: MDRC calculations based on data from the New York State Office of Court Administration, New York City Department of Correction, New York City Criminal Justice Agency, New York State Division of Criminal Justice Services, and SR providers.

NOTES: Sample includes all defendants with custodial arraignments in New York City for SR-eligible charges between May 2017 and April 2018. If a defendant had multiple qualifying arrests during this time frame, only the first case is included.

were facing more serious charges than was the case for unscreened defendants. This fact should not be surprising: Those characteristics make one more likely to receive bail at arraignment, and the screening approach for SR is designed to target defendants likely to have bail set.

Table 5.1 shows that, in fact, screened defendants who were not enrolled in SR were much more likely to have bail set at arraignment (50 percent) than were unscreened sample members (12 percent). In addition, nearly two-thirds (64 percent) of unscreened defendants were released on their own recognizance (ROR) at arraignment, compared with 45 percent of screened defendants who were not enrolled in SR. This pattern suggests that screening efforts were correctly directed toward a defendant population more likely to face bail. Furthermore, screened defendants who were not enrolled in SR were more likely than unscreened defendants

to be detained at arraignment (42 percent versus 10 percent), averaged longer periods of pretrial detention, and were more likely to be convicted. In contrast, there were few meaningful differences in defendants' demographic characteristics by screening status, suggesting that age, gender, and race/ethnicity did not influence SR screening decisions.

Overall, given the differences between the impact sample and most other defendants in New York City's criminal courts, the impact results described in this report may not represent what would occur if SR were available to most defendants, especially those who are at lower risk and face less serious criminal charges. In addition, the observed differences between screened and unscreened defendants has implications for interpreting outcome *levels* in the present analysis relative to those in other published data about New York City's full criminal caseload. Specifically, higher percentages of the impact (screened) sample had bail set, were detained while awaiting trial, and had pretrial failures (such as bench warrants and new arrests) than the rest of the criminal court caseload because screened defendants were at higher risk from the outset. Given New York State's 2020 bail reform implementation and subsequent rollbacks to portions of that legislation, the results for this higher-risk sample are particularly relevant to policymakers: Many policymakers are concerned that the elimination of bail for misdemeanors and nonviolent felonies may make defendants more likely to miss court appearances and incur new arrests, especially higher-risk defendants facing more serious charges.[4]

Note, lastly, that throughout the discussion of the present impact findings, the term "SR-eligible" represents members of the screened sample who were found to be eligible for SR based on the SR risk assessment, and the term "enrolled" represents members of the screened sample who were assigned to SR at arraignment.[5]

Overview of the Present Regression Discontinuity Design and Its Use in the SR Evaluation

The present analysis uses a regression discontinuity design (RDD) to assess the impacts of SR on defendant outcomes. Put simply, RDDs compare the outcomes of individuals who just met the eligibility criteria for a program (and therefore could have received program services) with those who just missed meeting those criteria (and therefore did not have access to program services). Because these two groups of defendants were comparable at the outset but differed in their potential access to a program, any differences in their future outcomes can be attributed to the program services with a high degree of confidence.

Put in more technical terms, RDD can be used in situations in which individuals are selected for participation in a program or set of services based on whether their values for a numeric rating fall just above or just below a certain "cut-point." An underlying premise of RDDs is that individuals just above and just below a cut-point are nearly identical, aside from exposure to the

[4]Rahman (2019).

[5]As noted earlier, to be fully eligible for SR, a charge-eligible defendant had to have an SR risk score of four or less and have an identifiable "community tie" (for example, a friend, family member, or case manager who could be reached).

program. Thus, any subsequent difference in outcomes can be attributed to the program. In the present analysis, the numeric rating is the SR risk score and the program is SR. Those with SR risk scores of four or below are eligible for the program and are therefore considered the "program group," while those with risk scores above four are ineligible and therefore comprise the "comparison group." As can be seen in Figure 5.1, which graphs the percentage of defendants in the impact sample who enrolled in SR by risk score, the determination of SR eligibility using a risk-score cut-point generates a large and abrupt "break," or discontinuity, in the probability of SR enrollment at a risk score of four. This discontinuity in enrollment between the program group who was eligible for SR and the comparison group who was not constitutes the SR participation contrast.

To estimate the SR participation contrast, Figure 5.1 includes a line that was fit through the program group points and a corresponding line that was fit through the comparison group points. The estimate of the impact of SR eligibility on the likelihood of enrolling in SR equals the vertical distance between the two lines at the cut-point risk score of four, labeled in red. For additional information regarding how to interpret the regression discontinuity figures in this report (including those in Appendixes B, C, and F), see Box 5.1.

- **Among those screened, the option of SR at arraignment increased enrollment in SR by about 55 percentage points for the program group (those who were eligible) versus the comparison group (those who were not eligible). This difference constitutes the "participation contrast" between the two groups.**

Table 5.2 shows the estimated impact of eligibility for SR on enrollment that was depicted visually in Figure 3. As can be seen, the impact estimate for a risk score of four (the RDD cut-point) is about 55 percentage points, meaning that the option of SR at arraignment increased enrollment in the program by that amount. This difference in enrollment between the program group and the comparison group quantifies the aforementioned participation contrast. Without this large participation contrast, there would be little reason to expect impacts.

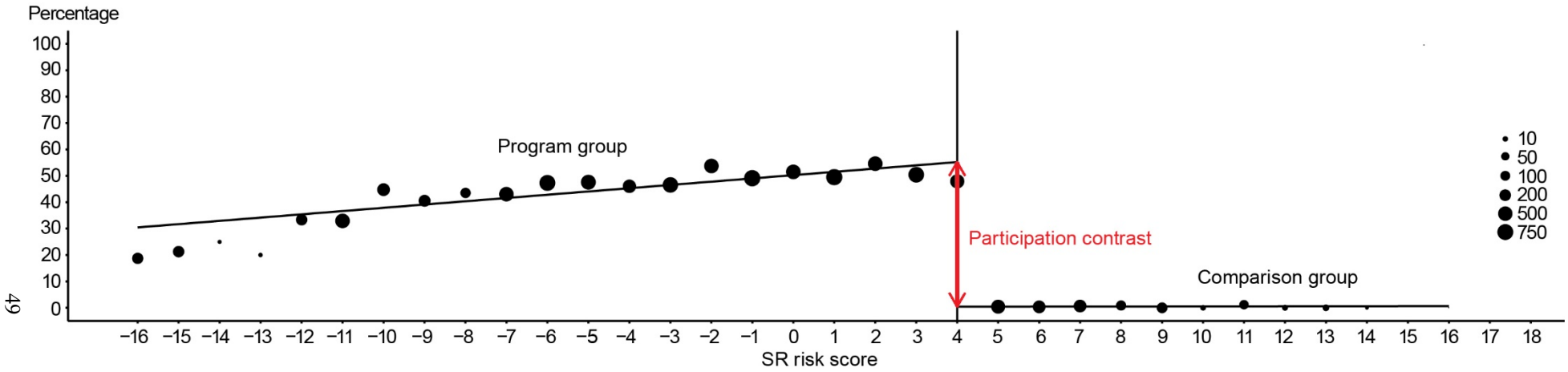
To present the most meaningful impact estimates possible, the SR analysis employs a treatment-on-treated approach (mentioned above and described in further detail in Appendix B) that estimates the impact for defendants who actually enrolled in SR, not all defendants who were screened and found eligible for SR.[6] This analysis is based on the premise that the effects of SR on defendant outcomes are produced solely through SR enrollment (a premise that is highly plausible, as explained in Appendix B). To calculate these treatment-on-treated estimates, the participation contrast of 55 percentage points is used to adjust the estimated impact of SR eligibility on defendant outcomes.

All impact estimates discussed throughout the remainder of this report represent the estimated effect of SR enrollment and are technically for defendants with a risk score of four, given

[6]For intent-to-treat impact estimates — which estimate the effects of SR for all screened defendants found eligible for the program, whether or not they actually enrolled — for all main outcomes, see Appendix D.

Figure 5.1

Percentage of Defendants Enrolled in Supervised Release, by SR Risk Score



SOURCE: MDRC calculations based on data from SR providers.

NOTE: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure.

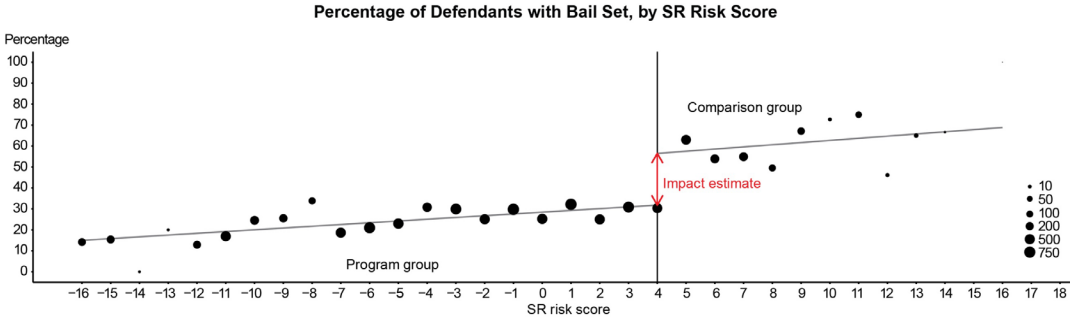
Box 5.1

How to Interpret Regression Discontinuity Figures

Regression discontinuity graphs in this report plot defendant outcomes by SR risk score, with the size of each point proportional to its sample size. Because only defendants with risk scores of four or less were eligible for SR, a risk score of four is the "cut-point" for the regression discontinuity design. Defendants with risk scores of four or less make up the "program group," and those with risk scores of more than four make up the "comparison group."

Lines are fit through the plotted points separately for the program group and the comparison group, accounting for the relative sample size at each point. The comparison group's predicted outcome at a risk score of four reflects the *mean counterfactual outcome*: that which would have occurred in the absence of SR eligibility (or, in a treatment-on-treated analysis such as the one presented in the impact tables in this chapter, that which would have occurred in the absence of SR enrollment). The difference between the outcome predicted by the program group line and the outcome predicted by the comparison group line for a risk score of four is the estimated impact of SR eligibility on that outcome for defendants with a risk score of four.

For example, consider the figure below, which plots by risk score the percentage of defendants who had bail set, with corresponding program group and comparison group lines. This plot illustrates the pronounced negative impact of SR eligibility on bail receipt for defendants with a risk score of four. The impact — which is almost 25 percentage points — is visible both from the pattern of points in the graph and from the vertical distance between the program group and comparison group lines at a risk score of four.



SOURCES: MDRC calculations based on data from the New York State Office of Court Administration and SR providers.

NOTES: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure.

the nature of the RDD method used to produce those estimates. However, the present data suggest that these findings generalize to a meaningful *range* of risk scores for most defendant outcomes. They do so because the discontinuity observed at the RDD cut-point for most outcome measures is reflected in similarly large and readily observable differences for a range of risk scores around the RDD cut-point.[7]

[7]For a more in-depth explanation of the generalizability of findings, including visual representations, see Appendix B. Additionally, Appendix C provides RDD graphs for each main outcome with red arrows to illustrate the generalizability range.

Table 5.2

Estimated Impact of SR Eligibility on SR Enrollment

Outcome (%)	Mean Program Outcome	Mean Counterfactual Outcome	Impact Estimate	P-Value	95 Percent Confidence Interval
Enrolled in SR	55.2	0.4	54.8***	< .00001	(51.03, 58.51)

SOURCE: MDRC calculations based on SR provider data.

NOTE: The estimated mean counterfactual outcome is that which would have occurred in the absence of SR eligibility. Statistical significance levels are indicated as follows: *** = 0.1 percent; ** = 1 percent; * = 5 percent.

Impacts on Defendant and Case Outcomes

Pretrial Release Outcomes

- **SR enrollment reduced the percentage of defendants for whom money bail was set, thus achieving its goal of serving as an alternative to bail. However, it simultaneously reduced the percentage of defendants who received ROR, exposing some defendants to SR's reporting requirements when otherwise they would have been released without added conditions.**

As discussed in Chapter 2, a judge determines a defendant's release status at the arraignment hearing. At this hearing, defendants who have been screened and found eligible for SR may: (1) be granted SR, (2) receive ROR (release with no conditions), (3) have bail set, (4) be remanded (detained without bail), or (5) have their cases resolved. (Note that the latter two possibilities are very rare for defendants in the impact [screened] sample).[8] SR is intended to serve as an alternative to bail, diverting eligible defendants to community-based supervision who would have had bail set otherwise (and typically have been detained while awaiting trial). Recall, however, that major stakeholders — particularly the defense bar — expressed concerns about the potential for "net widening" with the introduction of SR; that is, defendants who otherwise would have received ROR may have instead received SR, with its added requirements and risks, because judges preferred the extra assurances and services that SR offered.

As shown in Table 5.3 (which presents the estimated impacts on pretrial release outcomes for defendants *enrolled* in SR, as per the treatment-on-treated approach), SR eliminated bail for enrolled defendants (as reflected by the mean program outcome of 0 percent) while an estimated 45 percent of those enrolled in SR would have had bail set if SR did not exist (evidenced by the

[8]Defense attorneys were unlikely to pursue SR screening for clients with severe enough circumstances that remand was possible or for clients whose cases were likely to be dismissed or resolved in plea deals at the arraignment hearing. A defense attorney would be aware of both possibilities before arraignment.

Table 5.3

Estimated Impacts of SR Enrollment on Pretrial Release Conditions

Outcome (%)	Mean Program Outcome	Mean Counterfactual Outcome	Impact Estimate	P-Value	95 Percent Confidence Interval
Bail set	0.0	45.1	-45.1***	< .00001	(-63.58, -26.58)
Released on one's own recognizance (ROR)	0.0	44.0	-44.0***	< .00001	(-56.72, -31.19)

SOURCES: MDRC calculations based on data from the New York State Office of Court Administration and SR providers.

NOTES: The estimated mean counterfactual outcome is that which would have occurred in the absence of SR enrollment. Statistical significance levels are indicated as follows: *** = 0.1 percent; ** = 1 percent; * = 5 percent.

mean counterfactual outcome of 45 percent). Thus, SR enrollment reduced the percentage of defendants for whom money bail was set by about 45 percentage points relative to what would have occurred in its absence. This very large, highly statistically significant effect indicates that SR was successful in its mission of serving as an alternative to bail for many defendants. However, SR enrollment simultaneously reduced the percentage of defendants who received ROR by a similar amount, about 44 percentage points, affirming stakeholder concerns about net widening among SR enrollees.

In sum, SR diverted similar percentages of defendants from receiving bail and from receiving ROR. When assessing the latter finding, it is important to note that in practice, most defendants arraigned citywide were not considered for SR at arraignment and thus were unaffected by the existence of SR (and most received ROR). Nonetheless, avoiding net widening entirely would be nearly impossible, because it would require defense attorneys to anticipate judges' arraignment decisions perfectly. This reality may explain why the large, SR-induced decline in bail — a significant accomplishment of the SR program — was accompanied by a comparably sized SR-induced decrease in ROR.

Pretrial Detention

- **SR markedly reduced pretrial detention at arraignment for those enrolled in the program. On average for the full pretrial period — the time between arrest and case resolution — SR enrollees spent eight fewer days in jail than they would have in the absence of the program.**

Bail results in pretrial detention for defendants who cannot afford to pay the bail amount set as a condition of their release. For example, in 2017 (the most recent year for which data are available), 28 percent of New York City defendants whose cases continued past arraignment had

bail set and about 88 percent of these defendants were unable to pay bail at arraignment.[9] That is, the vast majority of defendants with bail set spent at least some time in pretrial detention until they were either able to make bail or were granted ROR or SR at a later court hearing.

SR's impact with respect to reducing the likelihood of having bail set had a corresponding effect on the likelihood of initial pretrial detention. As shown in Table 5.4, SR enrollment reduced the percentage of defendants who were detained immediately after arraignment by 34 percentage points.[10]

By reducing the proportion of defendants held on bail at arraignment, SR enrollment lowered the average number of days defendants spent in jail while their cases were pending: In the absence of SR enrollment, defendants would have spent eight additional days in pretrial detention. The measure of number of days detained while awaiting trial, shown in Table 5.4, includes all time defendants spent in jail between arraignment and case disposition.[11] Although SR defendants did not have bail set at arraignment,[12] there are other reasons defendants may have been detained after arraignment, such as violations of court rules that resulted in bail being set at later hearings. As evidenced by the declining mean counterfactual outcome levels for the bottom three descriptive measures presented in Table 5.4 that reflect duration of pretrial detention (*detained while awaiting trial 3 days or more, 8 days or more, and 30 days or more*), in the absence of SR enrollment relatively short stretches in pretrial detention were more common than longer ones. This pattern shows that SR enrollment caused the largest reductions in detention for shorter detention spells.

The next section of this chapter shows that SR enrollees experienced longer times to case resolution in addition to fewer days detained, exposing them to more potential for pretrial failures (that is, violations of court rules). Therefore, it is particularly notable that SR enrollees still spent fewer days in jail over the course of their pretrial periods than they would have absent the program. Defendants who avoid even short jail stays are less likely to suffer negative consequences such as loss of employment or housing.[13] Thus, SR's impact on pretrial detention may have farther-reaching effects on aspects of enrollees' lives that are not measured in this study.

Pretrial reforms that result in the release of additional defendants often ignite fears that, without detention, there will be an increased incidence of defendants missing court hearings,

[9]New York City Criminal Justice Agency (2019).

[10]Readers may expect the mean program outcome for the detained-at-arraignment measure to be zero, since by definition SR enrollees are not subject to bail and therefore should be released following their arraignment hearings. There are two reasons that it is not zero: (1) A small number of defendants in the impact sample were placed in SR at postarraignment court hearings, after previously having bail set; these individuals are included in the program group. (2) This outcome includes a small amount of measurement error due to the research team's strategy for matching the sample to jail data.

[11]This time is for the presenting case only; time in jail resulting from other criminal cases is not included in this measure.

[12]With the exception of the small number of defendants placed on SR at postarraignment hearings.

[13]Dobbie, Goldin, and Yang (2018); Digard and Swavola (2019).

Table 5.4

Estimated Impacts of SR Enrollment on Pretrial Detention

Outcome	Mean Program Outcome	Mean Counterfactual Outcome	Impact Estimate	P-Value	95 Percent Confidence Interval
Detained at arraignment (%)	2.3	36.5	-34.2***	< .00001	(-47.41, -21.08)
Number of days detained while awaiting trial	5.1	13.4	-8.3***	.00041	(-12.52, -4.05)
Detained 3 days or more while awaiting trial (%)	9.9	40.3	-30.4		
Detained 8 days or more while awaiting trial (%)	5.5	21.4	-15.9		
Detained 30 days or more while awaiting trial (%)	3.9	10.8	-6.9		

SOURCES: MDRC calculations based on data from the New York City Department of Correction and SR providers.

NOTES: The estimated mean counterfactual outcome is that which would have occurred in the absence of SR enrollment. Statistical significance levels are indicated as follows: *** = 0.1 percent; ** = 1 percent; * = 5 percent. The last three outcomes listed in this table (*detained while awaiting trial 3 days or more, 8 days or more, and 30 days or more)* are included to help unpack the overall effect on *days detained while awaiting trial*. However, these outcomes are not among the main impact outcomes for this analysis and were therefore not tested for statistical significance.

absconding (failing to return to court to face their criminal charges), or committing new crimes. The next few sections of this chapter explore the impacts of SR on bench warrants for failure to appear in court, new arrests, and case outcomes. In light of SR's sizable reduction in pretrial detention, its impacts on these subsequent outcomes may offer insights into what other jurisdictions can expect if they enact bail reforms that lead to higher rates of pretrial release.

Pretrial Exposure and Court Appearance

- **Despite increasing defendants' days of exposure to pretrial risk, SR enrollment does not appear to have increased failures to appear in court by a substantial amount, if at all.**

In New York State, the legal justification for setting bail for a defendant is to help ensure that the defendant returns to court to face charges. Thus, as an alternative to bail, one of the main goals of the SR program is to maintain defendants' court appearance rate without reliance on monetary bail or pretrial detention.

At the same time, previous research suggests that defendants' ability to avoid bail and pretrial detention — and therefore to maintain their freedom while their cases are open — may lengthen the time it takes to resolve cases, as defendants have less incentive to plead guilty in order to obtain releases from jail.[14] Speedy trial requirements that apply to detained defendants, but not released defendants, may also lead to released defendants having longer case-processing times. The additional time for cases to resolve in turn increases the pretrial period during which defendants are at risk of failing to comply with the terms of their release, whether through missing required court appearances (of which they are likely to have more, given the longer pretrial period), new arrests, or other violations. Reduced detention also increases exposure to pretrial risk, as those detained generally cannot fail to appear for court hearings or incur new charges.[15]

To illustrate SR enrollment's effects on pretrial exposure and appearances for court hearings, Table 5.5 presents estimated impacts on three pertinent outcomes: the length of the pretrial period, time exposed to pretrial risk, and the issuance of bench warrants (for failure to appear or, less frequently, other violations of release conditions).

Findings in Table 5.5 indicate that SR enrollment increased the average length of the pretrial period (time to case resolution) from 86 days to 143 days, or by close to two months. To fully consider differential time at risk of pretrial failures among SR enrollees, information regarding the length of pretrial periods was combined with information regarding days incarcerated during those pretrial periods (taking into account all possible defendant jail time, not just pretrial detention on the presenting case, as in the previous section) to produce a measure of days exposed to pretrial risk (or, put another way, pretrial days spent in the community). As can be seen, SR enrollment doubled time exposed to pretrial risk from 64 days to 128 days due to the combination of longer case duration and defendants not being in jail.

Bench warrants are primarily issued by judges when defendants fail to appear for required court dates, though they can also be issued for other forms of noncompliance with release conditions. For example, SR participants who miss required check-ins with their case managers and do not make contact within 48 hours of the missed appointment can be issued bench warrants. Unfortunately, given available data, there is no way to isolate bench warrants issued for failure to appear in court from those issued for other forms of noncompliance. However, court administrators and SR provider staff members report that bench warrants issued for noncompliance other than failures to appear are rare. Thus, bench warrants are considered a good proxy for court appearance.

[14]Dobbie, Goldin, and Yang (2018). Upon conviction, some defendants receive sentences such as community service, probation, or time served, so pleading guilty may not mean continued incarceration. Even for those facing jail or prison sentences, entering into a plea bargain often comes with the promise of reduced jail or prison time relative to what one might receive if one does not accept the plea offer.

[15]If a defendant is held in jail at the time of a required court appearance, he or she will be brought to court for the hearing by the New York City Department of Correction, making a failure to appear a near impossibility. Defendants who are not in jail may forget, have personal or family crises, abscond, or otherwise fail to appear.

Table 5.5

Estimated Impacts of SR Enrollment on Pretrial Exposure and Bench Warrants Issued for Missed Court Appearances

Outcome	Mean Program Outcome	Mean Counterfactual Outcome	Impact Estimate	P-Value	95 Percent Confidence Interval
Length of the pretrial period (days)	143.4	86.1	57.3**	.00382	(20.11, 94.55)
Time exposed to pretrial risk (days)	127.5	63.8	63.7***	< .00001	(34.96, 92.43)
Bench warrant issued (%)	33.7	24.0	9.7	.06099	(-0.48, 19.90)

SOURCES: MDRC calculations based on data from the New York State Office of Court Administration, New York City Department of Correction, and SR providers.

NOTES: The estimated mean counterfactual outcome is that which would have occurred in the absence of SR enrollment. Statistical significance levels are indicated as follows: *** = 0.1 percent; ** = 1 percent; * = 5 percent.

Bench warrants issued after sentencing were excluded from the measures in this table as those typically reflect a defendant's failure to appear before a cashier to pay fines or fees rather than a failure to appear for a court hearing in front of a judge.

As shown in Table 5.5, the estimate of the impact of SR enrollment on the percentage of defendants who received bench warrants at any time while their cases were open is an increase of about 10 percentage points.[16] However, this estimate is not statistically significant and thus contains considerable uncertainty. Furthermore, it does not account for the large increase in the number of days in the community at risk of pretrial failure, including a probable corresponding increase in the number of required court appearances.[17]

Considering all of the available evidence together, although enrollment in SR markedly increased the average length of the pretrial period for defendants (from 86 to 143 days) and *doubled* the amount of pretrial time they spent in the community (from 64 days to 128 days) — probably increasing their number of required court appearances substantially — the percentage of defendants who received bench warrants only increased from 24 percent to 34 percent. Proportionally, the estimated increase in time at risk is far greater than the estimated increase in bench warrants. Hence, when accounting for the time at risk of pretrial failure and the increased number of opportunities to miss court appearances, enrollment in SR does not appear to have increased

[16]MDRC created the *bench warrant issued* measure following the guidance of the New York State Office of Court Administration, and that guidance differs slightly from the approach taken by the New York City Criminal Justice Agency. As a sensitivity check, MDRC created a version of this measure according to the Criminal Justice Agency approach and estimated SR's effects on this new version. The results showed little meaningful difference from the original.

[17]Due to data limitations, it was not possible to identify the number of required court appearances for the cases of defendants in the sample. Therefore, while this is a well-supported assumption based on knowledge of criminal case processing in New York City, the research team was unable to confirm it empirically.

failures to appear to court appreciably, if at all. This pattern of findings suggests that the SR program approximated its goal of maintaining defendants' appearances at court hearings while reducing the use of money bail and pretrial detention.

New Arrests

- **During the first nine months after defendants were arraigned, SR enrollment did not increase the likelihood of new arrests overall or for any charge class, despite its reduction in rates of pretrial detention.**

In addition to maintaining defendants' appearances at court hearings, SR is also concerned with preserving public safety while defendants wait for their cases to be resolved. To gauge the impact of SR enrollment in this area, Table 5.6 presents estimated impacts on new arrests during the follow-up period for the present analysis: the first nine months after defendants were arraigned. As can be seen, enrollment in SR did not increase the likelihood of being arrested for a subsequent crime, overall or for any charge class. Specifically, although these differences are not statistically significant, the present findings suggest that enrollment in SR produced a 5 percentage point reduction in the likelihood of any subsequent new arrest, a 10 percentage point reduction in the likelihood of a new misdemeanor arrest, a 1 percentage point increase in the likelihood of a new felony arrest, and a 3 percentage point increase in the likelihood of a new violent felony arrest (from 6 percent to 9 percent, illustrating the relative rarity of such crimes). These findings are particularly encouraging in light of SR's marked reduction in rates of pretrial detention.[18] In sum, SR appears to have achieved its goal of preserving public safety (to the extent that new arrests reflect public safety).

Table 5.6

Estimated Impacts of SR Enrollment on New Arrests

Outcome (%)	Mean Program Outcome	Mean Counterfactual Outcome	Impact Estimate	P-Value	95 Percent Confidence Interval
New arrest within 9 months					
Any charge	66.5	71.6	-5.1	.24984	(-14.06, 3.81)
Misdemeanor	49.8	59.5	-9.7	.15146	(-23.13, 3.77)
Felony	37.6	36.7	.8	.78499	(-5.32, 6.98)
Violent felony	9.0	6.1	2.9	.05890	(-0.12, 5.92)

SOURCES: MDRC calculations based on data from the New York State Office of Court Administration and SR providers.

NOTES: The estimated mean counterfactual outcome is that which would have occurred in the absence of SR enrollment. Statistical significance levels are indicated as follows: *** = 0.1 percent; ** = 1 percent; * = 5 percent.

[18]This same result holds when the common follow-up period was set at four months instead of nine months. When the follow-up period used is the pretrial period (the time until cases are resolved), the analysis does detect statistically significant increases in new arrests, but these increases are a result of SR enrollees' lengthier pretrial periods. For a detailed look at impacts on new arrests during the pretrial period, see Appendix Table A.5.

Case Outcomes

- **SR enrollment increased case dismissals and correspondingly decreased guilty findings.**

As noted above, research has suggested that a defendant's ability to avoid bail and pretrial detention can affect the outcome of a case by lessening the incentive to plead guilty to obtain release from jail.[19] This pattern was observed in the present analysis: Table 5.7 shows that SR enrollment increased the proportion of defendants whose cases were dismissed by about 10 percentage points and reduced the proportion of defendants found guilty by about 11 percentage points. (Virtually no one in the impact sample was found not guilty, meaning case dismissals and guilty findings are the two potential — and opposing — case outcomes.)

Table 5.7

Estimated Impacts of SR Enrollment on Case Outcomes

Outcome (%)	Mean Program Outcome	Mean Counterfactual Outcome	Impact Estimate	P-Value	95 Percent Confidence Interval
Dismissed	22.7	12.8	9.9**	.00327	(3.59, 16.19)
Found guilty	76.8	87.8	-11.0**	.00195	(-17.60, -4.41)

SOURCES: MDRC calculations based on data from the New York State Office of Court Administration and SR providers

NOTES: The estimated mean counterfactual outcome is that which would have occurred in the absence of SR enrollment. Statistical significance levels are indicated as follows: *** = 0.1 percent; ** = 1 percent; * = 5 percent.

The probable explanation for these findings is the improved bargaining position of defendants who were released rather than detained. Released defendants — such as those on SR — may be more inclined to wait for better plea deals, or even for dismissals. It is likely that in some such cases, the defendants were innocent of their charges and avoiding pretrial detention allowed them not to plead guilty to crimes they did not commit. In other cases, the defendants might have been guilty, but the prosecution's cases were weak, resulting in dismissals (whereas if the defendants had been detained, they may have been compelled to enter into plea deals regardless of the strength of the evidence against them).

[19]Dobbie, Goldin, and Yang (2018).

Subgroup Impacts

- **SR enrollment's effects on money bail, pretrial detention, bench warrants, and new felony arrests did not differ meaningfully among defendants of different races/ethnicities or ages. However, SR enrollment had stronger effects on bail and pretrial detention for felonies than misdemeanors, and stronger effects in Manhattan than the Bronx, Brooklyn, or Queens.**

This section examines variation in the impacts of SR enrollment on defendant outcomes by subgroups of defendants based on charge class of offense (misdemeanor versus felony), borough of New York City,[20] race/ethnicity,[21] and age (16 to 24 versus 25 and older). These subgroups were chosen before the impact analysis was conducted, based on theoretical expectations that certain groups could experience different impacts of SR enrollment. These differences could occur because of differences in how their cases were processed by the New York City court system (for example, by charge class or by borough), because the characteristics of defendants affected how their cases were treated in the court system, or because their characteristics led them to respond differently to SR.

Subgroup impacts were estimated for four primary defendant outcomes: bail receipt, pretrial detention at arraignment, bench warrant receipt, and new felony arrests during a nine-month follow-up period. The veracity of *differences* between or among subgroup impacts was assessed based on statistical significance, visual inspection of RDD graphs to confirm discontinuities, and theoretical backing, to guard against the possibility of drawing incorrect conclusions given the relative imprecision of RDDs (which makes it difficult to detect small differences) and the smaller sample sizes of various subgroups.

There were no meaningful, statistically significant differences in impacts for any of these four outcome measures by defendants' race/ethnicity or age, nor were there any meaningful impact differences for bench warrants or new arrests by charge class or borough. All subgroup impact estimates are presented in Appendix E. Table 5.8 presents estimated impacts of SR enrollment on bail receipt and pretrial detention at arraignment, by the charge class of the presenting case in the analysis. Similarly, Table 5.9 presents the estimated impacts of SR enrollment on those same outcomes by borough.

Table 5.8 shows that the effect of SR enrollment on bail setting and detention at arraignment was larger for felony cases than misdemeanors. These impact differences did not quite reach statistical significance; however, examination of the respective RDD graphs indicated a clear discontinuity and the observed pattern fits with expectations: Monetary bail is set more frequently for more serious charges (and the amount of bail set is higher), and as a result there is more pretrial detention for more serious charges. Thus, SR had more room to reduce bail and pretrial detention for felony cases than misdemeanor cases.

[20]Excluding Staten Island due to its small sample size.

[21]Black, Hispanic, or White, excluding other racial/ethnic groups due to their small sample sizes.

Table 5.8

Estimated Impacts of SR Enrollment on Bail Setting and Pretrial Detention, by Charge Class of Offense

Outcome	Mean Program Outcome	Mean Counterfactual Outcome	Impact Estimate	P-Value	95 Percent Confidence Interval	Difference Between Subgroup Impacts
Misdemeanor						
Bail set	0.0	33.7	-33.7*	.01316	(-58.51, -8.96)	
Detained at arraignment	0.0	25.2	-25.2*	.02634	(-46.07, -4.27)	
Felony						
Bail set	0.0	54.0	-54.0***	< .00001	(-63.62, -44.28)	
Detained at arraignment	3.5	48.2	-44.7***	< .00001	(-49.98, -39.41)	

SOURCES: MDRC calculations based on data from the New York State Office of Court Administration, New York City Department of Correction, and SR providers.

NOTES: The estimated mean counterfactual outcome is that which would have occurred in the absence of SR enrollment. Statistical significance levels are indicated as follows: *** = 0.1 percent; ** = 1 percent; * = 5 percent. Impacts were calculated separately for each subgroup. Impact estimates were then examined for statistically significant differences between subgroups. Statistically significant differences between subgroups are indicated as: ††† = 0.1 percent; †† = 1 percent; † = 5 percent.

As described above, SR significantly reduced bail setting and pretrial detention citywide. Table 5.9 shows that it had the same types of impacts in each of the four boroughs analyzed individually. The largest effects were observed in Manhattan, where SR reduced bail setting by 64 percentage points, compared with an average reduction of 44 percentage points across the Bronx, Brooklyn, and Queens (not shown separately). Similarly, SR reduced pretrial detention by 62 percentage points in Manhattan, compared with an average impact of 28 percentage points across the other boroughs analyzed (not shown separately). Manhattan had higher rates of bail setting and pretrial detention than the other boroughs, which probably allowed SR enrollment to have a larger effect.

Table 5.9

Estimated Impacts of SR Enrollment on Bail Setting and Pretrial Detention, by Borough

Outcome	Mean Program Outcome	Mean Counterfactual Outcome	Impact Estimate	P-Value	95 Percent Confidence Interval	Difference Among Subgroup Impacts
Bronx						
Bail set	0.0	43.1	-43.1***	.00016	(-62.18, -23.96)	
Detained at arraignment	2.2	34.3	-32.1***	.00054	(-47.99, -16.25)	†††
Brooklyn						
Bail set	0.0	45.4	-45.4***	< .00001	(-61.77, -28.99)	
Detained at arraignment	2.3	26.8	-24.5**	.00598	(-40.52, -8.46)	†††
Manhattan						
Bail set	0.0	64.4	-64.4***	< .00001	(-73.52, -55.23)	
Detained at arraignment	3.3	65.1	-61.8***	< .00001	(-71.41, -52.17)	†††
Queens						
Bail set	0.0	42.1	-42.1*	.01938	(-75.14, -8.99)	
Detained at arraignment	0.0	26.7	-26.7*	.03898	(-50.81, -2.63)	†††

SOURCES: MDRC calculations based on data from the New York State Office of Court Administration, New York City Department of Correction, and SR providers.

NOTES: The estimated mean counterfactual outcome is that which would have occurred in the absence of SR enrollment. Statistical significance levels are indicated as follows: *** = 0.1 percent; ** = 1 percent; * = 5 percent. Impacts were calculated separately for each subgroup. Impact estimates were then examined for statistically significant differences among subgroups. Statistically significant differences among subgroups are indicated as: ††† = 0.1 percent; †† = 1 percent; † = 5 percent. Staten Island was excluded from the analysis due to its small sample size.

Chapter 6

Summary Discussion and Policy Implications

This report presents results from the first independent evaluation of the implementation and impacts of New York City's citywide pretrial Supervised Release (SR) program. Lessons from this evaluation will help criminal justice policymakers nationally and in New York statewide as the state implements changes to its pretrial system that limit the use of monetary bail for many cases involving misdemeanor and nonviolent felony charges. New York City has expanded the SR program significantly to support these changes.

The Mayor's Office of Criminal Justice developed the citywide SR program to reduce the number of defendants detained in jail because they could not afford to pay the monetary bail that was set as a condition for their release. Many jurisdictions across the nation are implementing pretrial supervision programs as part of similar pretrial system reforms. Most of these reforms share a common goal of reducing the unnecessary detention of defendants who pose little risk of failing to appear for their court dates or committing new crimes if released while awaiting trial. Underlying these efforts are concerns that the current money bail system favors defendants with the economic means to pay for their freedom while similar defendants without the ability to pay sit in jail. Pretrial detention also has implications for the outcomes of criminal cases. Specifically, defendants who are detained are more likely than others to plead guilty (whether they are in fact guilty or not) and thus are more likely to be convicted of their charges. In addition, defendants who are detained in jail before their trials are put at greater risk of future incarceration and personal hardships such as loss of employment and family ties.[1] Black and Latino defendants, who are disproportionately subject to pretrial detention due to racial discrimination in policing and in pretrial release conditions, among other racial inequities throughout the criminal justice system, are far more likely to experience these harms than comparable White defendants.[2]

The findings presented in this report offer strong evidence that SR was successful at achieving its overarching goals of reducing the use of money bail and pretrial detention, while maintaining court appearance rates and preserving public safety.

Specifically, the present evidence suggests that enrollment in SR did not increase subsequent new arrests during the follow-up period used in the analysis (the first nine months after each defendant was arraigned), overall or for any type of charge. Assessing the impact of SR on bench warrants for missed court hearings was more complicated (as described further below). Specifically, SR defendants experienced more exposure to pretrial failure (such as failure to appear to court hearings) because their cases were open for much longer and, accounting for pretrial detention, they

[1]Heaton, Mayson, and Stevenson (2017); Leslie and Pope (2017).

[2]Pretrial Justice Institute (2020); Sentencing Project (2018).

spent twice as many days in the community while their cases were pending. The results presented in Table 5.4 above show that there was little observable effect of SR on bench warrants for missing court appearances, particularly when considering the increased time defendants spent at risk (and implicitly, the added number of court appearances probably required per case).

Concerns about the potential for net widening (that is, imposing reporting conditions on defendants who would have been released without conditions otherwise) did prove to be warranted: About 44 percent of SR enrollees would have been released on their own recognizance (ROR) if SR had not existed. However, this net widening was not widespread in the total New York City defendant population because SR was not presented as an option at arraignment hearings for the vast majority of eligible defendants during the time frame examined. The majority of all defendants in New York City were released on their own recognizance after SR was implemented, as was true before SR rolled out. As shown in Table 3.1, fewer than 10 percent of charge-eligible defendants were even considered for SR (screened), and only about 4 percent of charge-eligible defendants were ultimately enrolled in the program (not shown). However, following changes to New York City's pretrial process resulting from the statewide bail reform, all defendants can now be considered for SR. It will be important for the city to implement strategies to protect against widespread net widening moving forward, given the increased conditions and risks it places on individuals awaiting trial.[3]

SR was designed for defendants who would have had bail set, typically those with more serious charges and those at higher risk for pretrial failures. As shown in Table 5.1, among those whose charges made them eligible for the program, higher-risk defendants with more serious charges comprised the majority of the cases enrolled in SR (73 percent were charged with felonies, most were at medium or medium-high risk for incurring new felony charges while awaiting trial, and about two-thirds were not recommended for ROR by the Criminal Justice Agency based on their elevated risk of failure to appear in court or for reasons such as having an active bench warrant). Thus, on average, SR served its intended target population.

Implementation Study Lessons

While New York City recognized the need to reduce the use of monetary bail, the city also determined that there had to be alternative options available to judges and other stakeholders to give them confidence that a broader swath of defendants could be released safely, with some assurances that they would appear at future court hearings. When making the decision to introduce SR more widely throughout New York City, stakeholders grappled with an added complication: Unlike judges in most other jurisdictions, judges in New York City may not legally consider the risk that a defendant may pose to public safety if released at arraignment. The only risk assessment that was conducted at the time, the one conducted by the Criminal Justice Agency, predicts the risk of failing

[3]In tandem with bail reform, the New York City Criminal Justice Agency began using an updated assessment that has greatly increased the proportion of cases recommended for ROR. The new assessment was developed based on a strategy of recommending as many individuals for release as possible while maintaining the city's high court appearance rate. The use of this new assessment may help counter the expanded potential for net widening.

to appear at court hearings. SR stakeholders felt that if they were to take on the responsibility of supervising defendants awaiting trial, they had to be able to assess a defendant's risk of incurring new felony charges. Additionally, stakeholders desired a tool that would help them better pinpoint a population of moderate-risk defendants with whom to begin the new SR program, with the goal of expanding to higher-risk defendants once they had a proven model (as they ultimately did with young people in the Youth Engagement Track expansion described in Chapter 4). These considerations led to the incorporation of a separate risk assessment, the SR risk assessment, that was integrated into the process between arrest and the arraignment hearing. This SR risk assessment is designed to predict the risk of being arrested for new felonies while awaiting trial.

- **Only a small fraction of defendants had SR presented as an option at the arraignment hearing.**

As stakeholders were deciding on a process for SR eligibility screening, they felt it was important that the procedures not place an additional burden on court staff members who operate under the pressures of large caseloads and short time frames to prepare for arraignment. Therefore, court liaisons from the SR providers were to be present at every arraignment shift in each borough's main criminal courthouse to shoulder any additional workload generated by SR screening. As discussed in Chapter 2, because New York City arraignment shifts operate all day and most of the night (until one a.m.) in each borough's main criminal courthouse except Staten Island's, SR screening was a significant undertaking. An additional consideration when developing SR procedures was to ensure that the arraignment judge not be given any information about the defendant's assessed risk of committing a new felony crime if released while awaiting trial, which meant that information from the SR assessment could not be shared with the arraignment judge. This set of circumstances led to a complicated screening process that left the important decision about whether SR could be considered for a defendant largely in the hands of public defenders. It also resulted in only a small fraction of charge-eligible defendants being considered for SR at arraignment (which aligned with the intention of the program to target only moderate-risk defendants who, it was believed, would otherwise have money bail set).

Despite the clear procedural guidelines for SR, and the presence of SR court liaisons at every arraignment shift for screening, there was still uncertainty about which types of defendants would end up having access to the program. It was also uncertain how judges would balance their need for discretion with the guidelines about assigning SR only in situations when bail would have been set. During the study time frame, defense attorneys had the difficult task of trying to predict what conditions judges would impose on their clients' release. In particular, they considered whether giving a judge the option of SR could result in conditions being imposed on a defendant who might otherwise have been released without conditions.

- **Some defendants were enrolled in SR who would have been released without conditions otherwise. Nonetheless, SR was largely successful in enrolling its intended target population of moderate-risk defendants. Most defendants who enrolled in SR were charged with felonies and were at higher risk of missing court appearances and being arrested for**

new felonies while awaiting trial than defendants who were not considered for the program.

As illustrated by the impacts on pretrial release conditions presented above, there were some defendants assigned to SR who otherwise would have been released without conditions (about 44 percent of SR enrollees would have been released on their own recognizance in the absence of SR). A similar proportion of defendants who enrolled in SR would have had bail set in the absence of the program (45 percent). Nonetheless, because SR enrollment only affected a very small proportion of cases in the New York City court system, the overwhelming majority of release conditions at arraignment were unaffected by the introduction of the program. Consequently, net widening was not an extensive problem during the study time frame.

Specifically, most defendants were excluded from consideration for SR because defense attorneys only requested screening for a small fraction of charge-eligible cases (fewer than 10 percent). Therefore, the SR option was not presented to judges at arraignment hearings for over 90 percent of defendants. Furthermore, most defendants were released on their own recognizance, as was true before SR existed.

Thus, it appears that the SR screening process and the discretion given to defense attorneys in determining access to SR was largely successful in making sure SR reached the intended target population for the program: moderate-risk defendants and those facing more serious charges (nearly three-fourths of SR enrollees were charged with felonies).

Furthermore, despite the variation across boroughs and program providers in court contexts, cultures, staffing, and the overall complexity of the process for determining if and when SR would be presented as an option, the present study found that the proportion of defendants who were ultimately screened and placed on SR was generally consistent (though Staten Island differed from the other four boroughs somewhat). There also appeared to be few meaningful differences across boroughs in the characteristics of defendants enrolled in SR.

In summary, moderate-risk defendants facing more serious charges were more likely to be considered for SR (that is, to be screened) and ultimately to be enrolled in the program. Most defendants who were not screened received ROR or had their cases resolved at arraignment through a dismissal or guilty plea. This pattern is largely aligned with policy guidelines that assert that release conditions should be consistent with the risk level of the defendant.[4]

- **When SR was presented as an option at arraignment hearings, enrollment was high.**

When SR was rolled out in 2016 it was a new option for most arraignment judges, since the program operated on a relatively small scale before its citywide implementation. Thus, it was not certain that judges would know enough about the program to feel comfortable using it in lieu of bail to ensure defendants returned to court hearings and did not incur new arrests. Although the program was not widely taken up by defense attorneys for reasons described

[4]National Association of Pretrial Services Agencies (2004); American Bar Association (2007).

above, judges did use it when presented with the option. Results offered in Chapter 3 show that judges assigned more than half of defendants to SR when presented with that option at arraignment hearings.

- **Once defendants were on SR, their supervision requirements usually aligned with those recommended by the SR supervision framework, which took into account defendants' risk levels, charge types, and any aggravating factors. This alignment suggests that providers followed the program guidelines closely when setting clients' reporting conditions.**

A defendant's score on the SR risk assessment was used to determine eligibility for the program and, along with a couple of other factors, how often the defendant would have to report to supervision if assigned to the program. Thus, one component of the implementation study examined whether the supervision levels and reporting frequency for SR defendants appeared to be implemented with fidelity to the SR supervision framework. The analysis confirmed that SR provider staff members set supervision levels at intake as intended, by using the framework: Once defendants were enrolled in SR, their supervision levels aligned with the levels recommended by the framework over 90 percent of the time. When the conditions were not aligned, it was most often the case that the SR reporting conditions were stricter than what was recommended.

- **SR emphasized clients' social service needs, an aspect of the program that made it appealing to some judges.**

New York City's approach to supervised release differs from that of many other jurisdictions in that the program is operated by community-based providers and strongly emphasizes counseling, case management, and voluntary connections to services to address clients' underlying needs. SR is staffed by trained social workers, clinicians, peer specialists, and others. (Many other jurisdictions that operate supervised release programs house them within an office of the courts or probation and focus more on monitoring and compliance than case management.)

Impact Study Lessons

One of New York City's primary goals in implementing SR was to provide an alternative to money bail, and in doing so, to reduce unnecessary detention for defendants who did not pose a serious risk to the court system or the public if released. At the same time, the city understood that releasing more defendants means more opportunity for pretrial failures. An underlying concern regarding SR was that releasing more defendants could mean higher rates of missed court dates or new criminal charges, even if defendants received more supervision support services.

- **SR enrollment substantially reduced money bail and pretrial detention.**

Among those defendants who were enrolled in SR, 45 percent would have been given bail if SR had not existed. Consequently, SR enrollment led to a large reduction in the proportion of defendants detained in jail after their arraignment hearings.

- **SR enrollment led to greater exposure to pretrial failures for SR enrollees because it reduced jail detention and simultaneously lengthened the amount of time that cases were open. In turn, this increased exposure probably increased the number of court appearances that were required in at least some of the cases.**

SR enrollment led to a reduction in pretrial detention and a large increase in the amount of time it took for cases to reach resolution (probably because of a combination of decreased incentive to plead guilty to obtain release from jail and laxer speedy trial requirements for released defendants). As a result, SR enrollees were exposed for a longer time to the risks of pretrial failures. One can only break a court rule, such as missing a court hearing, during the time that one is exposed to such court rules — that is, when one has an open case. This issue is critical when measuring the impacts of SR on outcomes such as bench warrants. Because the pretrial period was about two months longer for SR enrollees, it became an important challenge for the evaluation to attempt to disentangle the impact of the SR program on defendants attending their court appearances from the impact of defendants spending more time at risk.

- **SR produced no substantial or statistically significant increase in arrests for new crimes during the nine months after the initial arrest.**

The approach to isolating the impact of SR on new arrests was relatively straightforward. Rather than focus on the pretrial period — which is subject to wide variation across individual defendants, and which was dramatically affected by SR — the present analysis assesses the impact on new arrests for a nine-month follow-up period that is common to all defendants. These findings indicate that enrollment in SR did not produce a substantial or statistically significant increase in new arrests overall or by type of charge.

- **SR had no effect on bench warrants issued for failure to appear, which is notable, especially when considering that SR doubled the number of days that defendants were in the community with open cases and thus at risk of failing to make required court appearances.**

It is more difficult to isolate the effects of SR enrollment on bench warrants from the increased time at risk for pretrial failures that occurred for SR defendants because, unlike new arrests, bench warrants can only occur while a case is pending.. The impact analysis had to consider the additional exposure SR defendants had in the form of more time in the community and, especially, longer pretrial periods (probably with more court dates) alongside the estimated effect of SR on bench warrants. The study found that SR enrollment did not have a statistically significant effect on the likelihood of receiving a bench warrant for failing to appear in court, even though SR enrollees spent twice as much time at risk. In other words, the best evidence suggests that defendants enrolled in SR were not more likely to have a bench warrant issued for missing a court date.

- **Defendants enrolled in SR were less likely to be convicted and more likely to have their cases dismissed.**

Defendants who are detained while awaiting trial will often plead guilty to their charges without extensive negotiation because there is an incentive in the form of quicker release if they do so. Because SR reduced pretrial detention, it also reduced the incentive for defendants to plead guilty and lengthened the time to case disposition. This circumstance probably made it more difficult for prosecutors to obtain guilty pleas for cases, requiring them to conduct more thorough investigation and build more substantial evidence to support prosecutions. This combination of factors ultimately resulted in SR reducing convictions and increased the rates of case dismissal, meaning the prosecution dismissed charges or the judge determined the evidence was not sufficient for the case to proceed.

- **A new version of SR went into effect with New York's bail reform that is more expansive than the version of the program studied in this evaluation. However, the results from the present evaluation remain highly relevant to policymakers.**

Once New York's bail reform legislation took effect in January 2020, the vast majority of defendants were no longer eligible for bail based on their charges (excepting those arrested for most violent felony offenses). Instead they had to be released without monetary conditions, effectively limiting judges' options to ROR or SR. All defendants became eligible for SR at arraignment, with no exclusions based on charge or risk. These changes led to a significant expansion of the SR program: SR began serving both a larger number of defendants and defendants with different characteristics and types of cases than in the past (until the COVID-19 pandemic temporarily disrupted SR enrollment beginning in March 2020). Rollbacks to portions of the original bail reform legislation that went into effect in July 2020 may result in further changes to SR, though the program will probably continue to serve a larger, more varied caseload than it did before bail reform. Although this study cannot speak directly to the impact SR will have moving forward, the results presented in this report remain highly relevant as policymakers consider tools to support the goals of bail reform.

Appendix A

Miscellaneous Supplemental Exhibits

Appendix Table A.1

Supervised Release (SR) Risk Score Computation

Risk-Prediction Factor	Risk Points
Age at current arrest	
16 to 19	6
20 to 29	1
30 to 39	-3
40+	-4
Open cases	
No	-1
Yes	1
First arrest	
No	3
Yes	-3
Any warrants in the last 4 years	
No	-1
Yes	1
Misdemeanor conviction in the last year	
No	-2
Yes	2
Felony conviction in the last 9 years	
No	-1
Yes	1
Drug conviction in the last 9 years	
No	-2
Yes	2
Reports full-time activity (work, school, training, or caregiving)	
No	2
Yes	-2

SOURCE: New York City Mayor's Office of Criminal Justice.

Appendix Table A.2

SR Client Characteristics at Program Entry, by Borough

Characteristic	Citywide	Brooklyn	Bronx	Manhattan	Queens	Staten Island
Age	35.5	37.0	38.3	35.6	31.0	35.3
Gender (%)						
Male	83.3	84.4	86.7	78.0	84.7	82.1
Female	16.1	14.8	12.4	21.4	15.0	17.6
Other	0.6	0.8	0.8	0.6	0.3	0.3
Race/ethnicity (%)						
Hispanic of any race	35.8	28.5	46.8	39.6	37.1	23.3
Black, non-Hispanic	45.2	52.1	35.3	48.7	41.3	40.9
White, non-Hispanic	8.9	8.8	3.2	6.9	8.1	32.5
Other	10.1	10.7	14.7	4.8	13.6	3.3
Housing status (%)						
Private or market-rate housing	56.8	45.8	59.3	47.7	73.8	77.3
Affordable housing	23.0	28.2	18.8	29.3	16.8	9.4
Shelter or transitional living	9.0	11.2	5.9	14.8	4.1	3.7
Street homeless	1.4	2.3	1.5	0.6	1.2	0.6
Unknown or other	9.8	12.5	14.5	7.7	4.1	8.9
Engaged in full-time activity[a] (%)	40.7	37.3	40.7	48.9	44.1	36.4
Charge class and type (%)						
Felony	58.6	36.9	61.4	74.6	80.6	30.1
Drug	25.9	16.3	39.2	29.7	30.5	8.8
Property	25.3	13.2	14.7	40.2	40.4	14.1
Public order	5.3	5.6	5.3	2.8	7.1	7.0
Other	2.0	1.8	2.3	2.0	2.6	0.3
Misdemeanor	41.4	63.1	38.6	25.4	19.4	69.9
Drug	6.5	8.4	7.0	3.5	2.4	17.8
Property	12.6	17.3	10.8	8.9	8.0	22.2
Public order	10.7	15.1	9.1	7.7	5.3	20.6
Violent	11.6	22.3	11.7	5.2	3.7	9.3

(continued)

Appendix Table A.2 (continued)

Characteristic	Citywide	Brooklyn	Bronx	Manhattan	Queens	Staten Island
SR risk level (%)						
Low	9.8	8.3	5.8	10.7	15.3	7.8
Medium-low	15.9	14.4	13.3	16.0	20.6	16.0
Medium	41.5	41.2	44.5	41.5	38.8	43.4
Medium-high	32.6	36.0	36.5	31.5	25.4	32.8
High	0.1	0.1	0.0	0.4	0.0	0.0
Criminal Justice Agency recommendation (%)						
Recommended for ROR	20.2	14.3	13.4	18.7	36.4	18.9
Moderate risk for ROR	13.3	10.2	11.2	14.1	18.1	14.2
Not recommended for ROR	66.6	75.5	75.4	67.2	45.6	66.9
More than one case on SR (%)	4.3	6.4	4.4	2.4	3.0	5.3
Sample size	11,004	3,415	2,015	2,495	2,273	806

SOURCES: MDRC calculations based on SR provider data..

NOTES: Sample includes SR enrollees from March 2016 to January 2019. If a defendant was enrolled in SR more than once for different cases during this time frame, data from the first enrollment are used.

[a]Full-time activity is based on clients' own reports and includes work, school, training, and caretaking, and combinations thereof.

Appendix Table A.3

SR Client Intake Outcomes, Participation, and Compliance, by Borough

Outcome	Citywide	Brooklyn	Bronx	Manhattan	Queens	Staten Island
Intake outcomes						
Did not report for the intake appointment	4.4	6.2	5.1	3.4	2.5	3.2
Supervision level (%)						
Level 1	28.5	27.0	23.1	27.6	37.6	26.0
Level 2	43.3	52.4	46.7	41.9	27.9	44.4
Level 3	25.6	19.3	27.5	28.8	32.3	18.3
Level 4	2.6	1.4	2.8	1.6	2.3	11.3
Supervision level alignment (%)						
Supervision level at intake matched the framework recommendation	91.8	92.4	97.1	93.7	84.0	92.4
Lower supervision level assigned at intake than recommended by the framework	1.2	1.2	1.1	2.4	0.2	0.1
Higher supervision level assigned at intake than recommended by the framework	7.0	6.3	1.9	3.9	15.9	7.5
Participation						
Months enrolled, among those who have left the program	3.8	3.0	4.0	3.9	4.4	4.6
Number of contacts per month						
In-person	2.0	2.2	1.7	1.9	2.0	2.0
Phone	2.1	2.2	2.0	1.3	3.0	2.2
Ever referred to services (%)	26.0	22.4	22.6	33.8	24.1	30.6
Employment/vocational	10.5	8.9	9.1	14.3	10.0	10.8
Education	3.3	2.5	1.7	4.8	4.4	2.7
Substance abuse treatment	5.0	2.8	6.2	5.5	6.3	5.2
Housing/shelter	3.0	4.8	2.4	2.6	1.3	2.6
Mental health	4.0	2.8	4.5	7.3	2.2	3.0
Other	10.1	11.3	5.9	12.2	6.5	18.9

(continued)

Appendix Table A.3 (continued)

Outcome	Citywide	Brooklyn	Bronx	Manhattan	Queens	Staten Island
Compliance (%)						
Ever noncompliant while in the program	40.9	48.6	41.3	34.2	30.1	58.3
Ever missed a phone call or in-person check-in	24.4	34.6	26.6	16.0	8.5	46.8
Ever rearrested	19.4	18.9	16.4	20.3	21.2	21.8
Ever failed to appear for court	10.9	13.2	14.1	7.8	7.9	10.9
Sample size	11,004	3,415	2,015	2,495	2,273	806

SOURCE: MDRC calculations based on SR provider data.

NOTES: Sample includes SR enrollees from March 2016 to January 2019. If a defendant was enrolled in SR more than once for different cases during this time frame, data from the first enrollment are used.

Appendix Table A.4

SR Client Intake Outcomes, Participation, and Compliance, by Supervision Level

Measure	Level 1	Level 2	Level 3	Level 4	Total
Intake outcomes					
Did not report for the intake appointment (%)	1.7	5.4	5.8	2.4	4.4
Participation					
Months enrolled, among those who have left the program	4.2	3.6	3.6	3.9	3.8
Number of contacts per month					
In-person	1.7	1.9	2.3	3.6	2.0
Phone	1.7	2.2	2.3	3.5	2.1
Ever referred to services (%)	20.9	25.9	30.7	37.4	26.0
Employment/vocational	8.2	10.5	13.2	12.1	10.5
Education	2.5	2.9	4.8	4.2	3.3
Substance abuse treatment	3.8	4.2	7.1	9.3	5.0
Housing/shelter	2.0	3.6	2.9	4.8	3.0
Mental health	3.1	4.1	4.6	7.6	4.0
Other	8.4	10.3	10.8	17.0	10.1
Compliance					
Ever noncompliant while in the program (%)	26.0	45.2	48.4	58.1	40.9
Ever missed a phone call or in-person check-in	14.6	27.5	28.3	40.5	24.4
Ever rearrested	11.6	21.1	24.5	29.4	19.4
Ever failed to appear for court	6.0	12.1	13.8	14.9	10.9
Sample size	3,134	4,755	2,808	289	11,004

SOURCE: MDRC calculations based on SR provider data.

NOTES: Sample includes SR enrollees from March 2016 through January 2019. If a defendant was enrolled in SR more than once for different cases during this time frame, data from the first enrollment are used. There are 18 cases missing supervision-level information, therefore the individual sample sizes for each supervision level do not sum to the total sample size.

Appendix Table A.5

Estimated Impacts of SR Enrollment on New Pretrial Arrests

Outcome (%)	Mean Program Outcome	Mean Counterfactual Outcome	Impact Estimate	P-Value	95 Percent Confidence Interval
New arrest during the pretrial period					
Any charge	55.8	37.9	17.9***	.00035	(8.90, 26.95)
Misdemeanor	37.0	23.3	13.7***	.00039	(6.74, 20.69)
Felony	28.2	19.1	9.1**	.00690	(2.69, 15.41)
Violent felony	6.6	2.6	4.0	.07015	(-0.36, 8.44)

SOURCES: MDRC calculations based on data from the New York State Office of Court Administration and SR providers.

NOTES: The estimated mean counterfactual outcome is that which would have occurred in the absence of SR enrollment. Statistical significance levels are indicated as follows: *** = 0.1 percent; ** = 1 percent; * = 5 percent.

The statistically significant impacts shown in this table are due to the longer pretrial periods of SR enrollees. The main analysis, which uses a fixed, common follow-up period of nine months rather than variable pretrial periods, shows no statistically significant effects of SR enrollment on new arrests. See Table 5.6 for additional information.

Appendix B

Estimating the Impacts of Pretrial Supervised Release

This appendix describes how the impacts of New York City's Pretrial Supervised Release Program (SR) were estimated and how those estimates were assessed with respect to their internal validity (the degree to which they represent causal effects or impacts) and external validity (the degree to which they generalize to a meaningful population).

Introduction

Design

Estimates of SR impacts were based on a "fuzzy" parametric regression discontinuity design (RDD) with a discrete rating variable (defendants' SR risk score), one-sided treatment noncompliance (reflecting defendants who were eligible for SR based on their risk scores but did not enroll in the program), a placebo sample for robustness tests, and inference adjustments for multiple hypothesis testing.[1]

Samples

The first step of sample construction for the present analysis was to identify all new criminal cases in New York City between May 1, 2017 and April 30, 2018 involving defendants who were screened for SR, minus a few exclusions.[2] Only cases for defendants who were screened for SR were included in the sample because only they were considered for SR participation and thus only they had risk scores reported by SR providers. For defendants with multiple screened cases during the sample intake period, only their first cases were included. The resulting sample of 10,347 defendants (the present "impact sample") is the basis for the SR impact analysis.

This impact sample was used to estimate SR impacts on pretrial release outcomes, pretrial detention at arraignment, and new criminal charges within nine months of initial arrest because MDRC received data on those outcomes for all impact sample members. However, for defendant outcomes like days detained pretrial and case disposition that are not determined until a case is resolved, data were only available for the 8,894 impact sample members whose first cases were resolved during the present follow-up period.

To conduct robustness tests of bias from RDD model misspecification, a "placebo sample" was created. This sample comprised the first cases for all new defendants during the sample intake period who were not screened for SR but met all other conditions for membership in the impact sample. This placebo sample contains 66,753 defendants in total and 60,939 defendants whose first cases were resolved.

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[1]For detailed descriptions of regression discontinuity designs see Bloom (2012) and Imbens and Lemieux (2008).

[2]Cases for defendants previously enrolled in SR and cases ineligible for SR based on charge were excluded, as were desk appearance tickets, summonses, violations, and infractions. These exclusions comprise a negligible portion of all cases screened for SR.

Estimands

The SR impact analysis focuses on the impacts of *enrollment* in SR on defendants' outcomes, which as described later, are impacts of treatment on the treated (TOT). However, the first step in producing those findings was to estimate the impacts of defendants' *eligibility* for SR on their outcomes, which, as described later, are impacts of intent to treat (ITT). To produce an estimate of TOT, the ITT estimate was adjusted to account for the impact of SR eligibility on SR enrollment.

Step 1
Estimating the Impacts of SR Eligibility

The first step in the analysis of impacts of enrolling in SR (TOT) was to estimate the impacts of eligibility for SR (ITT). To be eligible for SR, a screened defendant needed a risk score of four or less plus a verified "community tie" (a family member, friend, case manager, or someone else in the community whom SR providers could reach). Thus, for the present analysis, the SR risk score is the RDD rating variable, a risk score of four is the RDD cut-point, and the absence of a community tie is one of many reasons why a screened defendant who is "risk-score" eligible for SR (hereafter referred to simply as "eligible") is not enrolled in the program.

Graphical Analysis

Appendix Figures B.1 and B.2 illustrate the core concepts and intuition of the present RDD analysis of the impacts of SR eligibility (ITT) on defendants' outcomes. Screened defendants with risk scores of four or less comprise the RDD program group and screened defendants with risk scores of more than four comprise the RDD comparison group.[3] Theoretically, the present RDD only identifies the mean impact of SR enrollment for screened defendants with a risk score of four (the RDD cut-point). However, as described later, it appears that most of the present impact estimates generalize to a range of risk scores around the RDD cut-point.

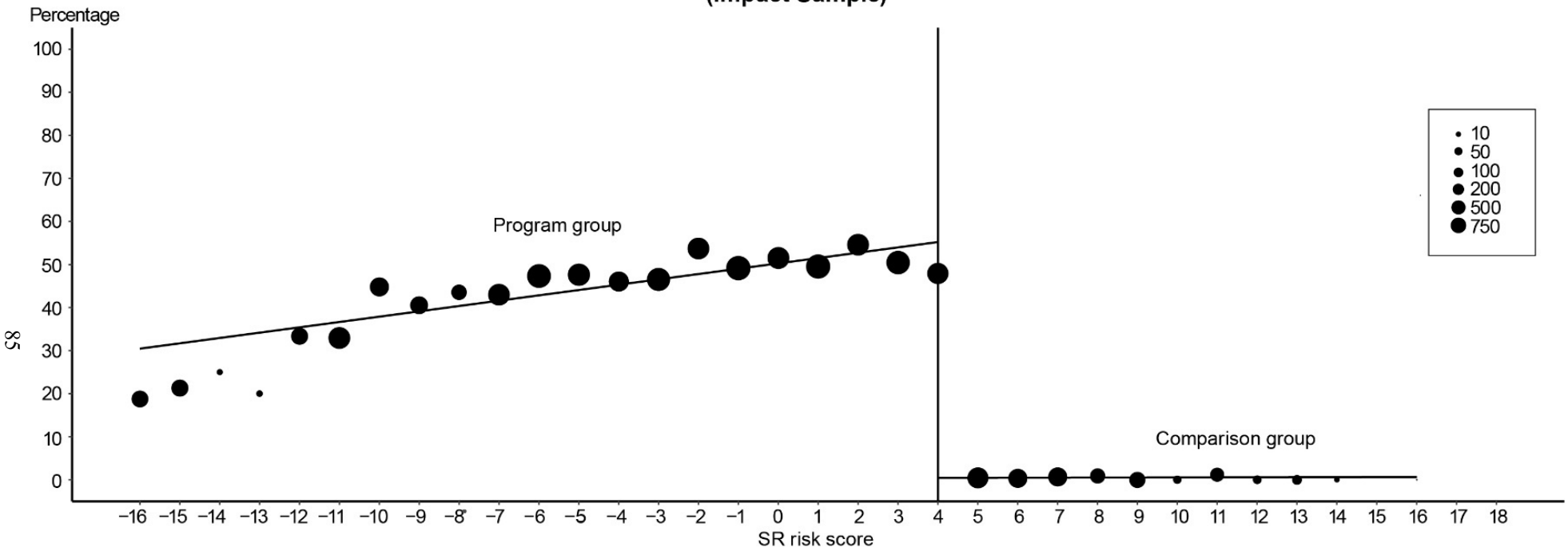
Appendix Figure B.1 plots by risk score the percentage of screened defendants who enrolled in SR, with the size of each point proportional to the size of its sample. As can be seen, this enrollment percentage is almost uniformly zero for defendants with risk scores above four (who were not eligible for SR).[4] At a risk score of four (the SR eligibility threshold), SR enrollment jumps to over 50 percent and declines gradually thereafter as risk scores decline. This pattern illustrates a pronounced positive impact of SR risk eligibility on SR enrollment. To quantify this impact, Appendix Figure B.1 also includes a weighted least squares regression line that was fit through the program-group points and a corresponding regression line that was fit

[3]Risk scores for the impact sample range from -16 to 18, with no sample members having scores of 15, 17, or 18.

[4]In a very small number of instances, judges have required SR providers to accept defendants with SR risk scores above four, explaining the nonzero enrollment percentages.

Appendix Figure B.1

Percentage of Defendants Enrolled in Supervised Release, by SR Risk Score (Impact Sample)

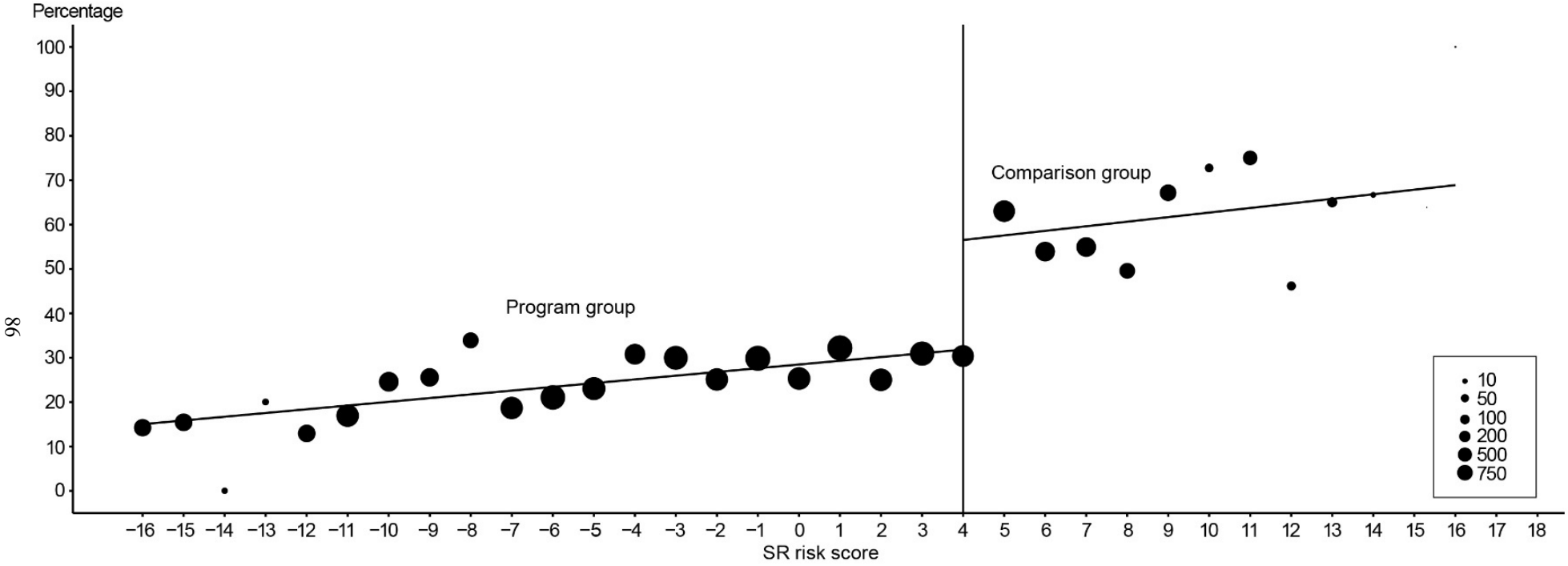


SOURCE: MDRC calculations based on data from SR providers.

NOTE: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure.

Appendix Figure B.2

Percentage of Defendants with Bail Set, by SR Risk Score (Impact Sample)



SOURCES: MDRC calculations based on data from the New York State Office of Court Administration and SR providers.

NOTE: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure.

through the comparison-group points.[5] The RDD estimate of the impact of SR eligibility on the likelihood of enrolling in SR for screened defendants with a risk score of four equals the vertical distance between the two regression lines at this risk score, which exceeds 50 percentage points.

Now consider Appendix Figure B.2, which plots by risk score (with dots proportional to sample size) the percentage of screened sample members who had bail set, with corresponding weighted least squares regression lines for the RDD program group and comparison group. This plot illustrates the pronounced *negative impact* of SR eligibility on bail receipt for defendants with a risk score of four. One can see this impact — which is almost 25 percentage points — both from the pattern of points in the graph and from the vertical distance between the program group and comparison group regression lines at a risk score of four.

In summary, Appendix Figures B.1 and B.2 indicate that for screened defendants with a risk score of four, eligibility for SR increased the likelihood of enrolling in SR by over 50 percentage points and reduced the likelihood of having bail set by almost 25 percentage points. Together, these two results indicate that *enrolling in SR* reduced the likelihood of having bail set substantially.

Statistical Analysis

To formalize the preceding graphical analysis, Equations B.1 and B.2 below were used to estimate the impact of SR eligibility (T) on SR enrollment (E) and on each defendant outcome (Y).

$$E_{ij} = \pi + \gamma_1 T_{ij} + \gamma_2 R_{ij} + \gamma_3 R_{ij} T_{ij} + e_j + v_{ij} \qquad \text{B.1}$$

$$Y_{ij} = \alpha + B_1 T_{ij} + B_2 R_{ij} + B_3 R_{ij} T_{ij} + y_j + w_{ij} \qquad \text{B.2}$$

where

E_{ij} = one if sample member i with risk-score j enrolled in SR and zero otherwise,

Y_{ij} = the outcome for sample member i with risk score j,

T_{ij} = one if sample member i with risk score j was eligible for SR and zero otherwise,

R_{ij} = the adjusted risk score for sample member i with risk score j, when risk scores were centered on a value of four,

e_j and y_j = random RDD specification errors that are distributed independently and identically across risk scores with a mean of zero, and

v_{ij} and w_{ij} = random errors that are distributed independently and identically across defendants within risk scores with a mean of zero.

[5]Estimates of these regressions, in effect, weight each point in the graph proportionally to its sample size.

Equation B.1 represents both the comparison-group regression line [$E_{ij} = \pi + \gamma_2 R_{ij}$] and the program-group regression line [$E_{ij} = (\pi + \gamma_1) + (\gamma_2 + \gamma_3) R_{ij}$] in Appendix Figure B.1. Hence, the impact of SR eligibility on SR enrollment for screened defendants with a risk score of four is γ_1. Likewise, Equation B.2 represents the program group and comparison group regression lines in Appendix Figure B.2, and B_1 is the impact of SR eligibility on the likelihood of having bail set for screened defendants with a risk score of four.

These models were estimated with risk-score, cluster-adjusted standard errors to account for possible random specification error ($e_j\ y_j$) due to the discrete nature of the RDD rating.[6] Lee and Card (2008) identified this issue and proposed a way to address part of the problem that it can cause. The issue arises because with a discrete rating, it is not possible to use the continuity properties of an RDD to *identify* program impacts. Instead, one must use a fully parametric estimator, the internal validity of which relies on the appropriateness of its functional form, which can be especially difficult to assess for ratings with discrete values. Although there is no way to overcome this problem completely — because true functional forms are unobservable — Lee and Card argue that researchers should at least account for uncertainty in parameter estimates caused by *random deviations* from an RDD functional form. To do so, they propose adjusting standard errors for the clustering of outcome values by rating values.

As noted above, the impact of SR eligibility on SR enrollment is represented by γ_1 in Equation B.1. This SR "participation contrast" is the intercept shift that occurs at the RDD cut-point when moving from the comparison-group regression line to the program-group regression line. As context, estimates of the "nonparticipating" comparison SR enrollment rate at the RDD cut-point (π) and the "participating" SR enrollment rate at the RDD cut-point ($\pi + \gamma_1$) are also reported.[7]

Similarly, the impact of SR eligibility on a defendant outcome is represented by B_1 in Equation B.2. This ITT impact estimate is the intercept shift that occurs at the RDD cut-point. As context, the mean "nonparticipating" counterfactual outcome at the cut-point (α) and the mean "participating" outcome at the cut-point ($\alpha + B_1$) are also reported.

Step 2
Estimating the Impacts of SR Enrollment

Equation B.1 above is the first-stage equation of an instrumental variables (IV) model of the impact of SR enrollment on defendants' outcomes, and Equation B.2 is the corresponding reduced form equation.[8] Appendix Figure B.3 below outlines the logic that motivates the use of these two equations to estimate the impacts of SR enrollment on defendants' outcomes.

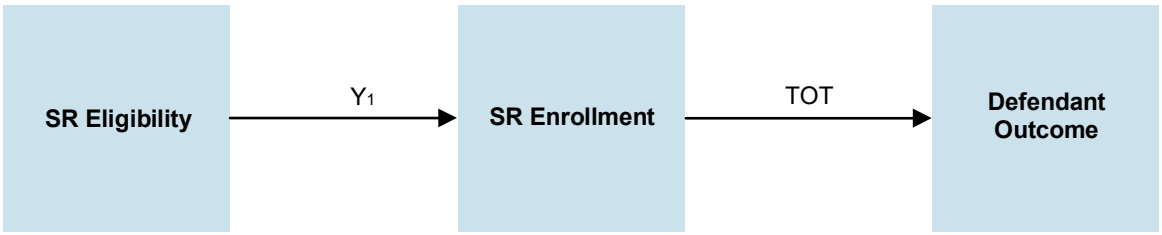
[6]SAS PROC SURVEY REG was used for this purpose.

[7]The terms "participating" and "nonparticipating" used here represent being "eligible for" or "not eligible for" SR, respectively.

[8]See Angrist and Pischke (2009) for an excellent discussion of instrumental variables analysis.

Appendix Figure B.3

Conceptual Model of SR Impacts on Defendants' Outcomes



The figure first indicates that being eligible for SR produces an impact (γ_1) on defendants' likelihood of enrolling in SR. This eligibility-induced enrollment produces an impact on defendants' outcomes, which is the impact of treatment on the treated (TOT). If the only way for SR eligibility to produce an impact on screened defendants' outcomes is *through* SR enrollment (a plausible approximation),[9] then the impact of SR eligibility on a mean outcome (B_1 in Equation B.2) is approximately $\gamma_1 TOT$. Consequently:

$$TOT \approx \frac{B_1}{\gamma_1} \qquad \text{B.3}$$

The statistical significance level (p-value) of an estimate of TOT in Equation B.3 is approximately the same as that for an estimate of B_1 in Equation B.2. To see this, note first that

$$se(\widehat{TOT}) \approx \frac{se(\hat{B}_1)}{\gamma_1} \qquad \text{B.4}$$

where $se(\widehat{TOT})$ is the standard error of the estimate of TOT and $se(\hat{B}_1)$ is the standard error of the estimate of B_1. This approximation holds well for large samples, like the present one, where γ_1 is estimated with great precision.[10] Together, Equations B.3 and B.4 imply that the t-statistic for an estimate of TOT in the present analysis is approximately the same as that for an estimate of $\hat{B}_1$, because:

$$\hat{t}_{TOT} \equiv \frac{\widehat{TOT}}{\widehat{se}(\widehat{TOT})} \approx \frac{\frac{\hat{B}_1}{\hat{\gamma}_1}}{\frac{\widehat{se}(\hat{B}_1)}{\hat{\gamma}_1}} = \frac{\hat{B}_1}{\widehat{se}(\hat{B}_1)} \equiv \hat{t}_{\hat{B}_1}. \qquad \text{B.5}$$

To provide context for TOT impact estimates, the present paper reports the mean "participating" outcome for SR enrollees with a risk score of four ($\bar{Y}_{E,R=4}$), which is estimated as the

[9]This is the well-known exclusion restriction for instrumental variables analysis. See Angrist and Pischke (2009).

[10]The estimated value of γ_1 is 54.8 percentage points and its estimated standard error is only 1.8 percentage points.

observed mean outcome for *SR enrollees* with a risk score of four. In addition, the paper reports an estimate of the mean "nonparticipating" counterfactual outcome for SR enrollees with a risk score of four, which is obtained as $(\bar{Y}_{E,R=4} - \widehat{TOT})$.[11]

Lastly, note that noncompliance with SR service is almost entirely one-sided (only reflecting SR-eligible defendants who do not enroll in the program) instead of two-sided (also reflecting SR-ineligible defendants who do enroll in the program). Therefore the internal validity of the present IV extension to TOT relies on a single assumption: that defendants who do not enroll in SR cannot experience an impact of SR (the IV exclusion restriction). This validity does not also rely on the additional assumption typically used for two-sided IV noncompliance: that control group members who experience the service have the same mean impact as their program group counterparts who experience the service.[12]

It is possible however, that the mere *existence* of SR can impact defendants' outcomes, whether they enroll in SR or not (perhaps by changing other features of the criminal justice system). If so, the present RDD comparison of program group and comparison group outcomes cannot identify those impacts. It can only identify the impact represented by the difference in mean defendant outcomes caused by SR enrollment versus nonenrollment for otherwise comparable defendants.

Step 3
Robustness Tests of Internal and External Validity

The following robustness tests were conducted to assess the internal and external validity of impact estimates for the present paper.

Internal Validity

As noted earlier, the internal validity of an impact estimate is its ability to support a causal claim. To assess this property of the present findings, five potential threats to their internal validity were examined: (1) estimation bias due to manipulation of RDD ratings, (2) estimation bias due to misspecification of the RDD model, (3) estimation bias for some outcome measures due to sample censoring (attrition) caused by missing data for cases that were not resolved during the present follow-up period, (4) estimation bias due to violation of the IV exclusion restriction for extensions of ITT findings to TOT, and (5) inferential bias due to multiple hypothesis testing.

Manipulation of RDD ratings can bias the present impact estimates if the RDD cut-point (a risk score of four) is known (which it is) and SR provider staff members rate defendants' risk

[11]Throughout the discussion of impacts of SR enrollment (TOT), the terms "participating" and "nonparticipating" mean having enrolled in SR or not having enrolled in SR, respectively.

[12]Such control group and program group members are referred to in the IV literature as "always-takers." See, for example, Angrist, Imbens, and Rubin (1996). When always-takers exist, the resulting impact of service receipt is referred to as a complier average causal effect (CACE) or a local average treatment effect (LATE). This estimand refers to an inference population of service recipients called "compliers": people who participate in a service because they were assigned to it (or in the present case, because they were eligible for it).

based in part on characteristics that are not in the risk score formula but are related to defendants' participating or nonparticipating potential outcomes.[13] For example, if reported risk scores were understated for some defendants whose true risks of unwanted future outcomes were just above the RDD cut-point (for example, to enable those defendants to receive SR services), an RDD impact estimate would tend to understate any reduction in those unwanted outcomes caused by SR. The impact estimate would also tend to understand the reduction if reported risk scores were overstated for some defendants whose true risk of unwanted future outcomes were just below the RDD cut-point (for example, to help avoid those unwanted outcomes for SR enrollees). These phenomena are forms of sample selection bias.

Fortunately, the present RDD ratings (defendants' risk scores) are computed from a well-defined formula (see Appendix Table A.1) and thus, in theory, are based solely on objective information. However, because SR provider staff members across the five boroughs of New York City compute these scores by hand, they are potentially susceptible to manipulation.

Three empirical tests were conducted to assess the likely severity of such manipulation, all of which indicate that it is unlikely to be problematic. The first test compared the distribution of provider-reported risk scores with the corresponding distribution of risk scores computed by MDRC based on administrative data for background characteristics of the 10,027 impact sample members with data for both risk scores.[14]

If provider-reported risk scores had been manipulated substantially, the two risk-score distributions would differ substantially, especially near the RDD cut-point. However, as Appendix Figure B.4 illustrates, the two distributions are strikingly similar, both overall and near the cut-point.[15]

The second test for risk-score manipulation was a regression of provider-reported risk scores (the dependent variable) on MDRC-computed risk scores (the independent variable) for impact sample members with both scores. The intercept of this regression is very close to zero (0.217 risk-score points) and its slope is very close to one (a 0.947-point increase in the mean provider-reported score per point increase in the MDRC-computed risk score), which is what one would expect in the absence of risk-score manipulation. Appendix Figure B.5 summarizes these regression results graphically. As the figure indicates, although the two risk scores are highly correlated, MDRC-computed scores do not predict provider-reported scores perfectly

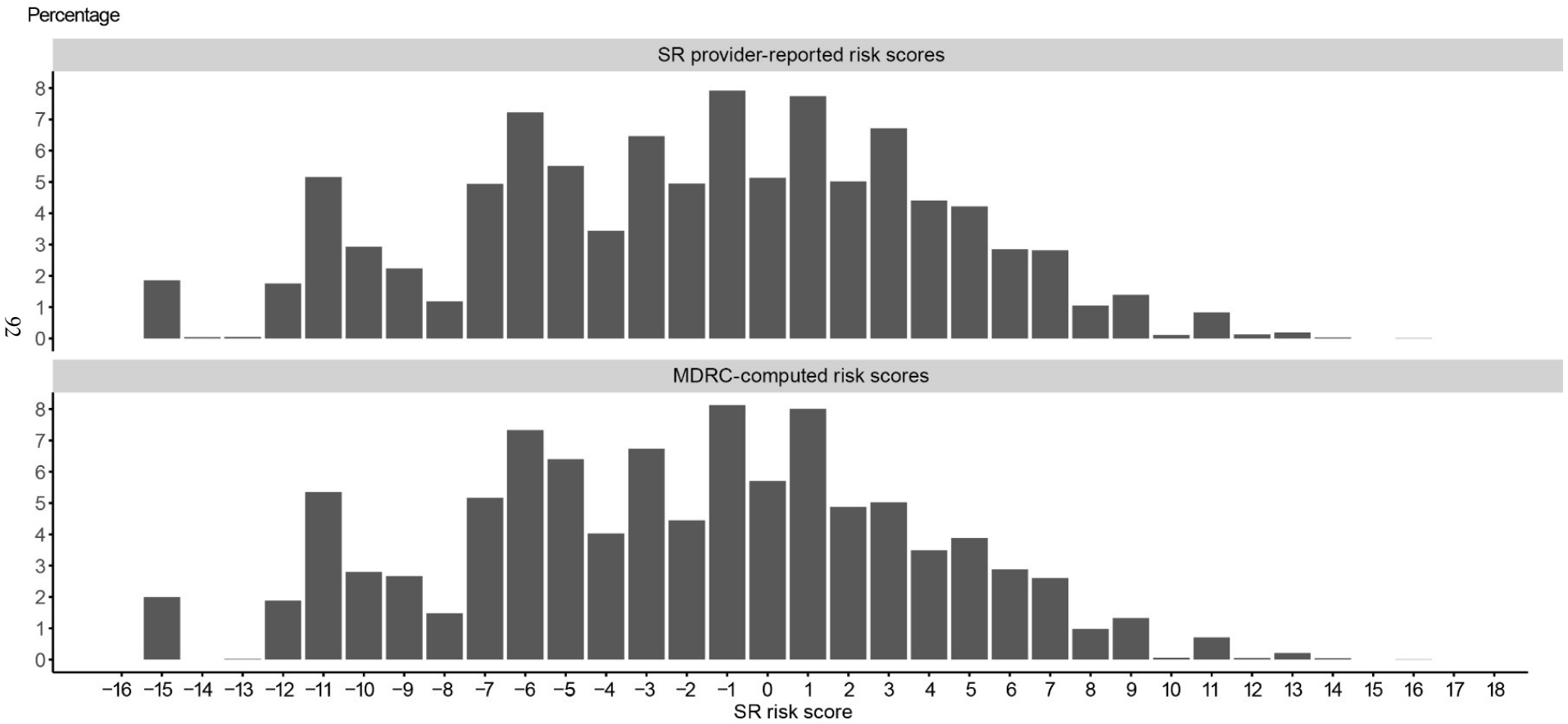
[13]For a discussion of the manipulation of RDD ratings and how to test for this phenomenon see McCrary (2008).

[14]While SR providers calculate risk scores using documents in electronic or physical case files, MDRC-calculated risk scores rely on large administrative data files from three different sources to capture the necessary information. Imperfect identifiers across these three sources mean that information from the files cannot always be correctly linked, resulting in missing information for some defendants. Because of this missing information, it was not possible to compute risk scores for 320 impact sample members.

[15]Across the full range of possible risk-score values, the two distributions are consistently similar. In addition, at the transition from SR ineligibility to SR eligibility (that is, at risk scores of five and four, respectively), sample densities for the two risk scores are quite similar.

Appendix Figure B.4

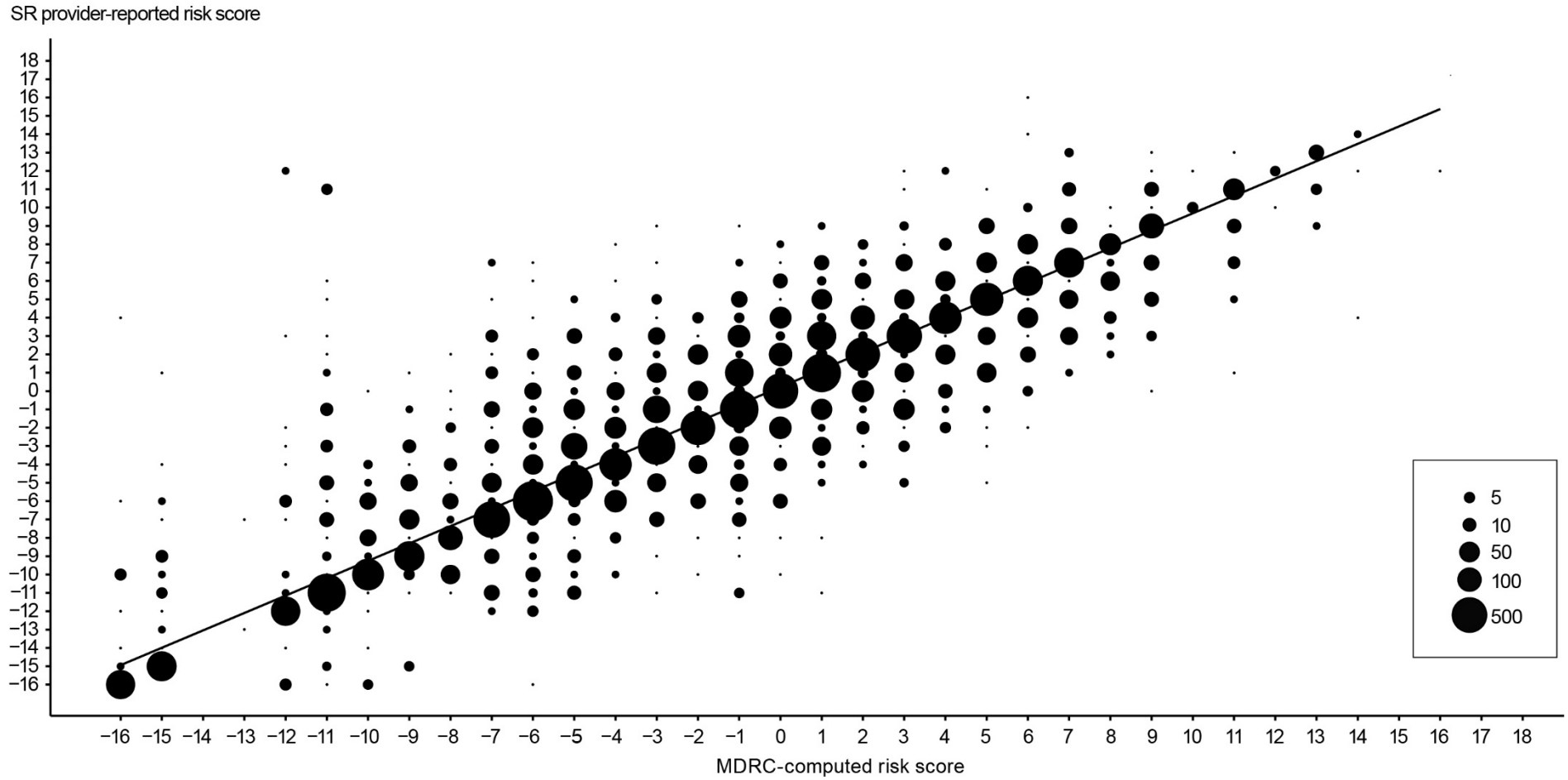
Distributions of Provider-Reported Risk Scores and MDRC-Computed Risk Scores for Impact Sample Members with Both Scores



SOURCES: MDRC calculations based on data from the New York State Department of Criminal Justice Services, the New York City Criminal Justice Agency, and SR providers.

Appendix Figure B.5

Summary Plot of SR Provider-Reported Risk Scores Versus MDRC-Computed Risk Scores



SOURCES: MDRC calculations based on data from the New York State Department of Criminal Justice Services, the New York City Criminal Justice Agency, and SR providers.

NOTE: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure.

($R^2 = 0.88$) and thus do not predict eligibility for SR perfectly. However, in the absence of risk-score manipulation, this prediction error should be approximately random.

The third test for risk-score manipulation compared ITT impact estimates for defendant outcomes based on the two risk scores for impact sample members with both scores. Appendix Table B.1 reports these findings for SR enrollment and for defendant outcomes.

To the extent that MDRC-computed risk scores incorrectly "*predict*" provider-reported risk scores, they incorrectly predict SR eligibility and enrollment. The incorrect prediction places some SR enrollees in the RDD comparison group and some SR-ineligible defendants in the RDD program group. This misplacement *understates* the SR enrollment discontinuity (the participation contrast) at the RDD cut-point, which in turn, understates ITT impact estimates for defendants' outcomes. If risk-score prediction error is random, the proportion by which the magnitudes of ITT impact estimates for defendants' outcomes are understated will equal the proportion by which the SR participation contrast is understated (in expectation).

Now consider the findings. The first two columns in Appendix Table A.2 report the estimated impact of SR eligibility on SR enrollment (the SR participation contrast) and on defendant outcomes (with p-values) based on SR provider-reported risk scores. The next two columns report corresponding findings based on MDRC-computed risk scores. The final column reports the *ratio* of the latter to the former. Note first that the estimated SR participation contrast for all impact sample members is 40 percentage points based on MDRC-computed risk scores versus 55 percentage points based on provider-reported risk scores — for a ratio of 0.74. This difference reflects the attenuation of the SR participation contrast produced by the incorrect prediction of RDD program group and comparison group members by MDRC-computed risk scores.[16]

Note next that the corresponding ratios for estimated impacts on defendant outcomes — especially for large and highly statistically significant impact estimates, which are less likely than others to reflect substantial random estimation error — are generally near the ratio for the SR participation contrast. For example, these ratios are 0.71, 0.74, 0.64 and 0.86 for bail set, detained at arraignment, length of pretrial period, and case dismissed, respectively. Consequently, error in the prediction of provider-reported risk scores (which in theory could have been manipulated) by MDRC-computed risk scores (which could not have been manipulated) appears to be random. This result implies that manipulation of provider-reported risk scores was not problematic.

Misspecification of the RDD functional form can produce biased impact estimates that overstate or understate true impacts.[17] One way to assess the potential for such bias is to examine

[16]Corresponding results for resolved cases are an estimated SR participation contrast of 39.9 percentage points based on MDRC-computed risk scores versus 53.5 percentage points based on SR provider-reported risk scores, for a ratio of 0.75.

[17]Given the "lumpy" point structure that relates defendants' risk scores to their baseline characteristics (Appendix Table A.1), the true underlying RDD model may have some modest, idiosyncratic nonlinearities that cannot be detected given the small number of risk scores involved.

Appendix Table B.1

ITT Impact Estimates Based on Provider-Reported and MDRC-Computed Risk Scores (Impact Sample Members with Both Risk Scores)

	For Provider-Reported Risk Scores		For MDRC-Computed Risk Scores		
Outcome[a]	Impact Estimate	P-Value	Impact Estimate	P-Value	Ratio[b]
Pretrial release outcomes (%)					
Enrolled in SR	54.8	$7.95 * 10^{-23}$	40.3	$4.97 * 10^{-14}$	0.74
Bail set	-24.7	$2.83 * 10^{-5}$	-17.5	0.0021	0.71
Released on one's own recognizance (ROR)	-24.1	$1.14 * 10^{-7}$	-19.4	$2.55 * 10^{-6}$	0.81
Pretrial detention					
Detained at arraignment (%)	-18.8	$1.13 * 10^{-5}$	-13.8	0.0003	0.74
Days detained while awaiting trial	-5.7	$1.07 * 10^{-5}$	-5.7	0.0025	1.00
Pretrial exposure and court appearance					
Length of the pretrial period (days)	30.6	0.0038	19.5	0.0233	0.63
Time exposed to pretrial risk (days)	34.0	$9.74 * 10^{-5}$	22.6	0.0067	0.66
Bench warrant issued (%)	5.2	0.0610	2.2	0.4960	0.43
New arrest within 9 months (%)					
Any charge	-2.8	0.2498	-0.7	0.8056	0.24
Misdemeanor	-5.3	0.1515	-5.0	0.1313	0.94
Felony	0.5	0.7850	1.9	0.2390	4.19
Violent felony	1.6	0.0589	1.9	0.1525	1.20
Case outcomes (%)					
Dismissed	5.3	0.0033	4.5	0.0219	0.86
Found guilty	-5.9	0.0020	-5.1	0.0139	0.87

SOURCES: MDRC calculations based on data from the New York State Office of Court Administration, the New York City Department of Correction, the New York State Division of Criminal Justice Services, the New York City Criminal Justice Agency, and SR providers.

NOTES: [a]Findings for pretrial release outcomes, detention at arraignment, and new arrests within nine months are based on data for all impact sample members with both risk scores. Findings for days detained while awaiting trial, pretrial exposure, court appearance rates, and case outcomes are based on data for impact sample members with resolved cases and both risk scores.

[b]This column reports the ratio of the estimated impact of SR eligibility on SR enrollment (the SR participation contrast) based on MDRC-computed risk scores to that based on SR provider-reported risk scores. This ratio is 0.74 for all impact sample members with both risk scores and 0.75 for impact sample members with resolved cases and both risk scores.

the graph of an RDD model superimposed on the data points it is modeling. This ability to validate impact findings visually is a major strength of RDDs.

Consider for example, Appendix Figure B.2, which graphs by risk score the percentage of impact sample members who received bail. Note the large and abrupt drop in bail receipt rates at the RDD cut-point, which provides strong evidence of a large negative impact of SR eligibility on bail receipt. Similar visual assessments for the other defendant outcomes studied did not provide evidence of substantial model misspecification error (see Appendix C).

To explore the possibility of model misspecification bias more systematically, a separate ITT impact analysis was conducted for the placebo sample of 66,753 unscreened defendants and its subsample of 60,939 unscreened defendants with resolved cases. Recall that no unscreened sample members could enroll in SR because they did not have provider-reported risk scores, which were only determined when defendants were screened for SR. However, based on MDRC-computed risk scores for unscreened defendants, it was possible to conduct an RDD placebo impact analysis. This analysis (1) plotted SR enrollment rates and mean outcomes by MDRC-computed risk score for the placebo sample; (2) estimated an RDD regression model of the statistical relationship among those data points; and (3) estimated placebo impacts of SR eligibility on SR enrollment and on each outcome measure as the vertical distance between the regression lines for the placebo program group and the placebo comparison group at a risk score of four.

Because SR enrollment rates are uniformly zero for all unscreened defendants, there is no SR enrollment discontinuity (participation contrast) to produce a defendant outcome discontinuity (impact) at the risk score of four. Consequently, any outcome discontinuity that is observed for unscreened sample members with that risk score represents either random error due to chance or systematic error due to model misspecification.[18]

To illustrate this point for bail receipt rates, Appendix Figure B.6 compares RDD findings for the impact sample based on SR provider-reported risk scores (at the top of the figure) with corresponding findings for the placebo sample based on MDRC-computed risk scores (at the bottom of the figure). Note the large drop in bail receipt rates for the impact sample at the SR eligibility threshold. This drop reflects the corresponding large jump in SR enrollment rates for screened defendants. In contrast, note the small decline in bail-receipt rates at the SR eligibility threshold for the placebo sample. The striking difference between these two results suggests there is no major misspecification bias in the actual RDD impact estimate.[19]

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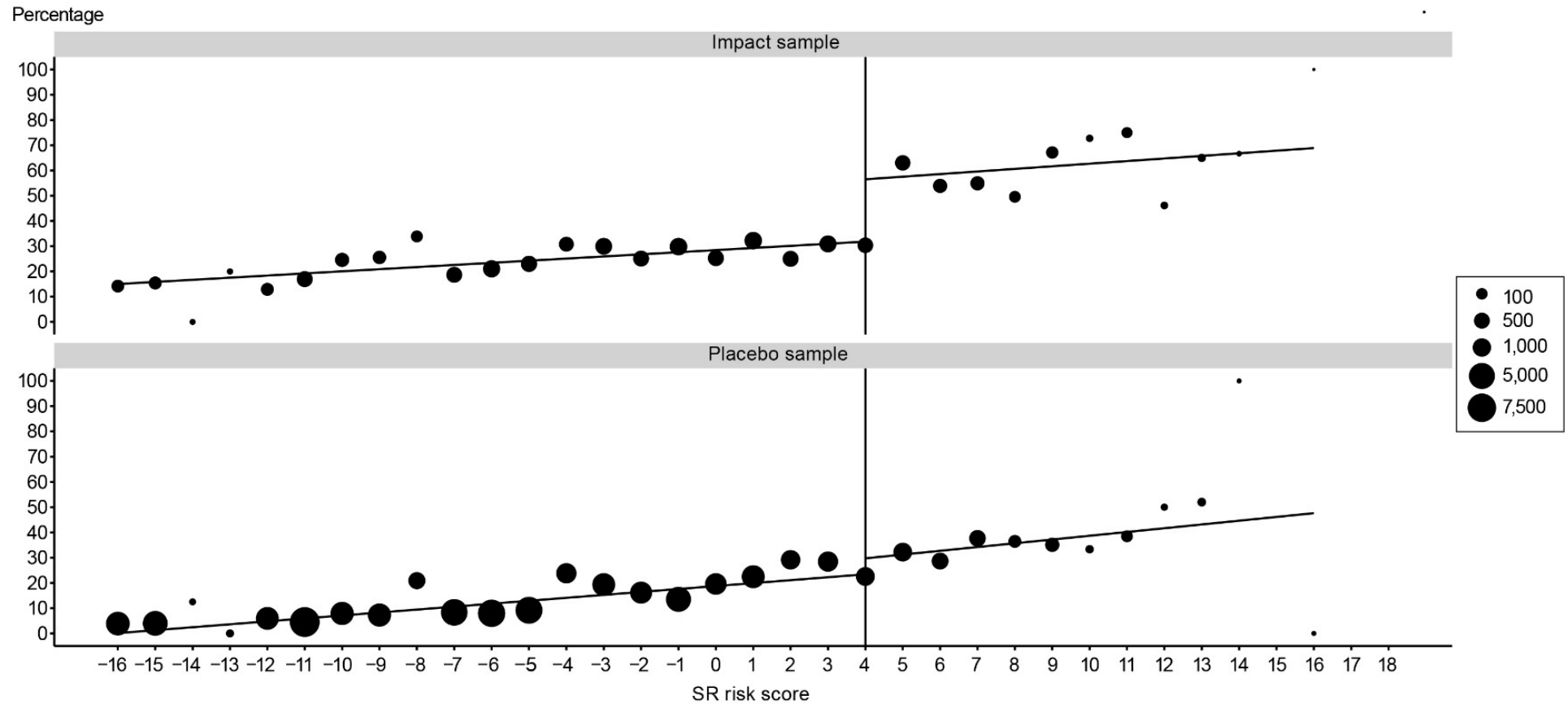
[18]The present analysis subsumes under model misspecification error the theoretical possibility that something in the local environment other than SR (for example, another intervention or a cultural shift) could produce a departure from the RDD model at a risk score of four.

[19]Although impact estimates for the impact and placebo samples are based on different risk scores, it is valid to compare them because of the strong association between the two risk scores ($R^2 = 0.88$). An alternative approach would be to compare impact sample estimates based on MDRC-computed risk scores with corresponding estimates for the placebo sample. However, doing so would not compare actual impact estimates with placebo impact estimates, which is more direct and transparent.

Appendix Figure B.6

Percentage of Defendants with Bail Set, by SR Risk Score (Impact Sample and Placebo Sample)



SOURCES: MDRC calculations based on data from the New York State Department of Criminal Justice Services, the New York City Criminal Justice Agency, and SR providers.

NOTE: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure.

Appendix Table B.2 quantifies the findings in Appendix Figure B.6 and corresponding findings for other defendant outcomes. The first two columns of findings report the ITT impact estimate and its statistical significance level (p-value) for the impact sample. The next two columns report the ITT impact estimate and its statistical significance level for the placebo sample. The last two columns report the difference between corresponding impact estimates and the p-value of this difference. When assessing the findings note that a large estimated impact difference supports the internal validity of an actual impact estimate.[20]

As can be seen, the evidence in Appendix Table B.2 clearly supports the internal validity of impact estimates for almost all outcomes. Consider for example, the findings for bail receipt rates. Eligibility for SR appears to reduce bail receipt rates by 25 percentage points for the impact sample versus only 6 percentage points for the placebo sample. The most likely explanations for the small but statistically significant placebo impact estimate are that (1) it is a false positive that occurred by chance or (2) it represents modest bias due to model misspecification. Nonetheless, the substantial and statistically significant difference between the two estimates (-18 percentage points) suggests that at most, misspecification bias is modest.

When examining the findings in Appendix Table B.2, note that they represent impacts of SR eligibility (ITT), not impacts of SR enrollment (TOT). However, because TOT estimates are a simple multiple of their ITT counterparts ($\widehat{TOT} = \widehat{ITT}(\frac{1}{\hat{\gamma}_1})$), an assessment of model misspecification bias for ITT estimates provides a corresponding assessment for TOT estimates.[21]

Also, when examining the findings in Appendix Figure B.6 and Appendix Table B.2 it is important to consider one further issue about any comparisons that are made between outcomes for impact sample members (all of whom were screened for SR) and outcomes for placebo sample members (none of whom were screened for SR). This issue involves the theoretical possibility that just being screened for SR (without enrolling in it) has a causal effect on court decisions, defendant behavior, or both.

MDRC's knowledge of the SR screening process and the New York City criminal court decision-making process strongly suggest that in practice, just being screened for SR is very unlikely to affect defendants' outcomes. However, Appendix Figure B.6 indicates that bail assignment rates for comparison group members (defendants with risk scores of five or higher) in the impact sample are almost twice those for comparison group members in the placebo sample. Thus, either the two comparison groups differ in ways that influence bail assignment decisions but are not accounted for by SR risk scores, or just being screened for SR somehow dramatically *increased* bail assignment rates.

[20]Although placebo sample findings are used as a robustness test for bias in the impact sample findings, they are not used to adjust impact sample findings, because to do so would inject further random error into those findings.

[21]The bias for estimates of TOT is approximately ($\frac{1}{\gamma_1}$) times the bias for estimates of ITT.

Appendix Table B.2
ITT Impact Estimates for the Impact Sample and Placebo Sample

	Impact Sample		Placebo Sample		Difference	
Outcome	ITT Impact Estimate	P-Value	ITT Impact Estimate	P-Value	Estimate	P-Value
Pretrial release outcomes (%)						
Enrolled in SR	54.8	$7.95 * 10^{-23}$	NA	NA	NA	NA
Bail set	-24.7	$2.83 * 10^{-5}$	-6.3	0.0202	-18.4	0.0027
Released on one's own recognizance (ROR)	-24.1	$1.14 * 10^{-7}$	4.2	0.3721	-28.2	$3.27 * 10^{-5}$
Pretrial detention						
Detained at arraignment (%)	-18.8	$1.13 * 10^{-5}$	-5.6	0.0223	-13.2	0.0041
Days detained while awaiting trial	-5.7	$1.07 * 10^{-5}$	-3.8	0.0051	-2.0	0.2423
Pretrial exposure and court appearance						
Length of the pretrial period (days)	30.6	0.0038	-3.0	0.7609	33.6	0.0210
Time exposed to pretrial risk (days)	34.0	$9.74 * 10^{-5}$	-0.7	0.9412	34.7	0.0054
Bench warrant issued (%)	5.2	0.0610	0.6	0.4025	4.6	0.1101
New arrest within 9 months (%)						
Any charge	-2.8	0.2498	-4.5	0.0179	1.6	0.5835
Misdemeanor	-5.3	0.1515	-2.8	0.1217	-2.5	0.5413
Felony	0.5	0.7850	-2.8	0.0579	3.3	0.1452
Violent felony	1.6	0.0589	0.4	0.5880	1.2	0.2687
Case outcomes (%)						
Dismissed	5.3	0.0033	6.8	0.0134	-1.6	0.6145
Found guilty	-5.9	0.0020	-7.1	0.0100	1.2	0.6918

SOURCES: MDRC calculations based on data from the New York State Office of Court Administration, New York City Department of Correction, New York State Division of Criminal Justice Services, New York City Criminal Justice Agency, and SR providers.

For example, it is theoretically possible that comparison group members in the impact sample — defendants who were screened for SR but found to be ineligible because they had risk scores above the SR eligibility cut-point of four — could have experienced increased bail assignment rates if judges were aware that they had been screened (a possibility if judges happened to observe defendants' attorneys interacting with SR providers in the courtroom to request screenings) and inferred from SR not being presented as an option at arraignment hearings that a defendant was too high risk to be eligible. This information could have led judges to view comparison group members as riskier than they otherwise would have, in turn leading to increased bail setting. While this scenario is possible and probably occurred in some cases, it is highly

improbable that busy judges observed courtroom interactions between defense attorneys and SR providers so consistently that comparison group members were subject to increased bail setting en masse.

Furthermore, a potentially important influence on bail assignment rates that is not accounted for by SR risk scores — and thus might account for the difference between bail assignment rates for comparison group members in the impact and placebo samples — is the nature of the crime for which defendants were charged in their study-qualifying cases. To explore this potential explanation for the striking difference in bail assignment rates for comparison group members in the two samples, Appendix Figure B.7 plots by risk score the percentage of impact and placebo sample members whose charges for their study-qualifying cases were felonies.[22] As can be seen, this percentage for comparison group members in the impact sample is roughly twice that for comparison group members in the placebo sample. Thus, it is far more likely that the striking difference in the seriousness of the charges for study-qualifying cases for the two comparison groups explains their difference in bail assignment rates than it is that just being screened for SR caused this difference.

Differential censoring of sample data for program and comparison group members whose cases were not resolved during the present follow-up period could, in theory, bias estimates of SR impacts on defendants' outcomes that are not determined until a case is resolved (for example, the incidence of bench warrants, the duration of the pretrial period, or the incidence of convictions). This phenomenon is a form of sample attrition bias. To assess the likely severity of such bias, MDRC computed the percentage of program group cases and comparison group cases that were censored (14 percent and 15 percent, respectively). This overall level of attrition and its program versus comparison group difference is well within the standards set by the What Works Clearinghouse for assessing the ability of randomized trials to provide evidence that supports causal inferences "without reservation."[23] Furthermore, these attrition rates range mainly from only 10 percent to 15 percent across the full range of risk scores (as shown in Appendix Figure B.8). Hence, it is very unlikely that differential attrition produced appreciable bias for impact estimates based on data for resolved cases.

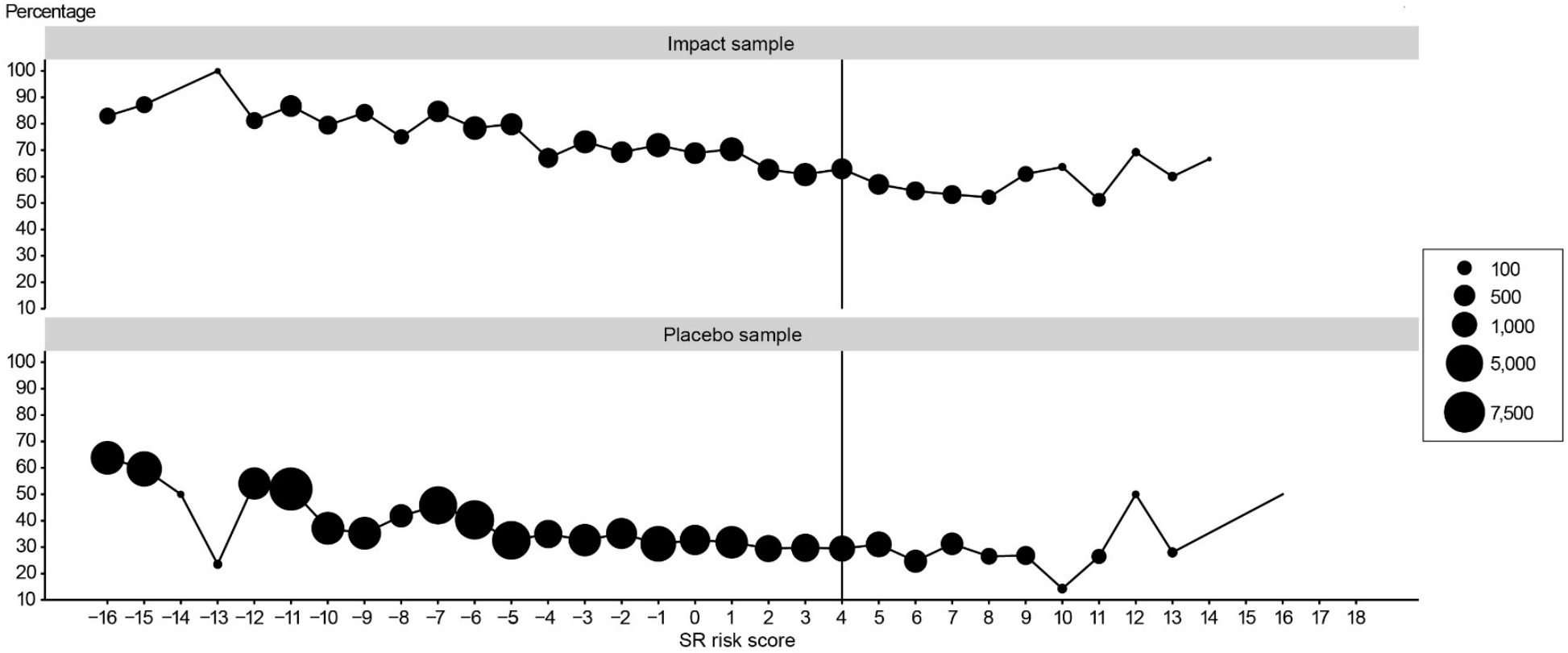
Violation of the IV exclusion restriction can in theory, bias estimates of the impacts of SR enrollment (TOT). In the present context, this restriction assumes that SR can only affect defendants' outcomes through their enrollment in the SR program. The present analysis therefore allocates the entire estimated impact of SR eligibility on a given outcome (for defendants with a risk score of four) to the roughly 55 percent of those defendants who enrolled in SR. This allocation is accomplished by dividing the estimated impact of SR eligibility on a given outcome (B_1 in Equation B.2) by approximately 0.55 (γ_1 in Equation B.1). However, if SR eligibility per se has

[22]Once again, SR provider-reported risk scores are used for the impact sample and MDRC-computed risk scores are used for the placebo sample.

[23]U.S. Department of Education, Institute of Education Sciences, What Works Clearinghouse (2017).

Appendix Figure B.7

Percentage of Defendants Whose Charges for Their Study-Qualifying Cases Included Felonies (Impact Sample and Placebo Sample)

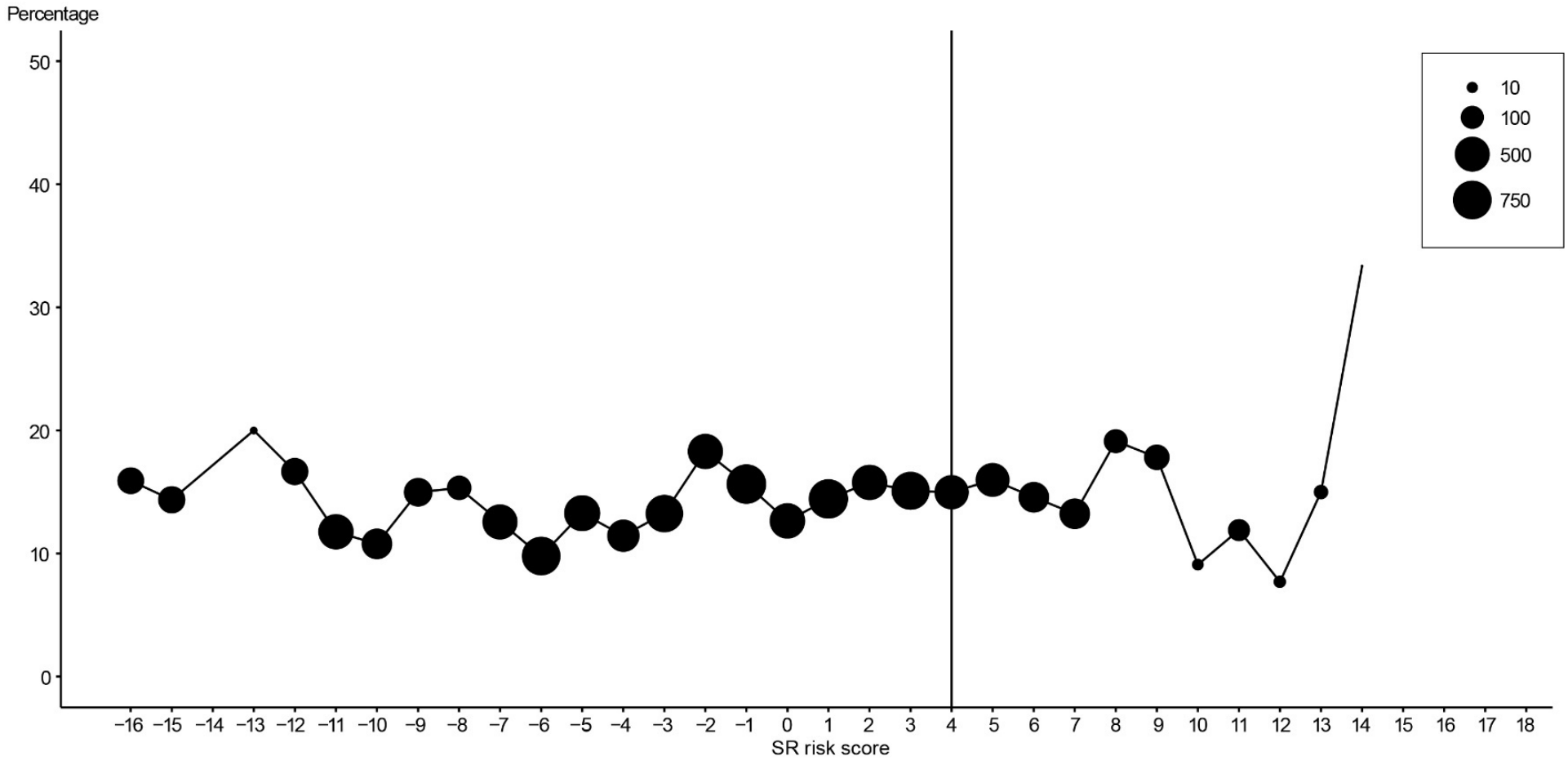


SOURCES: MDRC calculations based on data from the New York State Office of Court Administration, the New York State Department of Criminal Justice Services, the New York City Criminal Justice Agency, and SR providers.

NOTE: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure.

Appendix Figure B.8

Percentage of Cases That Were Censored, by SR Risk Score
(Impact Sample)



SOURCE: MDRC calculations based on data from the New York State Office of Court Administration and SR providers.

NOTE: TThe size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure.

a direct effect on screened defendants' outcomes, then allocating the entire impact of SR eligibility to SR enrollees will misstate the impact of SR enrollment.[24]

Consider this issue with respect to the impact of SR on the percentage of defendants for whom bail is set — a primary focus of the SR program. For defendants who would have had bail set if the SR program did not exist and who are eligible for and enroll in SR, being enrolled clearly causes them to not have bail set, because SR is a direct alternative to bail. This effect in turn causes those defendants not to experience impacts on outcomes that are produced by bail (for example, an increased likelihood of pretrial detention). However, for defendants who would have had bail set if the SR program did not exist and are eligible for but do not enroll in SR, it is highly implausible that just being eligible for SR would cause them not to have bail set (and thus be released on their own recognizance — ROR — which is virtually the only remaining option other than bail).[25] Consequently, it is highly plausible that the entire impact of SR eligibility on those defendants' outcomes is produced through SR enrollment.

Now consider the situation for defendants who would have been granted ROR if the SR program did not exist. For those defendants who are eligible for and enroll in SR, being enrolled cannot *cause* them to have bail set at their arraignment (again, because SR is a direct alternative to bail). In addition, for defendants who would have been given ROR if the SR program did not exist, and who were eligible for but did not enroll in SR, it is extremely unlikely that they would have bail set instead of being given ROR. For this situation to occur would imply that a judge who was willing to give a defendant ROR in the absence of the SR program would be more willing to assign bail if the defendant was known to be eligible for SR (and thus was judged not to be unduly risky) but did not enroll in the program.

Thus both for defendants who would have had bail set in the absence of the SR program or would have been granted ROR in the absence of the SR program (which in practice are virtually the only other arraignment options), it is highly plausible that the entire impact of SR eligibility on the likelihood of having bail set is produced through SR enrollment.

Multiple hypothesis testing due to estimating impacts for many outcomes can cause researchers to overstate the statistical significance (understate the p-value) of each impact estimate. Although this problem is well known, there is no consensus about how best to address it, and

[24]Theoretically, if the impact on a given outcome of being screened and deemed eligible for but not enrolling in SR is in the same direction as the corresponding impact of SR enrollment on that outcome, then imposing the exclusion restriction will tend to overstate the magnitude of the impact of SR enrollment. On the other hand, if the impact on a given outcome of being screened and deemed eligible for but not enrolling in SR is in the opposite direction as the corresponding impact of SR enrollment on that outcome, then imposing the IV restriction will tend to understate the magnitude of the impact of SR enrollment.

[25]In very rare cases, defendants are remanded to jail without the option of bail, which is the fourth possible outcome for cases that continue past arraignment (that is, those that are not dismissed or resolved).

numerous approaches for doing so have been developed.[26] With this lack of consensus in mind, the present analysis uses two different approaches to adjust p-values for ITT impact estimates.

One approach is the Holm (1979) method for maintaining an acceptable "familywise error" rate. The present analysis defines this rate as the percentage of the 13 impact estimates reported for defendant outcomes that are statistically significant at the 0.05 level for a two-tailed test but are, in fact, false positives. The Holm method is used to limit this rate to 5 percent. In other words, it is used to ensure that in expectation, only 5 percent of the present 13 impact estimates is a false positive. This restriction means that ex ante, the probability of being a false positive is 0.05 for each impact estimate reported.[27]

The second adjustment for multiple hypothesis testing is the Benjamini-Hochberg (1995) method for maintaining an acceptable "false discovery" rate. The present analysis defines this rate as the percentage of statistically significant ITT impact estimates for the 13 defendant outcomes reported that are false positives. The Benjamini-Hochberg method was used to limit this error rate to 5 percent. In other words, it was used to assure that in expectation, only 1 out of 20 statistically significant impact estimates are false positives. This restriction means that ex ante, the probability of being a false positive is 0.05 for each statistically significant impact estimate reported.

Appendix Table B.3 reports the results of these tests. The first two columns report the estimated ITT impact for each defendant outcome and its unadjusted p-value. The next two columns report the adjusted p-value for each outcome based on the Holm method and the Benjamini-Hochberg method, respectively. The last two columns indicate whether adjusted p-values for each method are statistically significant at the 0.05 level for a two-tailed test.

As can be seen, neither adjustment method changed the conclusion about statistical significance for any of the 13 outcomes studied. This result is especially impressive because both the Holm method and the Benjamini-Hochberg method tend to over-adjust (that is, understate) p-values of impact estimates for outcomes that are correlated with each other, which is the case for many outcomes in the present analysis.[28]

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[26]From a frequentist perspective, there are numerous ways to adjust p-values for individual impact estimates (for example, see Holm, 1979; Benjamini and Hochberg, 1995; and Westfall and Young, 1993) or to conduct an omnibus test that accounts for the multiplicity of these estimates (for example, see Caughey, Dafoe, and Seawright, 2017). However, each of these approaches has important limitations. Furthermore, although a Bayesian perspective (for example, see Gelman, Hill, and Yajima, 2012) eliminates the problem of multiple hypothesis testing by eliminating null hypothesis tests, it too has important limitations.

[27]Each impact estimate can be a true positive, a false positive, a true negative, or a false negative.

[28]Note that the correlation between the length of the pretrial period and time exposed to pretrial risk is 0.92 and the correlation between whether defendants had their cases dismissed or were found guilty is -0.97. Given these extremely strong correlations, each outcome pair was considered as a single independent outcome when making the Holm and Benjamini-Hochberg p-value adjustments.

Appendix Table B.3
Adjusted and Unadjusted P-Values for ITT Impact Estimates

Outcome	Impact Estimate	Unadjusted P-Value	Adjusted P-Value (Holm)	Adjusted P-Value (Benj./Hoch.)	Statistically Significant (Holm)[a]	Statistically Significant (Benj./Hoch.)[a]
Pretrial release outcomes (%)						
Enrolled in SR	54.8	$7.95 * 10^{-23}$	NA	NA	NA	NA
Bail set	-24.7	$2.83 * 10^{-5}$	0.0002	$7.79 * 10^{-5}$	TRUE	TRUE
Released on one's own recognizance (ROR)	-24.1	$1.14 * 10^{-7}$	$1.25 * 10^{-6}$	$1.25 * 10^{-6}$	TRUE	TRUE
Pretrial detention						
Detained at arraignment (%)	-18.8	$1.13 * 10^{-5}$	0.0001	$4.15 * 10^{-5}$	TRUE	TRUE
Days detained while awaiting trial	-5.7	$1.07 * 10^{-5}$	0.0001	$5.88 * 10^{-5}$	TRUE	TRUE
Pretrial exposure and court appearance						
Length of the pretrial period (days)	30.6	0.0038	0.0267	0.0084	TRUE	TRUE
Time exposed to pretrial risk (days)	34.0	$9.74 * 10^{-5}$	0.0007	0.0002	TRUE	TRUE
Bench warrant issued (%)	5.2	0.0610	0.2440	0.0839	FALSE	FALSE
New arrest within 9 months (%)						
Any charge	-2.8	0.2498	0.4997	0.2748	FALSE	FALSE
Misdemeanor	-5.3	0.1515	0.4544	0.1851	FALSE	FALSE
Felony	0.5	0.7850	0.7850	0.7850	FALSE	FALSE
Violent felony	1.6	0.0589	0.2945	0.0926	FALSE	FALSE
Case outcomes (%)						
Dismissed	5.3	0.0033	0.0196	0.0060	TRUE	TRUE
Found guilty	-5.9	0.0020	0.0117	0.0036	TRUE	TRUE

SOURCES: MDRC calculations based on data from the New York State Office of Court Administration, New York City Department of Correction, and SR providers.

NOTE: [a]Statistical significance is assessed at the 0.05 level for a two-tailed test.

On balance then, multiple hypothesis testing does not appear to be a problem for statistical inferences about the present ITT impact estimates. This conclusion also holds for corresponding TOT estimates because they are approximately a multiple of their ITT counterparts.

External Validity

There are two main threats to the external validity or generalizability of the present findings: (1) the fact that in theory, RDD impact estimates only apply to a subpopulation of defendants at the RDD cut-point, and (2) the possibility that in practice, the present sample of screened defendants do not represent a policy-relevant subpopulation of SR enrollees in New York City.

The primary theoretical threat to the external validity of RDD impact estimates is the localized nature of the impacts that can be identified. However, recent research has begun to demonstrate that in practice, RDD findings can sometimes (and perhaps often) generalize to policy-relevant subpopulations defined by a range of rating values.[29] To explore this issue for the present analysis, consider Appendix Figures B.9 and B.10, which expand on Appendix Figures B.1 and B.2, presented earlier.

Appendix Figure B.9 plots, by risk score, the percentage of screened defendants who enrolled in SR. In addition, it superimposes the linear regression for comparison group members as a solid line to the right of the cut-point and superimposes the linear regression for program group members as a solid line to the left of the cut-point. The RDD estimate of the impact of SR eligibility on SR enrollment at the SR eligibility threshold is the vertical distance between the two solid lines at a risk score of four.

Now consider what the graph implies about the generalizability of this finding to a range of risk scores below and above four. To facilitate a visual analysis, the comparison group regression was extended as a dashed line to the left of the RDD cut-point. Similarly, the program group regression was extended as a dashed line to the right of the cut-point. For a given risk score below four, the vertical distance (illustrated by a double-headed arrow) between the solid program group line and the dashed extension of the comparison group line is an estimate of the impact of SR eligibility on SR enrollment. This distance implies a large positive enrollment impact for a broad range of risk scores below four, and the corresponding points in the graph visually reinforce this interpretation. A similar comparison of lines (and points) to the right of the cut-point indicates that if defendants within a broad range of risk scores above four had been eligible for SR, it would have greatly increased their SR enrollment rates.

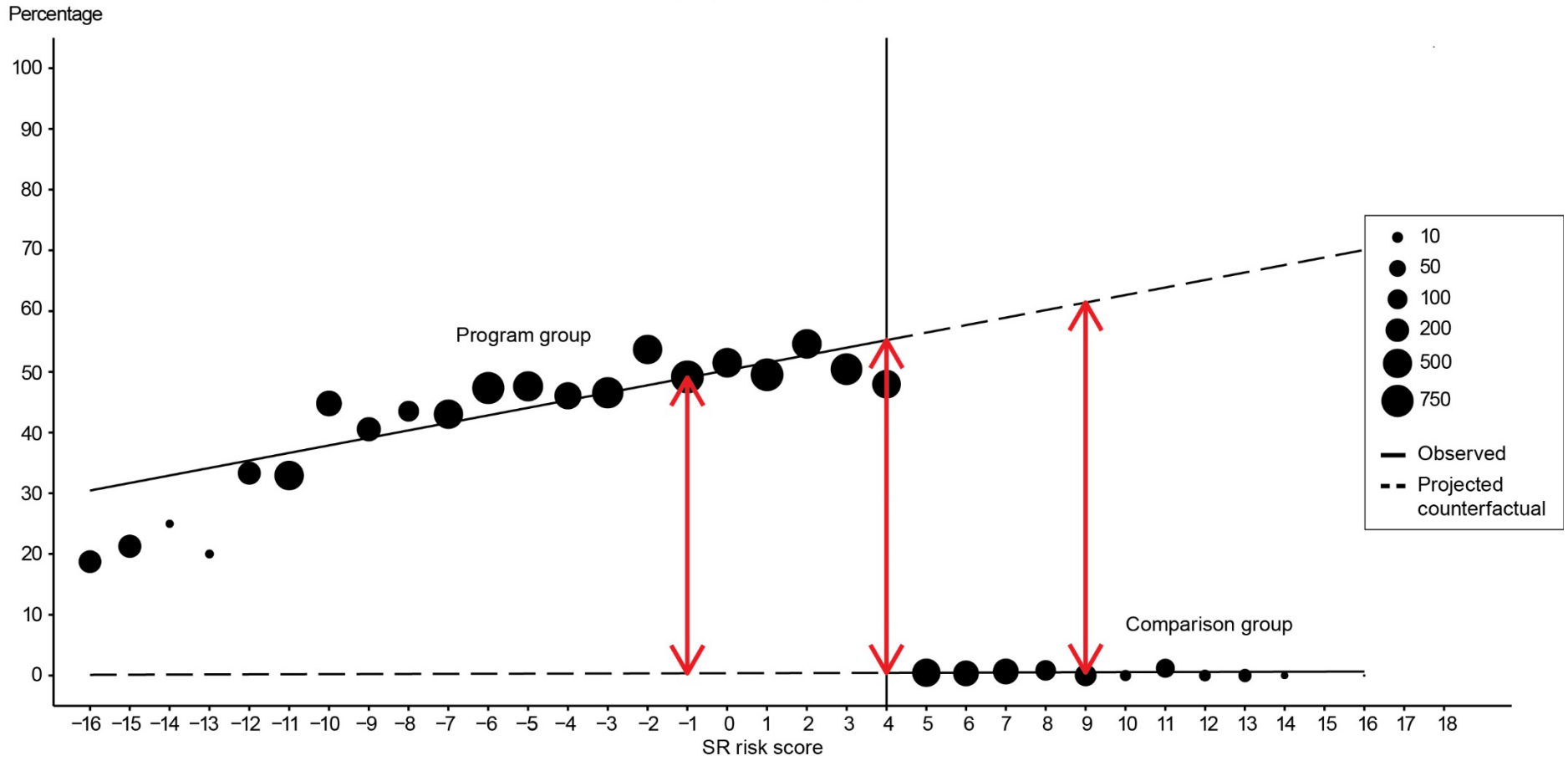
Appendix Figure B.10 presents similar information for the percentage of defendants who received bail. As can be seen, the large reduction in bail receipt rates for defendants with a risk score of four holds for defendants within an appreciable range of risk scores below and above four. Hence, the large negative impact of SR eligibility (ITT) on bail receipt appears to hold for an appreciable (and potentially policy-relevant) range of risk scores.

Now consider what the preceding findings imply about the generalizability of the present estimate of the impact of SR enrollment (TOT) on bail receipt rates. To do so, note first that the estimate of a TOT impact for a risk score of four — or any other risk score — equals the ratio of the vertical distance between the program group and comparison group regression lines for a given defendant outcome (for example, the bail receipt rate in Appendix Figure B.10) at that risk score to the corresponding vertical distance for the SR enrollment rate. Then note from Appendix Figures B.9 and B.10 that these vertical distances do not change markedly for a broad range of risk scores around four. Hence, the estimated value of TOT does not change markedly across this range of risk scores. This result, which holds for most defendant outcomes in the present analysis (see Appendix C), suggests considerable generalizability of the findings.

[29]For example, see Angrist and Rokkanen (2015); Bloom, Bell, and Reiman (2020).

Appendix Figure B.9

Percentage of Defendants Enrolled in Supervised Release, by SR Risk Score, with Extrapolations of the Program and Comparison Group Regression Lines (Impact Sample)



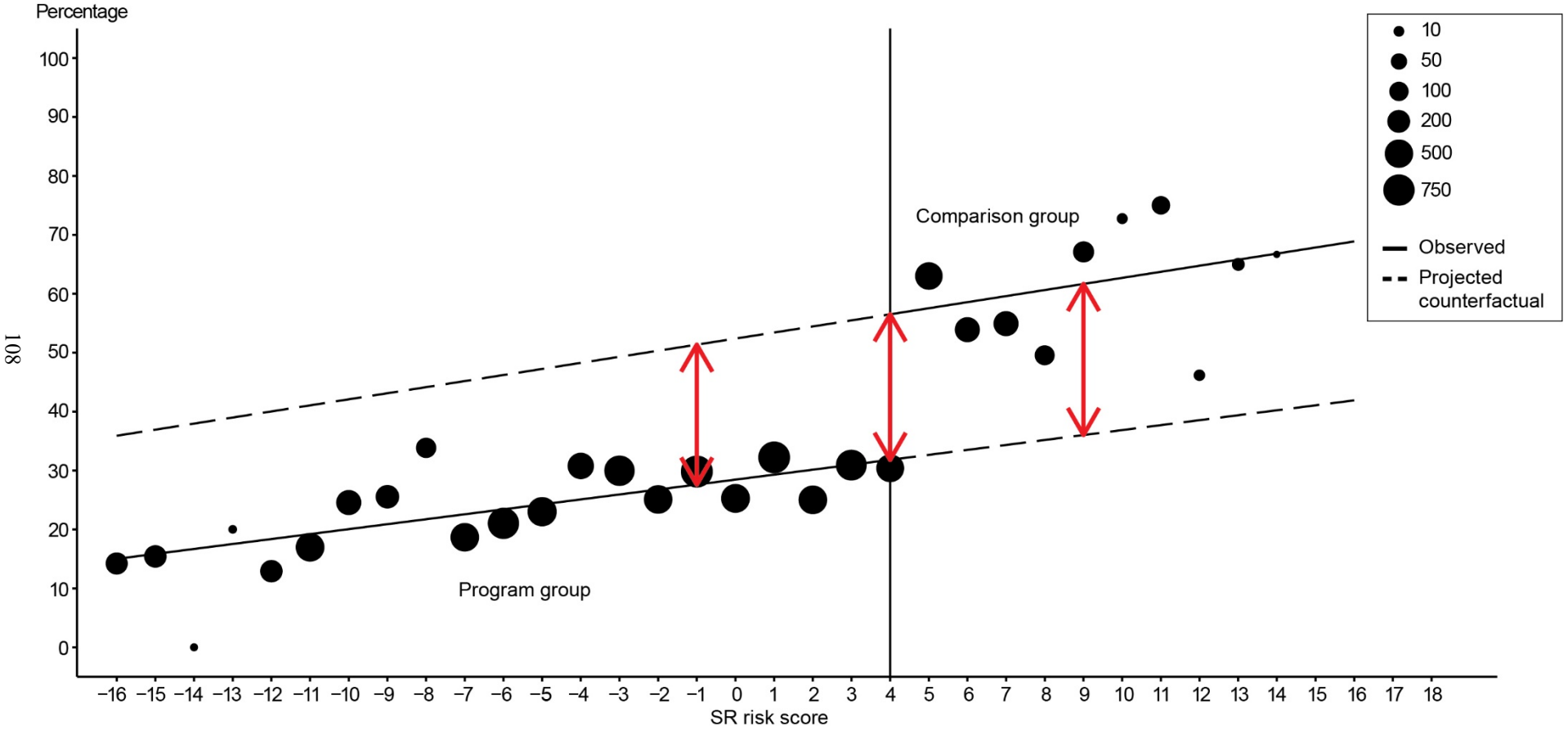
SOURCE: MDRC calculations based on data from SR providers.

NOTE: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure. The red vertical arrows represent the estimated impact of SR eligibility.

Appendix Figure B.10

Percentage of Defendants with Bail Set, by SR Risk Score, with Extrapolations of the Program and Comparison Group Regression Lines

(Impact Sample)



SOURCES: MDRC calculations based on data from the New York State Office of Court Administration and SR providers.

NOTE: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure. The red vertical arrows represent the estimated impact of SR eligibility.

A second potential threat to the external validity of the present RDD impact estimates is the possibility that the sample of defendants studied does not represent a policy-relevant subpopulation of New York City's SR enrollees. Fortunately, this sample contains 94 percent of all new SR enrollees during the sample intake period (May 1, 2017 to April 30, 2018).[30] Hence, it comprises virtually the entire population of new SR enrollees during that period. This fact, combined with the preceding result that for most outcomes, SR impact estimates appear to generalize to an appreciable range of risk scores, suggests that the present SR impact findings probably reflect the experience of a policy-relevant subpopulation of New York City criminal defendants during the time period represented by the present sample.

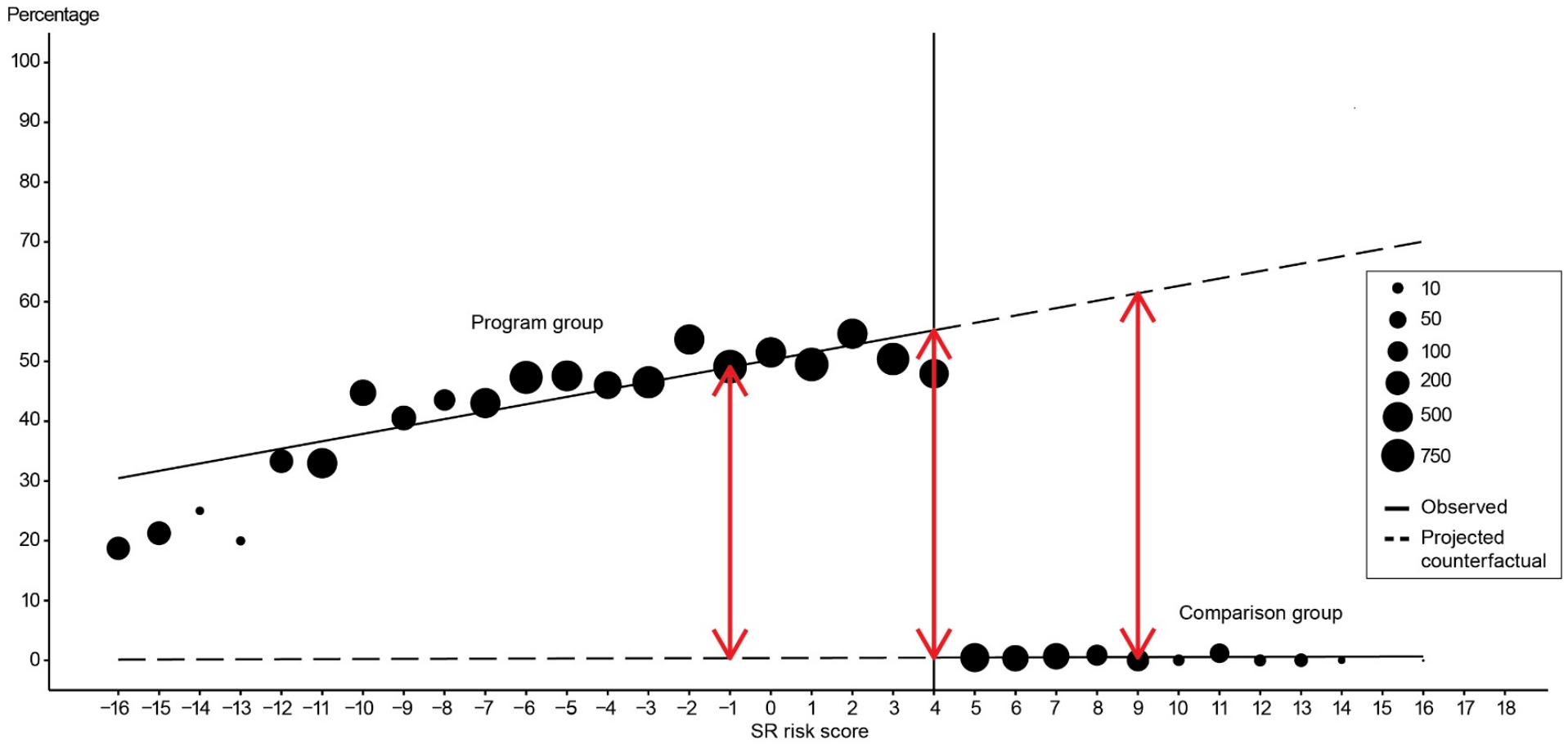
[30]The present sample contains 85 percent of the SR enrollees with a risk score of four.

Appendix C

Regression Discontinuity Graphs of Supervised Release Impacts for Each Defendant Outcome

Appendix Figure C.1

Percentage of Defendants Enrolled in Supervised Release (SR), by SR Risk Score



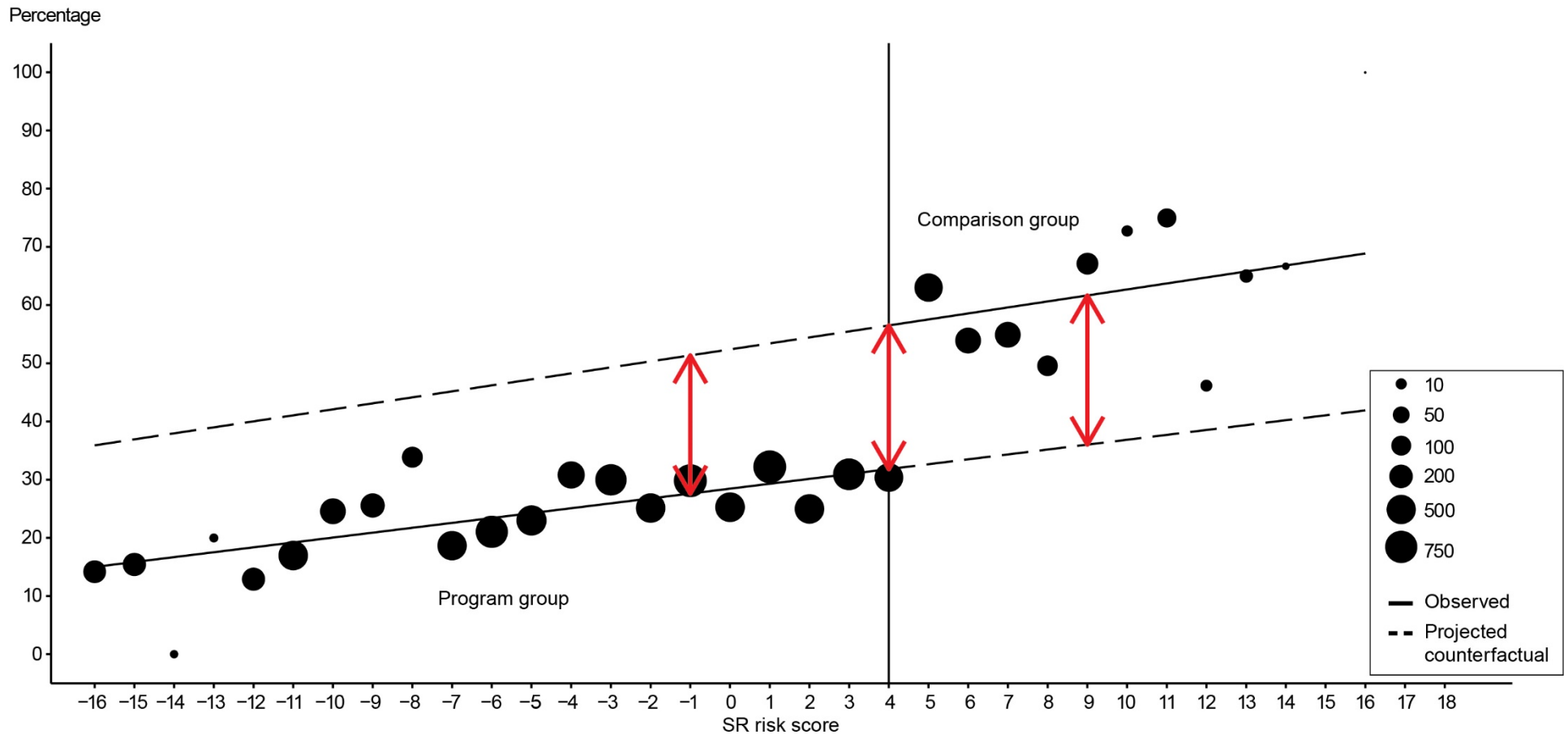
SOURCE: MDRC calculations based on data from SR providers.

NOTES: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure. The red vertical arrows represent the estimated impact of SR eligibility.

Projected counterfactuals are estimates of what the mean value of an outcome would have been without access to SR for program group members and with access to SR for comparison group members.

Appendix Figure C.2

Percentage of Defendants with Bail Set, by SR Risk Score



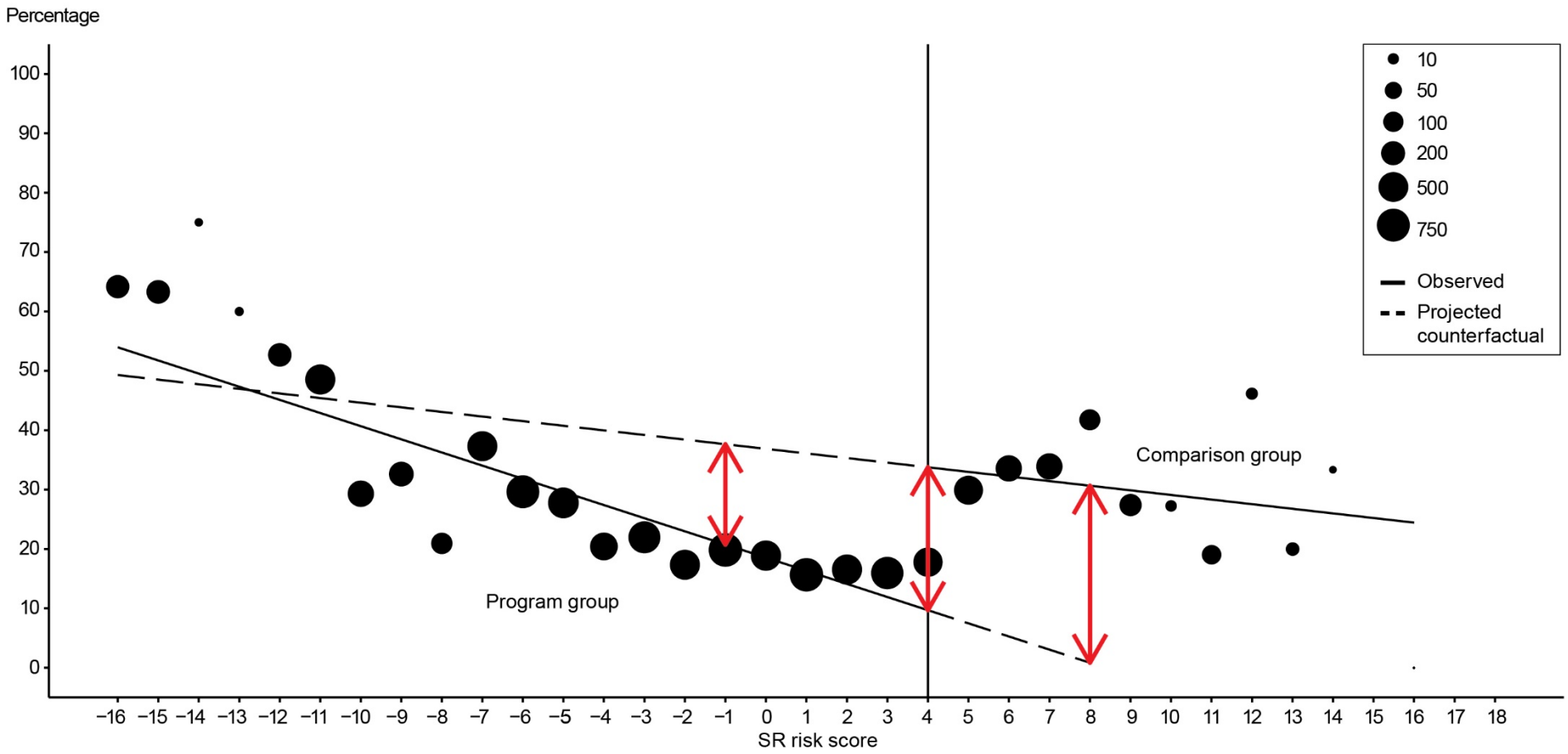
SOURCES: MDRC calculations based on data from the New York State Office of Court Administration and SR providers.

NOTES: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure. The red vertical arrows represent the estimated impact of SR eligibility.

Projected counterfactuals are estimates of what the mean value of an outcome would have been without access to SR for program group members and with access to SR for comparison group members.

Appendix Figure C.3

Percentage of Defendants Released on Their Own Recognizance, by SR Risk Score



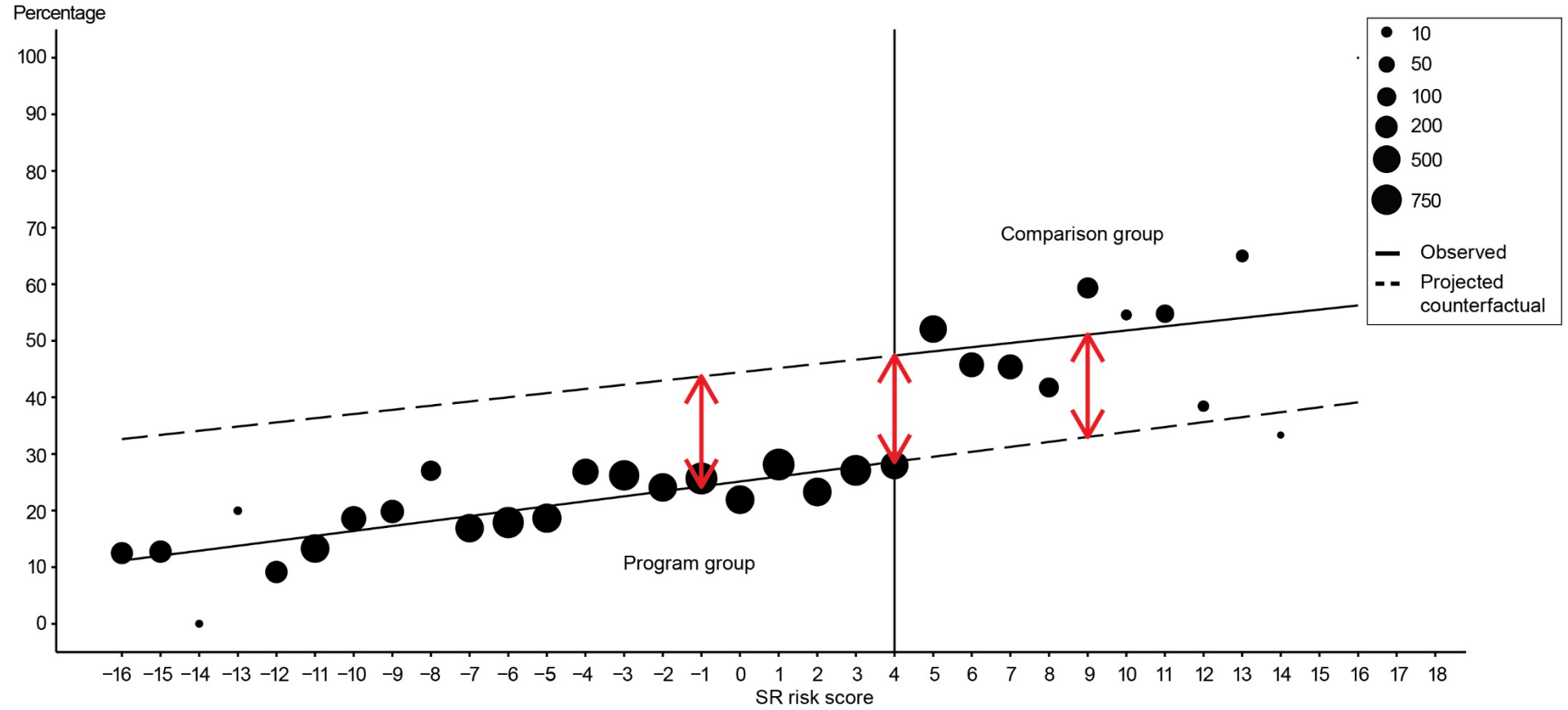
SOURCES: MDRC calculations based on data from the New York State Office of Court Administration and SR providers.

NOTES: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure. The red vertical arrows represent the estimated impact of SR eligibility.

Projected counterfactuals are estimates of what the mean value of an outcome would have been without access to SR for program group members and with access to SR for comparison group members.

Appendix Figure C.4

Percentage of Defendants Detained at Arraignment, by SR Risk Score



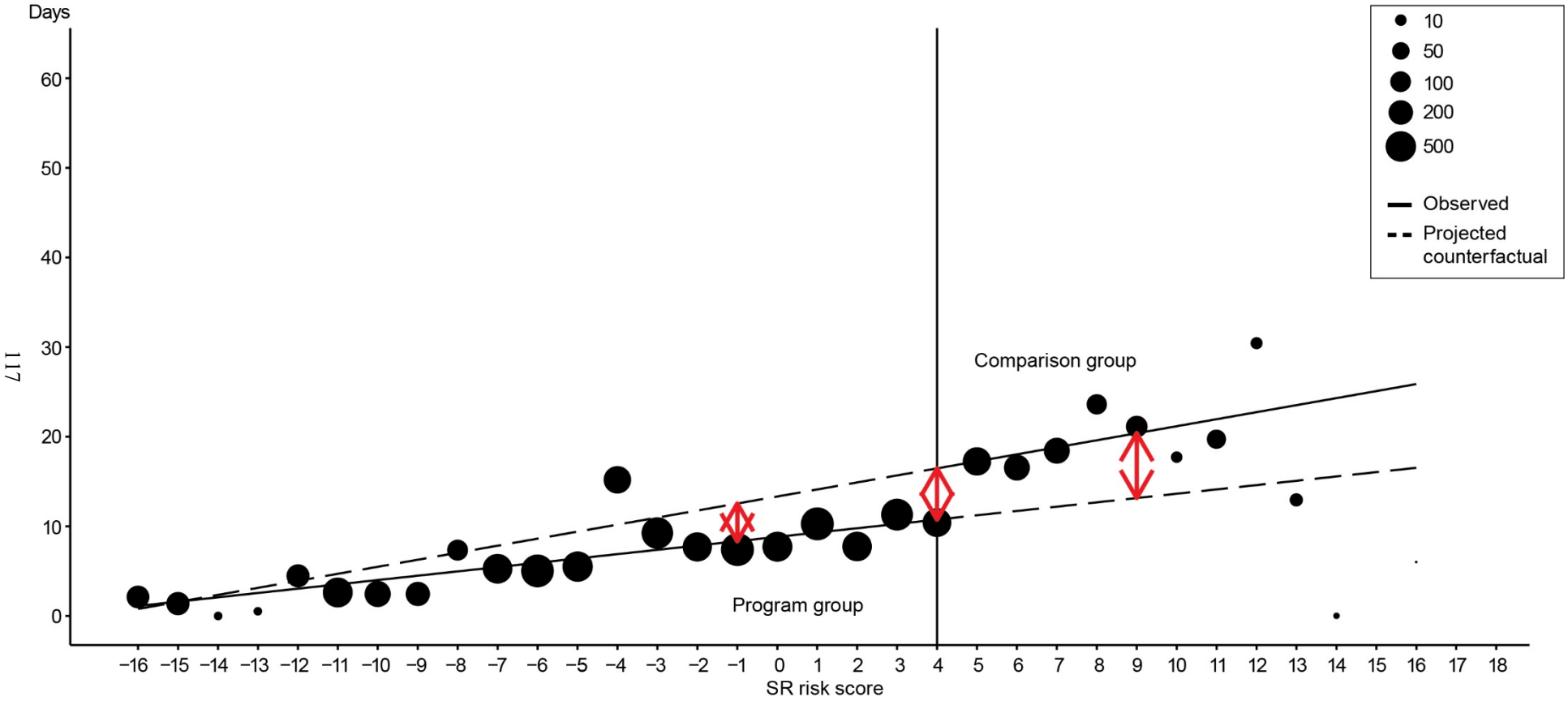
SOURCES: MDRC calculations based on data from the New York City Department of Correction and SR providers.

NOTES: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure. The red vertical arrows represent the estimated impact of SR eligibility.

Projected counterfactuals are estimates of what the mean value of an outcome would have been without access to SR for program group members and with access to SR for comparison group members.

Appendix Figure C.5

Days Detained While Awaiting Trial, by SR Risk Score



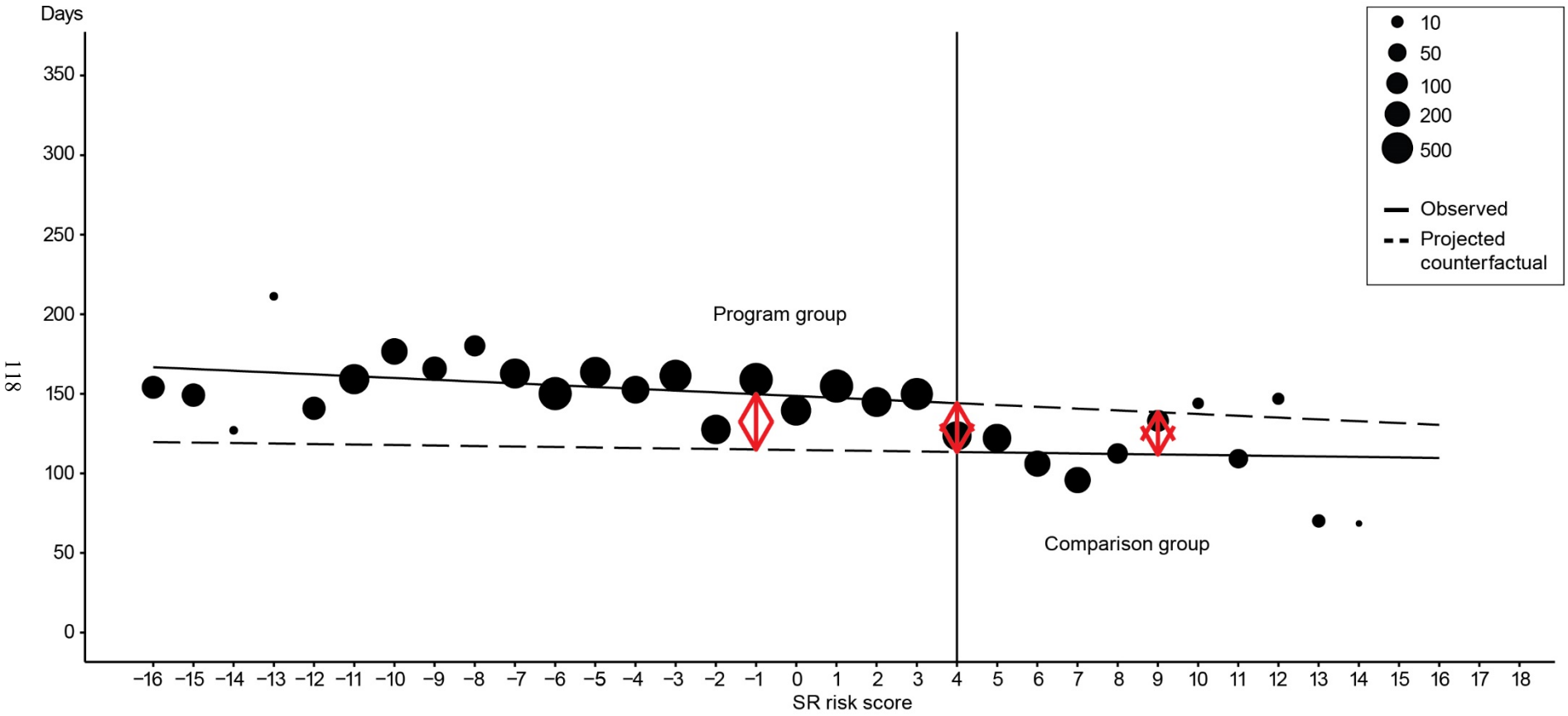
SOURCES: MDRC calculations based on data from the New York City Department of Correction and SR providers.

NOTES: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure. The red vertical arrows represent the estimated impact of SR eligibility.

Projected counterfactuals are estimates of what the mean value of an outcome would have been without access to SR for program group members and with access to SR for comparison group members.

Appendix Figure C.6

Length of the Pretrial Period, by SR Risk Score



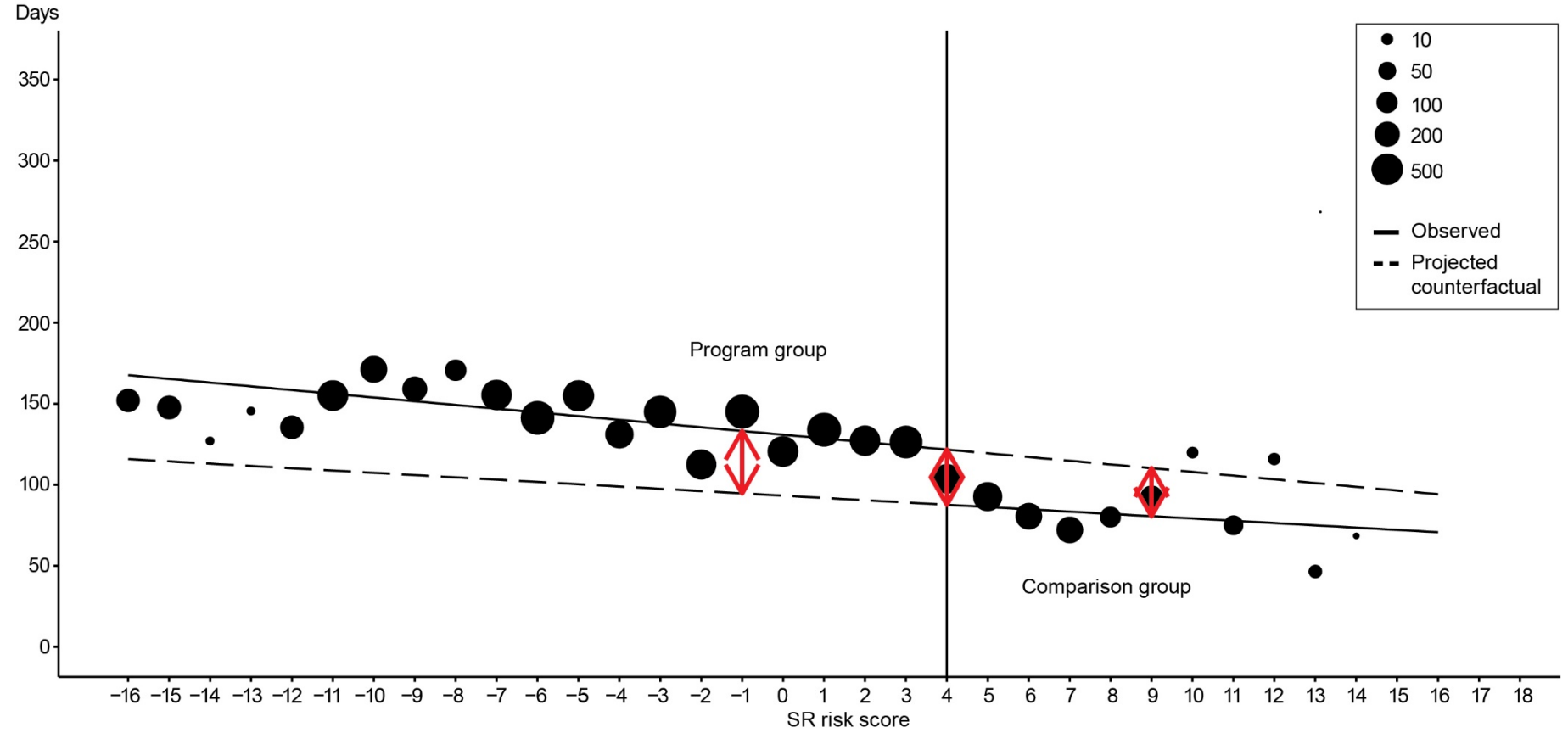
SOURCES: MDRC calculations based on data from the New York State Office of Court Administration and SR providers.

NOTES: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure. The red vertical arrows represent the estimated impact of SR eligibility.

Projected counterfactuals are estimates of what the mean value of an outcome would have been without access to SR for program group members and with access to SR for comparison group members.

Appendix Figure C.7

Days Exposed to Pretrial Risk, by SR Risk Score



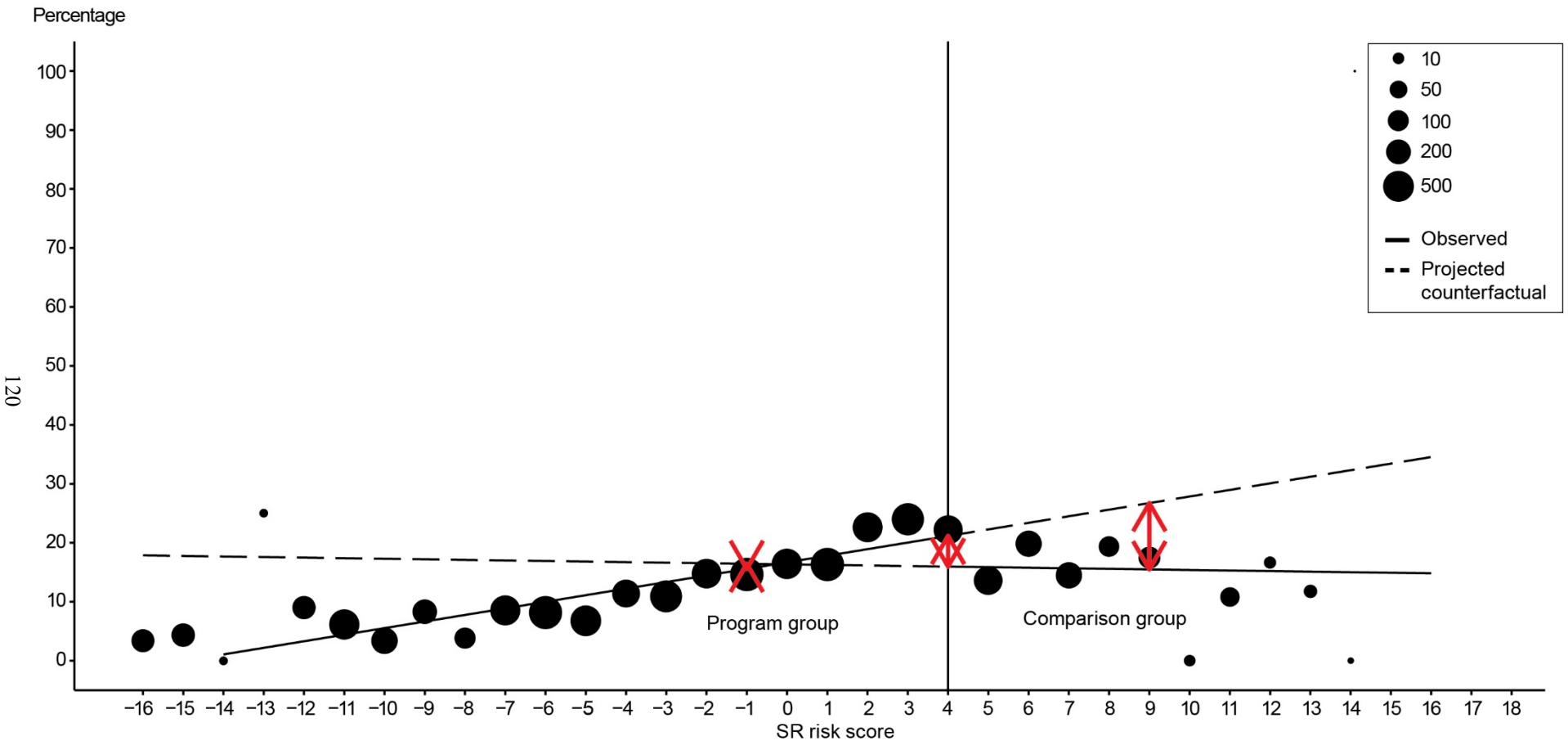
SOURCES: MDRC calculations based on data from the New York State Office of Court Administration, the New York City Department of Correction, and SR providers.

NOTES: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure. The red vertical arrows represent the estimated impact of SR eligibility.

Projected counterfactuals are estimates of what the mean value of an outcome would have been without access to SR for program group members and with access to SR for comparison group members.

Appendix Figure C.8

Percentage of Defendants Issued Bench Warrants, by SR Risk Score



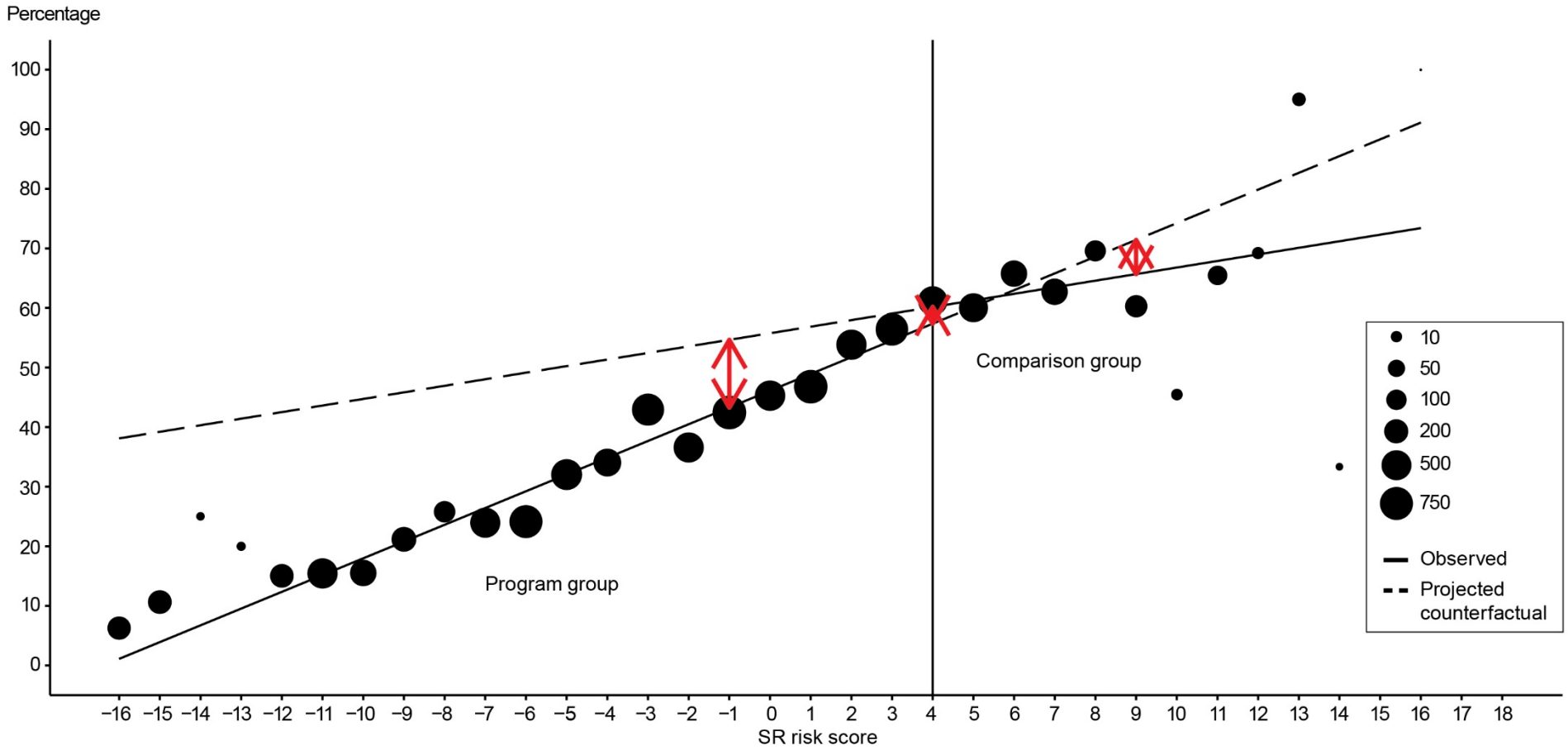
SOURCES: MDRC calculations based on data from the New York State Office of Court Administration and SR providers.

NOTES: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure. The red vertical arrows represent the estimated impact of SR eligibility.

Projected counterfactuals are estimates of what the mean value of an outcome would have been without access to SR for program group members and with access to SR for comparison group members.

Appendix Figure C.9

Percentage of Defendants with New Arrests Within Nine Months, by SR Risk Score



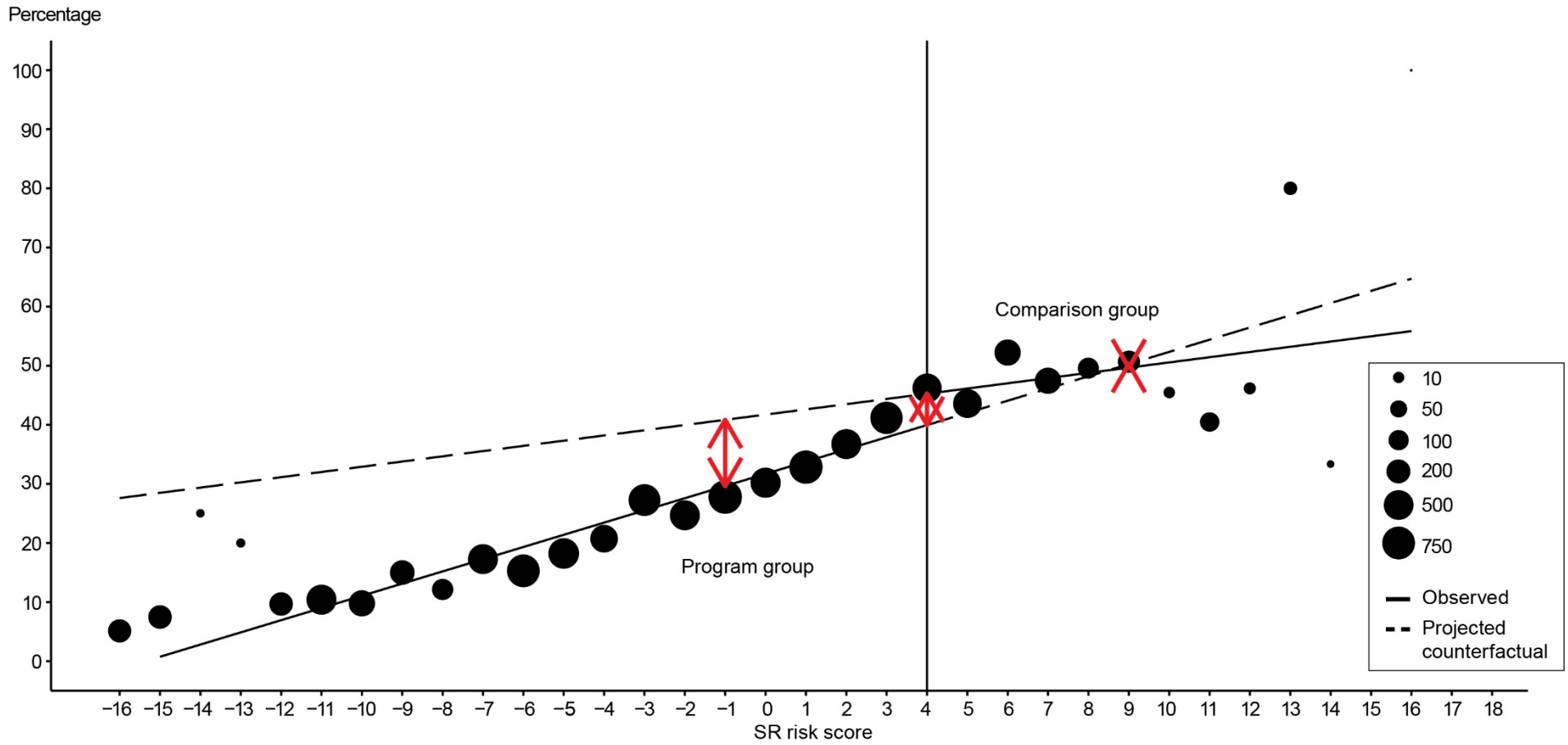
SOURCES: MDRC calculations based on data from the New York State Office of Court Administration and SR providers.

NOTES: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure. The red vertical arrows represent the estimated impact of SR eligibility.

Projected counterfactuals are estimates of what the mean value of an outcome would have been without access to SR for program group members and with access to SR for comparison group members.

Appendix Figure C.10

Percentage of Defendants with New Misdemeanor Arrests Within Nine Months, by SR Risk Score



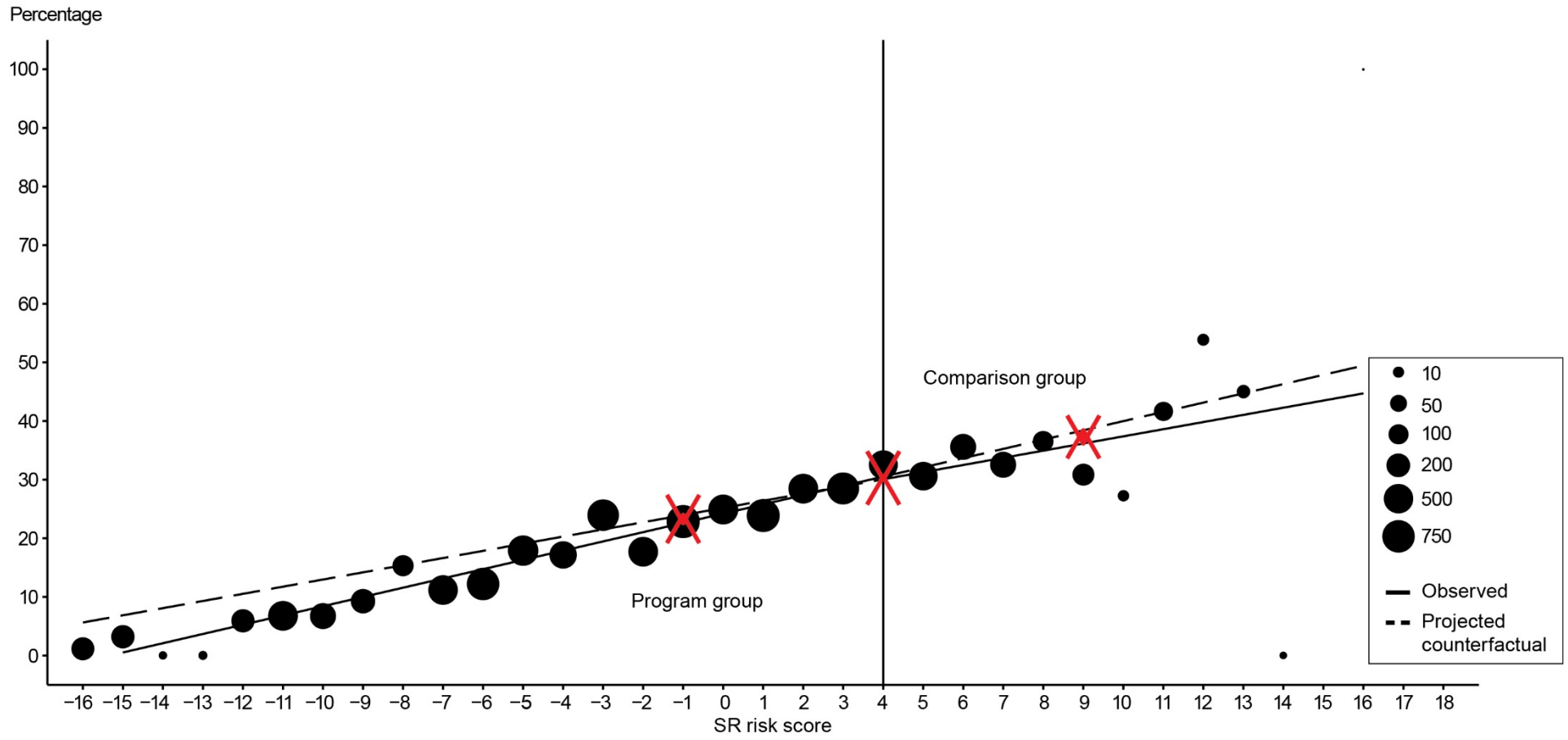
SOURCES: MDRC calculations based on data from the New York State Office of Court Administration and SR providers.

NOTES: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure. The red vertical arrows represent the estimated impact of SR eligibility.

Projected counterfactuals are estimates of what the mean value of an outcome would have been without access to SR for program group members and with access to SR for comparison group members.

Appendix Figure C.11

Percentage of Defendants with New Felony Arrests Within Nine Months, by SR Risk Score



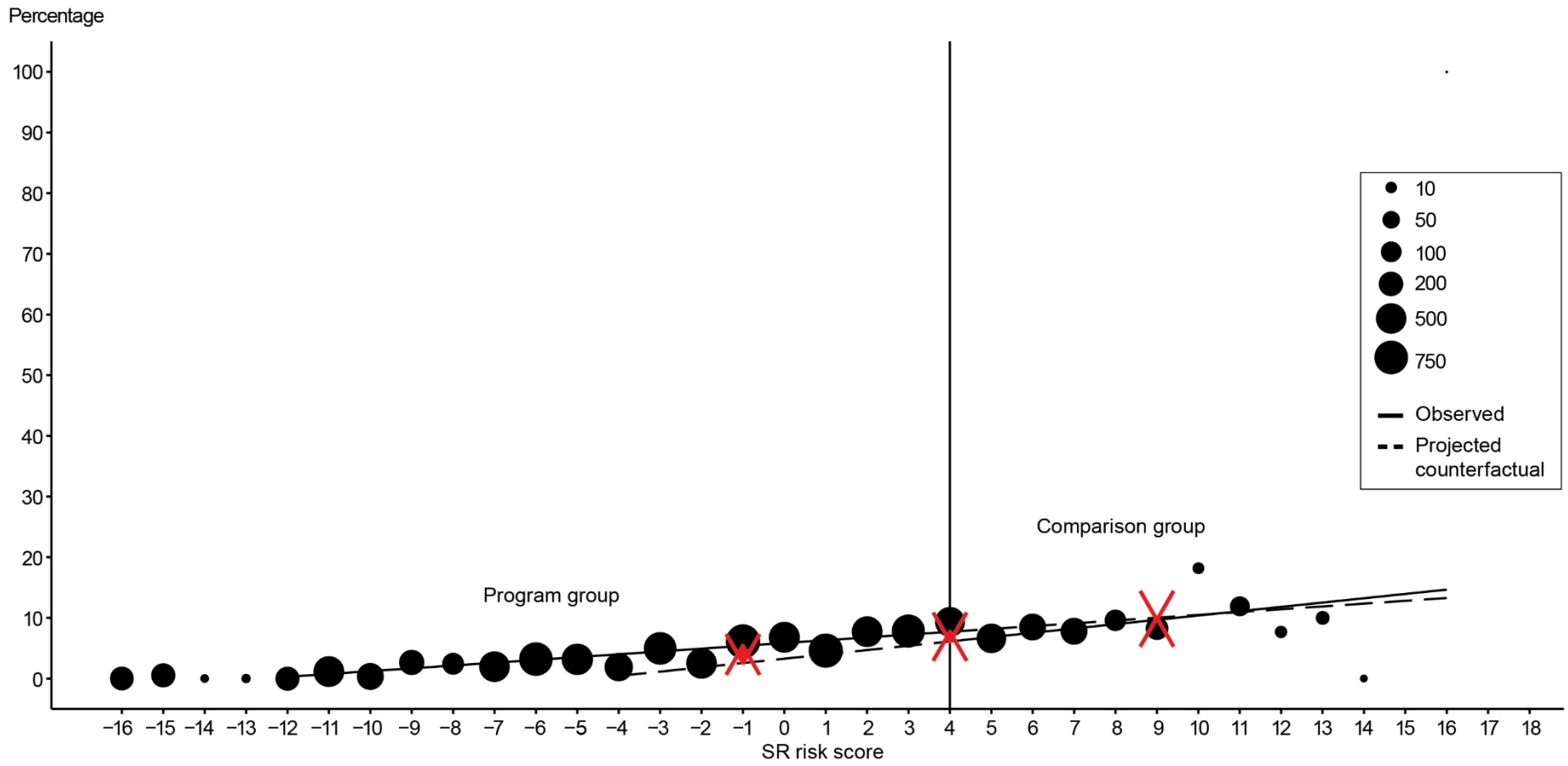
SOURCES: MDRC calculations based on data from the New York State Office of Court Administration and SR providers.

NOTES: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure. The red vertical arrows represent the estimated impact of SR eligibility.

Projected counterfactuals are estimates of what the mean value of an outcome would have been without access to SR for program group members and with access to SR for comparison group members.

Appendix Figure C.12

Percentage of Defendants with New Violent Felony Arrests Within Nine Months, by SR Risk Score



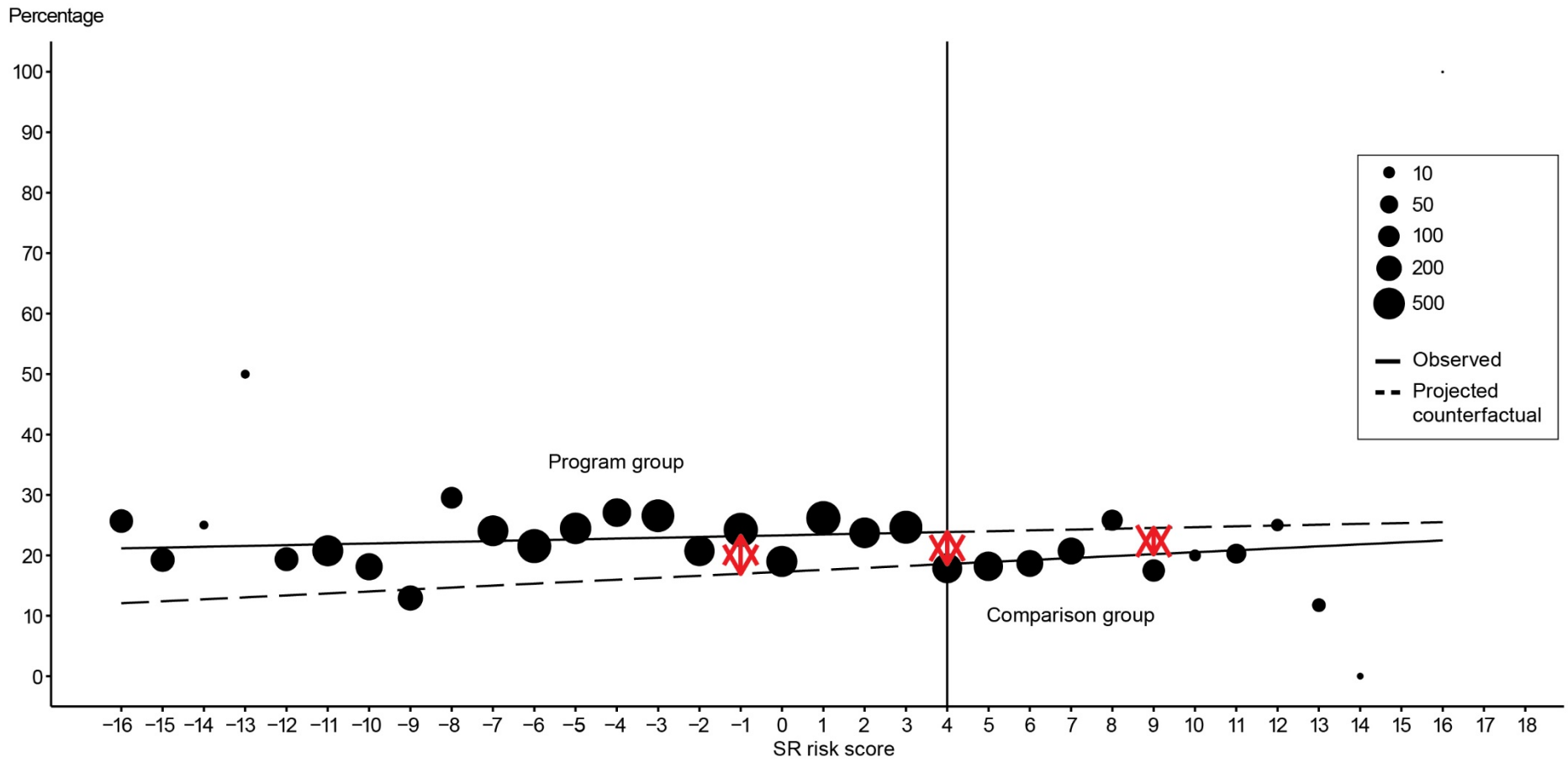
SOURCES: SOURCES: MDRC calculations based on data from the New York State Office of Court Administration and SR providers.

NOTES: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure. The red vertical arrows represent the estimated impact of SR eligibility.

Projected counterfactuals are estimates of what the mean value of an outcome would have been without access to SR for program group members and with access to SR for comparison group members.

Appendix Figure C.13

Percentage of Defendants Whose Cases Were Dismissed, by SR Risk Score



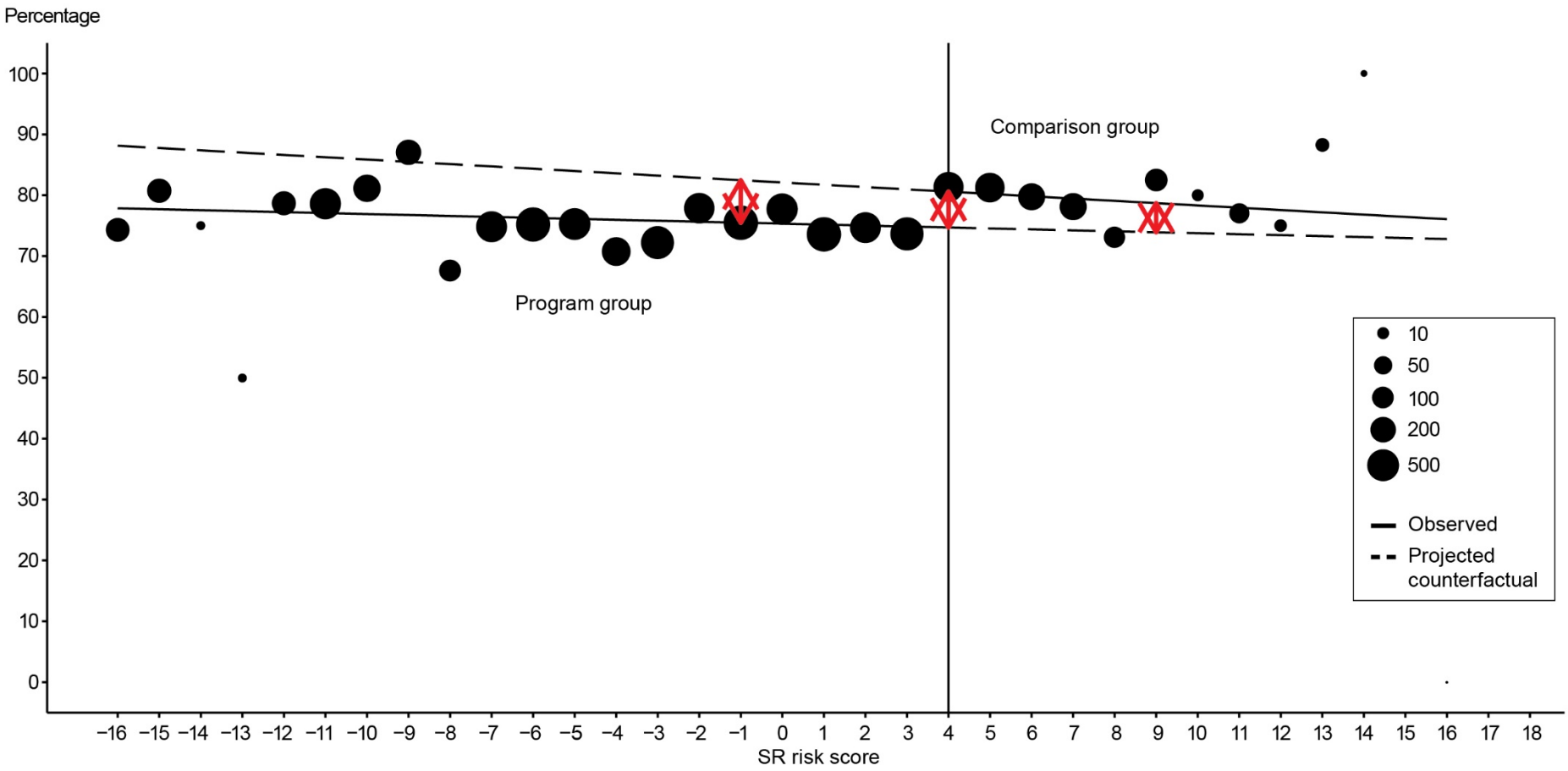
SOURCES: MDRC calculations based on data from the New York State Office of Court Administration and SR providers.

NOTES: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure. The red vertical arrows represent the estimated impact of SR eligibility.

Projected counterfactuals are estimates of what the mean value of an outcome would have been without access to SR for program group members and with access to SR for comparison group members.

Appendix Figure C.14

Percentage of Defendants Found Guilty, by SR Risk Score



SOURCES: MDRC calculations based on data from the New York State Office of Court Administration and SR providers.

NOTES: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure. The red vertical arrows represent the estimated impact of SR eligibility.

Projected counterfactuals are estimates of what the mean value of an outcome would have been without access to SR for program group members and with access to SR for comparison group members.

Appendix D

Estimated Intent-to-Treat (ITT) Impacts of Supervised Release for Each Defendant Outcome

Appendix Table D.1

Estimated Impact of Supervised Release (SR) Eligibility on SR Enrollment

Outcome (%)	Mean Program Outcome	Mean Counterfactual Outcome	Impact Estimate	P-Value	95 Percent Confidence Interval
Enrolled in SR	55.2	0.4	54.8***	$1.35 * 10^{-22}$	(51.03, 58.51)

SOURCE: MDRC calculations based on SR provider data.

NOTES: The estimated mean counterfactual outcome is that which would have occurred in the absence of SR eligibility. Statistical significance levels are indicated as follows: *** = 0.1 percent; ** = 1 percent; * = 5 percent.

Appendix Table D.2

Estimated Impacts of SR Eligibility on Pretrial Release Conditions

Outcome (%)	Mean Program Outcome	Mean Counterfactual Outcome	Impact Estimate	P-Value	95 Percent Confidence Interval
Bail set	31.8	56.5	-24.7***	$2.83 * 10^{-5}$	(-34.82, -14.56)
Released on one's own recognizance (ROR)	9.7	33.8	-24.1***	$1.14 * 10^{-7}$	(-31.06, -17.08)

SOURCES: MDRC calculations based on data from the New York State Office of Court Administration and SR providers.

NOTES: The estimated mean counterfactual outcome is that which would have occurred in the absence of SR eligibility. Statistical significance levels are indicated as follows: *** = 0.1 percent; ** = 1 percent; * = 5 percent.

Appendix Table D.3

Estimated Impacts of SR Eligibility on Pretrial Detention

Outcome	Mean Program Outcome	Mean Counterfactual Outcome	Impact Estimate	P-Value	95 Percent Confidence Interval
Detained at arraignment (%)	28.6	47.4	-18.8***	$1.13 * 10^{-5}$	(-25.97, -11.54)
Number of days detained while awaiting trial	10.7	16.5	-5.7***	$1.07 * 10^{-5}$	(-7.91, -3.53)

SOURCES: MDRC calculations based on data from the New York City Department of Correction and SR providers.

NOTES: The estimated mean counterfactual outcome is that which would have occurred in the absence of SR eligibility. Statistical significance levels are indicated as follows: *** = 0.1 percent; ** = 1 percent; * = 5 percent.

Appendix Table D.4

Estimated Impacts of SR Eligibility on Pretrial Exposure and Bench Warrants Issued for Missed Court Appearances

Outcome	Mean Program Outcome	Mean Counterfactual Outcome	Impact Estimate	P-Value	95 Percent Confidence Interval
Length of the pretrial period (days)	144.1	113.5	30.6**	0.0038	(10.75, 50.54)
Time exposed to pretrial risk (days)	121.7	87.7	34.0***	$9.74 * 10^{-5}$	(18.68, 49.40)
Bench warrant issued (%)	21.1	16.0	5.2	0.0610	(-0.26, 10.64)

SOURCES: MDRC calculations based on data from the New York State Office of Court Administration, New York City Department of Correction, and SR providers.

NOTES: The estimated mean counterfactual outcome is that which would have occurred in the absence of SR eligibility. Statistical significance levels are indicated as follows: *** = 0.1 percent; ** = 1 percent; * = 5 percent.

Appendix Table D.5

Estimated Impacts of SR Eligibility on New Arrests

Outcome (%)	Mean Program Outcome	Mean Counterfactual Outcome	Impact Estimate	P-Value	95 Percent Confidence Interval
New arrest within 9 months					
Any charge	57.4	60.2	-2.8	0.2498	(-7.70, 2.09)
Misdemeanor	39.9	45.2	-5.3	0.1515	(-12.67, 2.06)
Felony	30.5	30.1	0.5	0.7850	(-2.92, 3.82)
Violent felony	7.7	6.1	1.6	0.0589	(-0.06, 3.24)

SOURCES: MDRC calculations based on data from the New York State Office of Court Administration and SR providers.

NOTES: The estimated mean counterfactual outcome is that which would have occurred in the absence of SR eligibility. Statistical significance levels are indicated as follows: *** = 0.1 percent; ** = 1 percent; * = 5 percent.

Appendix Table D.6

Estimated Impacts of SR Eligibility on Case Outcomes

Outcome (%)	Mean Program Outcome	Mean Counterfactual Outcome	Impact Estimate		P-Value	95 Percent Confidence Interval
Dismissed	23.9	18.6	5.3	**	0.0033	(1.92, 8.65)
Found guilty	74.7	80.6	-5.9	**	0.0020	(-9.41, -2.36)

SOURCES: MDRC calculations based on data from the New York State Office of Court Administration and SR providers.

NOTES: The estimated mean counterfactual outcome is that which would have occurred in the absence of SR eligibility. Statistical significance levels are indicated as follows: *** = 0.1 percent; ** = 1 percent; * = 5 percent.

Appendix E

Supervised Release Impacts for Subgroups of Defendants

Appendix Table E.1

Estimated Impacts of Supervised Release (SR) Eligibility on SR Enrollment, by Charge Class of Offense

Outcome (%)	Charge Class of Offense	Mean Program Outcome	Mean Counterfactual Outcome	Impact Estimate		P-Value	95 Percent Confidence Interval	Difference Between Subgroup Impacts
Enrolled in SR	Misdemeanor	52.9	0.0	52.9	***	$8.13^{*}\ 10^{-21}$	(49.35, 56.46)	
Enrolled in SR	Felony	56.9	0.8	56.1	***	$6.15^{*}\ 10^{-20}$	(51.80, 60.41)	

SOURCES: MDRC calculations based on data from the New York State Office of Court Administration and SR providers.

NOTES: The estimated mean counterfactual outcome is that which would have occurred in the absence of SR eligibility. Statistical significance levels are indicated as follows: *** = 0.1 percent; ** = 1 percent; * = 5 percent. Impacts were calculated separately for each subgroup. Impact estimates were then examined for statistically significant differences between subgroups. Statistically significant differences between subgroups are indicated as: ††† = 0.1 percent; †† = 1 percent; † = 5 percent.

Appendix Table E.2

Estimated Impacts of SR Eligibility on SR Enrollment, by Borough

Outcome (%)	Borough	Mean Program Outcome	Mean Counterfactual Outcome	Impact Estimate		P-Value	95 Percent Confidence Interval	Difference Among Subgroup Impacts
Enrolled in SR	Bronx	49.8	0.0	49.8	***	$2.00^{*}\ 10^{-19}$	(46.00, 53.65)	†††
Enrolled in SR	Brooklyn	65.9	0.0	65.9	***	$1.70^{*}\ 10^{-22}$	(61.91, 69.92)	†††
Enrolled in SR	Manhattan	63.2	2.3	60.9	***	$2.46^{*}\ 10^{-20}$	(56.59, 65.17)	†††
Enrolled in SR	Queens	40.7	0.0	40.7	***	$1.38^{*}\ 10^{-15}$	(36.04, 45.45)	†††

SOURCES: MDRC calculations based on data from the New York State Office of Court Administration and SR providers.

NOTES: The estimated mean counterfactual outcome is that which would have occurred in the absence of SR eligibility. Statistical significance levels are indicated as follows: *** = 0.1 percent; ** = 1 percent; * = 5 percent. Impacts were calculated separately for each subgroup. Impact estimates were then examined for statistically significant differences among subgroups. Statistically significant differences among subgroups are indicated as: ††† = 0.1 percent; †† = 1 percent; † = 5 percent. Staten Island was excluded from the analysis due to its small sample size.

Appendix Table E.3

Estimated Impacts of SR Eligibility on SR Enrollment, by Race/Ethnicity

Outcome (%)	Race/Ethnicity	Mean Program Outcome	Mean Counterfactual Outcome	Impact Estimate		P-Value	95 Percent Confidence Interval	Difference Among Subgroup Impacts
Enrolled in SR	Black, non-Hispanic	54.7	0.3	54.4	***	$2.12 * 10^{-19}$	(49.79, 58.95)	
Enrolled in SR	Hispanic	55.7	0.7	55.0	***	$1.24 * 10^{-20}$	(51.02, 58.94)	
Enrolled in SR	White, non-Hispanic	51.6	0.6	51.0	***	$1.95 * 10^{-12}$	(42.96, 59.07)	

SOURCES: MDRC calculations based on data from the New York State Office of Court Administration and SR providers.

NOTES: The estimated mean counterfactual outcome is that which would have occurred in the absence of SR eligibility. Statistical significance levels are indicated as follows: *** = 0.1 percent; ** = 1 percent; * = 5 percent. Impacts were calculated separately for each subgroup. Impact estimates were then examined for statistically significant differences among subgroups. Statistically significant differences among subgroups are indicated as: ††† = 0.1 percent; †† = 1 percent; † = 5 percent. Other racial/ethnic categories were excluded from the analysis due to their small sample sizes.

Appendix Table E.4

Estimated Impacts of SR Eligibility on SR Enrollment, by Age

Outcome (%)	Age	Mean Program Outcome	Mean Counterfactual Outcome	Impact Estimate		P-Value	95 Percent Confidence Interval	Difference Between Subgroup Impacts
Enrolled in SR	16 to 24	59.3	0.4	58.8	***	$6.08 * 10^{-21}$	(54.90, 62.72)	
Enrolled in SR	25 and older	53.9	0.5	53.4	***	$2.45 * 10^{-19}$	(49.08, 57.75)	

SOURCES: MDRC calculations based on data from the New York State Office of Court Administration and SR providers.

NOTES: The estimated mean counterfactual outcome is that which would have occurred in the absence of SR eligibility. Statistical significance levels are indicated as follows: *** = 0.1 percent; ** = 1 percent; * = 5 percent. Impacts were calculated separately for each subgroup. Impact estimates were then examined for statistically significant differences between subgroups. Statistically significant differences between subgroups are indicated as: ††† = 0.1 percent; †† = 1 percent; † = 5 percent.

Appendix Table E.5

Estimated Impacts of SR Enrollment, by Charge Class of Offense

Outcome (%)	Charge Class of Offense	Mean Program Outcome	Mean Counterfactual Outcome	Impact Estimate		P-Value	95 Percent Confidence Interval	Difference Between Subgroup Impacts
Bail set	Misdemeanor	0.0	33.7	-33.7	*	0.0132	(-58.51, -8.96)	
Bail set	Felony	0.0	54.0	-54.0	***	$3.18 * 10^{-11}$	(-63.62, -44.28)	
Detained at arraignment	Misdemeanor	0.0	25.2	-25.2	*	0.0263	(-46.07, -4.27)	
Detained at arraignment	Felony	3.5	48.2	-44.7	***	$2.42 * 10^{-15}$	(-49.98, -39.41)	
Bench warrant issued	Misdemeanor	41.8	19.6	22.3	*	0.0246	(4.02, 40.50)	
Bench warrant issued	Felony	29.4	24.4	5.0		0.1548	(-1.68, 11.64)	
New felony arrest within 9 months	Misdemeanor	31.4	28.6	2.9		0.5862	(-7.29, 13.01)	
New felony arrest within 9 months	Felony	40.6	37.8	2.8		0.6340	(-8.53, 14.09)	

SOURCES: MDRC calculations based on data from the New York State Office of Court Administration, New York City Department of Correction, and SR providers.

NOTES: The estimated mean counterfactual outcome is that which would have occurred in the absence of SR enrollment. Statistical significance levels are indicated as follows: *** = 0.1 percent; ** = 1 percent; * = 5 percent. Impacts were calculated separately for each subgroup. Impact estimates were then examined for statistically significant differences between subgroups. Statistically significant differences between subgroups are indicated as: ††† = 0.1 percent; †† = 1 percent; † = 5 percent.

Appendix Table E.6

Estimated Impacts of SR Enrollment, by Borough

Outcome (%)	Borough	Mean Program Outcome	Mean Counterfactual Outcome	Impact Estimate		P-Value	95 Percent Confidence Interval	Difference Among Subgroup Impacts
Bail set	Bronx	0.0	43.1	-43.1	***	0.0002	(-62.18, -23.96)	
Bail set	Brooklyn	0.0	45.4	-45.4	***	$1.10 * 10^{-5}$	(-61.77, -28.99)	
Bail set	Manhattan	0.0	64.4	-64.4	***	$3.43 * 10^{-13}$	(-73.52, -55.23)	
Bail set	Queens	0.0	42.1	-42.1	*	0.0194	(-75.14, -8.99)	
Detained at arraignment	Bronx	2.2	34.3	-32.1	***	0.0005	(-47.99, -16.25)	†††
Detained at arraignment	Brooklyn	2.3	26.8	-24.5	**	0.0060	(-40.52, -8.46)	†††
Detained at arraignment	Manhattan	3.3	65.1	-61.8	***	$2.54 * 10^{-12}$	(-71.41, -52.17)	†††
Detained at arraignment	Queens	0.0	26.7	-26.7	*	0.0390	(-50.81, -2.63)	†††
Bench warrant issued	Bronx	14.3	9.6	4.6		0.4322	(-6.74, 16.02)	
Bench warrant issued	Brooklyn	50.8	39.7	11.1		0.2039	(-5.58, 27.79)	
Bench warrant issued	Manhattan	33.9	11.5	22.5	***	0.0003	(12.16, 32.80)	
Bench warrant issued	Queens	19.0	16.2	2.9		0.7020	(-11.69, 17.44)	
New felony arrest within 9 months	Bronx	34.8	26.1	8.6		0.1402	(-2.48, 19.75)	
New felony arrest within 9 months	Brooklyn	41.9	42.7	-0.8		0.8743	(-11.17, 9.49)	
New felony arrest within 9 months	Manhattan	38.3	40.5	-2.1		0.5350	(-8.79, 4.52)	
New felony arrest within 9 months	Queens	26.1	13.3	12.8		0.2280	(-7.49, 32.99)	

SOURCES: MDRC calculations based on data from the New York State Office of Court Administration, New York City Department of Correction, and SR providers.

NOTES: The estimated mean counterfactual outcome is that which would have occurred in the absence of SR enrollment. Statistical significance levels are indicated as follows: *** = 0.1 percent; ** = 1 percent; * = 5 percent. Impacts were calculated separately for each subgroup. Impact estimates were then examined for statistically significant differences among subgroups. Statistically significant differences among subgroups are indicated as: ††† = 0.1 percent; †† = 1 percent; † = 5 percent. Staten Island was excluded from the analysis due to its small sample size.

Appendix Table E.7

Estimated Impacts of SR Enrollment, by Race/Ethnicity

Outcome (%)	Race/Ethnicity	Mean Program Outcome	Mean Counterfactual Outcome	Impact Estimate		P-Value	95 Percent Confidence Interval	Difference Among Subgroup Impacts
Bail set	Black, non-Hispanic	0.0	47.7	-47.7	***	0.0001	(-68.25, -27.05)	
Bail set	Hispanic	0.0	37.7	-37.7	***	0.0001	(-54.37, -21.05)	
Bail set	White, non-Hispanic	0.0	49.3	-49.3	***	$1.08 * 10^{-6}$	(-64.64, -34.04)	
Detained at arraignment	Black, non-Hispanic	3.0	39.2	-36.1	***	0.0003	(-53.35, -18.94)	
Detained at arraignment	Hispanic	1.6	30.0	-28.4	***	$2.07 * 10^{-5}$	(-39.19, -17.69)	
Detained at arraignment	White, non-Hispanic	0.0	38.4	-38.4	***	0.0001	(-54.88, -21.86)	
Bench warrant issued	Black, non-Hispanic	37.6	23.3	14.4	*	0.0379	(1.47, 27.25)	
Bench warrant issued	Hispanic	32.0	27.7	4.3		0.3377	(-4.36, 13.03)	
Bench warrant issued	White, non-Hispanic	30.8	24.1	6.7		0.3749	(-7.83, 21.20)	
New felony arrest within 9 months	Black, non-Hispanic	37.1	33.5	3.6		0.6532	(-12.00, 19.24)	
New felony arrest within 9 months	Hispanic	43.8	42.5	1.3		0.8026	(-8.74, 11.32)	
New felony arrest within 9 months	White, non-Hispanic	20.0	32.4	-12.4		0.1315	(-27.95, 3.20)	

SOURCES: MDRC calculations based on data from the New York State Office of Court Administration, New York City Department of Correction, and SR providers.

NOTES: The estimated mean counterfactual outcome is that which would have occurred in the absence of SR enrollment. Statistical significance levels are indicated as follows: *** = 0.1 percent; ** = 1 percent; * = 5 percent. Impacts were calculated separately for each subgroup. Impact estimates were then examined for statistically significant differences among subgroups. Statistically significant differences among subgroups are indicated as: ††† = 0.1 percent; †† = 1 percent; † = 5 percent. Other racial/ethnic categories were excluded from the analysis due to their small sample sizes.

Appendix Table E.8

Estimated Impacts of SR Enrollment, by Age

Outcome (%)	Age	Mean Program Outcome	Mean Counterfactual Outcome	Impact Estimate		P-Value	95 Percent Confidence Interval	Difference Between Subgroup Impacts
Bail set	16 to 24	0.0	48.2	-48.2	**	0.0050	(-78.89, -17.54)	
Bail set	25 and older	0.0	41.7	-41.7	***	$2.88^{*}\ 10^{-6}$	(-55.48, -27.95)	
Detained at arraignment	16 to 24	2.5	39.1	-36.6	**	0.0079	(-61.50, -11.74)	
Detained at arraignment	25 and older	2.1	33.1	-31.0	***	$1.37^{*}\ 10^{-6}$	(-40.75, -21.24)	
Bench warrant issued	16 to 24	25.4	27.1	-1.7		0.7386	(-11.82, 8.35)	
Bench warrant issued	25 and older	39.1	25.4	13.6	*	0.0363	(1.53, 25.76)	
New felony arrest within 9 months	16 to 24	42.0	31.4	10.6		0.2544	(-7.18, 28.28)	
New felony arrest within 9 months	25 and older	35.0	38.2	-3.2		0.2703	(-8.69, 2.35)	

SOURCES: MDRC calculations based on data from the New York State Office of Court Administration, New York City Department of Correction, and SR providers.

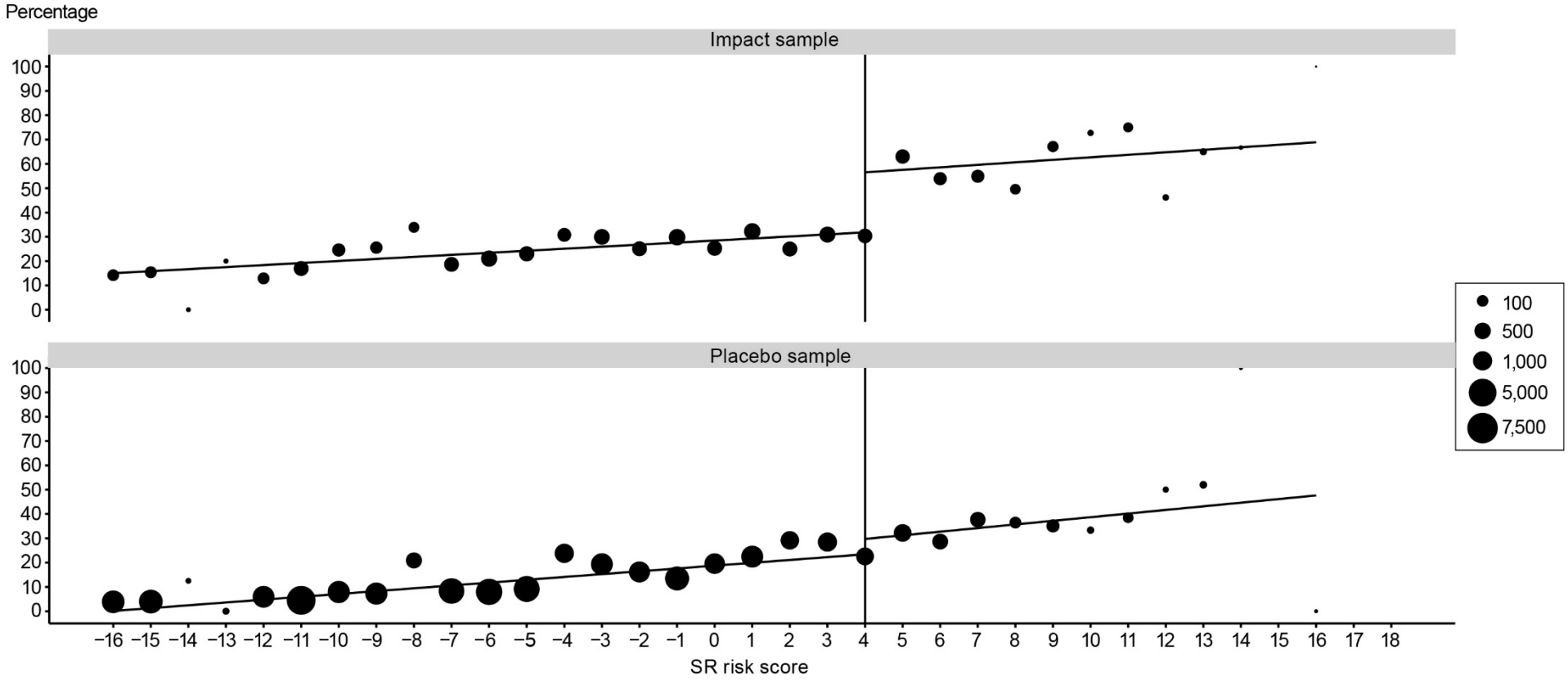
NOTES: The estimated mean counterfactual outcome is that which would have occurred in the absence of SR enrollment. Statistical significance levels are indicated as follows: *** = 0.1 percent; ** = 1 percent; * = 5 percent. Impacts were calculated separately for each subgroup. Impact estimates were then examined for statistically significant differences between subgroups. Statistically significant differences between subgroups are indicated as: ††† = 0.1 percent; †† = 1 percent; † = 5 percent.

Appendix F

Regression Discontinuity Graphs of Supervised Release Impacts for Each Defendant Outcome, Impact Sample Versus Placebo Sample

Appendix Figure F.1

Percentage of Defendants with Bail Set, by Supervised Release (SR) Risk Score
Impact Sample and Placebo Sample

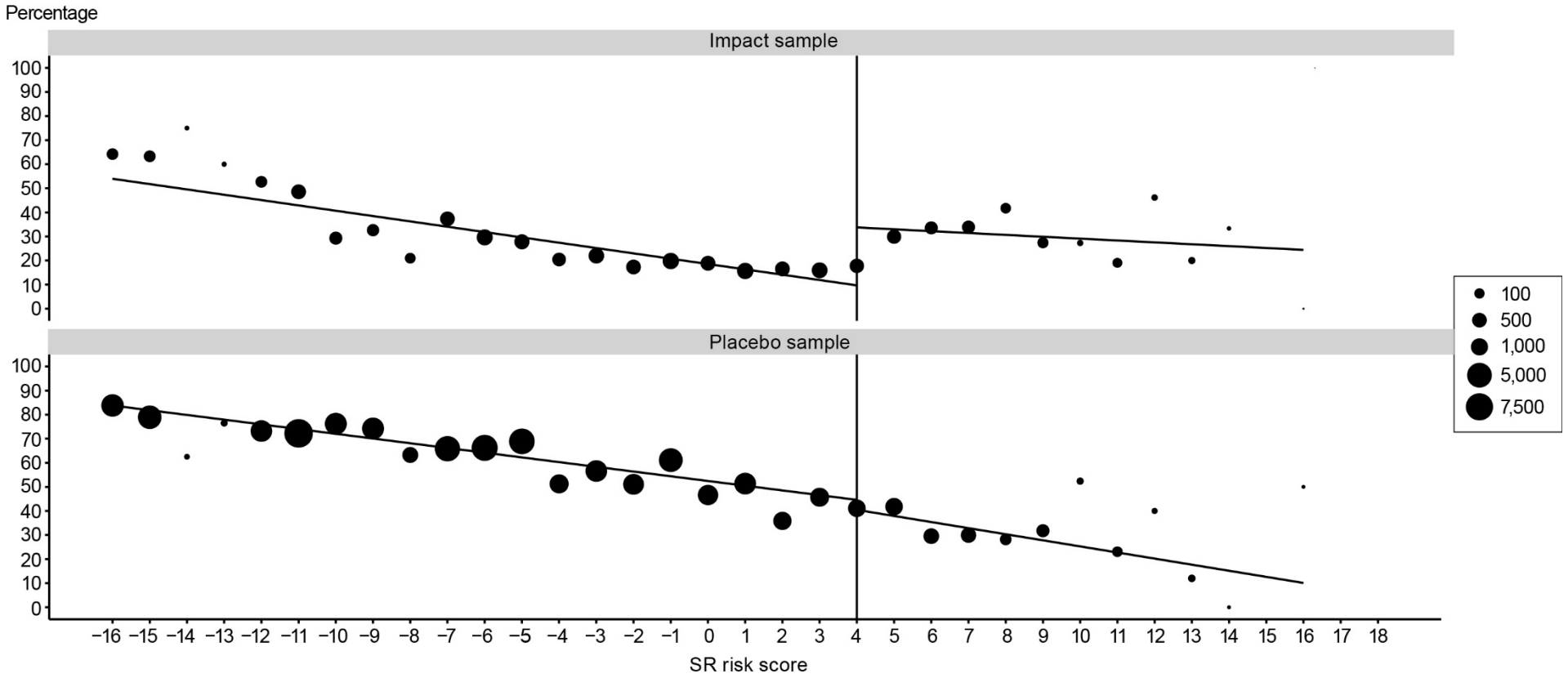


SOURCES: MDRC calculations based on data from the New York State Office of Court Administration, the New York State Division of Criminal Justice Services, the New York City Criminal Justice Agency, and SR providers.

NOTE: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure.

Appendix Figure F.2

Percentage of Defendants Released on Their Own Recognizance, by SR Risk Score
Impact Sample and Placebo Sample

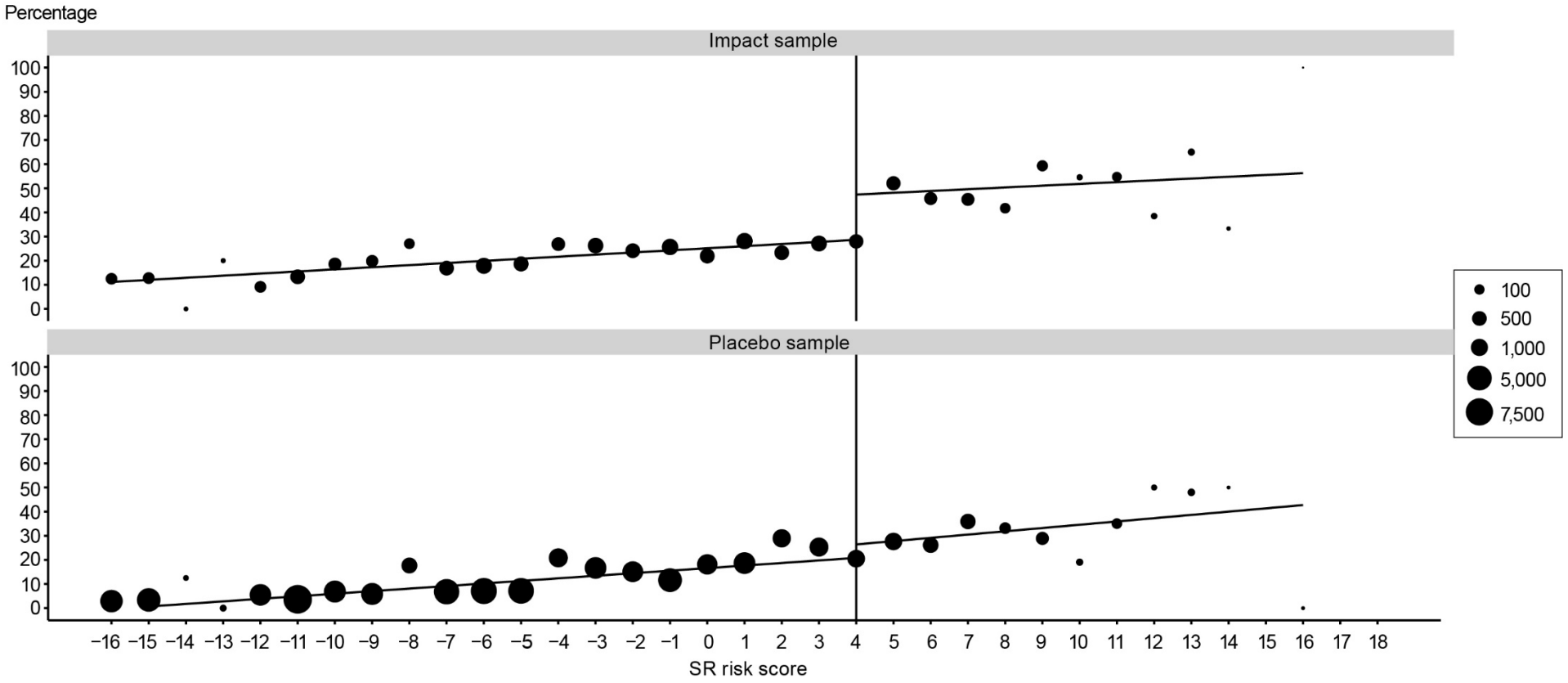


SOURCES: MDRC calculations based on data from the New York State Office of Court Administration, the New York State Division of Criminal Justice Services, the New York City Criminal Justice Agency, and SR providers.

NOTE: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure.

Appendix Figure F.3

Percentage of Defendants Detained at Arraignment, by SR Risk Score
Impact Sample and Placebo Sample

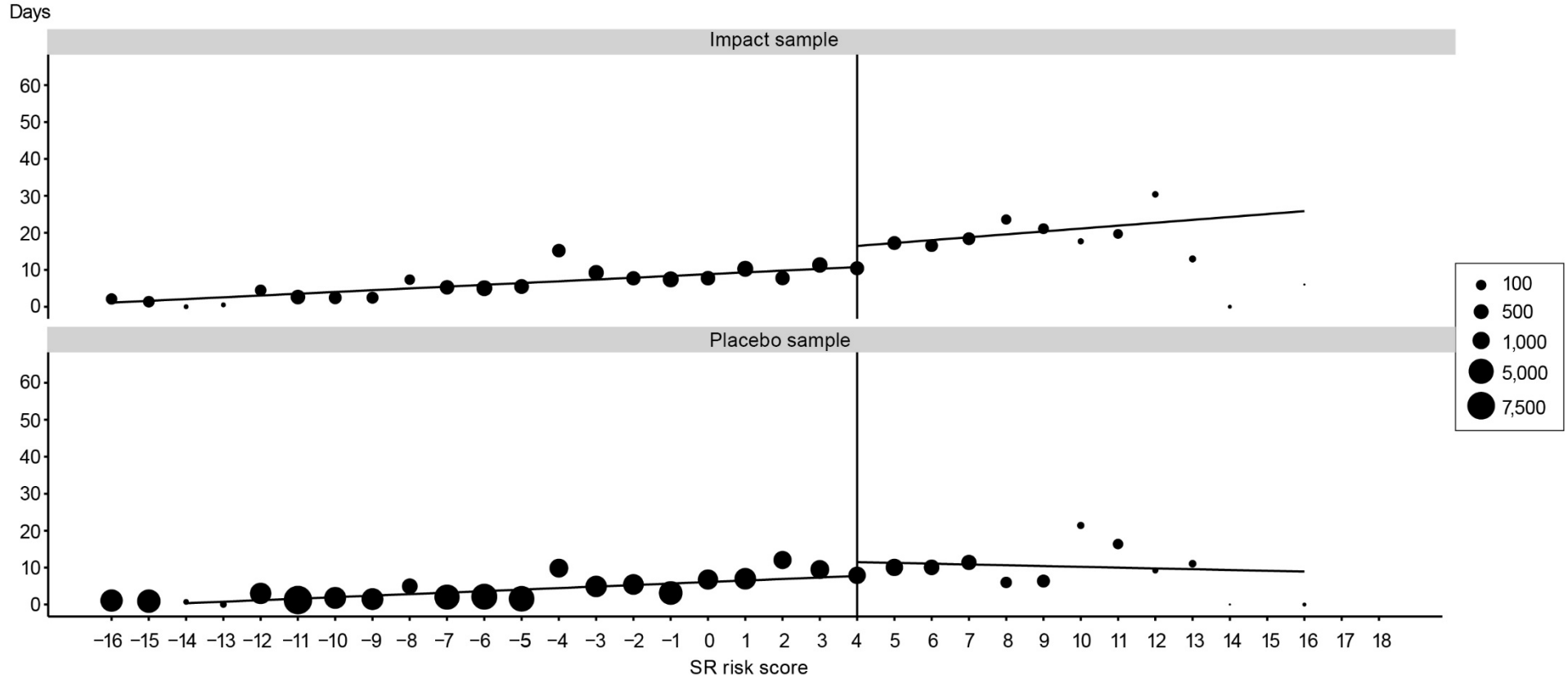


SOURCES: MDRC calculations based on data from the New York State Office of Court Administration, the New York State Division of Criminal Justice Services, the New York City Criminal Justice Agency, and SR providers.

NOTE: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure.

Appendix Figure F.4

Days Detained While Awaiting Trial, by SR Risk Score
Impact Sample and Placebo Sample

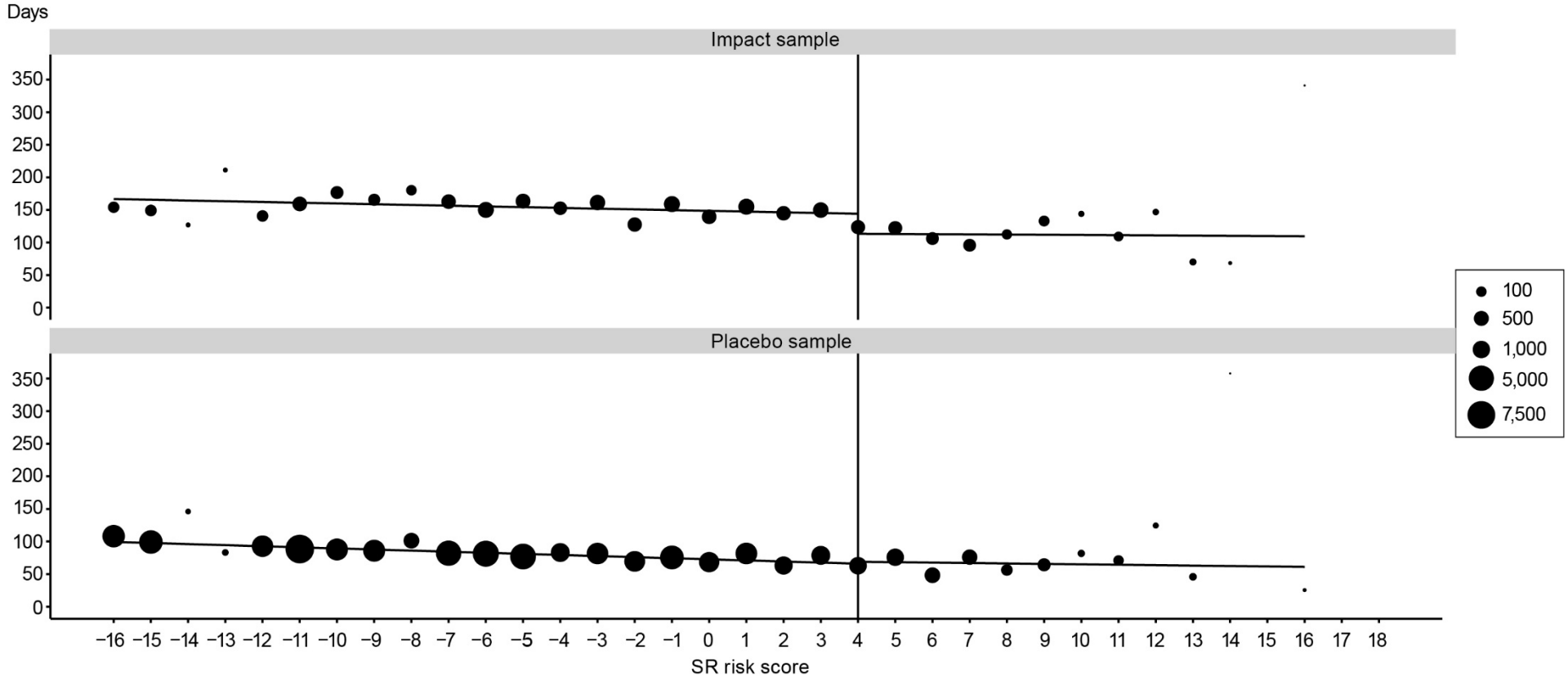


SOURCES: MDRC calculations based on data from the New York City Department of Correction, the New York State Office of Court Administration, the New York State Division of Criminal Justice Services, the New York City Criminal Justice Agency, and SR providers.

NOTE: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure.

Appendix Figure F.5

Length of the Pretrial Period, by SR Risk Score
Impact Sample and Placebo Sample

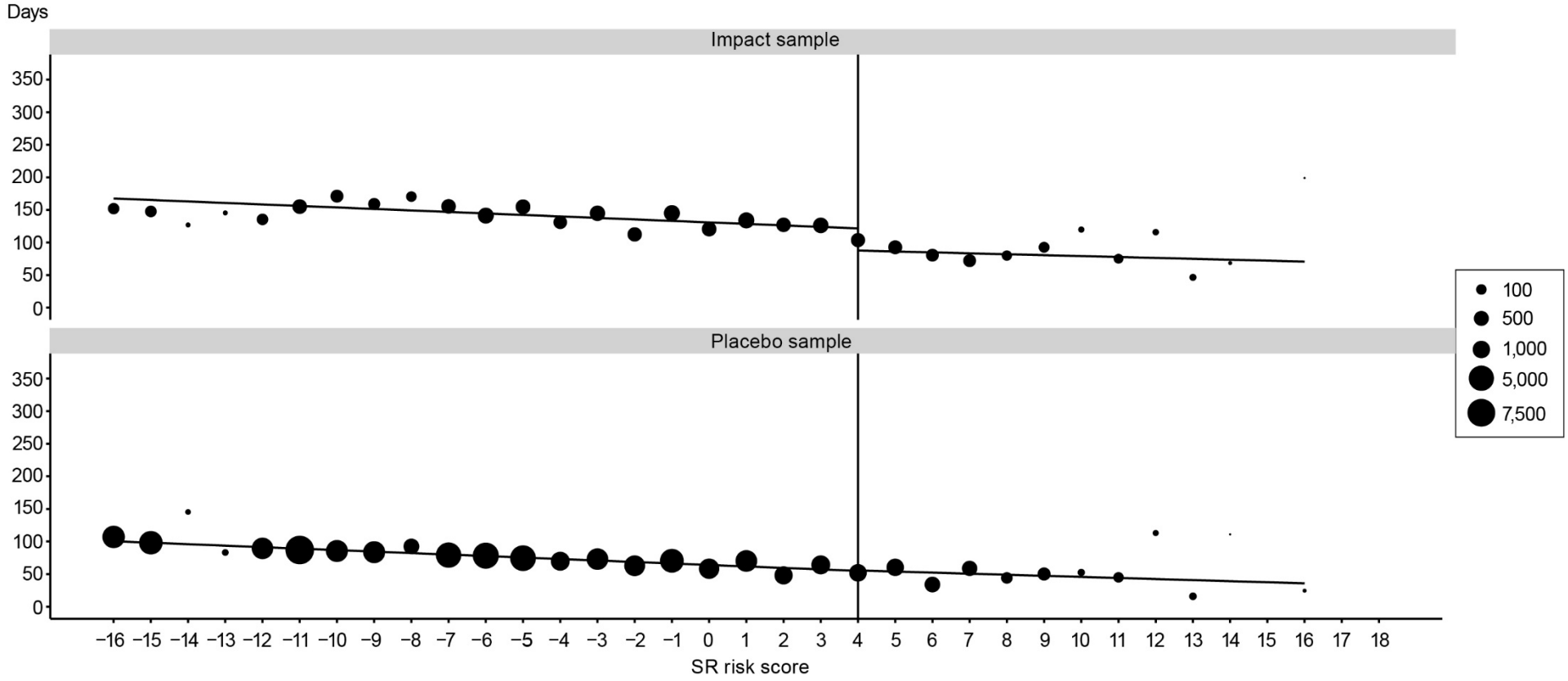


SOURCES: MDRC calculations based on data from the New York State Office of Court Administration, the New York State Division of Criminal Justice Services, the New York City Criminal Justice Agency, and SR providers.

NOTE: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure.

Appendix Figure F.6

Days Exposed to Pretrial Risk, by SR Risk Score
Impact Sample and Placebo Sample

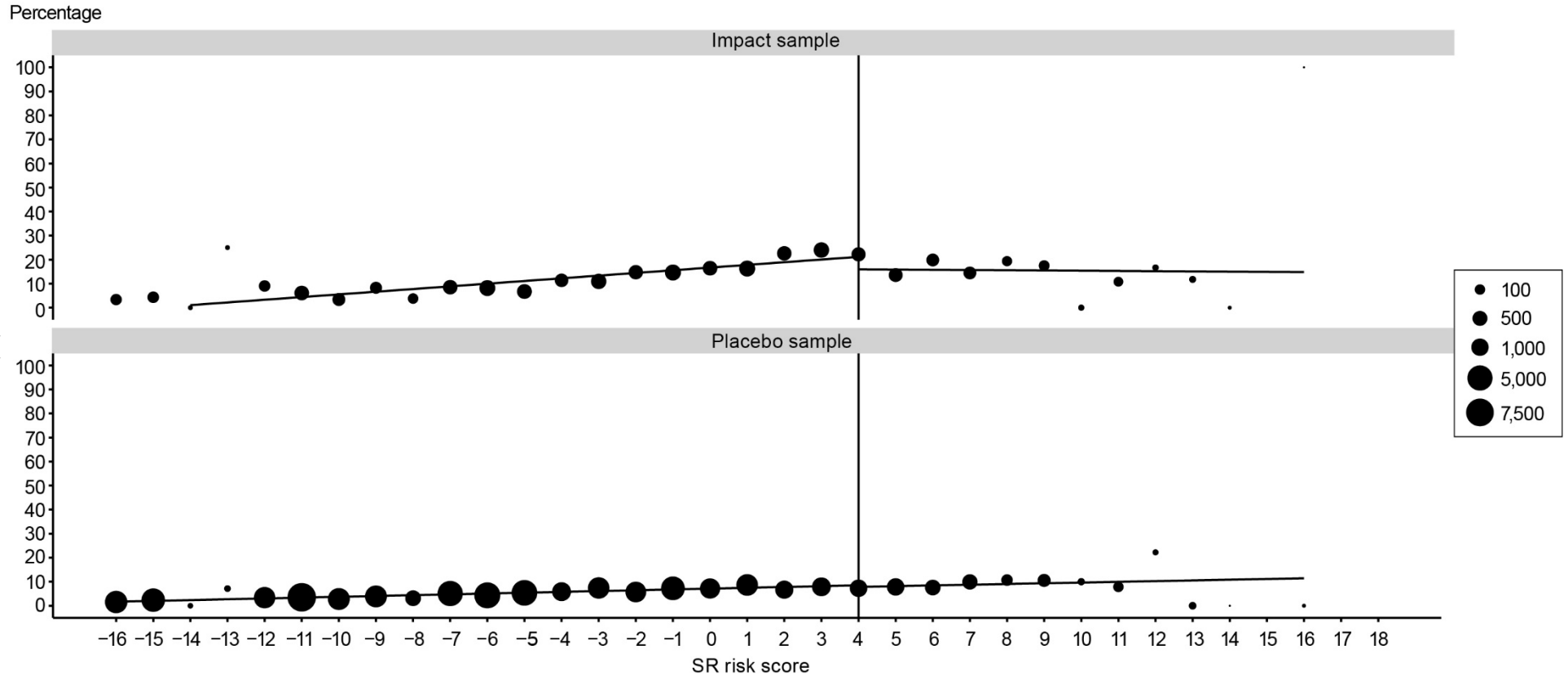


SOURCES: MDRC calculations based on data from the New York City Department of Correction, the New York State Office of Court Administration, the New York State Division of Criminal Justice Services, the New York City Criminal Justice Agency, and SR providers.

NOTE: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure.

Appendix Figure F.7

Percentage of Defendants Issued Bench Warrants, by SR Risk Score
Impact Sample and Placebo Sample

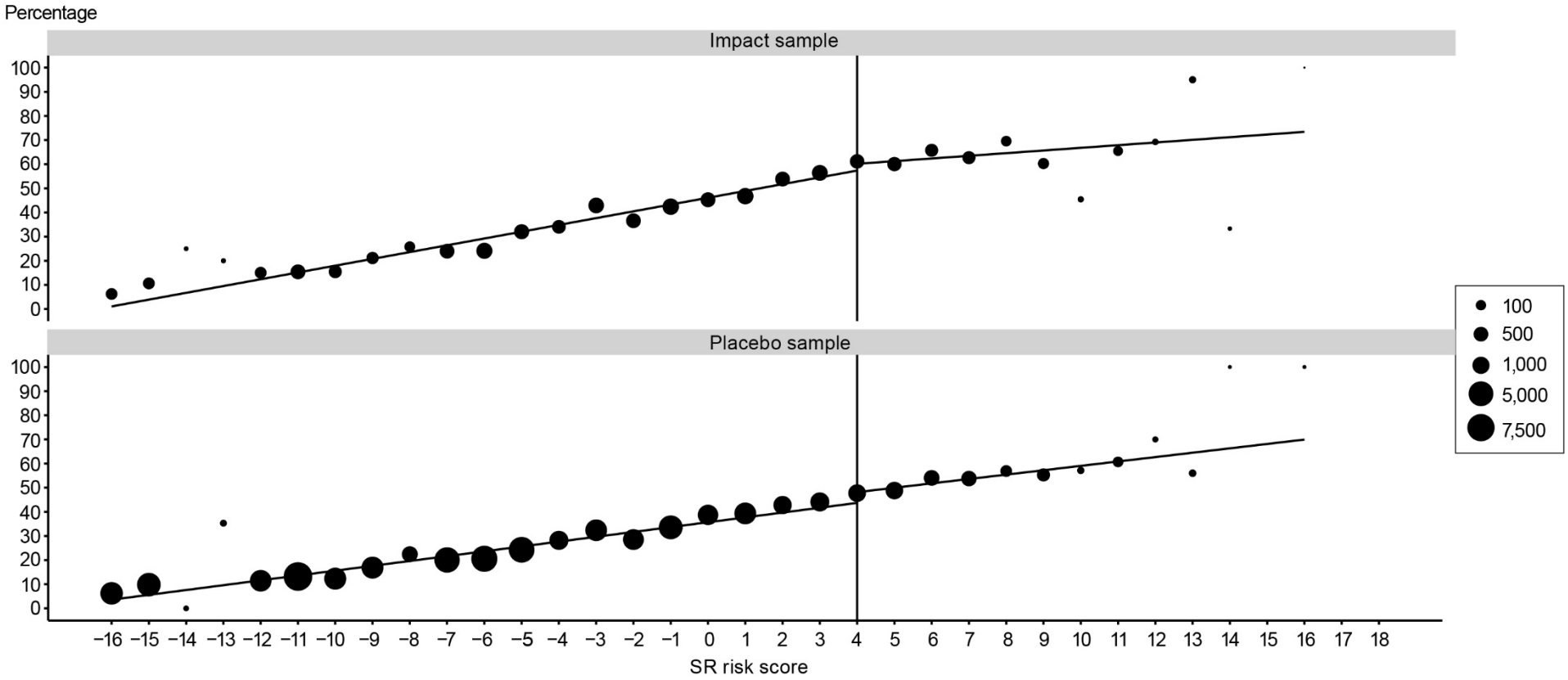


SOURCES: MDRC calculations based on data from the New York State Office of Court Administration, the New York State Division of Criminal Justice Services, the New York City Criminal Justice Agency, and SR providers.

NOTE: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure.

Appendix Figure F.8

Percentage of Defendants with New Arrests Within Nine Months, by SR Risk Score
Impact Sample and Placebo Sample

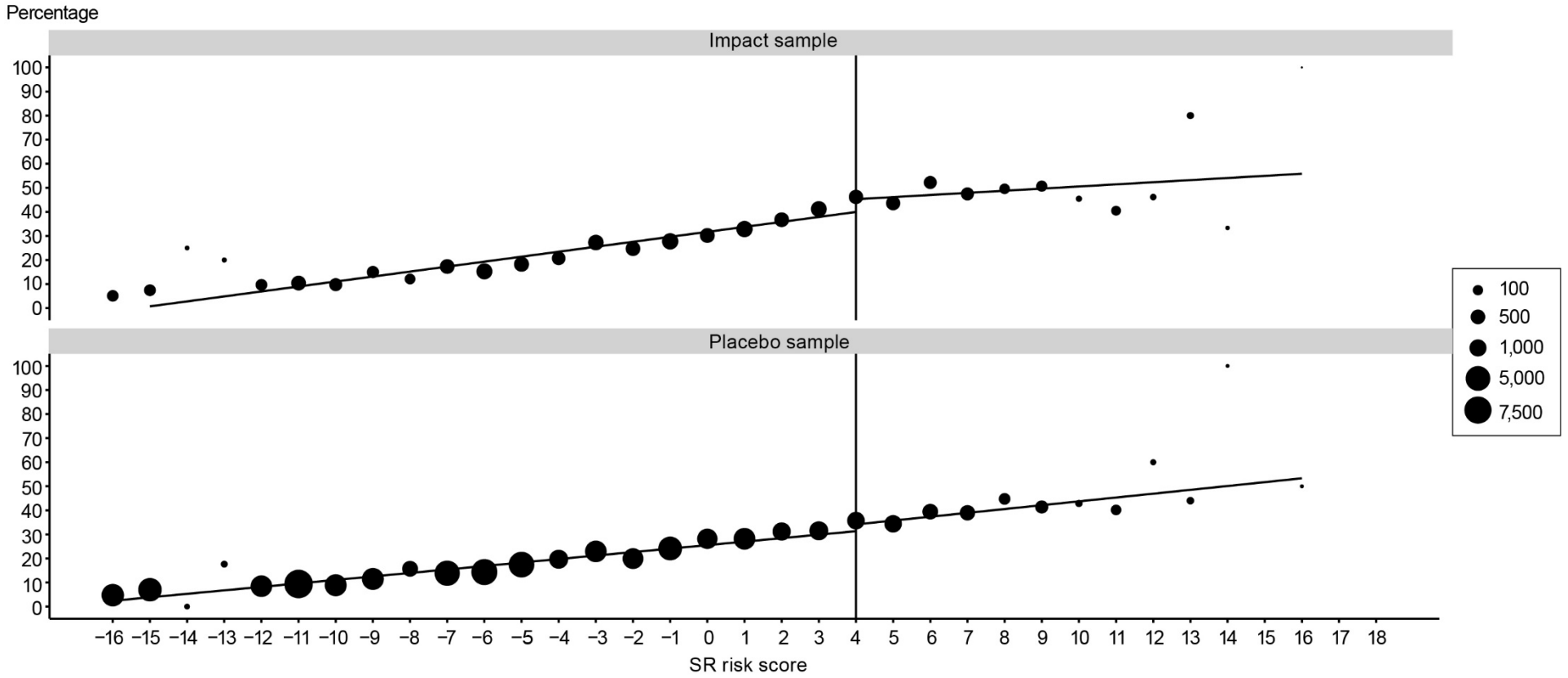


SOURCES: MDRC calculations based on data from the New York State Office of Court Administration, the New York State Division of Criminal Justice Services, the New York City Criminal Justice Agency, and SR providers.

NOTE: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure.

Appendix Figure F.9

Percentage of Defendants with New Misdemeanor Arrests Within Nine Months, by SR Risk Score
Impact Sample and Placebo Sample

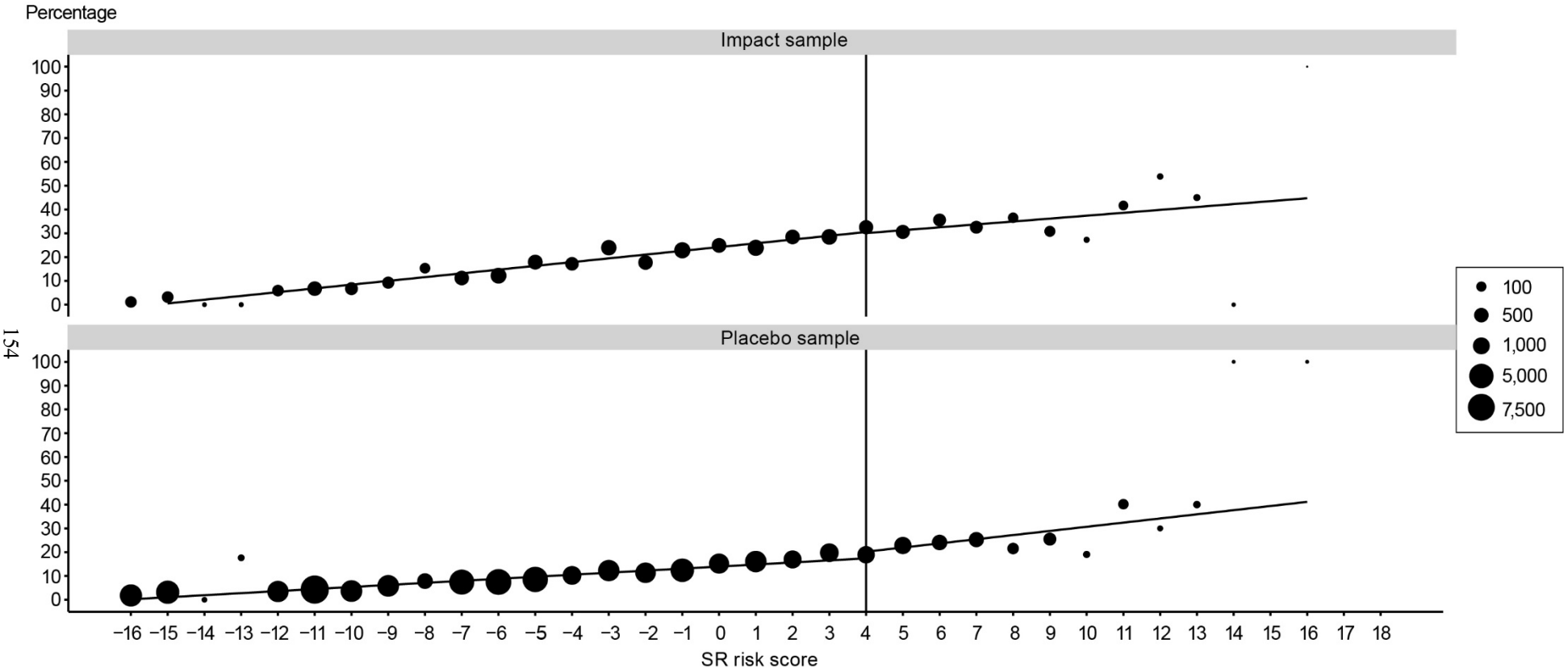


SOURCES: MDRC calculations based on data from the New York State Office of Court Administration, the New York State Division of Criminal Justice Services, the New York City Criminal Justice Agency, and SR providers.

NOTE: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure.

Appendix Figure F.10

Percentage of Defendants with New Felony Arrests Within Nine Months, by SR Risk Score
Impact Sample and Placebo Sample

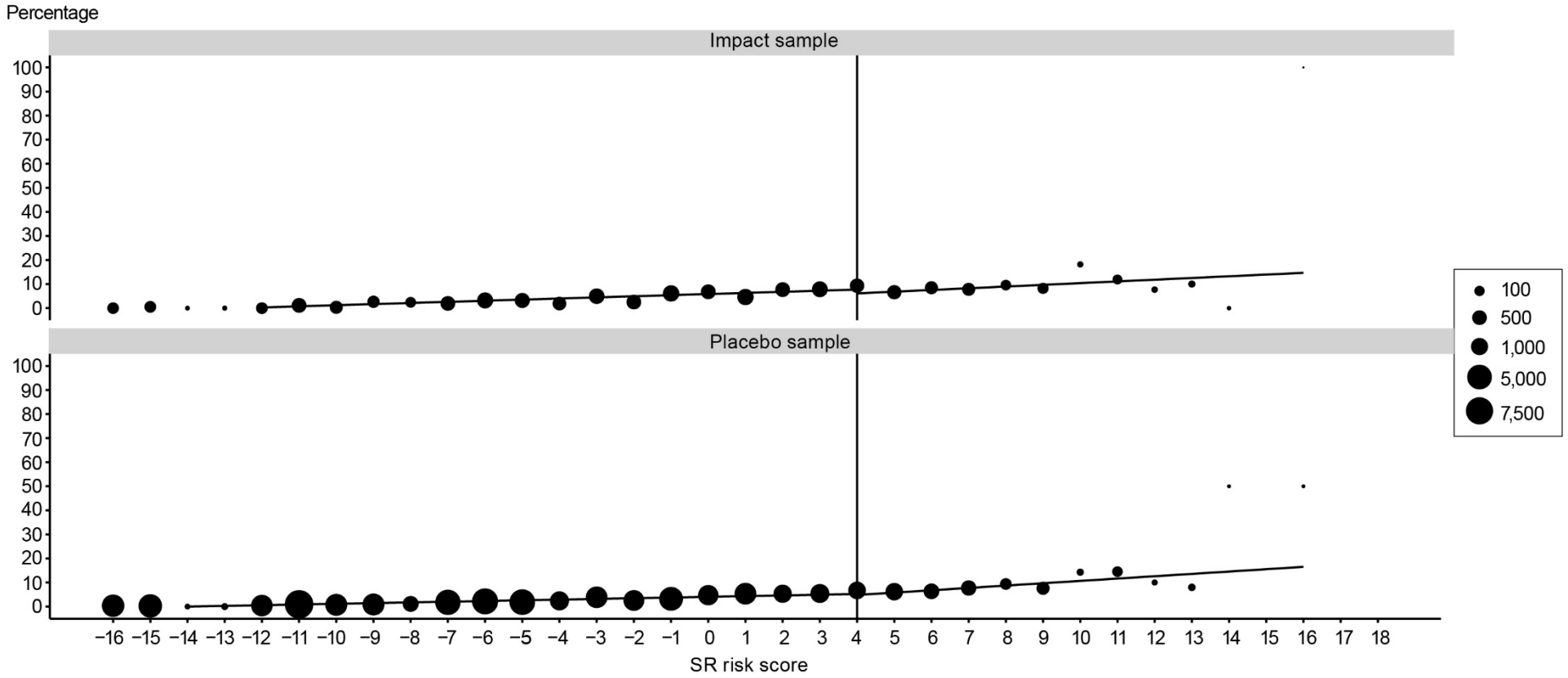


SOURCES: MDRC calculations based on data from the New York State Office of Court Administration, the New York State Division of Criminal Justice Services, the New York City Criminal Justice Agency, and SR providers.

NOTE: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure.

Appendix Figure F.11

Percentage of Defendants with New Violent Felony Arrests Within Nine Months, by SR Risk Score
Impact Sample and Placebo Sample

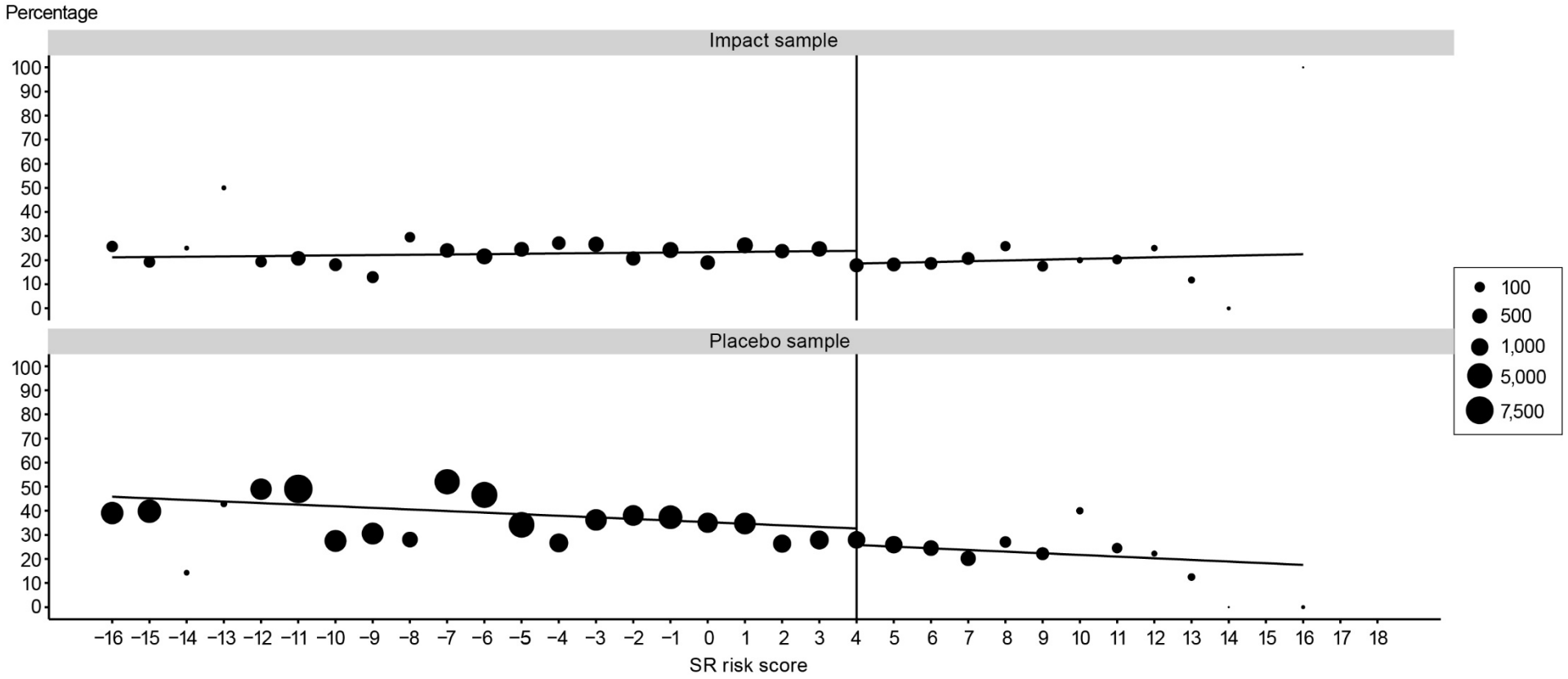


SOURCES: MDRC calculations based on data from the New York State Office of Court Administration, the New York State Division of Criminal Justice Services, the New York City Criminal Justice Agency, and SR providers.

NOTE: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure.

Appendix Figure F.12

Percentage of Defendants Whose Cases Were Dismissed, by SR Risk Score
Impact Sample and Placebo Sample

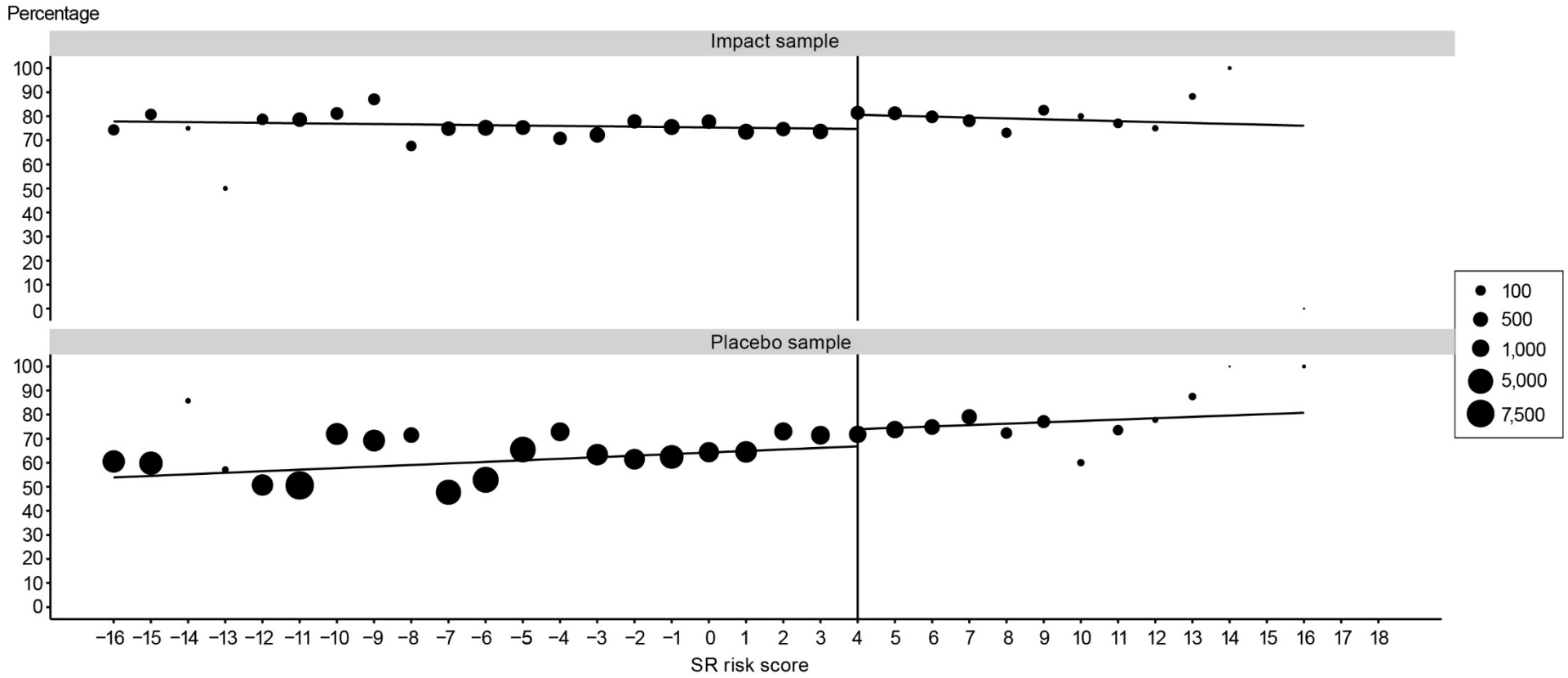


SOURCES: MDRC calculations based on data from the New York State Office of Court Administration, the New York State Division of Criminal Justice Services, the New York City Criminal Justice Agency, and SR providers.

NOTE: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure.

Appendix Figure F.13

Percentage of Defendants Found Guilty, by SR Risk Score
Impact Sample and Placebo Sample



SOURCES: MDRC calculations based on data from the New York State Office of Court Administration, the New York State Division of Criminal Justice Services, the New York City Criminal Justice Agency, and SR providers.

NOTE: The size of each dot represents the number of sample members involved, as reflected by the legend on the right-hand side of the figure.

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MDRC is a nonprofit, nonpartisan social and education policy research organization dedicated to learning what works to improve the well-being of low-income people. Through its research and the active communication of its findings, MDRC seeks to enhance the effectiveness of social and education policies and programs.

Founded in 1974 and located in New York; Oakland, California; Washington, DC; and Los Angeles, MDRC is best known for mounting rigorous, large-scale, real-world tests of new and existing policies and programs. Its projects are a mix of demonstrations (field tests of promising new program approaches) and evaluations of ongoing government and community initiatives. MDRC's staff members bring an unusual combination of research and organizational experience to their work, providing expertise on the latest in qualitative and quantitative methods and on program design, development, implementation, and management. MDRC seeks to learn not just whether a program is effective but also how and why the program's effects occur. In addition, it tries to place each project's findings in the broader context of related research — in order to build knowledge about what works across the social and education policy fields. MDRC's findings, lessons, and best practices are shared with a broad audience in the policy and practitioner community as well as with the general public and the media.

Over the years, MDRC has brought its unique approach to an ever-growing range of policy areas and target populations. Once known primarily for evaluations of state welfare-to-work programs, today MDRC is also studying public school reforms, employment programs for ex-prisoners, and programs to help low-income students succeed in college. MDRC's projects are organized into five areas:

- Promoting Family Well-Being and Children's Development
- Improving Public Education
- Raising Academic Achievement and Persistence in College
- Supporting Low-Wage Workers and Communities
- Overcoming Barriers to Employment

Working in almost every state, all of the nation's largest cities, and Canada and the United Kingdom, MDRC conducts its projects in partnership with national, state, and local governments, public school systems, community organizations, and numerous private philanthropies.