

Does New York's Bail Reform Law Impact Recidivism? A Quasi-Experimental Test in New York City

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Technical Supplement: Available [Here](#) on Report Landing Page.

The Data Collaborative for Justice (DCJ) at John Jay College of Criminal Justice houses a group of research initiatives that raise important questions and share critical research about the criminal justice system and its role in creating safe, just, and equitable communities. DCJ conducts data analysis and research on enforcement in the community, the adjudication of cases in the courts, and the use of confinement in jails and prisons. DCJ's work has informed policy reforms, facilitated partnerships between researchers and government agencies across the country, spurred new scholarly research on lower-level enforcement, and has been cited extensively in the press. For more information about the Data Collaborative for Justice please visit: <https://datacollaborativeforjustice.org/>

Chapter 1 - Introduction: Purpose of the Current Study

This report examines the impact of New York's bail reform law on recidivism in New York City. The analysis compares re-arrest on any charge, a felony, a violent felony, and a firearm charge between people released under the reforms and statistically similar people who faced bail or a remand order.

We tracked recidivism over a minimum of two years for everyone studied. This timeframe encompassed not only the pretrial period but, also, a post-disposition period during which the vast majority of people who initially faced bail and pretrial detention were ultimately released.

While addressing urgent “bottom-line” questions such as whether eliminating bail for select charges was associated with recidivism, we also examined for whom bail reform has had more or less beneficial effects. Additional analyses yielded results for people facing different charges (misdemeanor, nonviolent felony, and violent felony) and people with or without prior justice involvement. Finally, we examined whether and how the mid-2020 amendments that expanded legal eligibility for bail were associated with recidivism.

Key Components of New York's Bail Reform Law

Passed April 1, 2019, bail reform went into effect statewide on January 1, 2020. These [changes to the bail law](#) made the vast majority of misdemeanors and nonviolent felonies subject to mandatory release. Arraignment judges could release people with these charges on their own recognizance, order non-monetary conditions such as supervised release, or in limited circumstances order electronic monitoring.¹ But judges could no longer set bail or detain people.

Key exceptions to mandatory release included virtually all violent felonies; sex offenses; domestic violence cases in which the individual was accused of violating an order of protection; select offenses against children; and witness tampering and intimidation. These charges remained “bail eligible,” meaning eligible for money bail as a pretrial condition, and thereby, detention.

Bail reform also included provisions limiting bail and detention even in bail-eligible cases.²

- **Universal Eligibility for Supervised Release:** The law made all cases eligible for supervised release (often referred to as “pretrial supervision”), whereas New York State counties previously ranged from offering it for only some charges to not having pretrial supervision available at all.
- **Release Except in Cases of Flight Risk:** Regardless of the charge, the law requires judges to release people on their own recognizance unless they pose a demonstrable “risk of flight.”
- **Least Restrictive Condition:** Even when deeming a risk of flight to be present, judges must set the “least restrictive” condition(s) necessary to assure court attendance.
- **Affordable Bail:** When contemplating bail, judges must first consider what people can afford to pay.

The 2020 and 2022 Bail Amendments

Three months into implementation, legislators amended bail reform on April 3, 2020. Put into effect on July 2, 2020, the [2020 amendments](#) returned discretion to judges to set bail or detain people in certain circumstances originally made bail ineligible. The amendments also specified more non-monetary conditions that judges could order in any case, including mandatory treatment, restrictions on with whom people could associate, and conditions to protect victims in domestic violence cases.³

Legislators modified the reforms again on April 9, 2022, with these further amendments put into effect May 1, 2022. Their recency and [limited scope](#) precluded examining them in the current report.⁴

Research Questions

Further discussed below, research to-date does not include a valid analysis of bail reform's impact on crime or recidivism in New York, despite the matter's importance to legislators and the public. For New York City, we sought to fill this gap by answering the following five questions:

- 1. Estimated Impact of Eliminating Bail and Detention in Select Cases:** To what extent was eliminating the possibility of bail and pretrial detention for most misdemeanors and nonviolent felonies associated with recidivism over a timeframe encompassing both the pretrial and post-disposition period?
- 2. Estimated Impact of Reducing the Use of Bail in Cases Still Eligible for It:** In cases remaining legally eligible for bail, to what extent was reducing its frequency through provisions such as the universal availability of supervised release associated with recidivism?
- 3. Estimated Impact of the 2020 Bail Amendments:** How, if at all, was rolling back the original reforms by making more cases eligible for bail effective July 2, 2020 associated with recidivism?
- 4. Results After Excluding Time Incarcerated:** In modified analyses that omit days people spent in pretrial detention from the tracking period—leaving only time when people were truly at risk for re-arrest in the community—to what extent and how do any of the results change?
- 5. Subgroup Analyses:** How is pretrial release under bail reform associated with recidivism for people with different charge levels (misdemeanor, nonviolent felony, or violent felony) or prior criminal history (prior arrest or not; prior violent felony arrest or not; and open case or not)?

To maximize the validity of our findings, we employed statistical strategies designed to yield samples with comparable charges, criminal histories, and demographic characteristics.

The Goals of New York's Bail Reform and Research To-Date

Legislators passing recent bail reforms in New York (and elsewhere) sought to advance pretrial justice without jeopardizing public safety. Key goals included: (1) reducing pretrial detention while people are presumed innocent of a crime and entitled to due process; (2) curtailing the inequity of bail for people lacking access to money; (3) shrinking attendant racial disparities; and (4) prioritizing public safety.

A comprehensive assessment of New York's reform law depends on gaining credible information regarding the achievement of each of these goals. To date, a growing body of research has illuminated outcomes on the first three that involve reducing the use of bail, pretrial detention, and the inequities they tend to produce. Yet, the public conversation has focused predominantly on the fourth and least studied goal regarding the effects of bail reform on safety.⁵

Prior Research on Bail-Setting, Pretrial Detention, and Racial Disparities

Studies have consistently found that after New York's bail reform went into effect, fewer people statewide faced money bail and pretrial detention.⁶ Research has also pointed to several ways in which practical implementation of the reform has dampened the magnitude of resulting changes.⁷ For instance, when opting for bail, judges have set higher amounts, corresponding with lower bail payment rates than in 2019.⁸ Racial disparities in bail-setting have persisted or, according to some studies, widened.⁹ Among bail-eligible cases, the likelihood that judges will set bail has continued to vary considerably both by county,¹⁰ and by judge.¹¹

Dearth of Reliable Research Estimating the Impact on Recidivism

New York's bail reform law took effect January 2020. Since then, researchers had to wait for a meaningful follow-up period to elapse before they could draw valid conclusions about the law's impact on recidivism. In the interim, critics have pointed to increases in gun violence and murder, as well as a documented increase in street crime victimization,¹² since bail reform went into effect.¹³ Yet these violent crime trends synced with the onset of the COVID-19 pandemic and also took place in many cities that saw no changes to their bail laws.¹⁴ Further, many cases that became ineligible for bail, especially common misdemeanors such as petit larceny, were already routinely released by New York City judges in the pre-reform era, limiting the change brought about by new mandatory release provisions.¹⁵

Concrete Recidivism Statistics Reported To-Date and Their Limitations

Although several agencies have made re-arrest data available, they have not been conducive to a rigorous evaluation due to fundamental limitations that the current study seeks to overcome.

1. Inadequate Tracking Period: Reported re-arrest data prior to this study has been *exclusively limited to the pretrial period*,¹⁶ precluding an evaluation that might reveal both immediate-term (pretrial) and longer-term (post-disposition) recidivism effects of different release decisions. Another significant bias inherent in a pretrial-only tracking period stems from variations in its length for different people; exacerbating this bias, the average pretrial period grew significantly longer in 2020 than in 2019 due to court backlogs related to COVID-19.¹⁷

2. Improper Sample Exclusions: Some prior re-arrest data has solely looked at people *released* before trial,¹⁸ thwarting an essential purpose of any bail reform evaluation of comparing how *pretrial release versus detention* (the key practice bail reform seeks to change) affects recidivism.

3. No Effort to Distinguish Bail Eligibility: With notable exceptions,¹⁹ previously reported recidivism data have often not distinguished recidivism among cases made ineligible and remaining eligible for bail.

4. Unmatched Comparison Groups: Most fatally of all, past recidivism statistics have not represented a valid “apples-to-apples” analysis among people shown to have statistically comparable criminal histories, charges, and other baseline characteristics. Because people with prior arrests, or certain charges or backgrounds, are *generally* more likely to be re-arrested independent of the bail laws, it is imperative for these characteristics to be comparable between samples. *Aggregate re-arrest rate comparisons of 2019 versus 2020 cases with no effort to ensure similar characteristics across multiple measures is a recipe for untold bias, absent corrective statistical strategies such as those employed in the present study* (see Chapter 2).

With these principles in mind, two organizations have made recidivism data publicly available.

First, the New York State Division of Criminal Justice Services (DCJS) released case-level release decisions, failure to appear outcomes, and re-arrest data for 2019, 2020, and 2021. In doing this, DCJS went above and beyond the law’s data transparency requirements, facilitating a wide range of analyses.²⁰ DCJS also published a data summary indicating that about one out of five people statewide had a pretrial re-arrest, and from 1% to 5% were re-arrested on a violent felony, depending on region (NYC vs. non-NYC) and year.²¹ This re-arrest data yields important information regarding the prevalence of recidivism across the State. However, the data is limited to *pretrial* re-arrests and exclusively includes people *released* before trial, preventing a comparison of how release versus bail/detention impacts recidivism both before and after a disposition. Additionally, although the full dataset contains fields that could enable researchers to construct samples similar in criminal history and other baseline characteristics, such efforts were beyond the intended scope of DCJS’ data summary.

Second, the New York City Criminal Justice Agency created a dashboard showing pretrial re-arrest rates for people released before trial during each month from January 2019 onward.²² The dashboard shows, for example, that in November 2022, 5% of people released before trial were re-arrested, and just under 1% were re-arrested for a violent felony. Across all months from January 2019 onward, re-arrest rates do not appear to differ discernibly over time. At a high level, this information communicates that overall re-arrest rates have not dramatically shifted. But the dashboard contains the four limitations noted above and with the underlying case-level data not being available, researchers cannot draw upon it to address these limitations in their own evaluation studies.

Relevant Research from Other Jurisdictions

Bail reforms *have* been rigorously studied elsewhere. Evaluations in [Chicago](#)²³ and [Houston](#)²⁴ did not detect increased crime or recidivism, while a report on [New Jersey’s reform](#)²⁵ found a slight decrease in minor offenses but a modest increase in indictable offenses during the pretrial period. Likewise, other studies have found that pretrial detention—the most common outcome for people who face bail—modestly increases recidivism after people’s later release, at least for those whose characteristics put them at the margin between judges choosing detention or release.²⁶ Two studies that expressly distinguished pretrial from post-disposition timeframes—one based on older New York City data—found that pretrial detention modestly *reduced* pretrial recidivism (given its incapacitation effect), before then *increasing* recidivism afterwards.²⁷

A [New York City study](#) released in 2022 illustrates a few of the reasons why pretrial detention tends to increase recidivism following release. It points to significantly higher rates of unemployment and lack of housing among people released from NYC jails than people who were not detained in the first place.²⁸

The Current State of Our Knowledge

Despite prior research that is generally unfavorable to pretrial detention, bail laws and other pertinent factors vary across jurisdictions, making it unwarranted to draw conclusions on whether New York's reform maintained public safety based solely on studies conducted elsewhere. Furthermore, while prior research shows that pretrial detention can lead to elevated re-arrest rates among “marginal” individuals, there is less evidence that this holds for people who would have almost never been released in the absence of bail reform. In other words, most of the prior studies cited above achieved their apples-to-apples comparisons by limiting the samples to statistically similar people who different judges respectively released and detained, necessarily excluding people with characteristics that led nearly all judges to detain them.²⁹

To facilitate future data-driven policy in the State, it therefore remains important for researchers to offer credible evaluations of New York's reform, specifically.

Chapter 2 – Research Design and Methodology

This study estimates the impact of bail reform on recidivism in New York City. Later in 2023, we will publish a similar analysis for the rest of the state. Beyond this chapter's overview, methods are more fully described in a separate [technical supplement](#).³⁰

Data Source and Key Measures

We used New York State Office of Court Administration (OCA) data for all New York City Criminal Courts and Supreme Courts (Criminal Terms) from January 2017 to June 2022.

- **Defining the Analytic Samples:** Most analyses compared re-arrest rates for people who faced bail or remand at arraignment in the first half of 2019 (i.e., before the implementation of bail reform) with similar people who were released without bail in the first half of 2020 (i.e., after bail reform went into effect).
- **Recidivism Measures:** The study included four outcomes: (1) any re-arrest (i.e., all misdemeanor and felony arrests); (2) felony re-arrest; (3) violent felony re-arrest; and (4) firearm re-arrest.³¹
- **Follow-Up Timeframes:** For each set of analyses, we compared **two-year re-arrest rates** and conducted **survival analyses tracking days to re-arrest (if one occurred) for up to 30 months** with a method that adjusts for how long each individual could be tracked.³²

The data and design allowed us to construct criminal and warrant history variables for up to two years prior to each individual's initial arraignment date; i.e., for cases arraigned January 1, 2019, we can consider prior cases with arraignment dates as early as January 1, 2017. We could then consider re-arrest rates for at least two years after arraignment: i.e., the latest arraignment date in the analytic sample is June 30, 2020, and we can track re-arrest cases as late as June 30, 2022.

It is worth noting that OCA has released a public dataset for 2020 through midyear 2022 (to be updated every six months moving forward).³³ However, we instead relied on a larger non-public dataset going back to 2017, subject to a longstanding Data Use Agreement between OCA and the Data Collaborative for Justice. (Other research agencies have similar agreements.)

More Information About the Sampling Frame

For people to be included in the analysis, they had to have a misdemeanor or felony case that was continued at arraignment during the first half of 2019 or first half of 2020. When there were multiple cases for the same individual in either the pre-reform period or post-reform period, we used the case with the earliest arraignment date in each period.³⁴ The choice to pick the earliest date possibly inflates re-arrest rates since this means that everyone who had multiple arraignments during either period was coded as re-arrested. However, as this applies to the bail reform period and the comparison period alike, it does not bias the estimated recidivism impacts of bail reform.³⁵ Finally, we only included people who were born in 1998 or earlier to ensure complete criminal history data back to 2017 for everyone sampled.³⁶

Analysis Plan

Separate Evaluations for Mandatory Release and Bail-Eligible Cases

First, we estimated the impact of bail reform’s mandatory release provisions. Second, we estimated the impact of provisions designed to reduce the use of bail for cases still legally eligible for it. We defined mandatory release vs. bail eligible status based on the original reforms in effect from January 1 to July 1, 2020, since our bail reform samples came from this timespan.³⁷

Design for Analysis #1: Mandatory Release (MR) Cases. We conducted a *pre-post quasi-experiment*, comparing cases that had bail set or were remanded in the first half of 2019 (comparison group) with comparable cases that were mandatorily released without bail in the first half of 2020 (bail reform group).³⁸

Design for Analysis #2: Bail-Eligible (BE) Cases. To study the recidivism impact for cases that remained bail eligible but were released by judges, we used **two analytical approaches**.

a) Pre-Post Design: The first approach (“Analysis #2a”) is a *pre-post quasi-experiment* comparing cases that had bail set or were remanded in the first half of 2019 (comparison group) with comparable cases that were released without bail in the first half of 2020 (bail reform group).

b) Contemporaneous Design: The second approach (“Analysis #2b”) is a *contemporaneous quasi-experiment*, comparing cases arraigned in the first half of 2020 that had bail set or were remanded (comparison group) to comparable bail-eligible cases arraigned in the same period that were released without bail (bail reform group). This secondary analysis takes advantage of the randomness inherent in release decisions made by different judges in 2020—when some judges may have been more or less likely to interpret bail reform provisions for bail-eligible cases (such as expanded supervised release availability) in ways leading to more pretrial release.³⁹

Why Estimate Recidivism Effects for Bail-Eligible Cases? For cases remaining bail eligible, reasons why judges may have set bail less frequently include the expansion of non-monetary release conditions such as supervised release; the “risk of flight” provision limiting circumstances when judges may set any pretrial condition; and the “least restrictive condition” provision requiring non-monetary conditions to be considered initially, even in bail-eligible cases. Prior DCJ research already [confirms](#) that bail reform reduced bail and pretrial detention for both bail-ineligible and bail-eligible cases.⁴⁰

Evaluation of the 2020 Bail Amendments

We also conducted supplemental analyses (“Analysis #3”) pertaining to the bail reform amendments that went into effect on July 2, 2020. Specifically, we isolated cases subject to mandatory release in the first half of 2020 that became bail eligible again in July due to the amendments (bail reform group). We then compared their re-arrest rates to comparable people who had bail set or were remanded in the first half of 2019 (comparison group).

Quasi-Experimental Methodology: Propensity Scores and Weighting

We repeated the same statistical strategy for each analysis. It involved conducting propensity-score adjusted and inverse-probability weighted logistic regression models and Cox proportional hazards regression models to estimate the effects of release without bail. These methods correct for differences in observable baseline characteristics across groups, thereby reducing the effects of confounding variables and increasing the likelihood of more valid estimates of the causal connection between release status and recidivism.⁴¹ **Beyond the overview below, a more detailed description of these methods is in the technical supplement.**

Propensity Score Adjustment

Propensity scores are probabilities ranging between 0 and 1 predicting each person's likelihood of being in the treatment sample (0 = comparison, 1 = bail reform). In reality, we know as a fact who is in which sample, but people with some characteristics are *more statistically likely* to be in one over the other.

Using logistic regression models, we generated a propensity score for each case based on more than 60 baseline characteristics—including accused people's demographics, charges and other case characteristics, and extensive criminal history variables. (See Exhibits T1 – T3 in the [Technical Supplement](#) for the full list of baseline characteristics used for each analysis.) People with similar or identical propensity scores were, in effect, statistically comparable on the totality of all observable characteristics. But insofar as the treatment sample had disproportionately high scores and the comparison sample disproportionately low scores, the samples were initially biased without initiating further adjustments. *Using a propensity score covariate adjustment, we controlled for the propensity score as a single scalar covariate in all outcome analyses and reports "adjusted outcomes" after having initiated this control.*

Inverse Probability Weighting

We also used standardized inverse probability of treatment weights to increase the representation of "rare" cases. In this study, rare cases are: (1) ones with high propensity scores that had bail set or were remanded; and (2) ones with low propensity scores that were released without bail. Weighting statistically decreases the representation of "common" cases in each group, while increasing the representation of rare cases. This further facilitates the equal distribution of confounders across groups and reduces the need to trim cases to achieve covariate balance, thereby increasing the external validity of findings. External validity is achieved when in the end, results are generalizable across a wide spectrum of propensity scores/characteristics among people impacted by bail reform.

Impact Estimation

The logistic regression models estimate the impact of release without bail on two-year re-arrest rates. By comparison, the Cox survival models do not require a hard cut-off for the tracking period, which allows us to track recidivism beyond two years for people for whom we have more than two years of data available. For example, if a person's initial case began on January 1, 2020, we were able to track recidivism for that individual for two and a half years (i.e., until June 30, 2022).

Adjusting for Incarceration Time in Select Analyses

*This report primarily focuses on re-arrest outcomes without controlling for the time people spent incarcerated (either in pretrial detention or in jail or prison post-disposition), making the results likely more conservative estimates of the effects of bail reform. **This design speaks directly to opponents of bail reform who claim that pretrial detention increases public safety due to incapacitation. To the extent this is true, our design incorporates such an effect.*** However, this report also includes supplemental analyses that control for time spent in pretrial detention or post-disposition incarceration in the two years following people's initial arraignment.

Final Sample Characteristics

The technical supplement includes a series of exhibits displaying the baseline characteristics of the bail reform and comparison samples for each of the analyses described above. In their first two columns, these appendices show the sizable baseline differences that existed between the raw samples—before implementing statistical adjustments. In their final two columns, data in the technical supplement demonstrate the sizable positive effects of our propensity score and weighting methods in achieving comparability across a large number of observable characteristics. **For example, in Analysis #2 involving cases remaining eligible for bail post-reform, of 62 baseline characteristics, there were 53 statistically significant differences in the pre-adjusted samples but only one in the post-adjusted samples used in the actual analysis. This example demonstrates the biases that would have resulted had we not engaged in extensive statistical adjustments and the positive effects of those adjustments in limiting such bias.**

Study Limitations

This study has a few limitations to keep in mind.

1. Prosecuted Arrests. OCA data solely captures “prosecuted arrests,” omitting cases the prosecutor declined due to a lack of sufficient evidence or other reasons.

2. Unobserved Baseline Differences. Propensity score and weighting methods can only correct for differences in observable baseline characteristics, so the results may be biased due to unobserved confounders. For example, the criminal history measures used in this analysis are limited to cases that started no more than two years prior to people’s arraignment, but judges have access to people’s entire criminal history when they make release decisions. In general, prior New York City research indicates that recent priors are more predictive of recidivism than older priors.⁴² Nonetheless, there could be added risk created by older arrests for which we cannot statistically control.

3. Possible Pre-Post Design Bias. The results based on pre vs. post reform designs may be biased due to variations in arrests, clearance rates, and prosecution rates between 2019 and 2022. That is, there was a steep drop in overall arrests during the COVID-19 lockdowns from mid-March to May 2020. Likewise, [clearance rates plummeted at the onset of the pandemic, remained unusually low until the end of 2020, and then increased again but did not fully recover to pre-pandemic levels by the end of the study period](#)⁴³ (i.e., June 2022). Additionally, [the percentage of cases district attorneys declined to prosecute was much higher in 2020 than in 2019 or 2021](#).⁴⁴ These trends likely suppressed re-arrests during parts of the tracking periods (though it is unclear for exactly how long) for both the “pre” comparison groups (i.e., people who faced bail or remand in 2019) as well as the “post” bail reform groups (i.e., people released in 2020); however, it is difficult to ascertain the exact impact this has on our results.

4. Conceptual Limitations of the Contemporaneous Design. While the contemporaneous design cannot be biased due to COVID-19, it may be less able to isolate the impact of bail reform than the pre-post design.

That is, *in the pre-post design* (Analysis #2a), judges who made release decisions in 2019 were doing so under a different legal regime than judges who made release decisions in 2020. Specifically, before bail reform judges were more incentivized to set bail or remand people than after bail reform, as there were fewer non-monetary release options, no least restrictive release condition provision, and no “risk of flight” language. Thus, it is reasonable to expect that the same judge may make a more restrictive release decision for a case with the exact same (observed and unobserved) characteristics pre-reform vs. post-reform.

In the contemporaneous design (Analysis #2b), on the other hand, judges had available the same release options for all bail-eligible individuals. Thus, it may be more likely that different release decisions for people with similar observable characteristics were made based on factors that are not captured in our data. *Viewed from this perspective, the results based on the pre-post design may be more able to isolate the impact of the change of legal regime under bail reform than the results based on the contemporaneous design.*

5. Supervised Release Changes Early in the Pandemic: From March 17 to about mid-July 2020, the state court system made supervised release unavailable during a transition to video arraignments.⁴⁵ People already in the program continued to be served. Prior to March 17, 18% of the bail reform samples in fact received supervised release (or another non-monetary condition). Ostensibly, the lack of supervised release for some people who would have received it after March 17, 2020 may have impacted recidivism, though we lack rigorous research confirming or disconfirming as much: i.e., this may or may not be a study limitation.

6. Top Arraignment Charges: Our dataset only includes top charges, i.e., the most severe offense in the criminal complaint against someone. Since some cases may be eligible for bail due to additional charges not captured in our data, we cannot perfectly determine bail eligibility.⁴⁶ For the same reason, we may slightly undercount violent felony or firearm re-arrests.

7. Imperfect Data on Days Incarcerated: We cannot precisely measure the number of days people were incarcerated in the two years following their initial arraignment, possibly leading to slight inaccuracies in the results based on supplemental analyses that control for incarceration time. Specifically, we do not have data on whether individuals made bail between court appearances. For people who received a carceral sentence, we do not exactly know for how long they were incarcerated, as the imposed sentence length does not necessarily reflect the time they actually spent in jail or prison. Therefore, results that control for incarceration time should be interpreted with caution.

Chapter 3 – Estimated Impact of Eliminating Bail for Most Misdemeanors and Nonviolent Felonies

This chapter estimates the impact of bail reform on recidivism for cases where bail and pretrial detention were eliminated as options (virtually all misdemeanors and non-violent felonies). We compared re-arrest outcomes between:

- **Bail Reform Group:** Cases mandatorily released under bail reform in the first half of 2020.
- **Comparison Group:** Cases with comparable charges, criminal histories, and demographic characteristics that had bail set or were remanded in the first half of 2019 (pre-reform).

As described in Chapter 2, all results reported below were statistically adjusted using propensity scores and inverse probability weighting to minimize baseline differences between the groups.

Sample Characteristics

The final bail reform group had the following characteristics:

- **Charges:** The charges were 72% misdemeanors, 24% nonviolent felonies, and 4% violent felonies. (The reform made few violent felonies subject to mandatory release.) The most common charges were assault in the third degree (35%), drug offenses (13%), burglary (10%), and petit larceny (7%).
- **Criminal History:** The sample included 50% with a recent prior arrest, 10% with a recent violent felony arrest,⁴⁷ 15% with a pending case at the time of the current arraignment, and 20% with a prior warrant from an earlier case.
- **Demographics:** The sample was 53% Black, 32% Hispanic, 11% white, and 4% Asian or from additional racial/ethnic groups. Women were 9%, and the sample's average age was 37.

The characteristics of the final comparison group were highly similar. The [technical supplement](#) provides full sample characteristics for both the final bail reform and comparison samples after implementing statistical adjustments.

What is the Overall Recidivism Impact of Eliminating Bail for Select Charges?

Two-Year Re-Arrest Rates

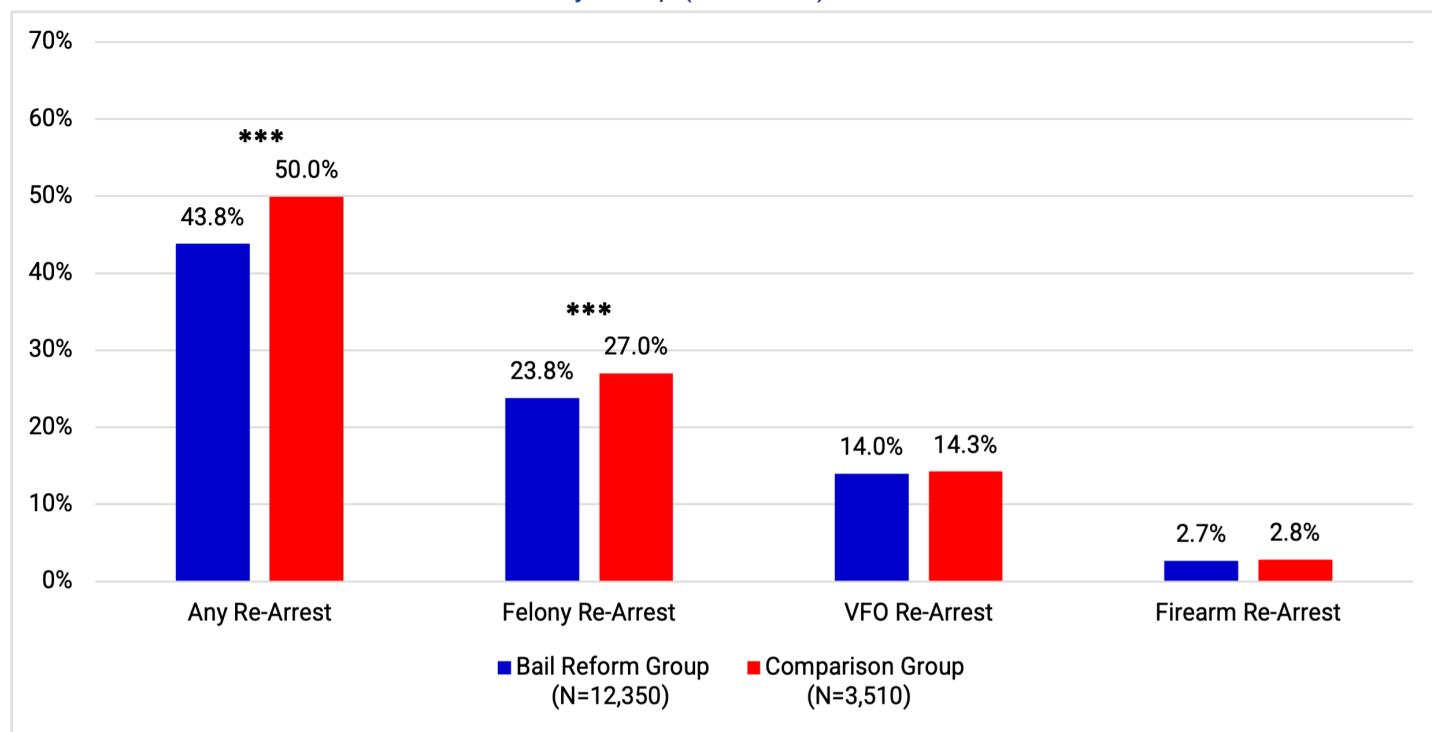
The results indicate that bail reform's mandatory release provisions significantly reduced two-year re-arrest rates for any charge (44% vs. 50%) and for a felony (24% vs. 27%). On the other hand, violent felony (VFO) and firearm re-arrest rates were unaffected (see Exhibit 3.1).

Survival Analysis

We also examined the timing of re-arrests (if they occurred) over a longer 30-month period. Exhibit 3.2 displays hazard ratios—essentially indicating the relative odds that people in the bail reform group were re-arrested more quickly than people in the comparison group. A value greater than 1.000 means

people in the bail reform group were re-arrested more quickly over 30 months (a negative finding), and a value less than 1.000 means people in that group were re-arrested less quickly (a positive finding). The magnitudes of any association are greater as the hazard ratios are farther below or above 1.000.⁴⁸

Exhibit 3.1: Two-Year Re-Arrest Outcomes by Group (N=15,860)



*** p < .001 ** p < .01 * p < .05

** Note: In the bail reform group, 89.8% were released on recognizance and 10.2% were released on non-monetary conditions; in the comparison group, 1.6% were remanded, 83.1% did not make bail, and 15.3% made bail.

The results indicate that people in the bail reform group less quickly experienced any re-arrest, felony re-arrest, and VFO re-arrest than people in the comparison group. There was no difference regarding firearm re-arrest.

Exhibit 3.2. Cox Proportional Hazards Regression Models of Time to Each Re-Arrest Outcome

Group	Any Re-Arrest		Felony Re-Arrest		VFO Re-Arrest		Firearm Re-Arrest	
	HR	p-value	HR	p-value	HR	p-value	HR	p-value
Comparison Group	1.000		1.000		1.000		1.000	
Bail Reform Group	0.814***	0.000	0.830***	0.000	0.906*	0.041	0.854	0.154

*** p < .001 ** p < .01 * p < .05

Exhibits 3.3-3.6 illustrate the survival curves for both samples.⁴⁹ Everyone starts at 100% survival (meaning not yet re-arrested). The graphs illustrate that as time passes, the gap tends to grow wider, with release producing greater benefits regarding overall re-arrest and felony re-arrest. In contrast, the lack of white space separating the curves for VFO and firearm re-arrest illustrate the comparatively smaller association with those two outcomes (not statistically significant for firearm re-arrest).⁵⁰

Exhibit 3.3. Probability of Survival without Any Re-Arrest by Group

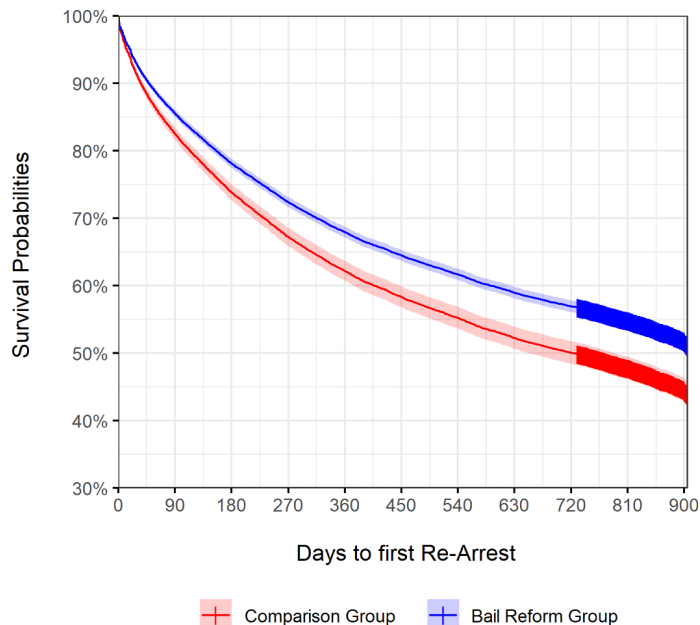


Exhibit 3.4. Probability of Survival without Felony Re-Arrest by Group

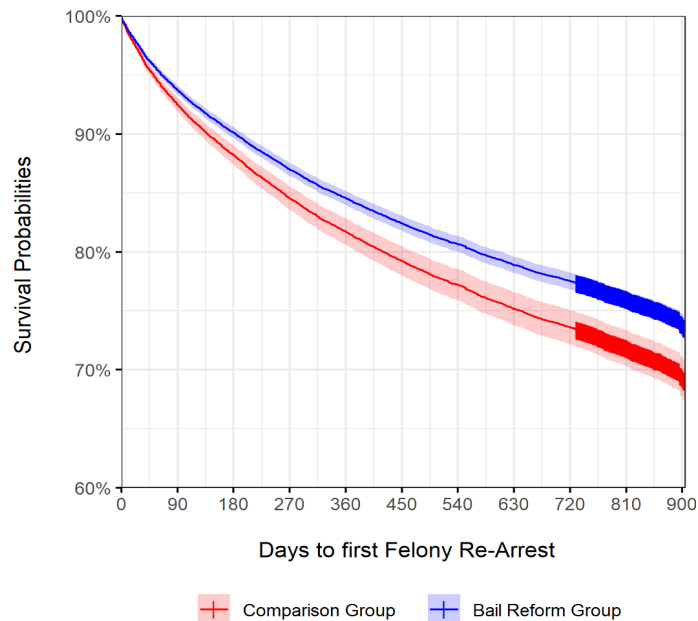


Exhibit 3.5. Probability of Survival without VFO Re-Arrest by Group

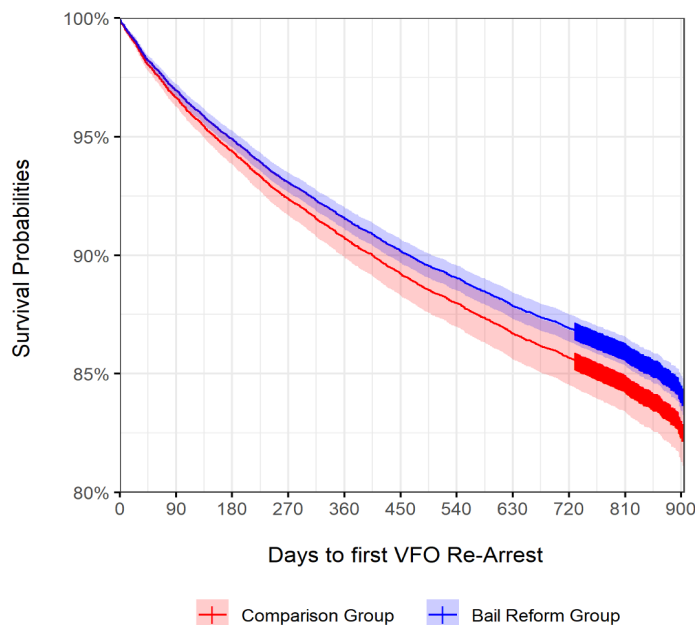
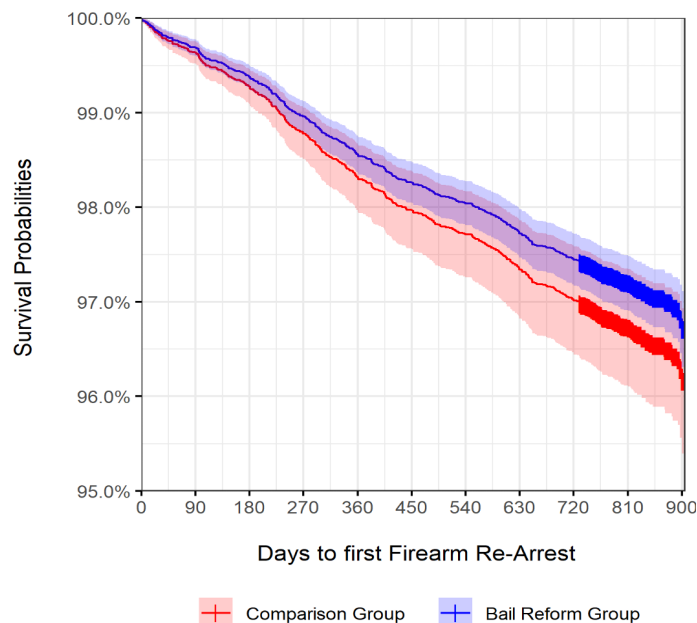


Exhibit 3.6. Probability of Survival without Firearm Re-Arrest by Group



Do the Results Change After Controlling for Incarceration Time?

We conducted the same re-arrest analyses presented above after accounting for the number of days people were incarcerated pre- or post-trial during the follow-up period. Further, we only included people we could definitively determine were at-risk of re-arrest (i.e., out in the community) for at least one day in the two years following arraignment.⁵¹

Variations in Incarceration Time. People in the bail reform group were incarcerated for an average of 2.7 days (median = 0 days) and those in the comparison group for an average of 60.8 days (median = 11 days). In effect, these results suggest that bail reform led to fewer days incarcerated in the two years following arraignment. It is worth noting that the equivalent pre-reform cases were only incapacitated for an average of two months. Results below compare re-arrest rates solely during periods of time when people were released back into the community.

Findings. Once incarceration time is accounted for, the results more strongly favored bail reform. The overall re-arrest rate for the comparison group increased by more than one percentage point (from 50.0% to 51.2%), slightly widening the gap between the two groups from 6.2 percentage points (see Exhibit 3.1) to 7.6 percentage points (see below, Exhibit 3.7). However, accounting for incarceration time led the estimated impacts on felony, VFO, and firearm re-arrest rates to remain virtually unchanged; release under reform continued to significantly reduce felony re-arrest at a nearly identical magnitude, and there continued to be no associations with two-year VFO or firearm re-arrest.

Exhibit 3.7. Two-Year Re-Arrest Outcomes After Controlling for Time Incarcerated by Group (N=15,695)

	Bail Reform Group (N=12,350)	Comparison Group (N=3,345)	Significance Level
	Likelihood of Re-arrest	Likelihood of Re-arrest	
Any Re-Arrest	43.6%	51.2%	***
Felony Re-Arrest	23.9%	27.0%	**
VFO Re-Arrest	14.1%	14.1%	
Firearm Re-Arrest	2.7%	3.0%	

*** p < .001 ** p < .01 * p < .05

** Note: In the bail reform group, 89.8% were released on recognizance and 10.2% were released on non-monetary conditions; in the comparison group, 1.6% were remanded, 82.4% did not make bail, and 16% made bail.

Does Eliminating Bail Have Varying Impacts for Different Subgroups?

We sought to gain more insight into the effects of bail reform for people with different characteristics. Specifically, we split the sample by: (1) current charge severity (misdemeanor vs. felony); (2) whether people had a recent prior arrest; (3) whether people had a pending case at the time of arraignment; and (4) whether people had a recent prior violent felony (VFO) arrest. (To clarify, the “felony” charge category primarily involves nonviolent felonies, since the bail reform law made virtually all violent felonies ineligible for bail and, therefore, not part of the analysis in this chapter.)

Once again, all reported subgroup results incorporate statistical adjustments to assure sample comparability. Exhibits 3.8 and 3.9 (see below at the end of this section) respectively illustrate the two-year re-arrest rates and the results from the survival analyses for each of these subgroups.

Estimated Recidivism Impact by Charge Severity

Misdemeanors. For people charged with misdemeanors, eliminating bail was associated with a significant reduction in two-year re-arrest rates for any charge (43% vs. 52%) and for a new felony (21% vs. 26%). While favoring bail reform, the difference was substantively negligible (one percentage point) for VFO re-arrest and not statistically significant for firearm re-arrest. Survival analyses extending the tracking period for up to 30 months showed that release was associated with a reduced re-arrest risk across all four outcomes.

Felonies. *For people charged with felonies, eliminating bail was associated with an increase in VFO re-arrest (17% vs. 14%), though there were no differences across the three other outcomes. Survival analyses that extended tracking for up to 30 months did not detect a change in re-arrest risk across any of the four outcomes, including VFO re-arrest.*

Estimated Recidivism Impact by Criminal History

Prior Arrest or Not. *Eliminating bail was associated with a statistically significant reduction in re-arrest on all four outcomes for people without a recent prior, while the analysis detected minimal effects in any direction for people with a prior.*

- **No Prior Arrest:** Release was associated with lower rates of overall re-arrest (26% vs. 38%), felony re-arrest (12% vs. 20%), and VFO re-arrest (6% vs. 10%); and a modestly lower firearm re-arrest rate (1.3% vs. 2.2%). The survival analyses confirmed the benefits of release across all four outcomes—with the relative risk reduction ranging from 40% (any re-arrest) to 47% (firearm re-arrest).
- **Recent Prior Arrest:** Release was associated with a higher VFO re-arrest rate (22% vs. 18%), though there were no differences across the two groups in overall, felony, or firearm re-arrest. None of the results across any of the four outcomes were different in the survival analyses (including for VFO re-arrest).

Pending Case or Not. *Eliminating bail for people without pending cases was associated with a statistically significant reduction in overall, felony, and VFO re-arrest, whereas the opposite was true for people with a pending case. There were no differences for firearm re-arrest. Results for the two-year re-arrest rates (see just below) were echoed in the survival analyses.*

- **No Pending Case:** Release of people with no pending cases was associated with lower rates of overall re-arrest (39% vs. 48%), felony re-arrest (20% vs. 26%), and VFO re-arrest (12% vs. 14%).
- **Pending Case:** In contrast, releasing people with a pending case was associated with higher rates of overall re-arrest (69% vs. 63%), felony re-arrest (44% vs. 34%), and VFO re-arrest (27% vs. 17%).

Prior VFO Arrest or Not. *Eliminating bail for people without a recent prior VFO arrest was associated with a reduction in overall, felony, and VFO re-arrest, whereas the opposite was true for people with a prior VFO arrest. There were no differences for firearm re-arrest. Results for the two-year re-arrest rates (see just below) were echoed in the survival analyses.*

- **No Prior VFO Arrest:** The release of people with no prior VFOs was associated with lower rates of overall re-arrest (41% vs. 49%) and felony re-arrest (21% vs. 26%), and a marginal association with VFO re-arrest (12% vs. 13%).
- **Prior VFO Arrest:** Releasing people with a recent VFO arrest was associated with higher rates of overall re-arrest (72% vs. 62%), felony re-arrest (50% vs. 38%), and VFO re-arrest (36% vs. 24%).

Exhibit 3.8. Two-Year Re-Arrest Outcomes with Different Charge Levels and Criminal History Characteristics by Group

	Re-Arrest Rates by Charge Level					
	Misdemeanor Charge (N=11,454)			Felony Charge (N=4,406)		
	Bail Reform Group (N=9,247)	Comparison Group (N=2,207)	sig. level	Bail Reform Group (N=3,103)	Comparison Group (N=1,303)	sig. level
Any Re-Arrest	42.6%	51.9%	***	47.0%	44.6%	
Felony Re-Arrest	21.0%	26.2%	***	31.4%	29.1%	
VFO Re-Arrest	12.7%	14.3%	*	17.3%	14.0%	*
Firearm Re-Arrest	2.4%	2.8%		3.6%	2.8%	
	Re-Arrest Rates by Whether Individuals had Any Recent Criminal History					
	No Criminal History (N=7,972)			Criminal History (N=7,888)		
	Bail Reform Group (N=6,456)	Comparison Group (N=1,516)	sig. level	Bail Reform Group (N=5,894)	Comparison Group (N=1,994)	sig. level
Any Re-Arrest	26.1%	37.7%	***	61.7%	62.5%	
Felony Re-Arrest	12.0%	20.1%	***	35.8%	34.1%	
VFO Re-Arrest	6.4%	10.4%	***	21.6%	18.3%	**
Firearm Re-Arrest	1.3%	2.2%	**	4.1%	3.5%	
	Re-Arrest Rates by Whether Individuals had a Pending Case at the Time of Arraignment					
	No Pending Case (N=13,489)			Pending Case (N=2,371)		
	Bail Reform Group (N=10,675)	Comparison Group (N=2,814)	sig. level	Bail Reform Group (N=1,675)	Comparison Group (N=696)	sig. level
Any Re-Arrest	39.4%	47.7%	***	68.8%	62.8%	**
Felony Re-Arrest	20.4%	25.7%	***	43.8%	34.0%	***
VFO Re-Arrest	11.7%	13.8%	**	27.2%	16.8%	***
Firearm Re-Arrest	2.4%	2.9%		4.2%	2.5%	
	Re-Arrest Rates by Whether Individuals had Recent VFO Arrests					
	No Prior VFO Arrests (N=14,376)			Prior VFO Arrest (N=1,484)		
	Bail Reform Group (N=11,270)	Comparison Group (N=3,106)	sig. level	Bail Reform Group (N=1,080)	Comparison Group (N=404)	sig. level
Any Re-Arrest	40.8%	48.7%	***	72.4%	61.9%	***
Felony Re-Arrest	21.1%	25.8%	***	50.4%	38.2%	***
VFO Re-Arrest	11.7%	13.2%	*	35.7%	23.9%	***
Firearm Re-Arrest	2.3%	2.6%		6.3%	4.7%	

* p < .05 ** p < .01 *** p < .001

Exhibit 3.9. Cox Proportional Hazards Regression Models of Time to Each Re-Arrest Outcome for People with Different Charge Levels and Criminal History Characteristics by Group

	Re-Arrest Rates by Charge Level			
	Misdemeanor Charge		Felony Charge	
	HR**	p-value	HR	p-value
Any Re-Arrest	0.727***	0.000	1.053	0.279
Felony Re-Arrest	0.739***	0.000	1.054	0.360
VFO Re-Arrest	0.815**	0.001	1.155	0.073
Firearm Re-Arrest	0.757*	0.041	1.120	0.538
	Re-Arrest Rates by Whether Individuals had Any Recent Criminal History			
	No Criminal History		Criminal History	
	HR	p-value	HR	p-value
Any Re-Arrest	0.602***	0.000	0.953	0.140
Felony Re-Arrest	0.541***	0.000	1.014	0.748
VFO Re-Arrest	0.551***	0.000	1.115	0.051
Firearm Re-Arrest	0.553**	0.002	1.058	0.666
	Re-Arrest Rates by Whether Individuals had a Pending Case at the Time of Arraignment			
	No Pending Case		Pending Case	
	HR	p-value	HR	p-value
Any Re-Arrest	0.755***	0.000	1.117*	0.044
Felony Re-Arrest	0.736***	0.000	1.296***	0.000
VFO Re-Arrest	0.780***	0.000	1.557***	0.000
Firearm Re-Arrest	0.781*	0.047	1.232	0.375
	Re-Arrest Rates by Whether Individuals had Recent VFO Arrests			
	No Prior VFO Arrests		Prior VFO Arrest	
	HR	p-value	HR	p-value
Any Re-Arrest	0.746***	0.000	1.284**	0.001
Felony Re-Arrest	0.764***	0.000	1.330**	0.001
VFO Re-Arrest	0.811***	0.000	1.494***	0.000
Firearm Re-Arrest	0.820	0.112	1.033	0.893

* p < .05 ** p < .01 *** p < .001

** Note: All hazard ratios for the comparison groups are 1.000.

What's the Upshot?

Overall, the results indicate that eliminating bail for select misdemeanor and nonviolent felony charges under New York's original bail reform law significantly reduced recidivism. We found significant reductions for any re-arrest and felony re-arrest over two years; a smaller reduction for VFO re-arrest that only appeared in "survival analyses" examining how quickly people were re-arrested over 30 months; and no effect on firearm re-arrest. Controlling for the time people spent incarcerated during the tracking period (and were not at risk for re-arrest in the community) did not change the thrust of any findings, though this led to slightly more favorable results for bail reform on re-arrest for any charge.

The results also indicate that the estimated impact of eliminating bail varied across key subgroups defined by their charge severity or criminal history. Across multiple outcomes, the reform's mandatory release provisions significantly *reduced* re-arrest for people charged with misdemeanors; without a recent prior arrest; without a recent violent felony arrest; and without a pending case. Conversely, mandatory release significantly *increased* re-arrest across multiple outcomes for people with a recent violent arrest and with a pending case at the time of the current arraignment.

Chapter 4 – Estimated Impact of Reforms Reducing the Use of Bail in Legally Eligible Cases

This chapter examines recidivism impacts for cases that remained eligible for bail post-reform; i.e., bail or detention were still legally permitted, but bail reform made supervised release universally available and required judges to select the least restrictive release conditions necessary to mitigate flight risk as well as to set affordable bail amounts. Prior research confirms that some combination of these provisions led to a significant reduction in bail-setting, even if the relationship was smaller than eliminating bail outright, as with the types of cases addressed previously in Chapter 3.⁵²

As described in Chapter 2, we used two research designs with competing advantages and limitations.

- **Pre vs. Post Design:** This analysis compared bail-eligible individuals released under reform in the first half of 2020 (bail reform group) to cases with comparable charges, criminal histories, and demographic characteristics that had bail set or were remanded in the first half of 2019 (pre-reform).
- **Contemporaneous Design:** This analysis compared bail-eligible individuals with similar characteristics who were, respectively, released (bail reform group) versus had bail set or were remanded (comparison group), *all within the first half of 2020*. This design, in effect, compares decisions made by different judges on comparable cases during the bail reform legal regime.

This chapter is organized similar to the previous one, except each section provides results for both designs. (Exhibits whose labels start with “4a” show the results based on the pre vs. post analyses and exhibits whose labels start with “4b” present the results based on the contemporaneous analyses.)

Like the analysis in Chapter 3, all results are statistically adjusted using propensity scores and inverse probability weighting to balance observed baseline individual and case characteristics between the bail reform and comparison groups.

Sample Characteristics

- **Charges:** The charges were 16% misdemeanors, 14% nonviolent felonies, and 70% violent felonies for the pre-post sample; and 13% misdemeanors, 11% nonviolent felonies, and 76% violent felonies for the contemporaneous sample. Across both designs, the most common charge types were assault, criminal contempt, firearms offenses, and robbery (combining for over 70% of charges).
- **Criminal History:** The samples included 46% with a recent prior arrest, 11% with a recent violent felony arrest, 16% (pre-post design) and 15% (contemporaneous) with a pending case at the time of the current arraignment, and 14% and 15% respectively with a prior warrant from an earlier case.
- **Demographics:** The pre-post sample was 50% Black, 35% Hispanic, 10% white, and 5% Asian or from additional racial/ethnic groups. The contemporaneous sample was a nearly identical 52% Black, 33% Hispanic, 11% white, and 4% Asian or from additional racial/ethnic groups. In both designs, women made up 13%, and the sample’s average age was 36.

The technical supplement includes comprehensive comparisons of the final bail reform and comparison samples, demonstrating that both designs successfully minimized observable sample differences.

What is the Overall Recidivism Impact of Reduced Bail-Setting?

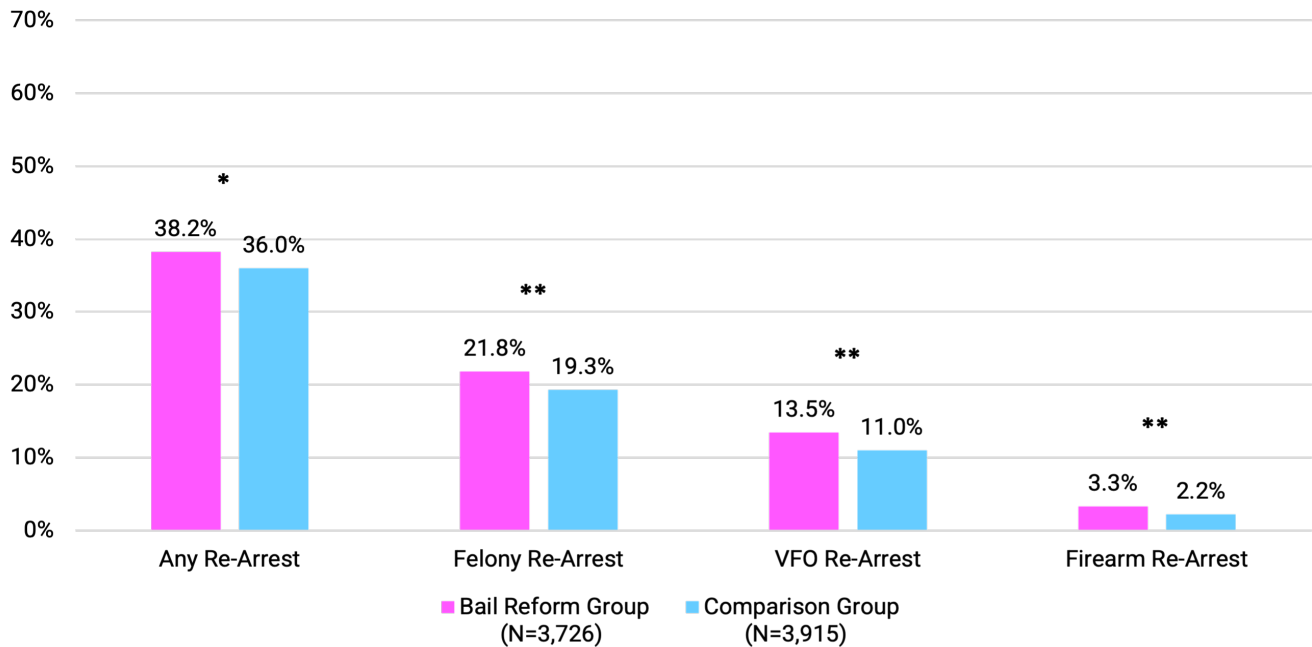
Two-Year Re-Arrest Rates

Both designs yielded small effects with no differences in two-year re-arrest rates that exceeded 3 percentage points. While some of the relationships were statistically significant, they were generally negligible in magnitude and trended in opposite directions across the two designs (Exhibits 4a.1 versus 4b.1).

Pre-Post Design. The results indicate that compared to setting bail or remand, release without bail modestly increased re-arrest for any charge (38% vs. 36%), a felony (22% vs. 19%), a violent felony (VFO, 14% vs. 11%), and a firearm charge (3.3% vs. 2.2%).

Contemporaneous Design. The results indicate that release without bail modestly reduced felony re-arrest (22% vs. 24%) and VFO re-arrest (13% vs. 16%), while there were no differences in overall or firearm re-arrest across the two groups.

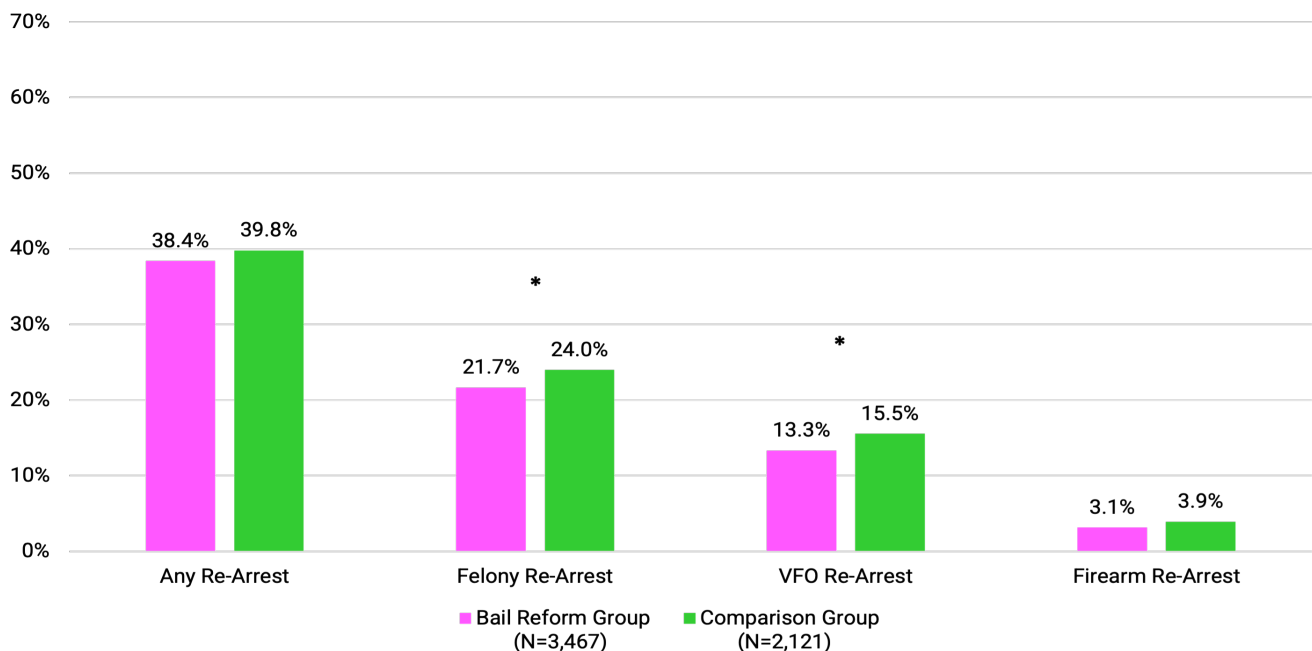
Exhibit 4a.1: Two-Year Re-Arrest Outcomes by Group – Pre-Post Analysis (N=7,641)



*** p < .001 ** p < .01 * p < .05

* Note: In the bail reform group, 78.4% were released on recognizance and 21.6% were released on non-monetary conditions; in the comparison group, 3.9% were remanded, 79.3% did not make bail, and 16.8% made bail.

Exhibit 4b.1: Two-Year Re-Arrest Outcomes by Group – Contemporaneous Analysis (N=5,588)



*** p < .001 ** p < .01 * p < .05

* Note: In the bail reform group, 77.2% were released on recognizance and 22.8% were released on non-monetary conditions; in the comparison group, 6.9% were remanded, 77.6% did not make bail, and 15.5% made bail.

Survival Analysis

We also examined how quickly people were re-arrested over 30 months following arraignment.

In general, the survival analyses show virtually no statistically significant differences across either design (see Exhibits 4a.2 and 4b.2). An exception is that in the pre-post design, people in the bail reform group were re-arrested more quickly for firearm offenses than comparable people in the comparison group (Exhibit 4a.2).

A visual inspection of the survival curves indicates that across all four outcomes, there is a slight trend towards the bail reform group having greater odds of re-arrest when relying on the pre-post design and lower odds of re-arrest when relying on the contemporaneous design; but again, nearly all effects appear substantively small and within the statistical margin of error.

Exhibit 4a.2: Cox Proportional Hazards Regression Models of Time to First Re-Arrest, Felony Re-Arrest, VFO Re-Arrest, and Firearm Re-Arrest based on the Pre-Post Analysis

Group	Any Re-Arrest		Felony Re-Arrest		VFO Re-Arrest		Firearm Re-Arrest	
	HR	p-value	HR	p-value	HR	p-value	HR	p-value
Comparison Group	1.000		1.000		1.000		1.000	
Bail Reform Group	1.025	0.573	1.090	0.148	1.148	0.067	1.369*	0.043

*** p < .001 ** p < .01 * p < .05

Exhibit 4a.3: Probability of Survival without Any Re-Arrest by Group (Pre-Post Analysis)

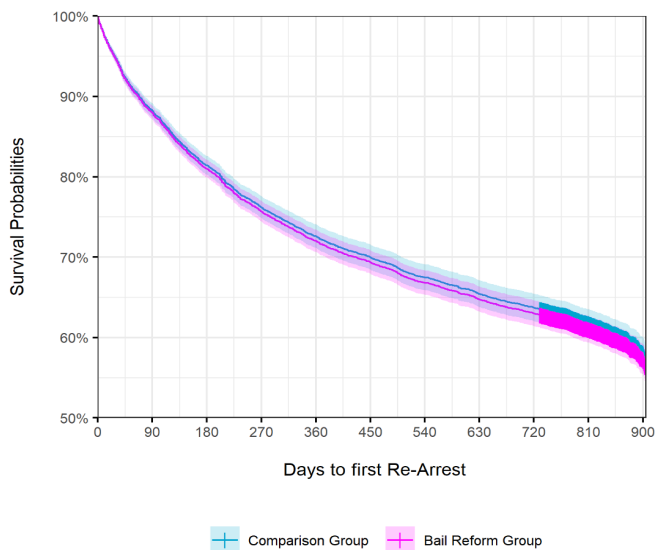


Exhibit 4a.4: Probability of Survival without Felony Re-Arrest by Group (Pre-Post Analysis)

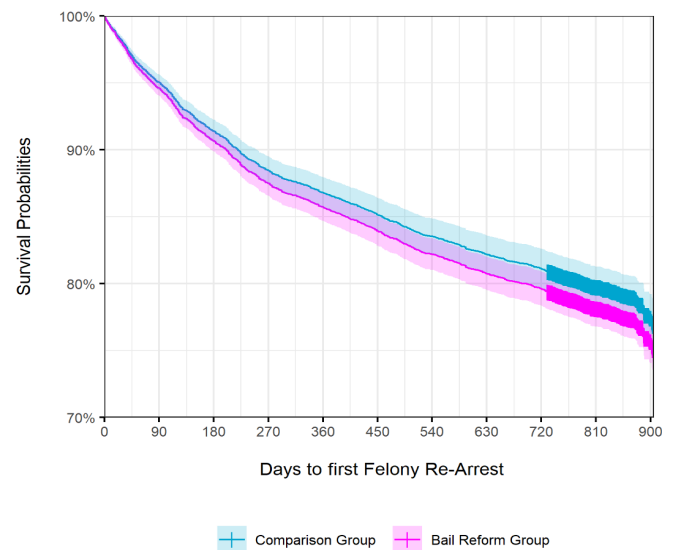


Exhibit 4a.5: Probability of Survival without VFO Re-Arrest by Group (Pre-Post Analysis)

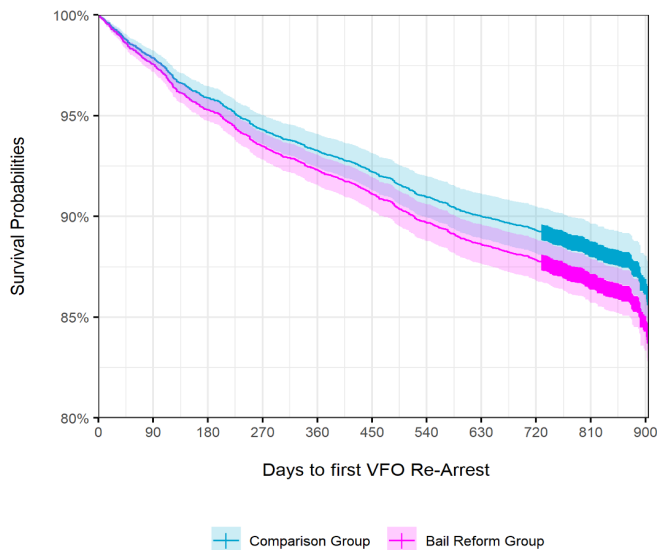


Exhibit 4a.6: Probability of Survival without Firearm Re-Arrest Group (Pre-Post Analysis)

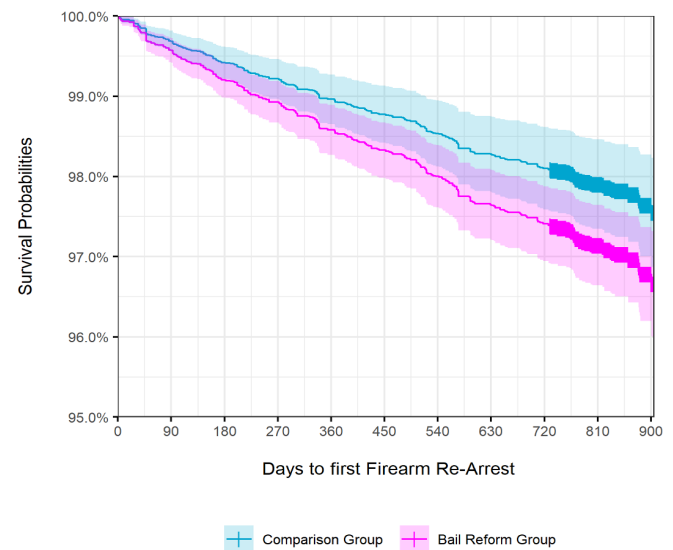


Exhibit 4b.2: Cox Proportional Hazards Regression Models of Time to First Re-Arrest, Felony Re-Arrest, VFO Re-arrest, and Firearm Re-Arrest based on the Contemporaneous Analysis

Group	Any Re-Arrest		Felony Re-Arrest		VFO Re-Arrest		Firearm Re-Arrest	
	HR	p-value	HR	p-value	HR	p-value	HR	p-value
Comparison Group	1.000		1.000		1.000		1.000	
Bail Reform Group	0.988	0.810	0.900	0.114	0.855	0.061	0.808	0.187

*** p < .001 ** p < .01 * p < .05

Exhibit 4b.3: Probability of Survival without Any Re-Arrest by Group (Contemporaneous Design)

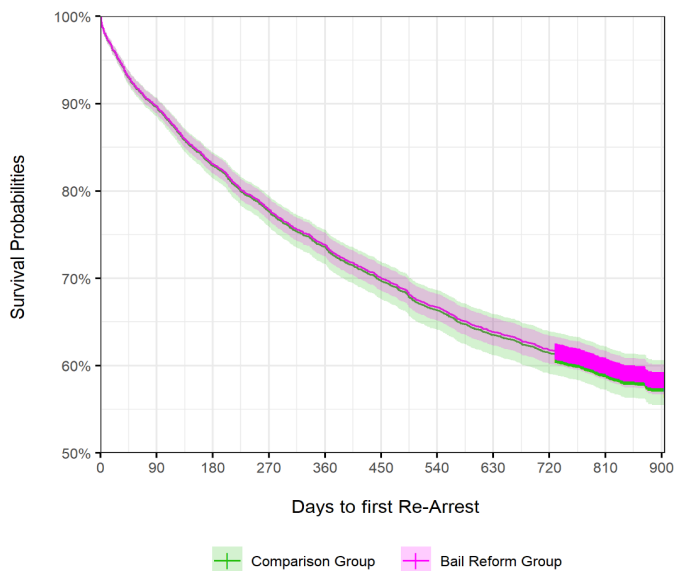


Exhibit 4b.4: Probability of Survival without Felony Re-Arrest by Group (Contemporaneous Design)

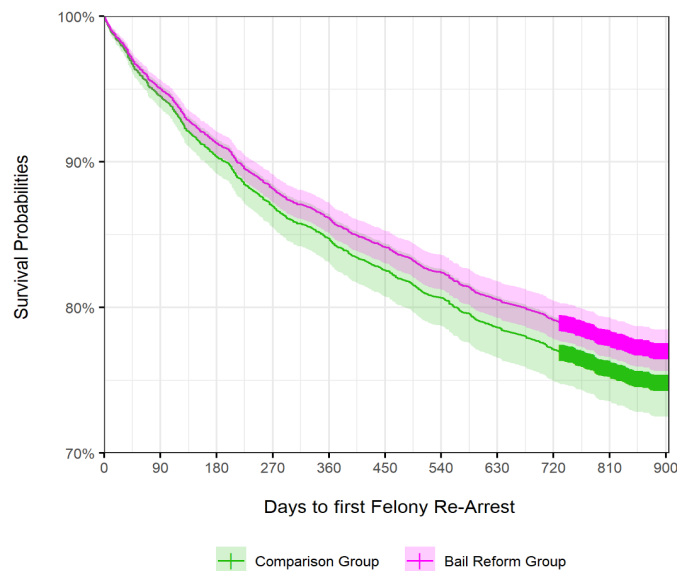


Exhibit 4b.5: Probability of Survival without VFO Re-Arrest by Group (Contemporaneous Design)

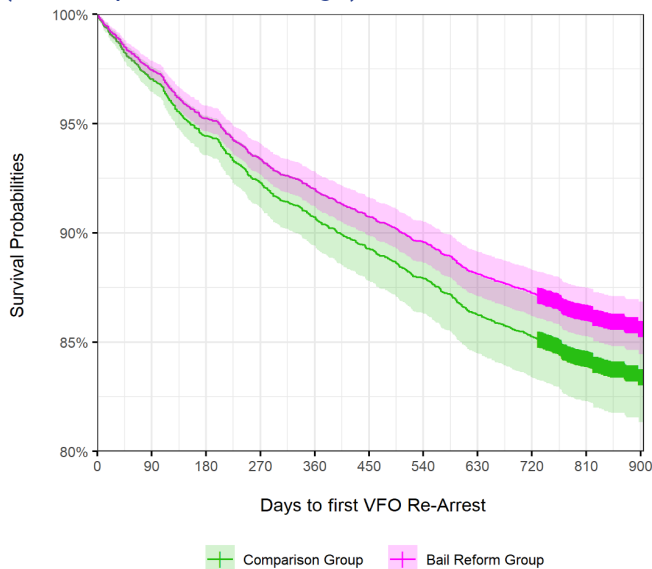
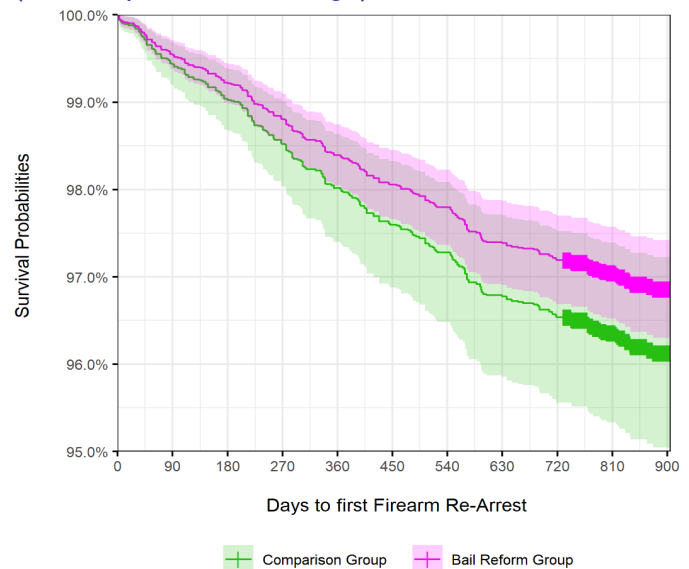


Exhibit 4b.6: Probability of Survival without Firearm Re-Arrest Group (Contemporaneous Design)



Takeaway: Overall, the results from the two designs were either non-statistically significant or involved substantively small differences, which suggest that the release of more bail-eligible people neither decreased nor increased recidivism over two-year and 30-month follow-up timeframes.

Do the Results Change After Controlling for Incarceration Time?

This section compares re-arrest rates after controlling for the number of days people spent in pretrial or post-disposition detention in the two years following arraignment. These analyses also exclude everyone coded in our data as incarcerated throughout the entire follow-up period.⁵³

Variations in Incarceration Time

In the pre-post design, people in the bail reform group were incarcerated for an average of 2.1 days (*median* = 0 days) and people in the comparison group were incarcerated for an average of 108.8 days (*median* = 8 days). In the contemporaneous design, people released without bail were incarcerated for an average of 2.2 days (*median* = 0 days) and people in the comparison group who had bail set or were remanded were incarcerated for an average of 117.9 days (*median* = 7 days). The findings in this section compare re-arrest rates solely during periods when individuals were released.

Findings

As expected, once the greater incarceration time of people in the comparison groups is accounted for, the results across both designs more strongly favored bail reform.

Pre-Post Design. The primary pre-post analyses indicated that re-arrest rates were consistently higher for the bail reform group than for the comparison group (see Exhibit 4a.1). *After controlling for days incarcerated, there were no differences in overall, felony, or VFO re-arrest rates between the two groups (Exhibit 4a.7). The difference in firearm re-arrest rates, however, remained virtually unchanged from before introducing this control (3.3% vs. 2.2%).*

Contemporaneous Design. The primary contemporaneous analyses indicated that the bail reform group had lower felony and VFO re-arrest rates than the comparison group while there were no differences in overall or firearm re-arrest rates (see Exhibit 4b.1). *After controlling for days incarcerated, the bail reform group had lower overall (38% vs. 42%), felony (21% vs. 25%), and VFO (13% vs. 16%) re-arrest rates than the comparison group, while the results for firearm recidivism remained the same as before introducing this control and continued to be non-significant (Exhibit 4b.7).*

Exhibit 4a.7: Likelihood of 2-Year Recidivism by Group, based on the Pre-Post Analysis (N=7,165)

	Bail Reform Group (N=3,726)	Comparison Group (N=3,439)	sig. level
	Likelihood of Re-arrest	Likelihood of Re-arrest	
Any Re-Arrest	37.0%	38.7%	
Felony Re-Arrest	20.9%	20.8%	
VFO Re-Arrest	12.8%	11.6%	
Firearm Re-Arrest	3.3%	2.2%	*

* Note: In the bail reform group, 78.4% were released on recognizance and 21.6% were released on non-monetary conditions; in the comparison group, 2.8% were remanded, 78.4% did not make bail, and 18.8% made bail.

Exhibit 4b.7: Likelihood of 2-Year Recidivism by Group, based on the Contemporaneous Analysis (N=5,502)

	Bail Reform Group (N=3,467)	Comparison Group (N=2,035)	sig. level
	Likelihood of Re-arrest	Likelihood of Re-arrest	
Any Re-Arrest	37.6%	41.6%	**
Felony Re-Arrest	21.4%	24.8%	**
VFO Re-Arrest	13.2%	16.0%	**
Firearm Re-Arrest	3.1%	3.9%	

* Note: In the bail reform group, 77.2% were released on recognizance and 22.8% were released on non-monetary conditions; in the comparison group, 6.2% were remanded, 77.7% did not make bail, and 16.1% made bail.

Does Reducing the Use of Bail in Bail-Eligible Cases Have Varying Impacts for Different Subgroups?

To estimate the effects of releasing more people with bail-eligible charges among particular subgroups of interest, we stratified the sample by: (1) charge severity (misdemeanor or nonviolent felony vs. VFO); (2) whether people had a recent prior arrest; (3) whether people had a pending case at the time of arraignment; and (4) whether people had any recent prior VFO arrest.

All subgroup-specific models are statistically adjusted to ensure sample comparability. The exhibits at the end of this section show the subgroup results based on the two research designs. Specifically, Exhibit 4a.8 and 4a.9 respectively show the two-year re-arrest rates and hazard ratios based on the pre-post design, and Exhibits 4b.8 and 4b.9 respectively display the re-arrest rates and hazard ratios based on the contemporaneous design.

Recidivism Results by Charge Severity

Misdemeanors/Nonviolent Felonies. The *pre-post* analysis shows that for people charged with misdemeanors or nonviolent felonies, release was not associated with any two-year re-arrest outcomes.⁵⁴ The results for the survival analyses show much the same non-effect for overall, felony, and VFO re-arrest, though released people had statistically greater odds of a firearm re-arrest.

The *contemporaneous* analysis shows that for people charged with misdemeanors or nonviolent felonies, release was associated with lower rates of overall re-arrest (44% vs. 56%), felony re-arrest (24% vs. 36%), and VFO re-arrest (12% vs. 21%), while firearm re-arrest rates did not differ by release status.⁵⁵ The survival analysis results are largely consistent with these patterns.

Violent Felonies. The *pre-post* analysis reveals that for people charged with VFOs, release was associated with higher overall re-arrest rates (37% vs. 33%), felony re-arrest rates (22% vs. 18%), VFO re-arrest rates (14% vs. 12%), and firearm re-arrest rates (4.0% vs. 2.7%). The survival analyses indicate that released people were re-arrested somewhat more quickly for a felony, while there were no differences across the two groups for overall, VFO, and firearm re-arrest.

In the *contemporaneous* analysis, pretrial release of individuals charged with VFOs was not associated with any of the four re-arrest outcomes (Exhibit 4b.8). Likewise, the survival models show that recidivism risk was not associated with release status regardless of re-arrest outcome (Exhibit 4b.9).

Recidivism Impact by Criminal History

When stratifying the analyses by criminal history characteristics, the general pattern is one in which the samples are within several percentage points of each other in nearly all analyses. Additionally, while there are variations between the two research designs, the results in totality are more favorable to release for people *without* indicators of criminal history and are more favorable to bail or remand for people with indicators of criminal history.

In addition, two specific findings are consistent regardless of the design:

First, for people *without* a recent prior arrest, release significantly *reduced* any two-year re-arrest (21% vs. 24% in the pre-post and 22% vs. 27% in the contemporaneous design).

Second, for people with a recent violent felony arrest, release significantly *increased* any two-year re-arrest (67% vs. 57% in the pre-post and 66% vs. 58% in the contemporaneous design).

Prior Arrest or Not. The pre-post analysis shows that pretrial release was associated with a small reduction in overall re-arrest rates for people without a recent prior arrest, while it was associated with an increase in re-arrest rates on all four outcomes for people with a recent arrest.⁵⁶

The contemporaneous analysis indicates lower re-arrest rates across all four outcomes for released people without a recent prior arrest, while there were no differences in recidivism for people with recent priors. For people respectively without and with a prior arrest, the survival models mirror the substantive direction of the two-year re-arrest findings.

Pending Case or Not. The pre-post analysis indicates that release was associated with slight increases in felony, VFO, and firearm re-arrest rates for people without pending cases, though the results for the survival analyses indicate no associations between release status and re-arrest for any outcome. For people with a pending case, the pre-post analysis points to increased VFO and firearm re-arrest rates (with these findings mirrored in the survival analyses).

The contemporaneous analysis shows that release was associated with lower felony, VFO, and firearm re-arrest rates for people without pending cases, while release status was not associated with any of the four re-arrest outcomes for people with a pending case. (These findings were all mirrored in the survival analyses.)

Prior VFO Arrest or Not. In the pre-post analysis, release status was not associated with recidivism among people without recent VFO arrests, but release without bail was associated with an increase in all four re-arrest outcomes for people with a recent VFO arrest (with results for three of those four outcomes statistically significant in the survival analyses).

The contemporaneous analysis shows that the release of individuals without recent VFO arrests was associated with lower felony and VFO re-arrest rates; individuals with a prior VFO were more likely to be re-arrested for any offense, though results for all four outcomes were non-significant in the survival analysis.

Exhibit 4a.8. Two-Year Re-Arrest Outcomes with Different Charge Levels and Criminal History Characteristics by Group – Pre vs. Post Design

	Re-Arrest Rates by Charge Level					
	Misdemeanor/NVF Charge (N=2,337)			VFO Charge (N=5,304)		
	Bail Reform Group (N=1,348)	Comparison Group (N=989)	sig. level	Bail Reform Group (N=2,378)	Comparison Group (N=2,926)	sig. level
Any Re-Arrest	41.3%	42.9%		37.0%	33.4%	**
Felony Re-Arrest	22.4%	23.2%		21.7%	17.8%	***
VFO Re-Arrest	11.5%	9.4%		14.4%	11.8%	**
Firearm Re-Arrest	1.8%	1.0%		4.0%	2.7%	*
	Re-Arrest Rates by Whether Individuals had Any Recent Criminal History					
	No Criminal History (N=4,061)			Criminal History (N=3,580)		
	Bail Reform Group (N=2,190)	Comparison Group (N=1,871)	sig. level	Bail Reform Group (N=1,536)	Comparison Group (N=2,044)	sig. level
Any Re-Arrest	21.0%	23.7%	*	58.0%	50.4%	***
Felony Re-Arrest	10.2%	11.0%		35.1%	29.0%	***
VFO Re-Arrest	5.9%	6.4%		22.1%	16.2%	***
Firearm Re-Arrest	1.5%	1.6%		5.4%	2.9%	***
	Re-Arrest Rates by Whether Individuals had a Pending Case at the Time of Arraignment					
	No Pending Case (N=6,451)			Pending Case (N=1,190)		
	Bail Reform Group (N=3,223)	Comparison Group (N=3,228)	sig. level	Bail Reform Group (N=503)	Comparison Group (N=687)	sig. level
Any Re-Arrest	34.5%	32.3%		58.6%	55.8%	
Felony Re-Arrest	18.7%	16.6%	*	38.8%	34.0%	
VFO Re-Arrest	11.5%	9.6%	*	24.1%	18.7%	*
Firearm Re-Arrest	2.8%	2.0%	*	5.7%	3.2%	*
	Re-Arrest Rates by Whether Individuals had Recent VFO Arrests					
	No Prior VFO Arrests (N=6,831)			Prior VFO Arrest (N=810)		
	Bail Reform Group (N=3,423)	Comparison Group (N=3,408)	sig. level	Bail Reform Group (N=303)	Comparison Group (N=507)	sig. level
Any Re-Arrest	34.8%	33.5%		67.0%	57.4%	**
Felony Re-Arrest	18.8%	17.3%		46.8%	36.3%	**
VFO Re-Arrest	11.0%	9.6%		33.4%	22.8%	**
Firearm Re-Arrest	2.5%	2.1%		9.8%	3.5%	**

* p < .05 ** p < .01 *** p < .001

Exhibit 4a.9. Cox Proportional Hazards Regression Models of Time to Each Re-Arrest Outcome for People with Different Charge Levels and Criminal History Characteristics by Group – Pre vs. Post Design

	Re-Arrest Rates by Charge Level			
	Misdemeanor/NVF Charge		VFO Charge	
	HR**	p-value	HR	p-value
<i>Any Re-Arrest</i>	0.893	0.153	1.093	0.092
<i>Felony Re-Arrest</i>	0.932	0.499	1.175*	0.025
<i>VFO Re-Arrest</i>	1.214	0.180	1.131	0.167
<i>Firearm Re-Arrest</i>	2.105*	0.031	1.267	0.181
	Re-Arrest Rates by Whether Individuals had Any Recent Criminal History			
	No Criminal History		Criminal History	
	HR	p-value	HR	p-value
<i>Any Re-Arrest</i>	0.833*	0.013	1.147*	0.012
<i>Felony Re-Arrest</i>	0.841	0.093	1.129**	0.006
<i>VFO Re-Arrest</i>	0.812	0.116	1.328**	0.002
<i>Firearm Re-Arrest</i>	0.796	0.369	1.746**	0.003
	Re-Arrest Rates by Whether Individuals had a Pending Case at the Time of Arraignment			
	No Pending Case		Pending Case	
	HR	p-value	HR	p-value
<i>Any Re-Arrest</i>	1.013	0.790	1.069	0.482
<i>Felony Re-Arrest</i>	1.056	0.417	1.202	0.130
<i>VFO Re-Arrest</i>	1.084	0.351	1.356*	0.043
<i>Firearm Re-Arrest</i>	1.224	0.259	1.914*	0.034
	Re-Arrest Rates by Whether Individuals had Recent VFO Arrests			
	No Prior VFO Arrests		Prior VFO Arrest	
	HR	p-value	HR	p-value
<i>Any Re-Arrest</i>	0.995	0.916	1.190	0.122
<i>Felony Re-Arrest</i>	1.029	0.659	1.384*	0.016
<i>VFO Re-Arrest</i>	1.062	0.473	1.512*	0.011
<i>Firearm Re-Arrest</i>	1.126	0.481	2.592**	0.004

* p < .05 ** p < .01 *** p < .001

** Note: The hazard ratios illustrated in the exhibit are for the bail reform groups. All hazard ratios for the comparison groups

Exhibit 4b.8. Two-Year Re-Arrest Outcomes with Different Charge Levels and Criminal History Characteristics by Group – Contemporaneous Design

	Re-Arrest Rates by Charge Level					
	Misdemeanor/NVF Charge (N=1,402)			VFO Charge (N=4,186)		
	Bail Reform Group (N=1,093)	Comparison Group (N=309)	sig. level	Bail Reform Group (N=2,374)	Comparison Group (N=1,812)	sig. level
Any Re-Arrest	44.1%	55.5%	***	36.4%	35.4%	
Felony Re-Arrest	23.5%	35.9%	***	20.9%	20.7%	
VFO Re-Arrest	12.3%	20.6%	***	13.6%	14.3%	
Firearm Re-Arrest	2.0%	3.4%		3.5%	4.2%	
	Re-Arrest Rates by Whether Individuals had Any Recent Criminal History					
	No Criminal History (N=3,068)			Criminal History (N=2,520)		
	Bail Reform Group (N=2,059)	Comparison Group (N=1,009)	sig. level	Bail Reform Group (N=1,408)	Comparison Group (N=1,112)	sig. level
Any Re-Arrest	21.6%	26.8%	**	58.7%	56.4%	
Felony Re-Arrest	10.5%	15.3%	***	35.1%	35.1%	
VFO Re-Arrest	6.1%	10.0%	***	22.0%	22.6%	
Firearm Re-Arrest	1.5%	3.2%	**	5.1%	4.9%	
	Re-Arrest Rates by Whether Individuals had a Pending Case at the Time of Arraignment					
	No Pending Case (N=4,755)			Pending Case (N=833)		
	Bail Reform Group (N=3,003)	Comparison Group (N=1,752)	sig. level	Bail Reform Group (N=464)	Comparison Group (N=369)	sig. level
Any Re-Arrest	34.7%	36.4%		59.7%	59.7%	
Felony Re-Arrest	18.7%	21.7%	*	38.2%	37.5%	
VFO Re-Arrest	11.6%	14.7%	**	23.1%	20.6%	
Firearm Re-Arrest	2.8%	4.1%	*	4.9%	3.0%	
	Re-Arrest Rates by Whether Individuals had Recent VFO Arrests					
	No Prior VFO Arrests (N=4,969)			Prior VFO Arrest (N=619)		
	Bail Reform Group (N=3,172)	Comparison Group (N=1,797)	sig. level	Bail Reform Group (N=295)	Comparison Group (N=324)	sig. level
Any Re-Arrest	34.9%	37.6%		66.1%	57.9%	*
Felony Re-Arrest	18.7%	22.1%	**	44.9%	39.3%	
VFO Re-Arrest	11.1%	14.1%	**	31.3%	27.4%	
Firearm Re-Arrest	2.4%	3.5%	**	8.6%	7.4%	

* p < .05 ** p < .01 *** p < .001

Exhibit 4b.9. Cox Proportional Hazards Regression Models of Time to Each Re-Arrest Outcome for People with Different Charge Levels and Criminal History Characteristics by Group – Contemporaneous Design

	Re-Arrest Rates by Charge Level			
	Misdemeanor/NVF Charge		VFO Charge	
	HR**	p-value	HR	p-value
Any Re-Arrest	0.753**	0.005	1.074	0.205
Felony Re-Arrest	0.602***	0.000	1.032	0.669
VFO Re-Arrest	0.619**	0.006	0.936	0.468
Firearm Re-Arrest	0.807	0.626	0.799	0.191
	Re-Arrest Rates by Whether Individuals had Any Recent Criminal History			
	No Criminal History		Criminal History	
	HR	p-value	HR	p-value
Any Re-Arrest	0.797**	0.008	1.092	0.156
Felony Re-Arrest	0.679**	0.001	1.005	0.950
VFO Re-Arrest	0.578***	0.000	1.005	0.960
Firearm Re-Arrest	0.469**	0.006	1.048	0.811
	Re-Arrest Rates by Whether Individuals had a Pending Case at the Time of Arraignment			
	No Pending Case		Pending Case	
	HR	p-value	HR	p-value
Any Re-Arrest	0.958	0.450	1.091	0.416
Felony Re-Arrest	0.860*	0.049	1.030	0.822
VFO Re-Arrest	0.787*	0.012	1.125	0.485
Firearm Re-Arrest	0.685*	0.039	1.617	0.152
	Re-Arrest Rates by Whether Individuals had Recent VFO Arrests			
	No Prior VFO Arrests		Prior VFO Arrest	
	HR	p-value	HR	p-value
Any Re-Arrest	0.943	0.288	1.211	0.119
Felony Re-Arrest	0.832*	0.014	1.215	0.173
VFO Re-Arrest	0.779**	0.008	1.177	0.352
Firearm Re-Arrest	0.731	0.086	1.082	0.810

* p < .05 ** p < .01 *** p < .001

** Note: The hazard ratios illustrated in the exhibit are for the bail reform groups. All hazard ratios for the comparison groups are 1.000.

What's the Upshot?

For cases remaining legally bail eligible, this study does not provide clear evidence that releasing more people impacted recidivism in any direction. On balance, different choices of method yielded modest variations in the “bottom line.” The pre vs. post analysis suggests a modest detrimental impact of bail reform on recidivism whereas the contemporaneous analysis suggests a modest beneficial impact on recidivism. And if choosing to remove days incarcerated from the tracking period, it shifts the pre-post results to, essentially, a null effect, while shifting the contemporaneous results further in a favorable direction to bail reform.

Given the modest relationships between release status and recidivism even when differences exceeded the statistical margin of error, our findings at best provide only weak support for the idea that releasing more bail-eligible people either increased or decreased recidivism.

However, the findings show that the association between pretrial release and recidivism varied across key subgroups defined by their charge severity or criminal history. While the subgroup results are also not entirely consistent with one another—for most subgroups, we found statistically significant results in one research design and null effects in the other—they do point to the following overarching patterns: The results from the two designs taken in tandem suggest that the release of bail-eligible people with recent violent felony arrests tended to *increase* recidivism (or certainly did not decrease it), and that releasing bail-eligible people accused of misdemeanors or nonviolent felonies and people with no prior arrests or prior VFOs tended to *decrease* recidivism (or at least did not increase it). Many, though not all, differences were small in magnitude. The most reliable effects across both designs were that pretrial release reduced any re-arrest for people without a recent prior arrest and increased re-arrest for people with a recent violent felony re-arrest.

Chapter 5 – Estimated Impact of the 2020 Amendments Expanding Bail Eligibility

New York's bail reform was amended effective July 2, 2020, reinstating bail eligibility for certain cases for which the original reforms had eliminated the option of bail or detention. These amendments affected 6% of all bail ineligible cases arraigned in the first half of 2020. The most impactful provisions of this rollback in leading more cases to face bail were: (1) making more burglary in the second degree cases bail eligible; (2) making cases bail eligible if a judge deems it to involve "harm to identifiable person or property" if there is a pending case meeting the same criterion (henceforth referred to as "harm-harm cases");⁵⁷ and (3) restoring bail eligibility for domestic violence cases involving the obstruction of breathing or blood circulation (i.e., misdemeanor choking offense).

To test whether this rollback promoted public safety, we compared recidivism outcomes between:

- **Original Bail Reforms:** People with the relevant case and criminal history characteristics mandatorily released without bail in the first half of 2020.
- **Bail Eligible Due to the 2020 Amendments:** People with comparable characteristics who had bail set or were remanded in the first half of 2019 (pre-reform).

Like all previous analyses, the results were statistically adjusted using propensity scores and inverse probability weighting to mitigate observed baseline differences across the groups.

Sample Characteristics

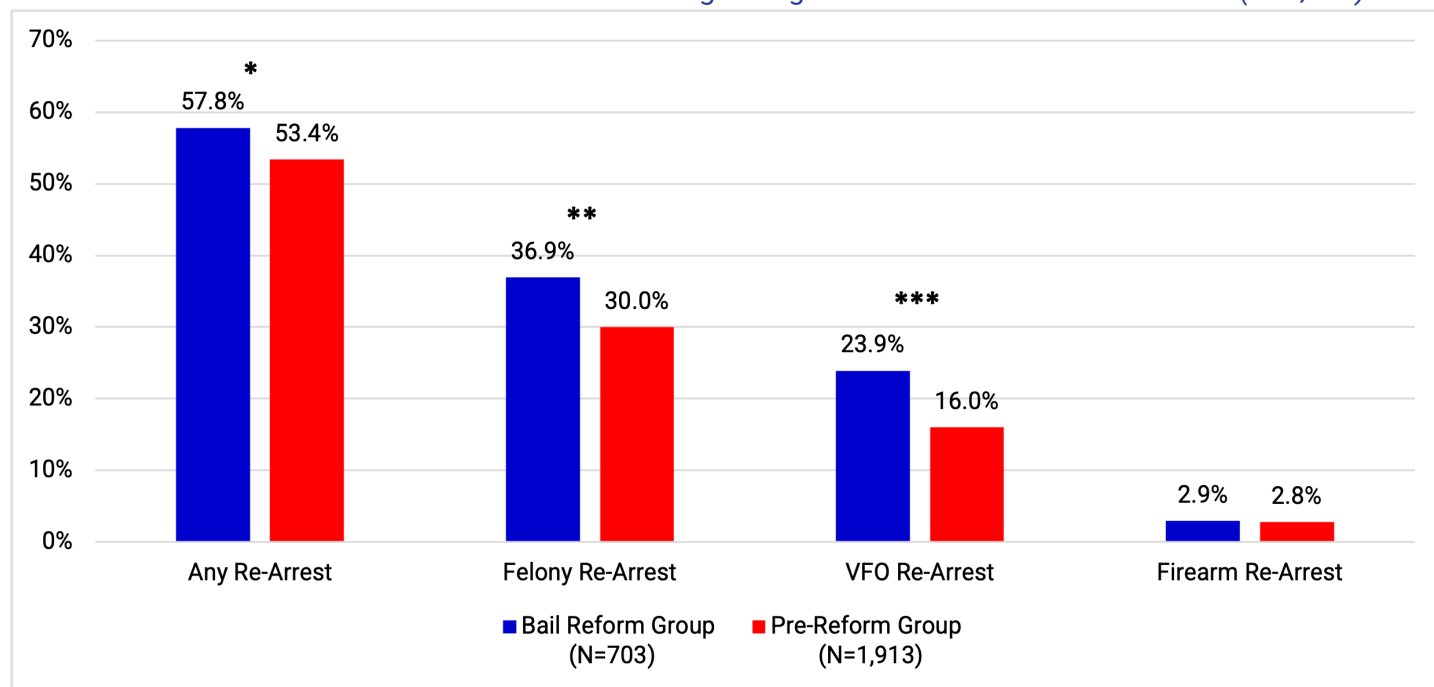
The two samples had the following characteristics:

- **Provision of the 2020 Amendments:** Cases falling under specific amendment provisions were as follows: burglary in the second degree (12% of the original reform and 13% of the pre-reform sample); obstruction of breathing cases (31% and 28%, respectively); harm to person or property (54% and 50%, respectively);⁵⁸ and all other provisions (4% and 9%, respectively).
- **Charges:** The charges for the respective original and pre-reform samples were 75% and 73% misdemeanors, 11% and 13% nonviolent felonies, and 14% and 14% violent felonies.⁵⁹
- **Criminal History:** The respective samples included 75% and 70% with a recent prior arrest, 21% and 21% with a recent violent felony arrest, 59% and 56% with a pending case at the time of the current arraignment, and 41% and 39% with a prior warrant from an earlier case.
- **Demographics:** The samples were 52% Black, 33% Hispanic, 11% white, and 4% Asian or from additional racial/ethnic groups. Women made up 11% and 10% of the respective bail reform and pre-reform samples, and the average ages were 36 and 35, respectively.

Two-Year Re-Arrest Rates

The results show that people subject to mandatory release under the original reform law were significantly more likely to be re-arrested for any offense (58% vs. 53%), a felony (37% vs. 30%) and a VFO (24% vs. 16%) than comparable people who had bail set or were remanded pre-reform. There were no statistically significant differences in firearm re-arrest rates (Exhibit 5.1). There were also minimal changes to the findings after controlling for the time people spent incarcerated during the tracking period.⁶⁰

Exhibit 5.1: Recidivism for Cases that Became Bail Eligible Again Under the 2020 Amendments (N=2,616)



* $p < .05$ ** $p < .01$ *** $p < .001$

In the bail reform group, 79.5% were released on recognizance and 20.5% were released on non-monetary conditions; in the pre-reform group, 1.7% were remanded, 85.4% did not make bail, and 12.9% made bail.

Survival Analysis

The survival analyses indicate that people who were mandatorily released were at an elevated risk of felony re-arrest and VFO re-arrest, at 24% and 39%, respectively, while there were no significant associations between release status and overall and firearm re-arrest. (Exhibit 5.2). Exhibits 5.3 through 5.6 illustrate the survival plots based on the Cox regression models for each re-arrest category; a simple visual inspection shows the greater gap between the survival curves of each sample on felony and VFO re-arrest than on overall and firearm re-arrest.

Exhibit 5.2: Cox Proportional Hazards Regression Models of Time to Each Re-Arrest Outcome

Group	Any Re-Arrest		Felony Re-Arrest		VFO Re-Arrest		Firearm Re-Arrest	
	HR	p-value	HR	p-value	HR	p-value	HR	p-value
Pre-Reform Group	1.000		1.000		1.000		1.000	
Bail Reform Group	1.099	0.108	1.235**	0.005	1.394***	0.001	0.909	0.693

Exhibit 5.3: Probability of Survival without Any Re-Arrest by Group

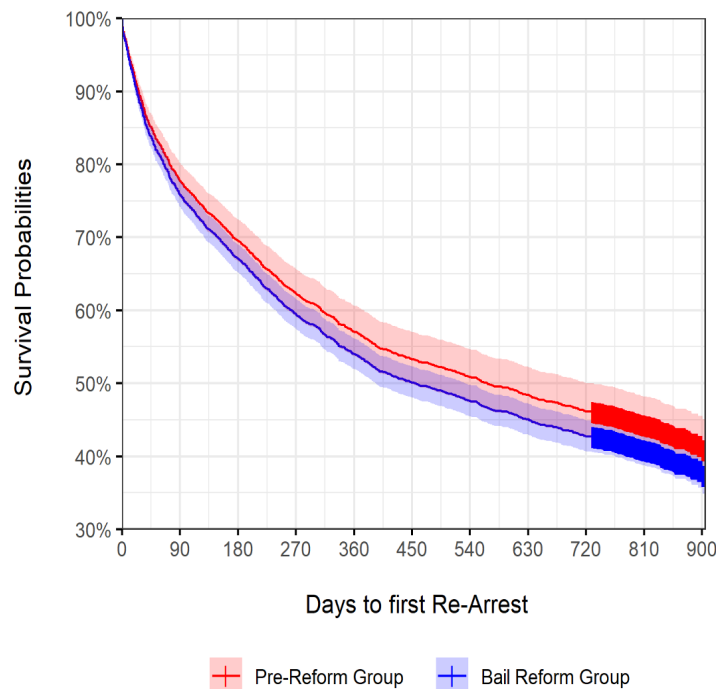


Exhibit 5.4: Probability of Survival without Felony Re-Arrest by Group

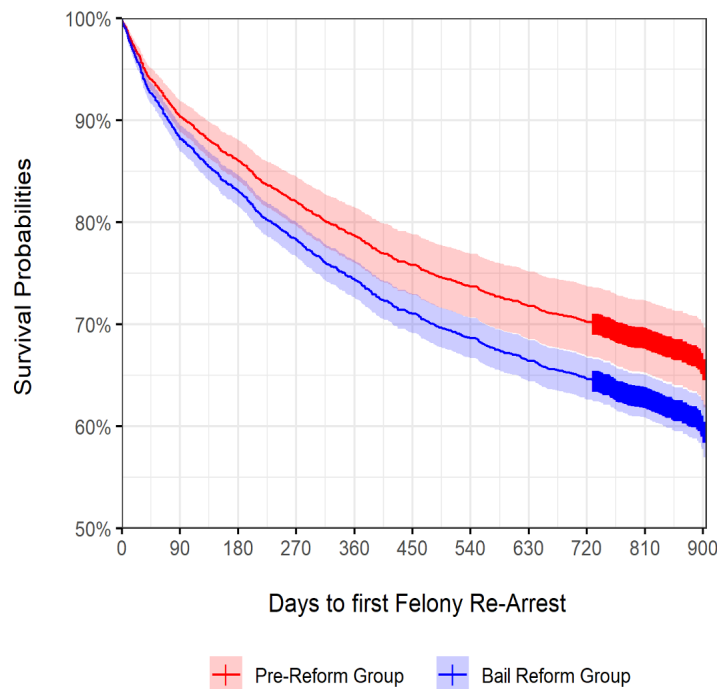


Exhibit 5.5: Probability of Survival without VFO Re-Arrest by Group

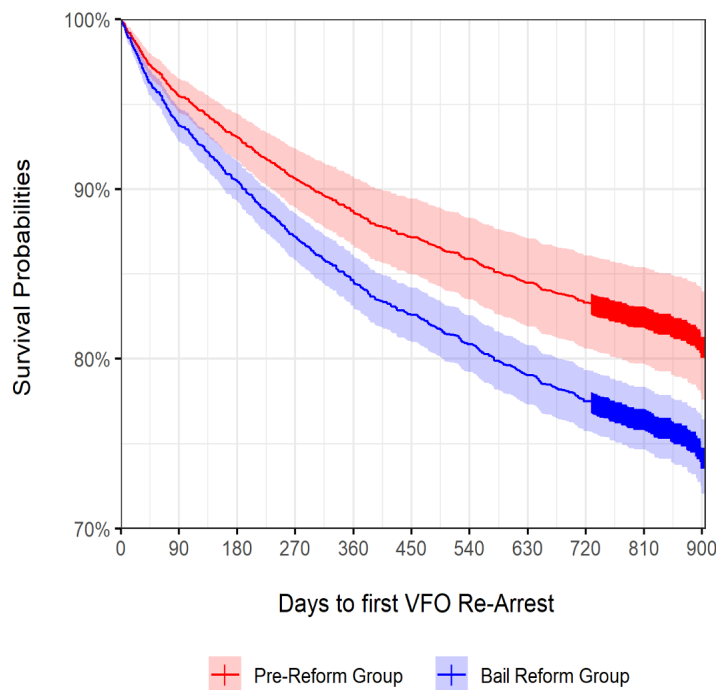
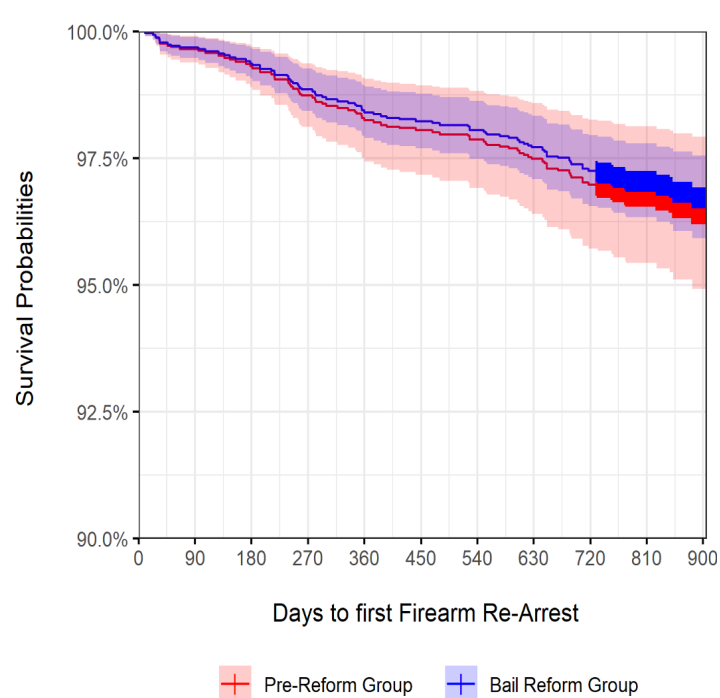


Exhibit 5.6: Probability of Survival without Firearm Re-Arrest by Group



Do the 2020 Amendments Have Varying Recidivism Impacts for Different Subgroups?

We conducted subgroup analyses to estimate the effects of the bail reform amendments pertaining to: (1) cases charged with burglary in the second degree; (2) “harm-harm” cases charged with misdemeanors; (3) “harm-harm” cases charged with felonies; and (4) domestic violence cases involving obstruction of breathing or blood circulation (New York State Penal Law 121.11).⁶¹

All subgroup-specific models are adjusted to assure sample comparability. Exhibits 5.7 and 5.8 at the end of this section below present the two-year re-arrest rates and hazard ratios, respectively, for any re-arrest, felony re-arrest, VFO re-arrest, and firearm re-arrest for each subgroup by release status.

Burglary in the Second Degree. *For people charged with burglary in the second degree, pretrial release was associated with increased rates of felony re-arrest (53% vs. 34%) and VFO re-arrest (38% vs. 20%). Although the effect was not statistically significant, the results for any re-arrest suggest a similar effect of higher re-arrest rates resulting from pretrial release. Firearm re-arrest rates, in contrast, were virtually identical across the two groups. The results for the survival analysis show the same patterns.*

Criminal Obstruction of Breathing Cases. *For domestic violence cases involving the misdemeanor obstruction of breathing or blood circulation charge, pretrial release drastically reduced rates of overall re-arrest (37% vs. 45%), felony re-arrest (16% vs. 27%), and VFO re-arrest (9% vs. 15%). The results for firearm re-arrest were not statistically significant, but they also suggest a decreased recidivism risk associated with release. The findings based on the survival analyses indicate the same patterns.*

Cases Impacted by the “Harm-Harm” Provision by Charge Severity. *Pretrial release of “harm-harm” cases was associated with higher felony and VFO re-arrest rates regardless of charge severity, but the effects were considerably larger for people charged with felonies than for people charged with misdemeanors. Likewise, people charged with felonies also experienced increased rates of any re-arrest, while this outcome was unaffected by release status for people charged with misdemeanors. In a sense, these findings are consistent with the particular subgroup result provided in Chapter 3 that found people subject to mandatory release with a pending case (a necessary component of “harm-harm” cases) saw increased recidivism.*

- **Misdemeanors.** *For “harm-harm” cases charged with misdemeanors, pretrial release was associated with higher rates of felony re-arrest (43% vs. 34%) and VFO re-arrest (28% vs. 18%), but release status did not affect overall or firearm re-arrest rates. The results for the survival analyses are consistent with these patterns.*
- **Felonies.** *For “harm-harm” cases charged with felonies, mandatory release increased rates of overall re-arrest (81% vs. 52%), felony re-arrest (64% vs. 32%), and VFO re-arrest (43% vs. 17%), but not for firearm re-arrest. Again, the results for the survival analysis are consistent.*

Exhibit 5.7. Two-Year Re-Arrest Outcomes with Different Charge Levels and Criminal History Characteristics by Group

	Re-Arrest Rates for Burglary 2 Cases (N=318)					
	Bail Reform Group (N=209)	Pre-Reform Group (N=109)	sig. level			
Any Re-Arrest	60.2%	49.0%				
Felony Re-Arrest	52.6%	33.7%	**			
VFO Re-Arrest	37.6%	19.5%	**			
Firearm Re-Arrest	0.8%	0.9%				
	Re-Arrest Rates for Obstruction of Breathing Cases** (N=795)					
	Bail Reform Group (N=653)	Pre-Reform Group (N=142)	sig. level			
Any Re-Arrest	36.7%	45.0%	*			
Felony Re-Arrest	15.9%	26.5%	**			
VFO Re-Arrest	8.7%	14.5%	*			
Firearm Re-Arrest	1.8%	3.7%				
	Re-Arrest Rates for Cases Impacted by the "Harm-Harm" Provision by Charge Severity					
	Misdemeanor (N=1,110)		Felony (N=361)			
	Bail Reform Group (N=780)	Pre-Reform Group (N=330)	sig. level	Bail Reform Group (N=263)	Pre-Reform Group (N=98)	sig. level
Any Re-Arrest	69.0%	66.9%		81.1%	51.9%	***
Felony Re-Arrest	42.5%	34.2%	*	63.8%	31.8%	***
VFO Re-Arrest	28.0%	18.1%	**	42.5%	17.0%	***
Firearm Re-Arrest	4.1%	3.1%		3.8%	2.8%	

* p < .05 ** p < .01 *** p < .001

** Note: Of the 795 criminal obstruction of breathing cases, 666 (84%) have third-degree assault as the top charge; however, as mentioned in Chapter 2, we are assuming that about a quarter of all third-degree assault cases also involved obstruction of breathing charges.

Exhibit 5.8. Cox Proportional Hazards Regression Models of Time to Each Re-Arrest Outcome for People with Different Charge Levels and Criminal History Characteristics by Group

	Re-Arrest Rates for People Charged with Burglary in the Second Degree			
	HR**		p-value	
Any Re-Arrest	1.244		0.152	
Felony Re-Arrest	1.612**		0.007	
VFO Re-Arrest	1.699*		0.013	
Firearm Re-Arrest	0.509		0.465	
	Re-Arrest Rates for Re-Arrest Rates for Obstruction of Breathing Cases			
	HR		p-value	
Any Re-Arrest	0.695**		0.008	
Felony Re-Arrest	0.545***		0.000	
VFO Re-Arrest	0.507**		0.004	
Firearm Re-Arrest	0.342		0.023	
	Re-Arrest Rates for Cases Impacted by the “Harm-Harm” Provision by Charge Severity			
	Misdemeanor		Felony	
	HR	p-value	HR	p-value
Any Re-Arrest	1.038	0.638	2.046***	0.000
Felony Re-Arrest	1.281*	0.017	2.250***	0.000
VFO Re-Arrest	1.501**	0.002	2.858***	0.000
Firearm Re-Arrest	1.129	0.703	1.367	0.640

* p < .05 ** p < .01 *** p < .001

** Note: The hazard ratios illustrated in the exhibit are for the bail reform groups. All hazard ratios for the pre-reform groups are 1.000.

What's the Upshot?

The results suggest that New York's original bail reforms led to an increase in overall, felony, and VFO recidivism among the subgroup of people whose case and criminal history characteristics would make them bail eligible again after the implementation of the 2020 amendments. This pattern holds true for all but one subgroup: For people facing the misdemeanor obstruction of breathing charge in a domestic violence matter, the mandatory release provision of the original bail reform was associated with substantially lower overall, felony, and VFO recidivism. Altogether, while the 2020 rollbacks likely decreased recidivism in New York City, restoring bail eligibility for the misdemeanor criminal obstruction of breathing cases appears to have had the opposite effect.

Chapter 6 – Summary and Conclusions

The many goals of New York’s bail reform law involve reducing unnecessary pretrial detention, increasing socioeconomic and racial fairness for people presumed innocent of a crime, and maintaining public safety. As we summarized in Chapter 1, ample studies have made clear that bail reform has [significantly reduced](#) the use of money bail and pretrial detention statewide, even after allowing that the scale of those effects has been dampened by deficits in the quality of state court implementation.⁶²

With this study, we endeavored to upgrade our knowledge of bail reform’s *recidivism* impacts, recognizing that maintaining public safety is one among many key goals of reform, and pretrial decision-making is one of the many possible tools (some of which exist outside the criminal legal system) that may help to increase safety.

The State of Our Imperfect Knowledge Concerning Recidivism

Since bail reform went into effect, its potential impact on crime and recidivism has received extensive media attention, despite a dearth of rigorous results.

Common shortcomings in previously reported recidivism data have included an inability to examine re-arrest beyond the oftentimes brief pretrial period; no effort to distinguish re-arrest among cases subject to bail elimination from cases still eligible for bail; and, most seriously, the omission of statistical strategies to ensure comparability of criminal histories, charges, and demographics between people who were released under reform and who previously faced bail or were remanded.

The present evaluation avoided all of these shortcomings. Nonetheless, in Chapter 2, we identified seven distinct limitations of our own chosen approach. One design limitation worth reiterating is that our maximum tracking period of 30 months arguably falls short of a true long-term evaluation of how pretrial release versus bail impacts people’s ultimate recidivism trajectories. We hope to release a follow-up update in the future with a longer tracking period.

Additionally, when estimating the recidivism effects of reduced bail-setting on cases *remaining legally eligible for bail*, we reported full findings using two research designs. Results were consistently several percentage-points different between these designs. This reality underscores that there is inevitably a margin of error between a credible impact estimate and objective truth.

With these caveats out of the way, our study yielded a number of unmistakable takeaways that illuminate our research questions.

Emergent Themes and Findings

1. What Was the Impact of Eliminating the Options of Bail and Detention in Select Cases?

Our analysis found that eliminating bail for most misdemeanor and nonviolent felony cases significantly reduced recidivism.⁶³ Over a two-year tracking period, this association reached a robust magnitude for overall re-arrest (44% vs. 50%) and was also statistically significant for felony re-arrest (24% vs. 27%). *In short, given the core policy tradeoffs at stake, our research indicates that a failure to have passed the original bail reforms would have maintained higher net recidivism rates in New York City.*

From a public safety perspective, *violent crime* is particularly destructive to its victims, the communities in which it is most concentrated, and society at large.⁶⁴ We found that eliminating bail was minimally associated with violent felony recidivism *in either direction*. Yet the one statistically significant finding regarding violence also showed that eliminating bail modestly reduced recidivism: Over 30 months, people who were mandatorily released were re-arrested less quickly (if at all) for a violent felony.

Hence, the results strongly favor bail reform's elimination of money bail, given that the policy did not only maintain public safety, but *increased* it by reducing recidivism rates.

2. What Was the Impact of Reducing the Use of Bail in Cases Remaining Eligible for It?

Here, our decision to minimize biases inherent in any particular approach by redoing the analysis with two alternative research designs provided the advantage of a more conservative overall methodology at the expense of our ability to assert unqualified “bottom line” answers. While our approach may be less satisfying, we believe it is was the more prudent one to limit premature conclusions in any direction.

Our “pre vs. post” design suggested that releasing people in bail-eligible cases led to modest increases in two-year re-arrest rates for all outcomes (though always by magnitudes of under 3 percentage-points). Our “contemporaneous” design pointed to similarly modest decreases in two-year felony and violent felony re-arrest rates. Survival analyses involving a longer 30-month tracking period saw almost no differences exceeding the statistical margin of error between bail reform and comparison groups coming out of either design. Finally, if omitting the time people spent incarcerated from the tracking period (since people cannot be re-arrested in the community when they are literally behind bars), most raw results shifted in a direction more favorable to pretrial release—but even after this small shift, only the contemporaneous design yielded results significantly favorable to bail reform. (Most of the results in the pre-post design shifted to null: i.e., no difference between the samples.)

Nevertheless, all findings taken together yield a clear overarching story: **The totality of our findings—all of which are either non-significant or involve substantively small differences—point to a lack of support for concluding that releasing more bail-eligible people either increased or decreased recidivism over two-year and 30-month follow-up timeframes.** In other words, reducing bail for cases remaining legally eligible for it maintained the same level of safety.

3. What Was the Impact of the Bail Amendments that Went into Effect July 2, 2020?

We sought to dig deeper into the question of for *whom* pretrial release under bail reform had more positive or negative effects. Foremost, we isolated cases made ineligible for bail under the original reforms and made eligible for bail again under the 2020 amendments.

While the original reforms reduced recidivism, overall, our findings were equally clear that the amendments implemented in July of 2020 impacted a specific subgroup for whom the original reforms had increased overall, felony, and violent felony recidivism (at magnitudes ranging from 5 to 8 percentage-points over two years).

Further analysis indicated that nearly all cases impacted by the amendments, *in practice*, involved: (1) burglary in the second degree; (2) a case the judge deemed to involve harm to a person or property where an open case met the same criterion (“harm-harm”); or (3) a domestic violence case involving

obstruction of breathing or blood circulation (usually with an assault in the third degree top charge). Our findings were that New York's original reforms increased re-arrest among affected second-degree burglary and "harm-harm" cases, while decreasing re-arrest among affected domestic violence cases involving obstruction of breathing (generally alongside third-degree assault).

4. For Which Additional Subgroups Was Pretrial Release More or Less Beneficial?

Further results suggest that bail reform had differential effects depending on people's charges and criminal history. **Both the mandatory release provisions and, to a lesser extent, provisions leading to the reduced use of bail in cases still eligible for it *reduced* recidivism for people facing *less serious charges* and with *limited or no recent criminal history*, but *increased* recidivism for people with *substantial recent criminal histories*.**

The above patterns were not, however, confirmed in every subgroup analysis. A few concrete examples will serve to demonstrate where these tendencies *did* apply:

- **Subgroups for Which Release Reduced Recidivism:** In cases made subject to mandatory release and (1) charged with a misdemeanor, (2) with no recent prior arrest, and/or (3) with no pending case, our estimates found that release without bail led to significantly *less* overall, felony, and violent felony re-arrest (by magnitudes frequently exceeding 5 percentage-points). In cases remaining eligible for bail, both of our two research designs pointed to a reduction in overall re-arrest in cases with no prior arrest.
- **Subgroups for Which Release Increased Recidivism:** In cases made subject to mandatory release and (1) with a pending case and/or (2) with a recent prior violent felony arrest, our estimates found that release without bail led to significantly *more* overall, felony, and violent felony re-arrest. In cases remaining eligible for bail, both of our two research designs pointed to an increase in overall re-arrest for people with a recent prior violent felony arrest.

Preventing re-arrest in the future could potentially involve careful, targeted policy changes based on these results. For example, future legislation or policy might make fewer "high risk" individuals (e.g., people with a prior violent felony) subject to mandatory release, allowing for wider judicial discretion in considering bail; or might enhance the range of non-monetary conditions for supporting "high risk" individuals.⁶⁵ (To avoid misinterpretation, we are not embracing weaker decision-making standards that might compromise due process, undermine the presumption of innocence, or contradict legal precedents concerning when bail or pretrial detention are permissible.)

On the other end of the spectrum, it could benefit public safety if legislation or policy encouraged the release of more bail-eligible people charged with relatively low-level offenses or with no or only minor criminal history, given that the incapacitation effects of incarceration (most people who face bail end up in pretrial detention) appear to be outweighed by its adverse "criminogenic" effects for these subgroups.

Alternatively, armed with the knowledge that, in totality, the bail elimination provisions of the original reforms reduced recidivism, and reducing the use of bail in cases legally eligible for it had little net effect in either direction, policymakers would be justified on public safety grounds in avoiding further legislative or policy changes while awaiting additional rigorous studies over longer tracking periods.

Forthcoming Research

Our hope is that researchers other than ourselves will apply rigorous methods of their own to the analysis of how New York's bail reform experiment impacts crime and recidivism. At the Data Collaborative for Justice (DCJ), we anticipate a minimum of three future reports on this topic.

- **Upstate Quasi-Experimental Recidivism Study:** Later in 2023, DCJ will release a similar analysis focused on estimating recidivism impacts throughout the remaining regions of New York State.
- **Time Series Analysis:** Also, in 2023, DCJ will release separate Controlled Interrupted Time Series (CITS) analyses for New York City and the rest of the state estimating the system-wide impact on recidivism of New York's bail reform law. Based on the criminal charge at arraignment, all cases arraigned between 2017 and the onset of the COVID-19 lockdowns in mid-March 2020 will be divided into either a mandatory release group (treatment) or a bail-eligible group (comparison). This approach will allow us to estimate not only whether recidivism rates changed after reform, but whether those changes were significantly different between those mandatorily released and those still eligible for bail. The CITS analysis will differ from the current study in two important ways: 1) the empirical strategy will be based on a quasi-experimental design exploiting differences in bail eligibility during the reform period, and 2) the focus will be on the aggregate-level effect of restricting bail eligibility rather than the individual-level effect of releasing an individual who may have otherwise had bail set or been remanded.
- **Longer-Term Follow-Up:** In 2024, DCJ will update the current report by estimating the impact of bail reform over a longer tracking period, enabling us to determine whether the recidivism trajectories of those who were respectively released and who had bail set or were remanded move closer together or farther apart over time.

Endnotes

¹ Rempel, M. & Rodriguez, K. (2019). Bail Reform in New York: Legislative Provisions and Implications for New York City. New York, NY: Center for Court Innovation. Available at: https://www.courtinnovation.org/sites/default/files/media/document/2019/Bail_Reform_NY_full_0.pdf.

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- ¹⁹ See, e.g., Solomon, J. (January 30, 2022). Bail Data: Key Takeaways on Judges' Discretion and Supervised Release. Times Union. Available at: <https://www.timesunion.com/state/article/Bail-data-Key-takeaways-on-judges-discretion-16815918.php>; Solomon, J. (January 13, 2022). New Data Shows Nearly 4 Percent of People Out Due to Bail Changes were Rearrested for Violent Felonies. Times Union. Available at: <https://www.timesunion.com/state/article/New-data-shows-less-than-4-of-people-out-on-bail-16736295.php>.
- ²⁰ On September 21, 2022, the Data Collaborative for Justice (DCJ) co-hosted a webinar with DCJS timed to the release of its public dataset with 2019 to 2021 information. At the webinar, DCJS' summary PowerPoint ([Webinar | Before and After: Data on the Impact of Bail Reform - Data Collaborative for Justice](#)) included useful data not only on recidivism but, also, on a wide range of added pretrial outcomes, including release decisions, bail payment, and failure to appear rates. To be clear, DCJ strongly did and does support the public release of this dataset and summary resource. We simply note that the highly aggregated re-arrest information made available is insufficient for a rigorous formal evaluation of bail reform—for all reasons cited in the body of this report: limited to pretrial re-arrest; bail-eligible vs. ineligible cases not isolated; re-arrest rates shown exclusively for released people (thwarting the need to compare release vs. detention); and no effort to employ statistical strategies to ensure statistical comparability in the 2019, 2020, and 2021 samples. With proper statistical strategies, researchers who analyze the case-level data themselves can overcome some of the limitations in the DCJS data summary, but will not be able to overcome the restriction to pretrial re-arrests only.

²¹ New York State Division of Criminal Justice Services. (2022), Op Cit.

²² New York City Criminal Justice Agency, Op Cit. See, also, NYC Criminal Justice (December 2021), whose publication essentially draws from the same data that populates the Criminal Justice Agency dashboard.

²³ Stemen, D. & Olson, D. (2020). Dollars and Sense in Cook County: Examining the Impact of General Order 18.8A on Felony Bond Court Decisions, Pretrial Release, and Crime. Loyola University Chicago.

²⁴ Heaton, P. (2022). The Effects of Misdemeanor Bail Reform. Quattrone Center. Available at: [12290-the-effects-of-misdemeanor-bail-reformpdf \(upenn.edu\)](https://www.upenn.edu/12290-the-effects-of-misdemeanor-bail-reformpdf).

²⁵ Grant, G.A. (2021). Annual Report to the Governor and the Legislature. Available at: <https://www.njcourts.gov/sites/default/files/courts/criminal/criminal-justice-reform/cjr2021.pdf>

²⁶ See Albright, A. (2021). No Money Bail, No Problems? Evidence from an Automatic Release Program. Available at: https://thelittledataset.com/research_papers/alex_albright_harvard_jmp.pdf; Leslie, E. & Pope, N.G. (2017). The Unintended Impact of Pretrial Detention on Case Outcomes: Evidence from New York City Arraignments. The Journal of Law and Economics, 60(3), 529-557. Available at: http://econweb.umd.edu/~pope/pretrial_paper.pdf; Dobbie, W., Goldin, J., & Yang, C.S. (2018). The Effects of Pretrial Detention on Conviction, Future Crime, and Employment: Evidence from Randomly Assigned Judges. American Economic Review, 108(2), 201-240. Available at: <https://pubs.aeaweb.org/doi/pdf/10.1257/aer.20161503>; Gupta, A., Hansman, C., & Frenchman, E. (2016). The Heavy Costs of High Bail: Evidence from Judge Randomization. The Journal of Legal Studies, 45(2), 471-505. Available at: <http://www.columbia.edu/~cjh2182/GuptaHansmanFrenchman.pdf>; Heaton, P., Mayson, S., & Stevenson, M. (2017). The Downstream Consequences of Misdemeanor Pretrial Detention. Stanford Law Review, 69 (March), 711-794. Available at: <https://review.law.stanford.edu/wp-content/uploads/sites/3/2017/02/69-Stan-L-Rev-711.pdf>; Mueller-Smith, M. (2015). The Criminal and Labor Market Impacts of Incarceration. Available at: <https://sites.lsa.umich.edu/mgms/wp-content/uploads/sites/283/2015/09/incar.pdf>. Note that studies on the effects of pretrial detention exploit the fact that people are quasi-randomly assigned to arraignment judges with varying propensities to release. However, people with certain charge and/or criminal history characteristics are almost always released or held regardless of how strict or lenient the judge is. Given this lack of counterfactual data, these studies cannot make any inferences on the effects of detention for such individuals. Rather, they can only estimate the causal effects of detention for people who would likely be released by a lenient judge or likely be held by a strict judge. In other words, this research may not be generalizable to very “high risk” or “low risk” individuals whose arraignment outcomes are overdetermined by factors unrelated to judge leniency.

²⁷ See Dobbie, W., et al. (2018), Op Cit.; and Leslie, E. & Pope, N.G. (2017), Op Cit.

²⁸ Bergin, T., Ropac, R., Randolph, I., & Joseph, H. (2022). The Initial Collateral Consequences of Pretrial Detention. New York, NY: New York City Criminal Justice Agency (CJA).

²⁹ Lehman, C.F. (2022). Is 3,300 Enough? Why the Borough-Based Jails Are Too Small to Keep NYC Safe. Manhattan Institute. Available at: <https://media4.manhattan-institute.org/sites/default/files/why-borough-based-jails-are-too-small-to-keep-nyc-safe.pdf>.

³⁰ Ropac, R. with Rempel, M. (2023). Technical Supplement: Quasi-Experimental Methods for Estimating the Impact of Bail Reform on Recidivism in New York City. New York, NY: Data Collaborative for Justice. Available at: <https://datacollaborativeforjustice.org/wp-content/uploads/2023/03/TechnicalSupplement-2.pdf>

³¹ Firearm charges include felony offenses whose top charge involved the use, display, possession, or sale of a firearm and displaying what appears to be a firearm.

³² It is critical to track recidivism beyond the pretrial period to test the overall effect of bail reform on public safety. That is, for the public safety question it is of primary relevance whether two individuals with comparable characteristics had different likelihoods of reoffending within the same time period when one person was released without bail and the other person had bail set or was remanded.

³³ New York State Unified Court System, Division of Technology & Court Research. Pretrial Release Data. Available at: <https://ww2.nycourts.gov/pretrial-release-data-33136>.

³⁴ When there were multiple arraignments on the same day for the same person (a highly infrequent occurrence), we used the case with the most restrictive release decision (e.g., if a person was released without bail at one arraignment but had bail set or was remanded at another arraignment on the same day, we used the latter case). For people who still had multiple arraignments on the same day after applying these exclusion criteria, we used the case with the most severe arraignment charge (i.e., keeping violent felony over nonviolent felony cases and nonviolent felony over misdemeanor cases).

³⁵ In general, both due to this sampling nuance and reflecting our use of a standard two-year follow-up period, the aggregate re-arrest rates in this report may vary from other published statistics that, for example, report re-arrests solely for the much shorter pretrial period (or for some other timeframe).

³⁶ Including people born in 1998 or earlier (i.e., people who were at least 18 years old on January 1, 2017) avoided a downward bias in criminal history for young people, as the person-level identifier in our data did not allow us to tally all prior arrests when people were minors. For this reason, applying this age cut-off enabled us to more accurately capture criminal history going back to 2017 for everyone sampled. Our date-of-birth cutoff also avoids confounding issues stemming from New York's Raise the Age reform; it went into effect in 2018 and 2019 for 16-year-olds and 17-year-olds, respectively, and has likely had an impact on recidivism rates for youth; see Gewirtz, M. (2021). Re-Arrest Among 16 Year-Olds Arrested in The First Year of Raise the Age. New York, NY: New York City Criminal Justice Agency (CJA). Available at: <https://www.nycja.org/publications/re-arrest-among-16-year-olds-arrested-in-the-first-year-of-raise-the-age>.

³⁷ See Rempel, M. & Rodriguez, K. (2019), Op Cit. for a list of bail-eligible and ineligible charges under the original reforms.

³⁸ This design estimates the local average treatment effect of bail reform, i.e., the recidivism impact for cases that were affected by the policy change in practice. In other words, our analyses do not include the types of cases that were already routinely released pre-reform (e.g. Desk Appearance Tickets or cases charged with the most low-level misdemeanors), since the mandatory release provisions did not actually affect how they were treated post-reform. (See the technical supplement for more details.)

³⁹ An alternative way of addressing this research question would be an instrumental variable (IV) analysis, which is a powerful research design that allows for an estimate of the effect of judges' release decisions on recidivism. However, this approach is not feasible, primarily due to the relatively small sample size of less than 6,000 observations. While such a sample size does not preclude one from using IV analyses per se, only 15 out of the 166 judges in the sample have more than 100 arraignments (and only one judge has more than 200 arraignments), so we cannot calculate a precise measure of a judge's propensity to release individuals without bail (as was done in Leslie & Pope, 2017, Op Cit.). In addition, we also evaluated instrument strength by using estimates of model fit from the first-stage regression, which produced an F-statistic of 3.0, indicating that the instrument would be too weak to yield reliable results; see Staiger, D. & Stock, J.H. (1994). Instrumental variables regression with weak instruments.

⁴⁰ Lu, O. & Rempel, M. (2022), Op Cit.; Lu, O., et al. (2022), Op Cit. Additional research found that when New York City expanded its supervised release program to violent felonies in December 2019, one month before bail reform went into effect, judges immediately began ordering supervision significantly more and bail less, pointing to this change as the most likely driver of reduced bail in bail-eligible cases. For a description of the expanded supervised release program put into effect December 1, 2019, see NYC Criminal Justice. (2021). A Guide to Supervised Release in 2020. Available at: https://criminaljustice.cityofnewyork.us/wp-content/uploads/2020/11/SR-2020-Benchcard_Citywide_Non_COVID.pdf. For research pointing to the immediate change in release decisions in 2019, see Rempel, M. & Rodriguez, K. (2020), Op Cit.

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⁴² Rempel, M., Kerodal, A., Spadafore, J. & Mai, C. (2017). Jail in New York City: Evidence-Based Opportunities for Reform. New York, NY: Center for Court Innovation and Vera Institute of Justice. Available at: <https://www.innovatingjustice.org/publications/jail-new-york-city-evidence-based-opportunities-reform>.

⁴³ NYPD Clearance Report. Available at: <https://www.nyc.gov/site/nypd/stats/reports-analysis/clearance.page>.

⁴⁴ Division of Criminal Justice Services (DCJS). Dispositions of Adult Arrests (18 and Older) 2017-2021. Available at: <https://view.officeapps.live.com/op/view.aspx?src=https%3A%2F%2Fwww.criminaljustice.ny.gov%2Fcrimnet%2Fojasa%2Fdispos%2FNYC.xls&wdOrigin=BROWSELINK>.

⁴⁵ Rempel, M. & Weill, J. (2021), Op Cit. See the timeline on page 5.

⁴⁶ Certain charges became bail eligible again under the 2020 amendments only under certain complex conditions. Second-degree burglary cases were only affected when a person is charged with entering the “living area” of a home, which applies to about 63% of all such cases (see Rempel & Rodriguez, 2020, Op Cit.). About one in four third-degree assault cases involving domestic violence have additional obstruction of breathing or blood circulation charges that made them eligible for bail again. We do not have this additional information in our data, so to identify cases affected by the 2020 amendments to the best of our ability, we included in the sample random subsets of 63% of all second-degree burglary cases and 25% of all third-degree assault cases involving domestic violence.

⁴⁷ We refer to “recent” criminal history since we only tallied prior cases that started up to two years prior to individuals’ initial arraignment.

⁴⁸ The hazard ratio of 0.814, for example, indicates that the hazard of any re-arrest is 18.6% lower for people in the bail reform group compared to individuals in the comparison group, based on this formula: $1 - (1 \times 0.814) = 0.186$. By comparison, the hazard ratio of 0.906 of VFO re-arrest indicates that the hazard of VFO re-arrest is just 9.4% lower for people in the bail reform group (though this effect too is statistically significant).

⁴⁹ To enhance the readability of the information presented in the survival plots, we do not show the full scale of the y-axis (0%-100%). Instead, the lowest values on the y-axis are a round percentage number slightly below the smallest value represented by the lower edge of the confidence interval for the group with the steepest survival curve (e.g., 30% in Exhibit 3.3; 60% in Exhibit 3.4); see: Pocock, S.J., Clayton, T.C., & Altman, D.G. (2002). Survival Plots of Time-to-event Outcomes in Clinical Trials: Good Practice and Pitfalls. *The Lancet*, 359(9318), 1686-1689.

⁵⁰ Note that overlapping confidence intervals on the survival plots (e.g., as shown in Exhibit 3.5) are not inconsistent with statistically significant results. See Cornell Statistical Consulting Unit. (2020). Overlapping Confidence Intervals and Statistical Significance. Statnews #73.

⁵¹ We excluded from the analysis 165 comparison group individuals who were likely not at risk during the two-year follow-up period.

⁵² Lu., O. & Rempel, M. (2022), Op Cit.; Lu, O., et al. (2022), Op Cit.; Rempel, M. & Weill, J. (2021), Op Cit.

⁵³ In the pre-post analysis, we excluded 476 comparison group individuals who were likely not at risk of re-arrest during the two-year follow-up period; in the contemporaneous analysis, we excluded 86 comparison group individuals based on this criterion.

⁵⁴ We also conducted separate analyses for individuals charged with misdemeanors versus individuals charged with nonviolent felonies. The results are largely the same as for both groups combined, i.e., the re-arrest rates between the bail reform and the comparison groups were not significantly different. The only exception was a positive association between release without bail and VFO re-arrest for people charged with misdemeanors (data not shown). However, this finding may be a false positive, since we did not find such a relationship for people charged with nonviolent felonies: i.e., for individuals accused of a more severe crime.

⁵⁵ The results based on separate analyses for individuals charged with misdemeanors or nonviolent felonies (data not shown) are consistent with the results based on the two subgroups combined, i.e., people charged with misdemeanors and people charged with nonviolent felonies were less likely to be re-arrested for any offense, for a felony offense, or for a VFO. Unsurprisingly, however, the effect sizes are larger for the misdemeanor subgroup (where release without bail was associated with 25% lower overall re-arrest rates, 43% lower felony re-arrest rates, and 42% lower VFO re-arrest rates) compared to the nonviolent felony subgroup (where the bail reform group had 16% lower overall re-arrest rates, 24% lower felony re-arrest rates, and 39% lower VFO re-arrest rates than the comparison group).

⁵⁶ As mentioned earlier, we refer to “recent” criminal history since we only tallied prior cases that started up to two years prior to individuals’ initial arraignment.

⁵⁷ Rempel, M. & Weill, J. (2021), Op Cit. This study found that in the second half of 2020, 62.5% of cases that had bail set or were remanded due to the amendments fell under the provision concerning harm to a person or property, and another 22.5% fell under the burglary in the second-degree provision.

⁵⁸ See Rempel, M. & Rodriguez, K. (2020a), Op Cit. for a discussion of operationalizing harm-harm cases in OCA data that we also adopted in the current study. To summarize, we assumed that any Class A misdemeanor or felony involving any physical threat or harm (including any weapons possession charge) involved alleged harm to a person, defined to encompass these penal law articles: PL 120 (assault and related); PL 121 (strangulation and related); PL 125 (homicide and related); PL 130 (sex offenses); PL 135 (kidnapping, coercion, and related); PL 150 (arson); PL 240 (public order offenses); PL 241 (harassment of rent regulated tenants); PL 260 (child, disabled person, and elderly person offenses); PL 263 (sexual performance by children offenses); PL 265 (firearms and weapons offenses); and PL 270 (public safety offenses). We also added these individual charges: witness intimidation and tampering (PL 215.10-15), juror tampering (PL 215.23-25), obstructing governmental administration (PL 195.05, 07, and 08), obstructing first responders (PL 195.15 and 16), obstructing government by means of a bomb or related device (PL 195.17), and promoting or compelling prostitution (PL 230.20-33). Conversely, three loitering charges (PL 240.35, 36, and 37) were removed from the harm to person criteria. Finally, regardless of the charge, any case involving domestic violence was also defined as involving harm to a person.

We then assumed that every property charge in the penal law involved alleged harm to property, a definition encompassing all charges in these penal law articles: PL 140 (burglary and related); PL 145 (criminal mischief and related); PL 155 (larceny); PL 156 (offenses involving computers); PL 160 (robbery); and PL 165 (other theft offenses—but excluding theft of services, PL 165.15).

As noted in Rempel & Rodriguez (2020a), we are not endorsing our inclusive definitions of harm per se; based on consultation with stakeholders at the time the bail amendments passed, we simply believe our definition is an appropriate one to capture how far judges might believe their discretion extends.

⁵⁹ The violent felony statistics include second-degree burglary cases, as described in the previous endnote, as well as a small number of burglary cases that happen to fall under the harm-harm provision due to criminal history factors rather than due to the specifics of the burglary cases themselves. Hence, the small discrepancy between reported percentages of second-degree burglary charges and VFO charges.

⁶⁰ As in the previous chapters, this analysis excluded individuals who were likely never at risk in the two years following arraignment (N=34). The results for two-year felony, VFO, and firearm re-arrest were virtually identical, but in contrast to the primary analysis, the relationship between release status and overall re-arrest was not statistically significant after controlling for time incarcerated (data not shown).

⁶¹ The coding of domestic violence cases with a criminal obstruction of breathing or blood circulation charge replicates that in Rempel, M. & Rodriguez, K. (2020a), Op Cit.

⁶² See, e.g., Envision Freedom Fund (2022), Op Cit.; Lu, O. & Rempel, M. (2022), Op Cit.; New York City Comptroller Brad Lander (2021), Op Cit.; Rempel, M. & Weill, J. (2021), Op Cit.

⁶³ As mentioned earlier, these estimated effects indicate the local average treatment effects of bail reform, since there was a subset of cases that were practically unaffected by the mandatory release guidelines as they were already routinely release pre-reform.

⁶⁴ Langton, L., & Truman, J. (2014). Socio-emotional Impact of Violent Crime. Bureau of Justice Statistics; Weisburg, D., & Zastrow, T. (2021). Crime Hot Spots: A Study of New York City Streets in 2010, 2015, and 2020; Hartinger-Saunders, R. M., Rine, C. M., Nochajski, T., & Wieczorek, W. (2012). Neighborhood Crime and Perception of Safety as Predictors of Victimization and Offending Among Youth: A Call for Macro-Level Prevention and Intervention Models. *Children and Youth Services Review*, 34(9), 1966–1973; Garcia, R. M., Taylor, R. B., & Lawton, B. A. (2007). Impacts of Violent Crime and Neighborhood Structure on Trusting Your Neighbors. *Justice Quarterly*, 24(4), 679-704; McCollister, K. E., French, M. T., & Fang, H. (2010). The Cost of Crime to Society: New Crime-Specific Estimates for Policy and Program Evaluation. *Drug and Alcohol Dependence*, 108(1-2), 98-109; American Public Health Association. (2018, November 3). Violence is a Public Health Issue: Public Health is Essential to Understanding and Treating Violence in the U.S. <https://apha.org/policies-and-advocacy/public-health-policy-statements/policy-database/2019/01/28/violence-is-a-public-health-issue>.

⁶⁵ It should be noted that a recent bail reform study from Bernalillo County, NM suggests that this would be a detrimental policy; see Moore C., Ferguson, E., Guerin, P. (2022). How Accurate are Rebuttable Presumptions of Pretrial Dangerousness? A Natural Experiment from New Mexico. Available at SSRN: https://papers.ssrn.com/sol3/papers.cfm?abstract_id=4143886. However, in contrast to the current study, this research only examined pretrial recidivism. Furthermore, every jurisdiction is unique, so the findings and implications regarding the effects of new bail laws may vary by location.

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