
Examining the Impact of Eliminating Bail on Recidivism in the New York City Suburbs and Upstate Regions: A Difference-in-Differences Study

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Executive Summary

Effective January through June 2020, New York’s initial bail reform law eliminated the option to set bail for most misdemeanor and nonviolent felony charges. Beginning in July 2020, subsequent amendments reinstated bail eligibility for certain charges as well as for people meeting specific criminal history criteria. The present study examines the recidivism impact of the initial reforms in place during the first half of 2020.

Research Design and Methods

This study used a difference-in-differences (DiD) design to estimate the system-wide effect of New York’s initial bail reform on recidivism in the New York City suburbs and upstate regions. The study complements [parallel research](#) using controlled-interrupted time series methods to examine this reform’s impact on recidivism in New York City.

With court data spanning January 2019 to June 2020, re-arrest rates were compared among cases arraigned before and after the initial reforms went into effect and between charges seeing the elimination of bail versus charges remaining bail-eligible. While certain legal provisions affected pretrial decision-making for both bail-ineligible and bail-eligible cases, such as making all cases universally eligible for supervised release, eliminating the option to set bail for many charges represented the most consequential component of the initial bail reform law.

To correct for any biases potentially resulting from shifting crime trends and/or arrest practices in the early months of the COVID-19 pandemic, the analysis also adjusted for changes in individual-level background characteristics from before to after the reforms went into effect among both the bail-ineligible and bail-eligible groups, as well as adjusted for county and month fixed effects over the study period.

Analyses were broken out by: (1) **Follow-up period** (immediate pretrial period capped at six months vs. a two-year follow-up including both pretrial and post-disposition periods); (2) **Re-arrest type** (any, felony, or violent felony); and (3) **Population** (full population, “high-risk” group defined by the presence of a pending case, and “low-risk” group defined by having neither a prior conviction nor a pending case).

Key Results

During the immediate pretrial period, eliminating the option to set bail for most misdemeanors and nonviolent felonies had no overall impact on recidivism, but was associated with statistically significant increases in both any re-arrest and felony re-arrest among “high-risk” individuals. However, when cases were followed over a longer 2-year period, the reform trended toward lower recidivism across all measures, including a statistically significant reduction in felony re-arrest in the full population.

Study Findings in Context

The findings are broadly consistent with prior studies examining the effects of bail reform on recidivism across different regions of New York State using a range of research methods. Across studies, any increases in recidivism appear driven by individuals already at high risk of recidivism. This and a prior New York City study found that the system-wide effects of reform were concentrated in the pretrial period but tend to diminish or disappear over the long term.

Chapter 1. Introduction

In January 2020, New York put into effect a bail reform law that eliminated the option to set bail or detain people in most misdemeanor and nonviolent felony cases. Subsequent amendments implemented in July 2020 reinstated bail eligibility for certain charges as well as for people meeting specific criminal history criteria. The present study examines the recidivism impact of the initial reforms in place during the first half of 2020.

Discussed below, previous studies by the Data Collaborative for Justice examined bail reform's impact across different regions of the State using a range of methods. In this study, we used a difference-in-differences (DiD) approach to assess the reform's effect on recidivism in the New York City suburbs and upstate regions. The study complements parallel research using controlled-interrupted time series (CITS) methods to examine recidivism effects in New York City.¹

Specifically, we compared re-arrest rates among cases arraigned before and after the initial reforms went into effect and between charges seeing the elimination of bail (*treatment group*) versus charges remaining bail-eligible (*control group*). While certain legal provisions affected pretrial decision-making for both bail-ineligible and bail-eligible cases, such as making all cases universally eligible for supervised release and making many cases newly eligible for electronic monitoring, the complete elimination of bail and pretrial detention for many charges represented the most consequential component of the initial bail reform law.

The DiD design allowed us to account for changes in re-arrest rates over time that would have occurred absent the reform and to control for other potential observed and unobserved confounders, providing robust causal estimates of the effect of eliminating bail for certain cases. By controlling for changes in individual-level background characteristics as well as adjusting for county and month fixed effects over the study period, we sought to correct for any biases potentially resulting from shifting crime trends and/or arrest practices in the early months of the COVID-19 pandemic.

Analyses were broken out by: (1) **Follow-up period** (immediate pretrial period capped at six months vs. a two-year follow-up including both pretrial and post-disposition periods); (2) **Re-arrest type** (any, felony, or violent felony); and (3) **Population** (full population, “high-risk” group defined by the presence of a pending case, and “low-risk” group defined by having neither a prior conviction nor a pending case).

Background: New York's Bail Reform Law

In April 2019, New York passed the Bail Elimination Act, a reform law limiting judges' discretion to set bail or remand individuals charged with certain offenses. Prior to the law's enactment, judges had the ability to set bail in all cases. After it took effect in January 2020, the option to set bail was eliminated for nearly all misdemeanor and nonviolent felony cases.²

Besides eliminating bail for certain charges, the reform gave judges a universal option to set supervised release in any case; gave judges the option to set electronic monitoring for any felony and select misdemeanors; required judges to consider the affordability of bail when setting an amount; and introduced new legal standards, including a requirement that judges release a person on their own recognizance unless they pose a “risk of flight,” and, if such a risk exists, impose the “least restrictive” conditions necessary to ensure the person's return to court.³

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Several months after going into effect, New York amended the bail reform law in response to concerns over public safety. Put into effect on July 2, 2020, the amendments reinstated bail eligibility for certain charges (primarily misdemeanors and nonviolent felonies), and gave judges the discretion to consider certain aspects of a person's criminal history (e.g., bail was allowed if a person had both a current and pending charge involving “harm to an identifiable person or property”).⁴ Effectively, besides making significantly more cases bail-eligible,⁵ these rollbacks ended New York's initial experiment with an exclusively charge-based approach to bail eligibility.

Consistent with most prior research on New York's bail reform, the present study focuses on the impact of the *initial* reform (January to June 2020). Given the substantial changes introduced by the July 2020 amendments, the Data Collaborative for Justice has separately examined the impact of the post-amendment bail regime (see last study in the list below).

Prior Research on the Impact of New York's Bail Reform Law

Past research on the impact of New York's bail reform on crime and recidivism has been mixed. Below we summarize the study designs and key findings from this literature.

- **Evaluation of Recidivism in New York City:** In the first study of the Data Collaborative for Justice's *Bail Reform & Recidivism Series*, inverse probability of treatment weighting (IPTW) was used to estimate the effect of bail reform on 2-year recidivism rates in New York City. The research design isolated people who were likely to have been impacted by reform and estimated the effect of pretrial release among: (1) cases made ineligible for money bail and pretrial detention; and (2) cases that remained eligible for bail but where judges became more likely to opt for release due to provisions such as the universal availability of pretrial supervision. Results showed a decrease in recidivism for bail-ineligible cases released under reform but no effects in either direction for bail-eligible cases. Subgroup analyses found that recidivism tended to increase for people with a recent violent felony arrest and, in some analyses, a current open case. In contrast, recidivism decreased for people with no recent criminal history and people with no prior violent felony arrests.⁶
- **Evaluation of Select Crime Incidents in New York City:** Researchers at the University of Southern California, Cornell University, and the NYC Criminal Justice Agency used a synthetic control design to estimate the effect of reform in New York City on multiple incident-level crime types, including assault, theft, robbery, burglary, and drug crime. Findings showed no significant increases in New York City compared to the synthetically matched cities for all crime types except robbery.⁷
- **Evaluation of Index Crime Incidents in New York State:** Researchers at the University of Albany used a synthetic control design to estimate the effect of reform throughout New York State on index crime. Findings showed that while murder, larceny and motor vehicle theft increased post-reform, the increases were not significantly different from those in the synthetically matched states.⁸
- **Evaluation of Recidivism in New York City:** In the second study of the Data Collaborative for Justice's *Bail Reform & Recidivism Series*, controlled-interrupted time series models were used to estimate the impact of New York's initial bail reform law on pretrial recidivism

CHAPTER 1. INTRODUCTION

(capped at 6 months) and two-year recidivism rates. Findings showed that in the overall population the reform had no impact on recidivism (measured as any re-arrest, any felony re-arrest, or any violent felony re-arrest). However, among a “high-risk” population of individuals who had an open case at the time of prosecution, the reform was associated with an increase in violent felony re-arrest.⁹

- **Evaluation of Recidivism in the New York City Suburbs and Upstate Regions:** In the third study of the *Bail Reform & Recidivism Series*, researchers used inverse probability of treatment weighting (IPTW) to estimate the impact of bail reform in New York’s suburban and upstate regions. The study compared recidivism outcomes before and after the reform for people who would likely have been held on bail or detained pre-reform but were released under the new law. Results showed little overall change in re-arrest. However, subgroup analyses revealed that recidivism increased among people charged with nonviolent felonies and those with recent prior arrests, while it decreased among people charged with misdemeanors and those with no recent prior record. The starkest and most consistent increases in recidivism were seen among people with recent violent felony arrests and those charged with violent felonies who had a prior record.¹⁰
- **Long-Term Recidivism Impact of New York’s Bail Reform Law:** Released in 2025 at nearly the same time as the current study, the fourth report of the *Bail Reform & Recidivism Series* examined the impact of the bail law on recidivism across all regions of the State after the first and most substantial round of amendments took effect in July 2020. Besides representing the first study to examine the recidivism impact of New York’s reform as it currently operates (subsequent amendments were relatively minor and did not significantly alter the use of bail and pretrial detention), this study also tracked recidivism over a lengthy follow-up period of 50 months. Researchers used IPTW to estimate (1) the effects of eliminating bail and (2) the reduced use of bail in legally eligible cases among people who were likely released without bail due to reform. Findings were largely consistent with similar evaluations of “initial” bail reform: the prohibition of money bail for select cases substantially reduced recidivism in New York City, with no changes in the rest of the state; the reduced use of bail in cases for which it was still permitted was not associated with changes in recidivism. Subgroup analyses showed significant recidivism reductions for “low-risk” subgroups (people with no recent criminal history and no recent prior violent felony arrest) and notable recidivism increases for “high-risk” subgroups (people with any recent criminal history and, most especially, recent prior violent felony arrests).¹¹

This Report’s Original Contribution

Prior research has evaluated the effect of New York’s bail law on crime incidents,¹² recidivism among matched samples of impacted individuals,¹³ and system-wide recidivism in *New York City*.¹⁴ In this study, we used a difference-in-differences approach to examine the impact of eliminating bail for certain charges on recidivism in the New York City suburban and upstate regions. Specifically, within these regions we addressed the question: *Did eliminating the option to set bail for certain charges cause a change in an individual’s likelihood of recidivism?*

Chapter 2. Data, Definitions, and Methods

Treatment vs. Control Groups

As discussed in Chapter 1, this study does not comprehensively evaluate all aspects of New York’s initial bail reform law. Rather, we study the single most impactful provision: eliminating the option to set bail at arraignment for most misdemeanor and nonviolent felony charges.¹⁵ Accordingly, cases were divided into “treatment” and “control” groups based on whether a person’s charges qualified for bail under the initial 2020 bail reform law in effect from January to June 2020 as follows:

- **Treatment cases:** individuals charged with an offense that became ineligible for bail under the reform.
- **Control cases:** individuals charged with an offense that remained eligible for under the reform.

Pretrial Recidivism (Capped at 6 Months)

For the analysis of pretrial recidivism, we used publicly available pretrial data collected jointly by New York State’s Division of Criminal Justice Services (DCJS) and Office of Court Administration (OCA) for the purposes of studying the impact of bail reform on pretrial decision-making and pretrial recidivism (the “DCJS Pre and Post Bail Data”).¹⁶ The data include case information on all prosecuted arrests for fingerprintable offenses (to allow for accurate tracking of re-arrests) of people 18 or older from 2019 to 2022.¹⁷ Each observation represents a unique criminal case (“cycle”) following an arrest, which may include multiple offenses.

The DCJS/OCA dataset includes several pretrial recidivism measures. To limit bias due to variability in pretrial length, we used recidivism measures which capture whether an individual was re-arrested within 6 months after arraignment or, if the case was resolved in less than 6 months, at any time before the case disposition (yes/no). These measures were broken out by charge severity: **any re-arrest**, **felony re-arrest**, and **violent felony re-arrest**.

Two-Year Recidivism

A growing body of research suggests that pretrial detention may have criminogenic effects that extend into the initial post-release period. To account for these potential long-term impacts, we examined recidivism outcomes beyond just the pretrial window. Using data provided by the Office of Court Administration (OCA), we constructed recidivism measures with a follow-up period of two years after arraignment, regardless of whether a case had been resolved before that time. These measures capture whether an individual was re-arrested during the follow-up (yes/no), disaggregated by the severity of the new charge: **any re-arrest**, **felony re-arrest**, and **violent felony re-arrest**.

Like the pretrial dataset described above, each observation represents a unique criminal case following an arrest, restricted to cases against adults (18 or older) for a fingerprintable offense.

Note: Due to data limitations, only re-arrests that resulted in prosecution are captured by these measures (i.e., re-arrests that prosecutors declined to pursue are not counted).

CHAPTER 2. DATA, DEFINITIONS, AND METHODS

High-Risk Subgroup (Pending Case)

As described above, two earlier reports in the Data Collaborative for Justice's *Bail Reform & Recidivism Series* found that eliminating bail led to an increase in recidivism among a relatively small "high-risk" subgroup, with the primary risk factors being a pending case or a prior violent felony arrest in the two years before the current case.

In the present study, we defined "high-risk" as people with at least one pending case at the time of arraignment. Among bail-ineligible cases, 30.0% of cases were defined as "high-risk." In the year prior to the reform's implementation, these individuals were significantly more likely to have bail or remand set compared to the general population (53.9% vs. 37.8%). Thus, this analysis captures the effect of the reform on a subgroup especially likely to have had bail or remand set absent reform.

Low-Risk Subgroup (No Prior Conviction or Pending Case)

Additionally, we assessed the impact of the reform among a subgroup of "low-risk" individuals, defined as having neither a prior conviction or current pending case. Of those charged with a bail-ineligible offense, 33.8% of individuals fell within the "low-risk" subgroup. In the year preceding the reform, 18.3% of these individuals had bail set or were remanded.

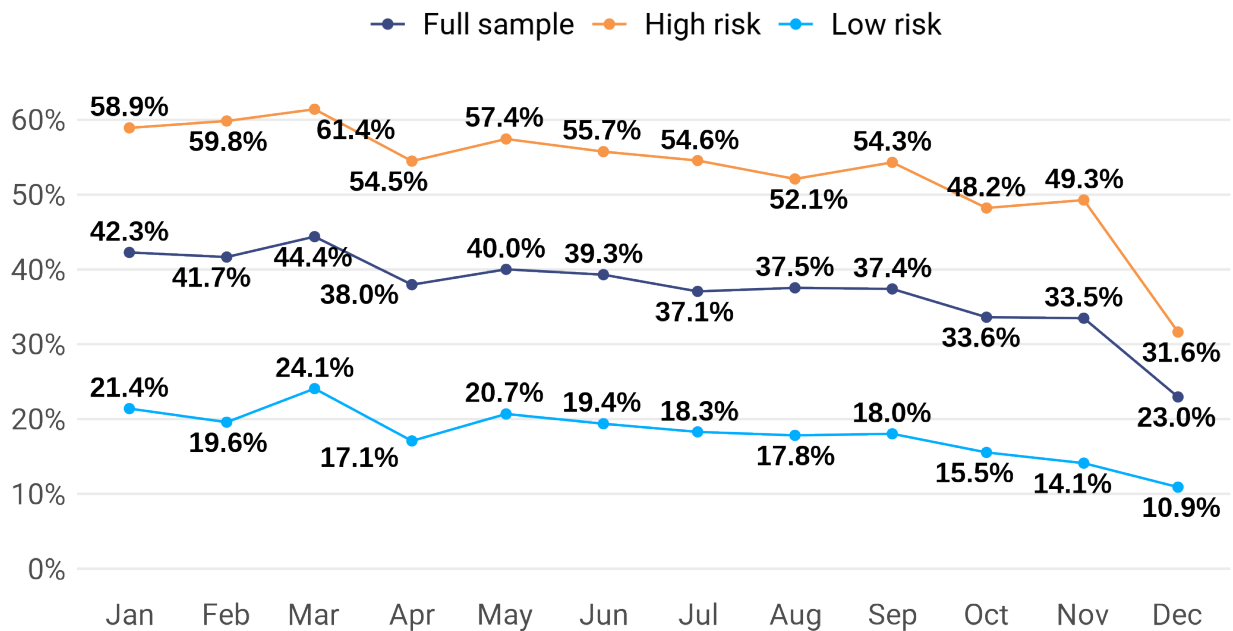
Intervention Timing

In April 2019, the Bail Elimination Act was signed into law with an effective date of January 1, 2020. The new law's bail eligibility requirements were retroactive, meaning that people who were already held in detention for failure to post bail on an ineligible offense were subject to immediate release on the implementation date.

Partly to avoid a "rush on the courts," the Office of Court Administration encouraged judges to align their decisions with reform prior to the official rollout date.¹⁸ **Figure 2.1** shows monthly bail-setting trends in 2019 among people facing charges that became ineligible for bail the following year (2020). A substantial decrease in bail setting was observed starting in December 2019 in the general population, as well as in the high- and low-risk subgroups. Accordingly, we used December 2019 as the interruption date for all analyses.

Figure 2.1.

**Percentage with Bail-Set or Remanded at Arraignment
Among Those Charged with a Bail-Ineligible Offense (2019)**



Difference-in-Differences Design

To estimate the impact of New York’s initial bail reform on an individual’s likelihood of recidivism, we employed a difference-in-differences (DiD) design. This approach is appropriate when observations can be sorted into clearly defined treatment and control groups, and outcomes are available before and after a policy change. In this setting, cases were classified depending on whether a person’s charge in a case became bail-ineligible (treated) or remained bail-eligible (control) after reform. By comparing pre- and post-reform outcomes between the bail-ineligible and bail-eligible groups, this design allowed us to net out changes in recidivism that would have occurred in the absence of the reform, isolating its causal effect. For example, if the pre-post difference among the bail-eligible group was significantly greater than the pre-post difference among the bail-ineligible group, this *difference-in-differences* would represent the reform’s causal effect.

Within this DiD framework, we controlled for a range of individual demographic and criminal history characteristics previously shown to be associated with recidivism, including: **gender** (male/female), **race/ethnicity** (Black, Hispanic, White, other), **age** (<25, 25–44, 45+), the presence of a **pending case** at arraignment, and the **number of prior misdemeanor convictions** (0, 1, 2–4, 5+).¹⁹ To address unobserved confounding, we also included **county fixed effects** to absorb time-invariant jurisdictional-level differences (e.g., prosecutorial) and **month fixed effects** to capture common temporal shocks, such as systemwide disruptions during the early months of the COVID-19 pandemic (April–June 2020). Following Abadie et al. (2023), **standard errors** were clustered at the level of treatment assignment: the type of offense charged in a case.²⁰

CHAPTER 2. DATA, DEFINITIONS, AND METHODS

Importantly, our models do not control for the time individuals spent in pretrial detention. *This was a deliberate design choice rather than a limitation, as the purpose of the analysis was to capture the overall effect of the reform on recidivism—including any changes due to reduced incapacitation among people who would have otherwise had bail set and potentially been detained absent reform.*

We estimated the following linear probability model:

$$Y_{it} = \beta^0 + \beta^1 \cdot Post_t + \beta^2 \cdot Treated_i + \beta^3 \cdot Post_t \times Treated_i + X'_i \cdot \gamma + \mu_t + \delta_j + \varepsilon_{it}$$

Where:

- Y_{it} equals 1 if individual i arraigned in month t was re-arrested with the follow-up period;
- $Post_t$ is a binary indicator for observations after the reform's implementation;
- $Treated_i$ is an indicator for whether the individual's charge was made ineligible for bail under the reform;
- $Post_t \times Treated_i$ captures the effect of the reform;
- X'_i is a vector of the individual-level covariates described above;
- μ_t are month fixed-effects;
- δ_j are county fixed-effects;
- ε_{it} is the error term, clustered at the charge level.

Parallel Trends Assumption

A key assumption of the DiD design is that, absent reform, outcomes in the treatment and control groups would have followed parallel trends. If the parallel trends assumption (PTA) holds, the control group can then serve as a credible counterfactual for the treated group in the post-reform period. While the assumption cannot be tested after implementation of a reform, it can be assessed with pre-reform data.

To assess the PTA and examine post-reform dynamics, we estimated event study models with the above-described DiD framework. These models included leads and lags of the treatment indicator relative to the month immediately preceding the reform, yielding month-specific treatment effect estimates compared to that baseline. The coefficients on the pre-reform leads provided a diagnostic check of the PTA, while the coefficient on the post-reform lags allowed us to evaluate whether any effects were concentrated during periods of systemwide disruption during the COVID-19 pandemic (April-June).

Difference-in-Differences versus Controlled-Interrupted Time Series Methods

Difference-in-differences (DiD) and controlled-interrupted time series (CITS) methods are similar in that both compare outcomes before and after a reform between treatment and control groups. In a previous analysis of New York’s initial bail reform, we employed a CITS design using linear regression models to estimate the law’s impact on monthly aggregate recidivism rates in New York City.²¹

Preliminary analyses aimed at replicating that study for regions outside New York City revealed sharply declining monthly case volumes following the onset of the COVID-19 pandemic—with substantially larger decreases than those seen in New York City. Because these months (April to June 2020) overlapped with the final months of the initial bail reform, applying the same CITS design posed a substantial risk of confounding due to COVID-19.

Switching to a DiD approach offered a number of advantages in addressing this concern. First, the use of individual-level data allowed us to account for potential shifts in the composition of prosecuted individuals during the COVID period. Second, county and month fixed effects helped adjust for unobserved sources of confounding. Third, modeling outcomes at the individual level prevented outcomes from months with low case volumes from being overweighted. Finally, whereas CITS models assume a linear functional form for pre-reform trends, the DiD approach relies on the weaker parallel trends assumption.

Chapter 3. The Impact of Bail Elimination on Pretrial Recidivism

Results are shown below for recidivism during the immediate pretrial period (up to 6 months or disposition, whichever came first), first for the full population and then for high- and low-risk subgroups.

Findings are presented as follows: (1) descriptive comparisons of re-arrest rates before and after reform across the treatment and control groups; (2) our main adjusted DiD model estimates; and (3) event study plots to assess pre- and post-reform dynamics.

Pretrial Recidivism Results (Tracking Capped at 6 Months)

Table 3.1 shows unadjusted (“raw”) re-arrest rates for the treatment and control groups across the pre- and post-reform periods.

In the **bail-ineligible** group (treatment), re-arrest rates rose post-reform by 6.2 percentage points for any re-arrest, 5.0 percentage points for felony re-arrest, and 2.0 percentage points for violent felony (VFO) re-arrest. In the **bail-eligible** group (control), rates rose by 3.7 percentage points for any re-arrest, 3.1 percentage points for felony re-arrest, and 2.1 percentage points for VFO re-arrest.

Table 3.1
Pretrial Recidivism (Full Population)

	Treatment (Bail Ineligible)			Control (Bail Eligible)		
	Pre-Reform	Post-Reform	Diff.	Pre-Reform	Post-Reform	Diff.
Any Re-Arrest	16.3%	22.5%	+6.2	19.7%	23.4%	+3.7
Felony Re-Arrest	6.7%	11.7%	+5.0	9.4%	12.5%	+3.1
VFO Re-Arrest	1.6%	3.6%	+2.0	2.2%	4.3%	+2.1

Using a DiD framework, we estimated the *difference* in pre–post changes between the treatment and control groups, while also adjusting for individual demographic and criminal history characteristics, as well as county and month fixed effects. Results from the DiD models are shown below in **Table 3.2**, with monthly event study estimates plotted in **Figure 3.1**.

For the overall population, the reform had no statistically significant effect on any of the three recidivism measures (any re-arrest, felony re-arrest, violent felony re-arrest), though the estimates trended upward for any re-arrest and felony re-arrest.

For the overall population, the reform had no statistically significant effect on any of the three recidivism measures (any re-arrest, felony re-arrest, violent felony re-arrest), though the estimates trended upward for any re-arrest and felony re-arrest.

Table 3.2

Difference-in-Differences Models: Pretrial Recidivism in the Full Population

	Estimated Effect	Standard Error	p-value
Any Re-Arrest	+1.8	2.5	0.462
Felony Re-Arrest	+1.1	1.1	0.330
VFO Re-Arrest	-0.1	0.4	0.766

How to Read the Findings from the DiD Tables

Our main coefficient of interest (β^3) represents the difference-in-differences estimate. Below are definitions of terms used to describe the coefficient (i.e., the table *columns* in **Table 3.2** and all similar tables below).

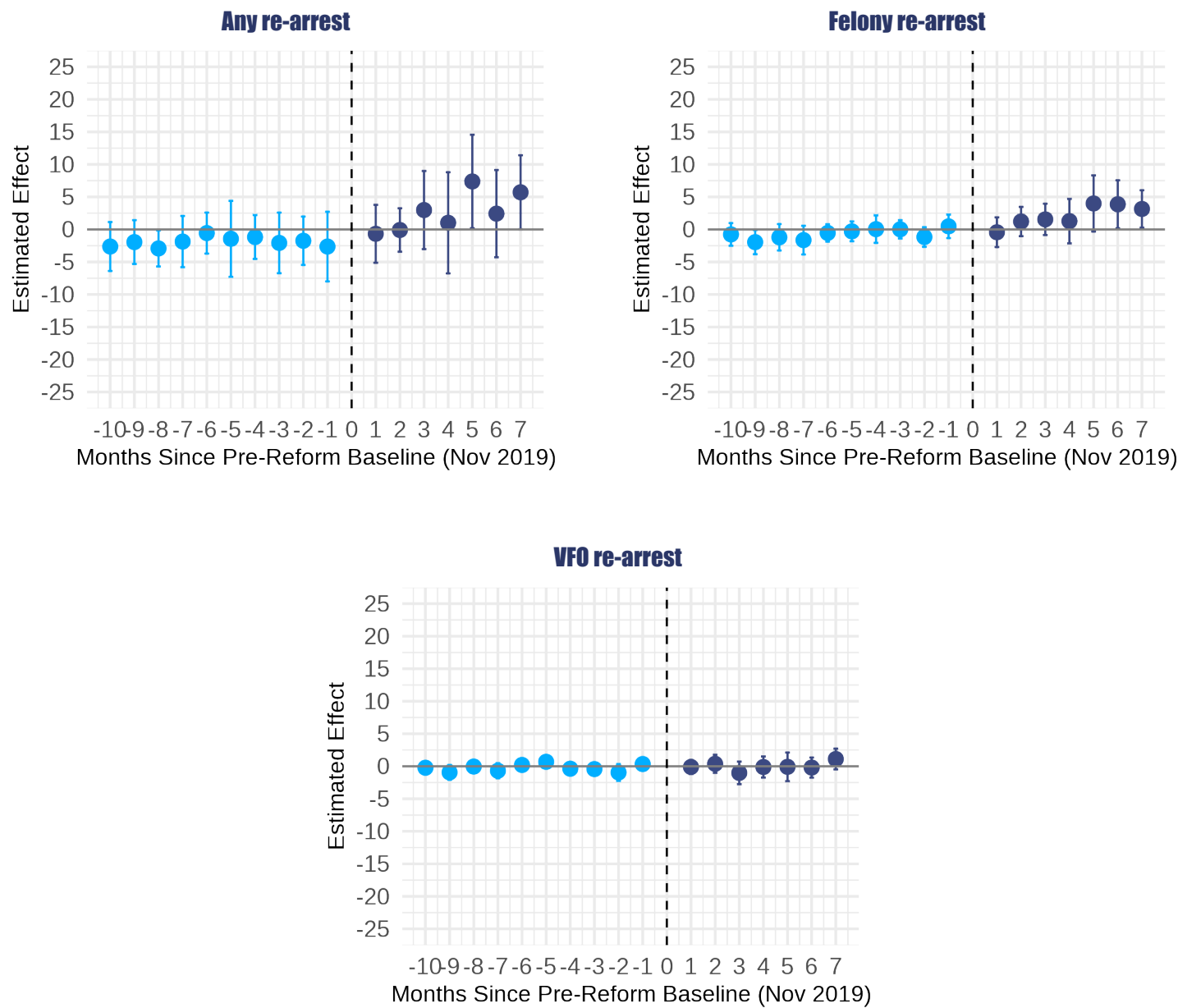
- **Estimated effect:** The magnitude and direction of the post-reform difference in treatment and control groups expressed in percentage points (e.g., +5.0 indicates the reform increased an individual's likelihood of re-arrest by 5 percentage points).
- **Standard error:** The uncertainty around the estimate expressed in percentage points. Smaller values indicate more precise estimates, while larger values indicate greater uncertainty.
- **p-value:** The probability of seeing a particular percentage-point difference by chance. A very low p-value (typically .05 or less) suggests that the difference was very unlikely to be due to chance and is therefore considered “*statistically significant*.”

Event study estimates for any re-arrest, felony re-arrest, and violent felony re-arrest indicate that the treatment and control groups had parallel pre-reform trends (**Figure 3.1**), with confidence intervals overlapping or close to zero.

CHAPTER 3. THE IMPACT OF BAIL ELIMINATION ON PRETRIAL RECIDIVISM

Figure 3.1

Event Study: Pretrial Recidivism Among the Full Population



How to Interpret the Event Study Plots

The event study coefficients in **Figure 3.1** and all similar figures below represent the estimated treatment effect for each month relative to the baseline month (i.e., the month immediately prior to the intervention). In the figures:

- **Light blue and dark blue circles:** represent the estimated effect for a given month before (light blue) and after (dark blue) the reform relative to the baseline month. A positive value indicates an increase in the outcome following treatment, while a negative value indicates a decrease.
- **Vertical lines above and below the circles:** represent the confidence interval for the estimate. Narrow intervals imply greater precision, while wider intervals imply more uncertainty. Confidence intervals that exclude zero indicate a statistically significant effect.
- **Months since pre-reform baseline:** Shown on the x-axis, these indicate the number of months since the baseline month (November 2019).

High-Risk Subgroup (Pending Case)

Among the high-risk subgroup, recidivism rates rose post-reform more sharply in the treatment group than in the control group, particularly for any re-arrest (+14.2 percentage points vs. +5.7 percentage points) and felony re-arrest (+11.9 percentage points vs. +5.1 percentage points) (**Table 3.3**).

Using DiD models, we found two statistically significant effects: a 6.6 percentage-point increase in any re-arrest ($p < .05$) and a 5.0 percentage points increase in felony re-arrest ($p < .001$) (**Table 3.4**). The event studies showed that treatment and control groups had roughly parallel pre-reform trends across all measures, with confidence intervals overlapping or close to zero (**Figure 3.2**). *Importantly, post-reform monthly coefficients reached statistical significance even prior to the onset of the COVID-19 pandemic (in February for any re-arrest and January for felony re-arrest), suggesting that these effects were not entirely driven by pandemic-related changes.*

Table 3.3

Average Pretrial Recidivism (High-Risk Subgroup)

	Treatment (Bail Ineligible)			Control (Bail Eligible)		
	Pre-Reform	Post-Reform	Diff.	Pre-Reform	Post-Reform	Diff.
Any Re-Arrest	25.9%	40.1%	+14.2	27.7%	33.4%	+5.7
Felony Re-Arrest	11.0%	22.9%	+11.9	13.2%	18.3%	+5.1
VFO Re-Arrest	2.5%	7.2%	+4.7	3.3%	6.4%	+3.1

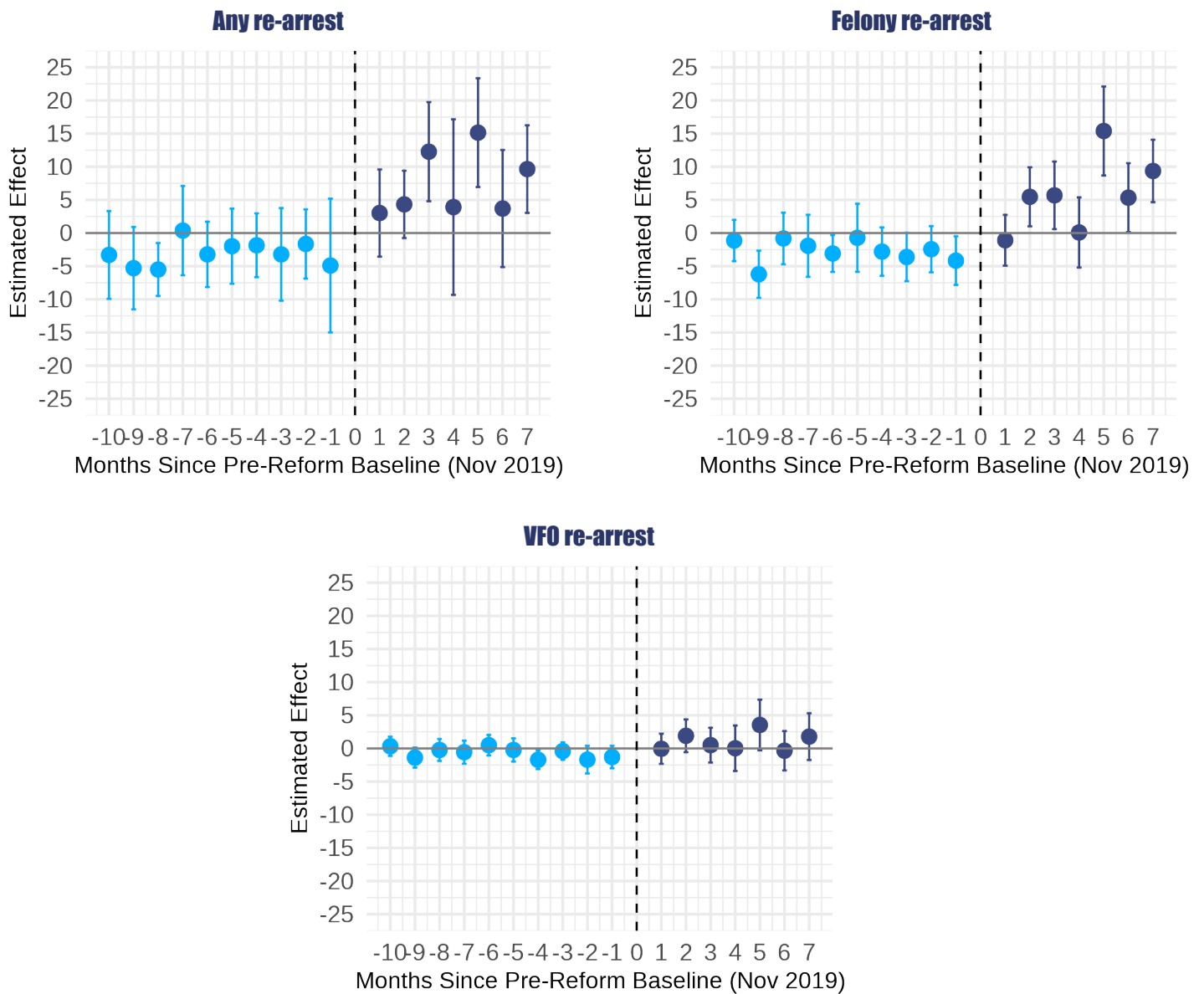
Table 3.4

**Difference-in-Differences Models:
Pretrial Recidivism in the High-Risk Subgroup**

	Estimated Effect	Standard Error	p-value
Any Re-Arrest	+6.6	2.8	0.017
Felony Re-Arrest	+5.0	1.4	0.000
VFO Re-Arrest	+0.9	0.8	0.272

Figure 3.2

Event Study: Pretrial Recidivism Among the High-Risk Population



CHAPTER 3. THE IMPACT OF BAIL ELIMINATION ON PRETRIAL RECIDIVISM

Low-Risk Subgroup (No Prior Conviction or Pending Case)

Among the low-risk subgroup, recidivism rates increased post-reform slightly for both the treatment and control groups (Table 3.5). The DiD estimates suggested modest reductions in recidivism across all measures, though none reached statistical significance (Table 3.6). Event study results indicated that the treatment and control groups had roughly parallel pre-reform trends across all measures, with confidence intervals overlapping or close to zero (Figure 3.3).

Table 3.5
Average Pretrial Recidivism (Low-Risk Subgroup)

	Treatment (Bail Ineligible)			Control (Bail Eligible)		
	Pre-Reform	Post-Reform	Diff.	Pre-Reform	Post-Reform	Diff.
Any Re-Arrest	7.9%	8.6%	+0.7	12.4%	11.2%	-1.2
Felony Re-Arrest	2.6%	3.6%	+1.0	4.6%	5.2%	+0.6
VFO Re-Arrest	0.7%	1.0%	+0.3	1.1%	1.8%	+0.7

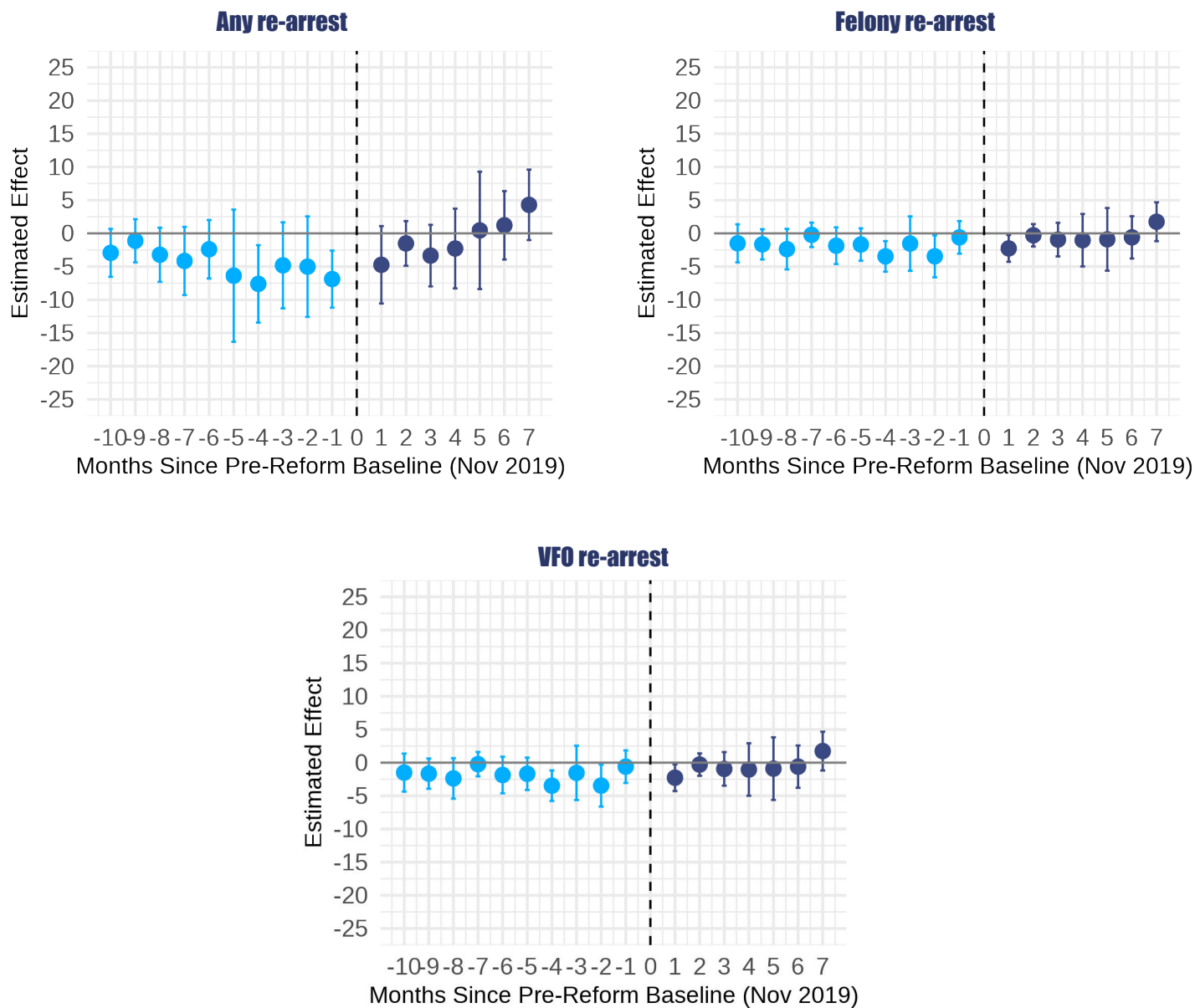
Table 3.6
Difference-in-Differences Models:
Pretrial Recidivism in the Low-Risk Subgroup

	Estimated Effect	Standard Error	p-value
Any Re-Arrest	-1.7	1.8	0.344
Felony Re-Arrest	-0.9	0.7	0.195
VFO Re-Arrest	-0.5	0.4	0.220

CHAPTER 3. ESTIMATED IMPACT OF ELIMINATING BAIL FOR SELECT CASES

Figure 3.3

Event Study: Pretrial Recidivism Among the Low-Risk Population



Chapter 4. The Impact of Bail Elimination on Long-Term Recidivism

This chapter replicates the analyses described above, extending the recidivism follow-up window to two years after arraignment, regardless of whether the case was resolved within that time. Note that the two-year measures were defined somewhat differently: only re-arrests that resulted in prosecution were counted (due to a limitation of the alternative non-public data source).

Two-Year Recidivism Results

When tracked for a full two-years, recidivism rates in the full population increased slightly in both the treatment and control groups (ranging from +1.4 to +3.2 percentage points) (**Table 4.1**).

Using DiD models to compare these post-reform changes, we found that eliminating the option to set bail was associated with a statistically significant decrease in felony recidivism (–2.7 percentage points; $p < .05$) (**Table 4.2**). Although reform was associated with decreases in any re-arrest and violent felony re-arrest, these changes were not statistically significant.

Table 4.1
Average Two-Year Recidivism
(Full Population)

	Treatment (Bail Ineligible)			Control (Bail Eligible)		
	Pre-Reform	Post-Reform	Diff.	Pre-Reform	Post-Reform	Diff.
Any Re-Arrest	39.1%	40.5%	+1.4	46.4%	49.5%	+3.2
Felony Re-Arrest	21.9%	24.8%	+2.9	30.6%	33.7%	+3.1
VFO Re-Arrest	7.4%	9.3%	+1.9	10.7%	12.2%	+1.5

Table 4.2
Difference-in-Differences Models:
Two-Year Recidivism in the Full Population

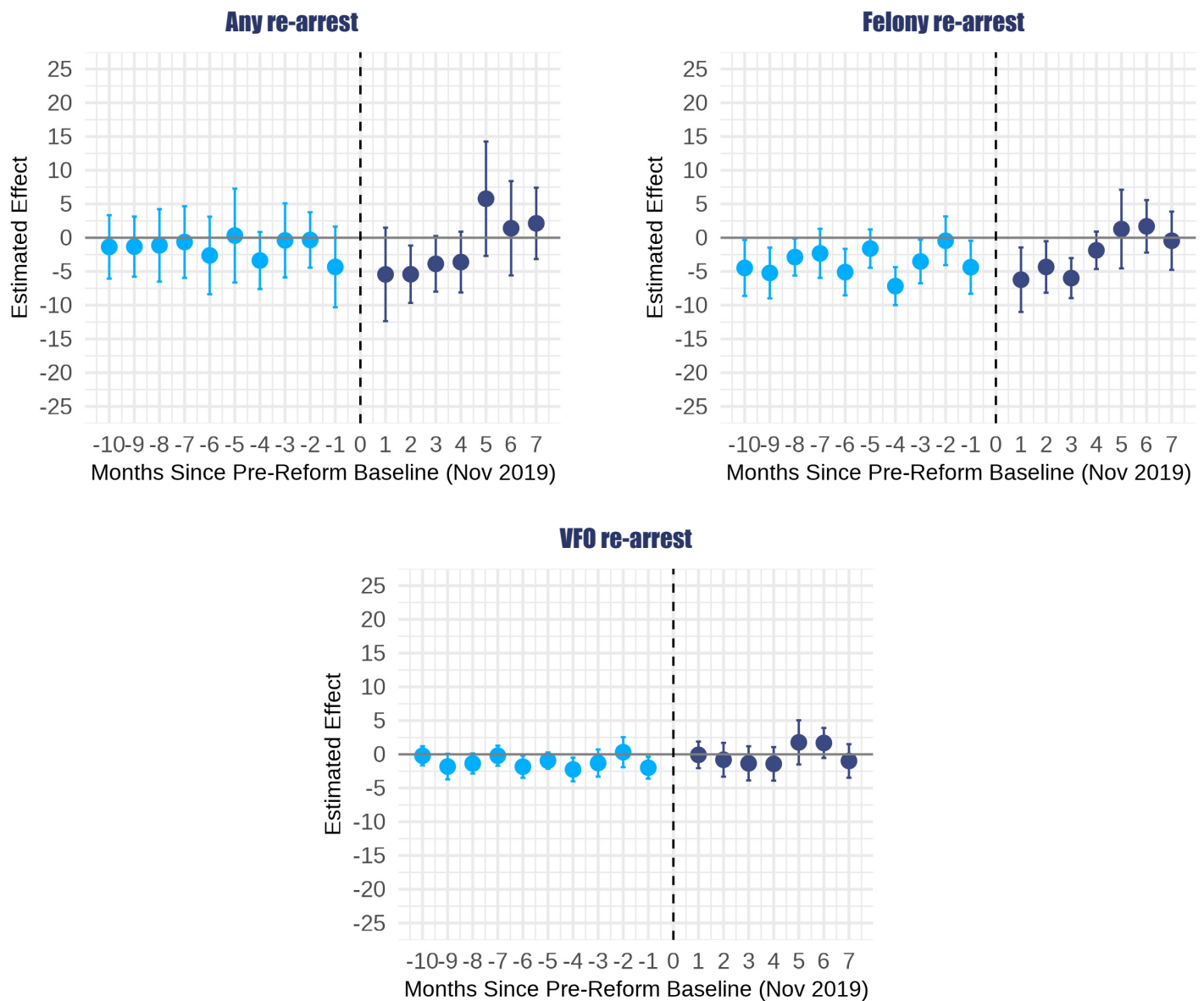
	Estimated Effect	Standard Error	p -value
Any Re-Arrest	-2.0	2.3	0.388
Felony Re-Arrest	-2.7	1.3	0.039
VFO Re-Arrest	-0.3	0.7	0.662

CHAPTER 4. THE IMPACT OF BAIL ELIMINATION ON LONG-TERM RECIDIVISM

Event study estimates for any re-arrest and violent felony re-arrest indicate that the treatment and control groups had parallel pre-reform trends (**Figure 4.1**), with confidence intervals overlapping or close to zero. For felony re-arrest, however, several pre-reform estimates indicated substantially lower recidivism rates in the treatment group, raising uncertainty about whether the observed post-reform decline can be attributed to reform. *In general, the pattern of larger effects during the early months of COVID-19 suggests that pandemic-related disruptions may have upwardly biased the results—potentially dampening any long-run benefits of mandatory pretrial release.*

Figure 4.1

Event Study: Two-Year Recidivism Among the Full Population



CHAPTER 4. THE IMPACT OF BAIL ELIMINATION ON LONG-TERM RECIDIVISM

High-Risk Subgroup (Pending Case)

In the “high-risk” subgroup, recidivism increased post-reform across all measures, with substantially larger increases among bail-ineligible (treatment) cases for felony re-arrest (+7.3 percentage points vs. +4.5 percentage points) and violent felony re-arrest (+4.2 percentage points vs. +1.7 percentage points) (**Table 4.3**). However, the DiD models indicate that these changes were not statistically significant (**Table 4.4**).

The event studies showed the treatment and control groups had roughly parallel pre-reform trends for any re-arrest and violent felony re-arrest, with confidence intervals overlapping or close to zero (**Figure 4.2**). By contrast, several of the pre-reform coefficients for felony re-arrest were statistically significant, raising concerns about the credibility of the control group as a counterfactual.

Table 4.3
Average Two-Year Recidivism
(High-Risk Population)

	Treatment (Bail Ineligible)			Control (Bail Eligible)		
	Pre-Reform	Post-Reform	Diff.	Pre-Reform	Post-Reform	Diff.
Any Re-Arrest	60.7%	64.5%	+3.8	61.0%	65.6%	+4.5
Felony Re-Arrest	36.8%	44.1%	+7.3	42.1%	46.6%	+4.5
VFO Re-Arrest	12.5%	16.8%	+4.2	14.7%	16.5%	+1.7

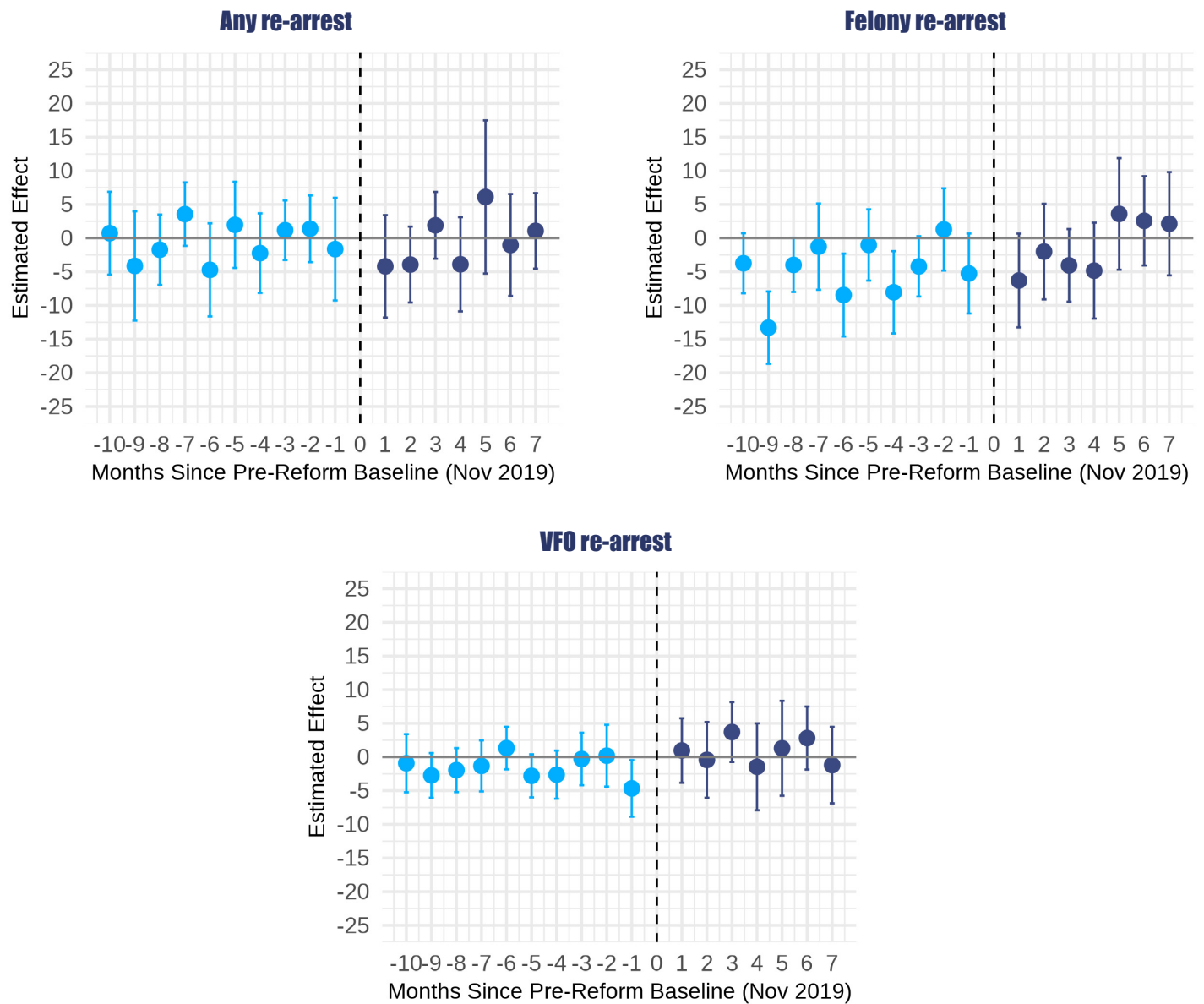
Table 4.4
Difference-in-Differences Models:
Two-Year Recidivism in the High-Risk Population

	Estimated Effect	Standard Error	p-value
Any Re-Arrest	-1.1	1.7	0.525
Felony Re-Arrest	-1.9	1.7	0.279
VFO Re-Arrest	+0.9	1.4	0.511

CHAPTER 4. THE IMPACT OF BAIL ELIMINATION ON LONG-TERM RECIDIVISM

Figure 4.2.

Event Study: Two-Year Recidivism Among the High-Risk Population



CHAPTER 4. THE IMPACT OF BAIL ELIMINATION ON LONG-TERM RECIDIVISM

Low-Risk Subgroup (No Prior Conviction or Pending Case)

Among the low-risk subgroup, recidivism rates slightly increased post-reform for both the treatment and control groups (**Table 4.5**).

The DiD estimates suggest modest reductions in recidivism across all measures, though none reached statistical significance (**Table 4.6**). Event study results indicate that the treatment and control groups had roughly parallel pre-reform trends across all measures, with confidence intervals overlapping or close to zero (**Figure 4.3**).

Table 4.5
Average Two-Year Recidivism
(Low-Risk Population)

	Treatment (Bail Ineligible)			Control (Bail Eligible)		
	Pre-Reform	Post-Reform	Diff.	Pre-Reform	Post-Reform	Diff.
Any Re-Arrest	28.3%	29.0%	+0.8	32.7%	34.2%	+1.4
Felony Re-Arrest	14.2%	16.0%	+1.8	18.7%	20.1%	+1.4
VFO Re-Arrest	4.8%	6.1%	+1.3	6.9%	8.3%	+1.4

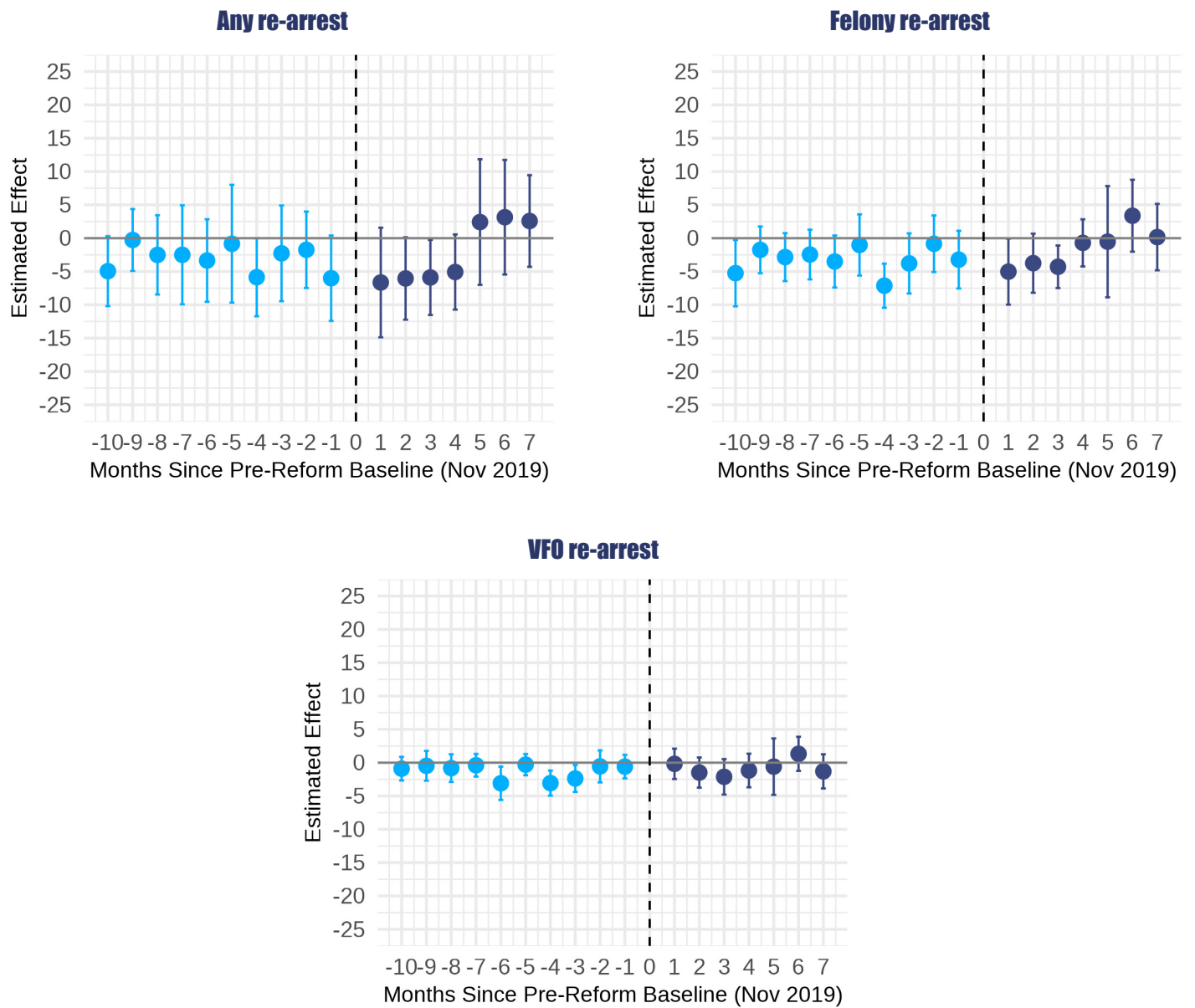
Table 4.6
Difference-in-Differences Models:
Two-Year Recidivism in the Low-Risk Population

	Estimated Effect	Standard Error	<i>p</i> -value
Any Re-Arrest	-2.8	2.9	0.343
Felony Re-Arrest	-1.8	1.5	0.243
VFO Re-Arrest	-0.8	0.7	0.234

CHAPTER 4. THE IMPACT OF BAIL ELIMINATION ON LONG-TERM RECIDIVISM

Figure 4.3

Event Study: Two-Year Recidivism Among the Low-Risk Population



Chapter 5. Conclusion & Limitations

This study used a difference-in-differences design to assess the impact of New York’s initial bail reform law on recidivism in the New York City suburban and upstate regions. Effective from January to June 2020, the initial reform eliminated judges’ option to set bail for certain charges, with bail-eligibility eliminated for most misdemeanors and nonviolent felonies. By dividing cases into treatment and control groups depending on bail-eligibility, this design allowed us to net out changes in recidivism over time that would have occurred in the absence of the reform, while also adjusting for individual-level background characteristics as well as county and month “fixed” differences.

We found that eliminating bail eligibility for certain charges was associated with several statistically significant changes in recidivism outcomes (**Table 5.1**). Among people classified as high-risk (defined as having a pending case), the reform was associated with a 6.6 percentage-point increase in the likelihood of any re-arrest and a 5.0 percentage-point increase in the likelihood of felony re-arrest during the immediate six-month pretrial period. By contrast, when outcomes were examined over a longer two-year follow-up window, the reform trended toward *reductions* in recidivism across all measures, including a statistically significant decline in felony re-arrest for the full population.

Table 5.1

**Summary of Difference-in-Differences Estimates
Change in Likelihood of Recidivism**

	Any Re-arrest		Felony Re-arrest		VFO Re-arrest	
	Pretrial	2 Years	Pretrial	2 Years	Pretrial	2 Years
Full Population	+1.8	-2.0	+1.1	-2.7	-0.1	-0.3
High Risk	+6.6	-1.1	+5.0	-1.9	+0.9	-0.9
Low Risk	-1.7	-2.8	-0.9	-1.8	-0.5	-0.8

Note: Highlighted cells indicate a statistically significant finding at a p -value of .05 or lower.

Study Findings in Context

The findings are broadly consistent with prior studies examining the effects of bail reform on recidivism across different regions of New York State using a range of research methods. Across studies, any increases in recidivism appear driven by individuals already at high risk of recidivism. This and a prior New York City study found that the system-wide effects of reform were concentrated in the pretrial period but tend to diminish or disappear over the long term.

CHAPTER 5. CONCLUSION & LIMITATIONS

Study Limitations

There are several important limitations to keep in mind when interpreting the findings of this study.

First, our analysis specifically examined the effects of the reform’s elimination of the option to set bail for certain charges. This element was arguably the most consequential component of the initial bail reform law. However, the legislation also introduced other provisions that could have influenced recidivism for both treatment and control groups—for example, expanding the availability of pretrial supervision and requiring judges to impose the “least restrictive” release conditions necessary to ensure court appearance. Since our research design was unable to isolate the impact of these provisions, it is possible that some of the observed recidivism increases after the reform were influenced by these additional changes.

Second, this study did not examine the direct effect of setting bail, but rather the *discretion* to set bail (or not) based on the type of charge. This allowed us to capture the broader *system-wide effects* of the reform as implemented in the NYC suburbs and upstate regions. However, it also means that our estimates are shaped by how judges used their discretion before and after the law changed. In both periods, judges often chose to *release* people even in cases where a person was charged with a bail-eligible offense (the “control” group).

Third, although we controlled for a range of demographic and criminal history characteristics known to be associated with recidivism, unmeasured individual-level factors remain a potential source of confounding. If such unobserved characteristics led to compositional differences between the treatment and control groups at baseline or over time, this would represent a potential source of residual confounding.

Fourth, while the inclusion of county and month fixed effects helped to account for unobserved, time-invariant county characteristics and statewide temporal shocks such as the COVID-19 pandemic, our estimates could still be affected by unmeasured local changes over time. County-specific shifts in prosecutorial practices, court operations, or pretrial supervision availability, for example, may have coincided with the reform and differentially influenced the treatment and control groups.

Fifth, during the final months of the study period (April to June 2020), the COVID-19 pandemic caused major disruptions to criminal justice operations, affecting criminal activity, arrests, case processing times, and the types of arrests that were prosecuted and reached arraignment. While our main DiD specification purposefully sought to minimize any pandemic-related biases by controlling for observable individual-level shifts as well as unobserved monthly “fixed” effects, other unmeasured pandemic-related factors may have still influenced the estimates. However, while the event study results suggest some upward bias during the COVID months, the statistically significant *pre*-COVID coefficients indicate that the main findings were not entirely driven by pandemic-related disruptions.

Sixth, the outcome measures used here capture whether a person was re-arrested within a given timeframe, but not how many times. (This limitation stems from the data; specifically, for the public pretrial re-arrest measures, only binary re-arrest indicators were available.)

CHAPTER 5. CONCLUSION & LIMITATIONS

It is therefore possible that changes in the likelihood of re-arrest do not fully reflect changes in its frequency. That said, in separate analyses examining the impact of the reform on the number of new prosecutions, we found results broadly consistent with those based on the binary re-arrest measures.

Finally, there are important differences between the datasets used to estimate pretrial and two-year recidivism outcomes. The dataset used for pretrial recidivism includes people's complete criminal histories, whereas the two-year recidivism dataset contains only recent history dating back to 2017 (though the two-year dataset includes a greater array of potential criminal history measures for this timeframe). Additionally, the two-year outcome measure captures only re-arrests that resulted in prosecution, while the pretrial recidivism measure also includes arrests that prosecutors ultimately declined to file with the court. These differences may affect the comparability of results across follow-up periods and could partly explain any inconsistencies between the short-term and long-term findings.

Endnotes

- ¹ Koppel, S. & Ropac, R. (2023). [Examining the System-Wide Effect of Eliminating Bail in New York City: A Controlled-Interrupted Time Series Study](#). Data Collaborative for Justice.
- ² Rahman, I. (2019). [New York, New York: Highlights of the 2019 Bail Reform Law](#). Vera Institute for Justice; Rempel, M. & Rodriguez, K. (2019). [Bail Reform in New York: Legislative Provisions and Implications for New York City](#). Center for Justice Innovation.
- ³ Rempel, M. & Rodriguez, K. (2019), Op Cit.
- ⁴ Rempel, M. & Rodriguez, K. (2020). [Bail Reform Revisited: The Impact of New York’s Amended Bail Law on Pretrial Detention](#). Center for Justice Innovation.
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- ⁶ Ropac, R. & Rempel, M. (2023). [Does New York’s Bail Reform Law Impact Recidivism? A Quasi-Experimental Test in New York City](#). Data Collaborative for Justice.
- ⁷ Zhou, A., Koo, A., Kallus, N., Ropac, R., Peterson, R., Koppel, S., & Bergin, T. (2023). [Synthetic Control Analysis of the Short-Term Impact of New York State’s Bail Elimination Act on Aggregate Crime](#). *Statistics and Public Policy*.
- ⁸ Wu, S., & McDowall, D. (2023). [Does Bail Reform Increase Crime in New York State: Evidence from Interrupted Time-Series Analyses and Synthetic Control Methods](#). *Justice Quarterly*, 1-29.
- ⁹ Koppel, S. & Ropac, R. (2023), Op Cit.
- ¹⁰ Ropac, R. (2024). [Does New York’s Bail Reform Law Impact Recidivism? A Quasi-Experimental Test in the State’s Suburban and Upstate Regions](#). Data Collaborative for Justice.
- ¹¹ Ropac, R. (2025). [Testing the Long-Term Impact of Bail Reform Across New York State: A Quasi-Experimental Evaluation](#). Data Collaborative for Justice.
- ¹² Wu & MacDowell (2023), Op Cit.; Zhou, et al. (2023), Op Cit.
- ¹³ Ropac & Rempel (2023), Op Cit.; Ropac 2025, Op Cit.
- ¹⁴ Koppel, S. & Ropac, R. (2023), Op Cit.
- ¹⁵ For an accessible summary of bail-eligible charges, see Rempel, M. & Rodriguez, K. (2019), Op Cit.; for the actual bail reform legislation, see New York State Senate. (2019). [Senate Bill S.1509-C](#).
- ¹⁶ Division of Criminal Justice Services (DCJS). (n.d.) [Pretrial Release Data](#).
- ¹⁷ Because our strategy was to test for a system-wide effect, we studied the vast majority of cases arraigned during the study period. However, we omitted cases arraigned for low-level, [non-fingerprintable](#) offenses outside New York’s penal code (e.g., certain violations of vehicle traffic law, tax law, or workers’ compensation law); and cases where judges did not have an opportunity to set bail (i.e., cases disposed at arraignment or subject to a pretrial hold in another case).

ENDNOTES

¹⁸ Abruzzese, R. (2019). [Judges say that Brooklyn courts will be ready for criminal justice reform changes](#). *Brooklyn Daily Eagle*; Conviser, D. (2019). [New York's Bail Reform Law: A Bench Book for Judges](#). *Criminal Justice Legislation - Recommendations*.

¹⁹ Gendreau, P., Little, T., & Goggin, C. (1996). [A Meta-Analysis of the Predictors of Adult Offender Recidivism: What Works!](#) *Criminology*, 34(4), 575-608; Piquero, A. R., Jennings, W. G., Diamond, B., & Reingle, J. M. (2015). [A Systematic Review of Age, Sex, Ethnicity, and Race as Predictors of Violent Recidivism](#). *International Journal of Offender Therapy and Comparative Criminology*, 59(1), 5-26. Siddiqi (2003). [Predicting the Likelihood of Pretrial Re-Arrest Among New York City Defendants: An Analysis of the 2001 Dataset](#). New York City Criminal Justice Agency; Lowenkamp, C., VanNostrand, M., & Holsinger, A. (2013). [The Hidden Costs of Pretrial Detention](#). Arnold Ventures.

²⁰ Abadie, A., Athey, S., Imbens, G. W., & Wooldridge, J. M. (2023). [When Should You Adjust Standard Errors for Clustering?](#) *Quarterly Journal of Economics*, 138(1), 1-35. To deal with uncommon and singleton charge categories, we combined charges with less than 20 cases during the study period into two catchall categories: one for rare “treated” charges and one for rare “control” charges. In all, less than 3% of all charges were reclassified.

²¹ Koppel, S. & Ropac, R. (2023), Op Cit.