The Impact of Expanding Pretrial Release: Evidence from the Repeal of Presumptions Against Bail in Virginia *

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Debbie Amewu

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Abstract

In many jurisdictions, defendants charged with certain offenses are presumed ineligible for bail based on the charge alone, a categorical rule that treats the charge as a proxy for risk regardless of a defendant's individual circumstances. This paper studies Virginia's 2021 repeal of its Presumptive Denial of Bail statute, which previously made defendants charged with specified violent and serious offenses presumptively ineligible for bail on the basis of charge alone. Using administrative court records from the Virginia court system and a difference-in-differences design comparing defendants with affected and unaffected charges before and after the reform, I estimate the effects of removing charge-based bail presumptions. The repeal increased pretrial release among affected defendants, accompanied by a shift away from monetary bail toward other forms of release. Consistent with the literature on the costs of detention, this expansion in releases coincided with more lenient case dispositions—incarceration and guilty pleas both declined, though the decline in guilty pleas did not translate into a statistically significant drop in conviction rates. The reform did not increase failures to appear or short-run rearrest, but longer-run rearrest rose modestly. These results provide new evidence on bail reform for defendants charged with violent and serious offenses, a population largely absent from a literature focused primarily on lower-level offenses.

Keywords: Bail Reform, Pretrial Release, Presumptive Detention, Criminal Justice Reform

JEL classification codes: K42, K14, K41

*Amewu: Ph.D Candidate, University of South Carolina, Darla Moore School of Business, damewu@email.sc.edu.

1 Introduction

In the United States, roughly 70 percent of people held in local jails on any given day are awaiting trial rather than serving an actual sentence [Bureau of Justice Statistics, 2025]. This high share is a relatively recent phenomenon; between 1985 and 2023, the number of pretrial detainees rose by 268 percent. Pretrial detention is a driving factor in the rapid increase in jail populations and jail costs around the country. The Prison Policy Initiative reports the costs of detaining individuals pretrial at $13.6 billion in 2014. Moreover, detention stays are often lengthy, averaging between 26 and over 100 days depending on jurisdiction and offense severity, meaning a substantial share of jail populations are held for extended periods before their cases are even resolved.

Beyond the immediate deprivation of liberty, pretrial detention imposes several economic costs on defendants. First, detention disrupts defendants' economic and social stability, as detained defendants risk losing employment, housing, or child custody. Moreover, these disruptions could extend to defendants' households, leading to higher rates of insolvency, including bankruptcy and foreclosure [Slutzky and Xu, 2025]. To the extent that these losses erode a defendant's legitimate economic opportunities, detention may raise the likelihood of future criminal behavior. Second, detention exposes defendants to other accused individuals and to criminal networks that could increase defendants' criminal capital. This is especially critical for defendants who are ultimately not convicted post-case disposition. Third, and most directly documented in the literature, detention distorts the plea-bargaining process. Because the fastest route out of jail for most detained defendants is a guilty plea paired with a time-served sentence, defendants are forced to plead to secure early release. However, the resulting consequence is an almost certain guilty verdict; therefore, detention mechanically increases conviction rates by pressuring defendants to plead guilty to secure release. [Dobbie, Goldin, and Yang, 2018; Leslie and Pope, 2017; Gupta, Hansman, and Frenchman, 2016].

In many jurisdictions, the more common method of pretrial detention is monetary bail, which helps incentivize appearance in court and reduce recidivism. However, in other jurisdictions it is becoming more common for pretrial detention to occur outside of bail determinations and be based on the individual's charge, leading to a presumption against release that the defendant bears the burden of overcoming.[1] This channel to

[1] At least 22 states and the District of Columbia, as of 2019, authorize preventive detention of defendants charged with specified serious offenses, in addition to the federal system [Warren et al., 2020]. States have continued to move in this direction: New Jersey authorized preventive detention through a 2014 constitutional amendment and New Mexico did so in 2016 [Warren et al., 2020]. By contrast, seventeen states limit preventive detention to capital or life-sentence offenses or prohibit it entirely [Petis, 2025].

pretrial detention is understudied in the literature, as most work done focuses on either aggregate forms of pretrial detention regardless of the mode of detention or focuses on reforms that eliminate monetary bail in certain jurisdictions. Understanding this distinct channel matters because, unlike monetary bail, charge-based denial removes ability to pay from the release decision entirely — detention is determined by the statutory charge rather than the defendant's resources or circumstances, making its effects on defendant outcomes an open empirical question.

This paper examines Virginia's repeal of its Presumptive Denial of Bail (I hereby term it PDoB) statute, which eliminated the presumption against bail for a specified set of offenses in July 2021. Before the reform, defendants charged with any of the specified offenses, which are violent and serious in nature, faced a statutory presumption against bail, where, conditional on their charged offense, courts were directed to treat them as presumptively ineligible for pretrial release unless the presumption was rebutted. The July 2021 repeal removed this charge-based restriction, returning these defendants to the pre-repeal standard bail determination process, in which release decisions are based on individualized assessments of defendants' flight risk and threat to public safety. This repeal generated variation in the strength of the barrier to release for defendants charged with any of the specified offenses. Removal of the presumption expanded the likelihood of pretrial release, since these defendants no longer had to rebut the presumption against them to be released. Using criminal case records web-scraped from the Virginia Court System, comprising cases from about 3.8 million defendants from 2010-2024, I exploit this variation, comparing outcomes for defendants whose charges were eligible for presumptive denial to those whose charges were not, before and after the repeal.

I begin by estimating the reform's impact on pretrial release, which is the direct impact of the policy. The results show that eliminating charge-based presumptions expanded pretrial release for eligible defendants but shifted away from monetary bail toward less financially burdensome forms of pretrial release. I find that pretrial release increased for eligible defendants by 5.2 percentage points, or 6.4 percent relative to the pre-reform mean. Release on monetary bail fell by 28.7 percentage points, while release on secured/unsecured bond and on personal recognizance rose by 24.7 and 6.7 percentage points, respectively. This shift reflects how PDoB distorted release conditions prior to the reform. Because the presumption treated eligible defendants as high-risk by statute, judges who released these defendants had a strong incentive to attach monetary bail as a hedge against that presumed risk, even when the fuller circumstances of the case may not have warranted it. Once the reform removed the presumption, judges could instead condition release on defendants' actual risk profiles, reducing the need to default to a financial safeguard based on the charge alone.

Next, I investigate whether the reform impacted downstream criminal justice outcomes. The reform decreased guilty pleas by 2 percentage points, consistent with pretrial release reducing the pressure to accept a plea deal. In contrast to the mechanism emphasized in prior work, however, this decline in guilty pleas does not generate a statistically significant decline in convictions, albeit in the preferred specification. This pattern suggests that, among defendants charged with violent and serious offenses, expanded release reduced the pressure to plead guilty without substantially changing prosecutors' ability to ultimately secure a conviction through other means. On the other hand, incarceration rates decreased by 7.8 percentage points, although this did not translate into any statistically significant impact on sentence length.

Finally, I examine whether the reform increased flight risk or subsequent criminal justice contact. Because PDoB applied to defendants charged with violent and serious offenses, the public-safety rationale for presumptive detention is especially salient; if the charge against a defendant is an informative proxy for risk, removing the presumption should increase failure to appear in court or new arrests after release. I find little evidence of flight risk and recidivism, at least in the short-term. Failure-to-appear in court rates are unchanged in my preferred specification, while short-term recidivism (rearrest within six months of release), which is likely to cover pretrial misconduct, also shows no significant effect. However, long-run recidivism, which likely covers the post-trial period, increases by 2.5 percentage points.

Because racial disparities in pretrial outcomes are well documented (black defendants are less likely to be released and face more restrictive conditions [Arnold, Dobbie, and Yang, 2018; Anderson, 2024]), I next ask whether the reform closed or widened this gap. Black defendants saw a larger gain in release (5.8 percentage points versus 4.6 for White defendants) but fewer downstream effects. Incarceration fell by 6.3 percentage points, with no detectable change in guilty pleas, convictions, failure to appear, or recidivism. White defendants' downstream outcomes mirrored the main results, suggesting that results are driven by white defendants.

I subject the main results to multiple robustness checks. First, I verify whether other policy changes occurred around the revocation of PDoB that could confound the estimates. Notably, Virginia decriminalized simple marijuana possession in July 2020 and legalized recreational marijuana in July 2021, which is the same date that PDoB was revoked. After excluding the small share of defendants (0.15 percent of the treated group) whose charges also involved marijuana-related offenses, I find that the results are similar to the main estimates. Second, I plot charges and arrests around the policy change date and find that they are parallel to each other and move smoothly through the threshold. This also helps to test whether offenses are charged the same way before and after the

policy. The time trends show no indication of bulking around the policy reform date. Thirdly, I test whether the policy led to cases being upcharged or downcharged and find no statistically significant effects. Fourth, since the majority of the cases result in release on summons, I exclude this population to narrow down to more serious offenses within the sample, and this also helps to identify cases that are more likely to have bail determination directly conducted by the magistrate rather than law enforcement, as is the case for most traffic infractions. The results largely match the main results, with a significant difference being an increase in guilty pleas, which does not translate into any significant impact on conviction rates. Finally, I restrict the treated sample to offenses in the PDoB law that do not have prior criminal history conditions attached to them, as I am unable to test long-term criminal history due to lack of data. The results here also mirror the main results with a strong reduction in conviction rates and failure-to-appear in court rates, possibly due to the fact that the excluded offenses are more severe in nature while the included offenses in this sample are less severe in nature.

Related Literature

This paper contributes to several strands of the existing literature. First, it supplements the existing literature on the impacts of pretrial detention on defendant outcomes using quasi-experimental variation in judge bail decisions as an instrument. Dobbie, Goldin, and Yang, 2018 find in Philadelphia and Miami-Dade that detention raises the probability of conviction, primarily through an increase in guilty pleas, while having no net effect on future crime and reducing formal-sector employment. Leslie and Pope, 2017 and Gupta, Hansman, and Frenchman, 2016 find similar increases in conviction rates due to pretrial detention and assignment of money bail in New York City and Philadelphia. Slutzky and Xu, 2025 finds that pretrial detention increases household financial distress. My results mirror the findings in this strand of the literature where the outcomes in my paper overlap with the respective literature.

Second, this paper relates to a literature testing whether monetary and supervisory bail conditions deter pretrial misconduct. M. T. Stevenson, 2018 shows that inability to pay bail worsens case outcomes in Philadelphia, and Ouss and M. Stevenson, 2023 exploits a prosecutor-led elimination of low-level cash bail to show that bail amount has no deterrent effect on failure to appear or pretrial crime. This paper's finding of no detectable increase in failure to appear echoes that null result at the opposite margin — the extensive decision to detain rather than the size of bail — over a considerably longer post-implementation horizon and for a substantially more serious offense population. A closely related set of natural-experiment studies reaches similar conclusions for other

reforms. Lacoe, Fischer, and Raphael, 2025 studies a pilot program providing early legal representation immediately following arrest in Santa Clara County and finds that access to a public defender pre-arraignment increased pretrial release and case dismissals. Lacoe, Skog, and Bird, 2025 studies the implementation of In re Humphrey in San Francisco County, which required judges to set bail based on defendants' ability to pay rather than a fixed county schedule. They find that reform led to a decline in pretrial detention, a drop in conviction rates, driven primarily by reduced plea bargaining, with no consistent change in subsequent arrests or recidivism. Premkumar et al., 2025 uses California's staggered county-level zero-bail orders during COVID and finds that implementation increased non-violent felony rearrests within 30 days. Albright, 2022 finds that replacing financial release conditions with automatic release for low-level offenses sharply reduces short jail stays without adverse consequences.

Finally, this paper complements the existing research evaluating the impact of PDoB on defendant outcomes. The only existing evaluation is a report by the Virginia Criminal Sentencing Commission, which focuses on pretrial release, failure to appear, and recidivism (Virginia Criminal Sentencing Commission, 2024). I supplement this research with significantly stronger empirical analysis and also include analysis of case outcomes, which the report largely excludes.

I make four contributions to the literature. First, I document a break from the mechanism established in prior work: while guilty pleas decline following expanded release, as the existing literature predicts, this decline does not translate into a statistically significant reduction in convictions in the Virginia context, indicating that the plea-to-conviction link is weaker for defendants charged with serious and violent offenses. Second, whereas judge-leniency designs identify a local average treatment effect for defendants near the margin of an individual judge's release decision, my reform generates variation at the extensive margin of eligibility itself, allowing me to estimate detention effects for a population — defendants categorically denied release regardless of judicial assessment — that this literature has previously studied. Third, unlike the existing bail-reform literature, which is concentrated on lower-level and nonviolent offenses, I provide new evidence on the public-safety trade-offs of extending individualized bail determinations to more serious charges. Fourth, I show that the reform's effects are not race-neutral. Despite gaining more access to release, Black defendants saw no corresponding improvement in case outcomes — evidence that expanding release does not mechanically translate into equitable gains across defendants.

Outline

The rest of this paper is organized as follows. Section 2 provides institutional background on Virginia's bail system and the 2021 PDoB repeal. Section 3 describes the court-record data and provides descriptive statistics. Section 4 presents the empirical strategy. Section 5 presents the results, while Section 6 discusses the implications of the results and estimates the benefits and costs of the policy reform. Section 7 concludes.

2 Institutional Context

2.1 The Pretrial Process in Virginia

The term "bail" refers to the temporary release of an accused person awaiting trial.[2] Its primary purpose is to ensure that a released defendant appears at all future court hearings, while also reducing the risk of reoffending during the pretrial period. In most jurisdictions, once the defendant appears for all required court dates and does not recidivate before trial, the bail money is returned after the case is resolved.

In Virginia, this general bail framework is carried out through a specific pretrial process. The Code of Virginia requires that when a law enforcement officer arrests a suspect under a warrant or capias, the officer must bring the arrested individual before a judicial officer without unnecessary delay. Before making an arrest, the officer investigates the facts of the case, identifies possible suspects, and locates witnesses. Once probable offenders are identified, the officer issues either a warrant of arrest or a summons, depending on the nature of the offense. Typically, misdemeanors committed in the presence of the officer, such as traffic violations, result in a summons unless the misdemeanor is jailable or the officer has probable cause to believe the defendant will not honor the summons. When a summons is issued, the officer takes the defendant into custody and has them sign a promise to appear for the initial court date, after which the defendant is released. For felony offenses and jailable misdemeanors, the offender is issued a warrant of arrest and brought before a magistrate, either in person or through two-way electronic video and audio communication, to determine bail. If bail is denied, the magistrate sets the date for the first court appearance. The arraignment, which informs the defendant of the charges and records the plea, then takes place on the next court date after arrest. If the individual is released under one of the available bail options, the arraignment takes place at the earliest available court date. When bail is denied, the arresting officer transports

[2]The right to bail is guaranteed by the Eighth Amendment of the United States Constitution, which also stipulates that "excessive bail shall not be required, nor excessive fines imposed, nor cruel and unusual punishments inflicted."

the defendant to the locality where the case will be tried.[3]

At the bail hearing, the magistrate considers all available information related to the case and the defendant. Specifically, the arresting officer or pretrial services agency must present the defendant's criminal history to assist the magistrate in making this decision. The magistrate considers factors such as the nature and circumstances of the offense, whether a firearm was used in the commission of the crime, the weight of the evidence against the accused, the defendant's financial resources, employment and education history, family ties, residence in the state, and criminal history, including past convictions and prior court appearance records.

Upon careful consideration of these factors, the magistrate makes one of five bail decisions: release on summons, release on recognizance, release on bond, release on money bail, or ineligible for bail.[4] Release on summons is a legal order requiring the defendant to appear in court on a specified date without being taken into custody, and is the mechanism used for most traffic infractions and minor misdemeanors. Release on recognizance requires the defendant to sign a promise to appear at all future court dates and comply with any release conditions, without a financial requirement. Release on bond requires the defendant to agree to a specified financial amount as a condition of release. Under a secured bond, the defendant must provide the financial security up front (through cash, property, or a licensed bail bondsman), while under an unsecured bond, the defendant is only required to pay the specified amount if they fail to appear or otherwise violate the conditions of release. Release on money bail requires the defendant to post the full bail amount in cash to secure release. If the magistrate deems none of these conditions sufficient to reasonably assure the defendant's appearance in court or the safety of the public, the defendant is deemed ineligible for bail and is detained pending trial.

2.2 Virginia's Repeal of the Presumptive Denial of Bail Statute

Virginia's pretrial release framework changed substantially following the passage of Senate Bill 1266 (SB1266) during the 2021 Special Legislative Session. Prior to July 2021, the Commonwealth's bail framework had been shaped by legislation dating back to 1996, when the Code of Virginia first introduced rebuttable presumptions against bail for a narrow class of offenses, inverting the burden such that defendants were required

[3]Most magistrate offices in Virginia are open 72 hours a day, 7 days a week, with offices in more rural cities or counties available on call.

[4]A magistrate's bail decision may later be modified by another magistrate if new information emerges that could alter the outcome. Because my analysis is intended to capture the magistrate's initial assessment of the defendant's eligibility for release, I restrict the release outcome to each defendant's first bail decision rather than any subsequent modification. Such later changes occur at subsequent hearings, not at arraignment, and before case disposition.

to demonstrate that release was appropriate. Over the following two and a half decades, the scope of these presumptions expanded considerably through ten successive legislative amendments, growing from a narrow cluster of drug-related conditions to encompass a broad range of violent offenses. SB1266 amended §19.2-120 to eliminate the rebuttable presumptions against bail previously codified in subsection (B), replacing them with a requirement that judicial officers consider a specified set of individualized factors when making bail determinations, similar to what magistrates would have done in the absence of the 1996 legislation.

The introduction of PDoB altered the bail-determination process described in Section 2.1. Instead of beginning with the magistrate's assessment of the defendant's individual circumstances, PDoB imposed a statutory presumption that no combination of conditions would reasonably assure the defendant's appearance or the safety of the public. Defendants subject to this presumption were therefore effectively denied bail at the outset, and any challenge to the magistrate's decision had to proceed through more cumbersome appellate procedures.[5] Because the presumption was rebuttable rather than absolute, however, PDoB shifted the burden to the defendant to justify release rather than foreclosing it entirely. A defendant who successfully rebutted the presumption could still obtain release under the standard bail options described in Section 2.1.

Figure 1 provides a visual illustration of the pretrial process in Virginia before and after the repeal of the Presumptive Denial of Bail statute. Panel (a) illustrates the pretrial decision-making process under the PDoB regime (pre-repeal). At the bail hearing, a defendant's case was first categorized according to whether the charged offense fell within the list of offenses enumerated under subsection B of §19.2-120. For defendants whose charges did not trigger a presumption — the non-PDoB eligible group — the magistrate retained full discretion to consider all available bail options (released on summons, on recognizance, on bond, or on money bail).[6] For defendants whose charges triggered a presumption — the PDoB eligible group — the statutory presumption meant that the starting point of the bail hearing was an assumption of detention rather than an assumption of release. These defendants were detained but could attempt to rebut the presumption; if the rebuttal failed, the defendant remained detained until trial.[7]

Panel (b) illustrates the post-repeal framework following the passage of SB1266 in July

[5]Rebutting this presumption was a demanding process. Defendants could not secure release through a standard magistrate hearing alone but were required to appear before a judge and, in many cases, obtain the concurrence of the attorney for the Commonwealth before any bail consideration could proceed. This created a structurally higher bar for release.

[6]However, defendants assigned to money bail or bond who were unable to meet the associated financial conditions faced detention as a consequence of their inability to satisfy those requirements.

[7]My data do not capture rebutted bail decisions, but because I restrict the sample to each defendant's initial bail decision, rebuttals should not affect the outcome measure.

2021. The reform repealed all presumptions against bail contained in subsection B of §19.2-120, shifting the burden from the defendant to justify release to the Commonwealth to justify detention. Under the new framework, both groups are subject to the same set of release and detention possibilities, with the magistrate or judge determining the appropriate outcome based on assessment of the individual defendant's circumstances. The reform, therefore, did not eliminate judicial discretion to detain defendants charged with offenses listed in subsection B of §19.2-120; rather, it removed the statutory presumption that made detention the default outcome for this group and restored the magistrate's obligation to give meaningful individualized consideration to each defendant's pretrial release.

3 Data

My primary data source consists of court records web-scraped from the Virginia Court Systems [Virginia Court Data, n.d.].[8] I sourced district criminal court cases for all defendants whose cases were heard from 2010 to 2024. The data are at the person-hearing-charge level and include information on the defendant (name, date of birth[9], race, gender, address, and a unique person identifier), the charge (the offense date, arrest date, original arrest charge, amended arrest charge, offense type), and the case (case number, hearing dates, hearing type, file date, status of the case which determines the release or detention outcome, final disposition and sentence length of each guilty charge).

I restrict the sample to the first hearing of each defendant's first case within the sample, and collapse the data so that the unit of analysis is the individual defendant rather than the charge. The first hearing contains both the bail decision and the final disposition of the charges within the case. Because a defendant's first case can involve multiple charges, I code the treatment and outcome variables as the maximum value observed across all charges within that case. This approach reflects the fact that, if any charge in a defendant's case results in a conviction, the defendant should be classified as convicted rather than not convicted. The same logic applies to other outcomes such as guilty plea or failure to appear. Likewise, if any charge within an individual's case is an offense listed in the bail reform legislation, that individual is coded as treated. The same logic applies to release outcomes, since a defendant's custody status is determined by the case as a whole rather than by any single charge in isolation. Accordingly, I code release outcomes

[8]These data were compiled by an independent private contractor who sourced court dockets directly from the Virginia Courts website under an MIT license.

[9]The defendant's date of birth excludes the year of birth, as that is censored from Virginia's Court System website. Additionally, it does not show the defendant's full address but rather the state, city, and zip code of residence.

using the maximum value observed across charges within the case, which ensures that the defendant is classified according to the most restrictive applicable bail outcome. After applying this coding, I collapse the data to one observation per defendant.

Using these data, I construct my main outcome variables and controls, with full variable definitions provided in the Data Appendix. Conviction and guilty plea are derived from the final disposition and hearing plea variables, respectively. My pretrial misconduct outcome, failure to appear, is constructed from bench warrants and offense codes indicating an arrest for failure to appear, and additionally includes cases in which the defendant was tried in absentia. Recidivism is constructed using the defendant's unique person identifier together with subsequent arrest dates to identify individuals rearrested following their first hearing. Because the court records do not distinguish whether a subsequent arrest precedes or follows case disposition, this measure captures rearrest generally rather than a specifically pretrial or post-disposition outcome. Pretrial release is constructed from the case status variable, which records the bail outcome assigned at the first hearing. I include controls for defendant race and gender, an indicator for representation by a public defender derived from the attorney representation variable, and an indicator for the judicial circuit in which the case was heard.

Virginia's bail reform legislation bases treatment assignment on both the offense charged and prior convictions of that offense. The legislation contains 16 clauses covering 32 unique offense codes and their subsections, each specifying the conditions under which an offense would have triggered presumptions against bail prior to the reform.[10] Six of the 16 clauses specifically require an individual to have prior convictions or violations of the same offense in addition to the current charge.[11] Since I use each defendant's first case observed in the data, I do not directly observe each defendant's criminal history at the time of their first case. However, using each defendant's last case within the sample, I can back out whether they had accumulated the required number of prior convictions or violations by the end of the observed period, which provides an upper-bound estimate of how many defendants could have met the prior-conviction threshold. The share of defendants with such prior convictions or violations, across each of the six clauses I detected in the data, was extremely low. For example, among individuals charged with manslaughter, only 0.006% had the number of prior convictions required by the legislation (three or more). Similarly, only 0.51% of defendants charged with any offense indexed by the legislation had the required number of prior convictions to be considered treated.

The low frequency of prior convictions likely reflects three data limitations. First,

[10]The conditions typically include the maximum sentence for the offense, minimum imprisonment terms, prior conviction of the offense, age of the victim, and the relationship of the victim to the offender.

[11]Appendix Table A1 outlines the full list of clauses and specifies which of them require past conviction conditions.

my dataset may not span a sufficient time period to fully capture defendants' criminal histories. Second, the offenses that trigger these prior conviction requirements typically carry long sentences, meaning the gap between an initial conviction, sentence completion, and a subsequent charge on the same offense may exceed the observation window of the data. Third, several of these provisions require two or more prior convictions before they apply, making it even less likely that the full conviction history of affected defendants is captured within the current data span.

Given this, I code cases as "treated" if they meet the criteria for offense type specified in the legislation, and I do not consider past convictions of that offense. This may introduce some level of measurement error in the treatment classification; however, this would likely lead to an understatement of the true treatment effect, since if some untreated cases are mistakenly coded as treated, I would expect that the empirical estimates would be watered down and conservative.[12]

Descriptive Statistics

Table 1 presents descriptive statistics for the full sample of defendants, split by PDoB eligibility status, for the period before the repeal of PDoB.

Panel A of Table 1 reports descriptive statistics for pretrial release outcomes, both overall and by release type.[13] Eligible and non-eligible defendants differ substantially in the type of release they receive. Non-eligible defendants are released on summons the large majority of the time (92.3 percent), while eligible defendants are much more likely to be released on bail (31.5 percent) or recognizance (43.9 percent) and only released on summons 28.4 percent of the time. This pattern is consistent with PDoB attaching to offenses that are, on average, more serious than the typical non-eligible charge, making magistrates less likely to resolve these cases via summons. As seen in Column 1, defendants in Virginia are very likely to be released pretrial, with an average of 97 percent prior to PDoB repeal.

Panel B presents defendant characteristics. Eligible defendants are considerably more likely to be represented by a public defender (11.9 percent versus 2.3 percent) and live in zip codes with modestly lower median household income ($69,599 versus $76,710). They are also less likely to be female (23.1 percent versus 39.0 percent) or Hispanic (1.1 percent versus 3.9 percent), while Black representation is only modestly higher (29.9 percent versus 26.1 percent).

[12]However, as a robustness check, I run regressions where I restrict to the 10 clauses that do not require prior convictions, and the results are similar. These results are included in Appendix Tables **??** – **??**

[13]I am unable to observe the exact length of time between arrest and release. In Virginia, however, defendants who are released pretrial are generally released shortly (typically within 72 hours on average) after arrest, either by a law enforcement officer or following an appearance before a magistrate.

Panel C reports charge characteristics for defendants' first observed cases. Most cases involve misdemeanor offenses, with felonies accounting for only 3.5 percent of cases in the full sample. Eligible defendants are far more likely to face a felony charge (24.7 percent versus 2.8 percent), a violent crime charge (11.7 percent versus 1.1 percent), and carry more charges per case on average (2.123 versus 1.176).

Panel D presents case outcomes. Eligible defendants are convicted at a somewhat lower rate (81.8 percent versus 89.3 percent), while the incarceration rate is substantially higher (66.8 percent versus 4.6 percent) — a gap far larger than the difference in conviction rates alone would predict, consistent with the more serious charge composition of the eligible group, which carries a higher likelihood of an incarceration sentence conditional on conviction. Additionally, although non-eligible defendants are convicted, they are more likely to be fined rather than incarcerated. As such, sentences are about 20 percent longer for eligible defendants (166.3 days versus 138.9 days). Eligible defendants also plead guilty at a somewhat higher rate (81.9 percent versus 76.7 percent), reflecting greater pressure to resolve their case quickly given the higher likelihood of pretrial detention while awaiting trial.

Panel E reports pretrial misconduct measured by failure to appear in court and recidivism. Eligible defendants fail to appear at roughly twice the rate of non-eligible defendants (2.4 percent versus 1.1 percent), despite being detained pretrial far more often (18.8 percent are not released, versus 1.8 percent among non-eligible defendants). This is notable because detention should mechanically suppress FTA — a detained defendant cannot fail to appear. The fact that FTA is nonetheless higher for eligible defendants suggests that the released eligible population is disproportionately high-risk relative to the released non-eligible population: since PDoB is presumptively applied to the eligible group, the subset who still secure release are likely more lower risk defendants within that group who need to be brought in front of a magistrate more often given the number of charges and case complexity, resulting in more court dates and thus more opportunities to miss one. In other words, the composition effect among the released — eligible defendants facing more complex, multi-hearing cases — appears to dominate the mechanical incapacitation effect of higher detention rates. Recidivism shows the opposite pattern in the short run: at 0–6 months, eligible defendants are rearrested less often than non-eligible defendants (18.0 percent versus 23.9 percent), consistent with incapacitation — a larger share of the eligible group remains detained during this window and is mechanically unable to reoffend. By the 1–2 year horizon, this gap nearly disappears (17.6 percent versus 17.4 percent), as previously detained eligible defendants are released or have their cases resolved and the incapacitation effect fades.

4 Empirical Strategy

I estimate the impact of Virginia's 2021 repeal of the Presumptive Denial of Bail (PDoB) statute using a difference-in-differences design. Prior to the reform, defendants charged with offenses enumerated in the legislation faced a statutory presumption against bail — the PDoB-eligible group — under which the bail hearing began from an assumption of detention rather than release, while defendants charged with other offenses — the non-PDoB-eligible group — were subject to the standard, individualized bail determination described in Section 2.1. The reform repealed this presumption, extending standard bail consideration to the PDoB-eligible group and creating quasi-experimental variation that I exploit by comparing outcomes for PDoB-eligible defendants against non-eligible defendants, before and after July 2021. This approach allows me to assess both the immediate effect on pretrial release and the downstream consequences for case disposition, pretrial misconduct, and recidivism, thereby providing some of the first evidence on how bail expansion affects defendants charged with serious offenses and public safety.

My baseline specification is:

$$Y_{ict} = \alpha + \beta_1(Eligible \times Post)_{ct} + \beta_2 Eligible_c + \beta_3 Post_t + \epsilon_{ict} \tag{1}$$

where Y_{ict} represents the outcome of interest for individual i with charge c at time t, $Eligible_c = 1$ if any charged offense within the individual's first case is impacted by the bail reform and 0 otherwise, and $Post_t = 1$ from July 2021 onwards and 0 otherwise. The coefficient of interest, β_1, captures the average treatment effect of the reform on eligible defendants relative to ineligible defendants before and after implementation.

My preferred specification replaces $Eligible_c$ and $Post_t$ with offense-by-eligibility and time fixed effects:

$$Y_{ict} = \alpha + \phi_1(Eligible \times Post)_{ct} + \gamma_{ct} + \mu_c + \lambda_t + X_i\theta + \epsilon_{ict} \tag{2}$$

where γ_{ct} denotes offense type by treatment status fixed effects that absorb time-invariant differences in outcomes across offense-eligibility cells, subsuming the main effect of $Eligible_c$.[14] μ_i denotes fixed effects for the number of charges within each offense-eligibility cell, which I include as a control for case complexity absorbed nonparametrically. λ_t denotes year-month fixed effects that absorb aggregate time-varying shocks common to both groups, subsuming the main effect of $Post_t$. I include controls for defendant race and gender, an indicator for whether the defendant was represented by a public

[14] These fixed effects absorb the full set of offense types within the defendant's first case, interacted with eligibility status.

defender, and an indicator for the judicial circuit in which the defendant's case was heard. These controls are represented by X_i. The coefficient of interest, ϕ_1, captures the average treatment effect of the reform on eligible defendants relative to ineligible defendants before and after implementation. The identifying assumption is that, absent the reform, outcomes for eligible and ineligible defendants would have evolved in parallel. Standard errors are clustered at the level of broad offense category[15] to account for correlation in outcomes among defendants charged under the same offense category.

To support the parallel trends assumption, I estimate the following specification:

$$Y_{ict} = \alpha + \sum_{k \neq -1} \delta_k \left(Eligible_c \times \mathbf{1}[t = k] \right) + \gamma_{ct} + \mu_c + \lambda_t + X_i\theta + \epsilon_{ict} \tag{3}$$

where $\mathbf{1}[t = k]$ replaces $Post_t$ in Equations 1 and 2 and denotes indicators for quarters relative to the repeal of presumptions against bail in July 2021. The omitted category is January–June 2021, the quarter immediately preceding the reform. Thus, $k = -1$ corresponds to July–December 2020, $k = -2$ to January–June 2020, and $k = 1$ to July–December 2021. The parameters of interest are the δ_k's, which capture the dynamic effects of the bail reform on the outcomes of interest relative to the January–June 2021 period.

5 Results

In this section, I present some visual evidence of the policy's impact on jail populations. I then move on to show the main results, beginning with the impact of revoking PDoB on pretrial release, and then I show the consequent impacts on defendant outcomes. I explore heterogeneity by defendant characteristics and also show some robustness checks to increase confidence in the difference-in-differences estimates. Finally, I present IV estimates which analyse the impact of pretrial release on defendant outcomes using the revocation of PDoB as an instrument for pretrial release.

5.1 Visual Evidence

To begin, I present visual evidence of how jail populations evolved in Virginia before versus after treatment in Figure 2. Using data from the Virginia Compensation Board and residualised trend analysis, I show that after the reform in July 2021, the average daily population decreased substantially, which is driven by both a larger decrease in the sentenced population relative to the pretrial population. This might be because the

[15] Offenses are grouped into broad categories using Virginia Crime Code prefixes, yielding 118 clusters.

policy affects a small proportion of arrestees; hence, an increase in pretrial release for that demographic might not contribute greatly to reductions in jail populations. However, the moderate decrease in pretrial population provides suggestive evidence of the impact of the policy on defendant release outcomes.

5.2 Pretrial Release

Table 2 shows the impact of the bail reform on any pretrial release (Panel A) and pretrial release by bail type (Panel B). Overall, I find compelling evidence that the expansion of bail eligibility increased pretrial release. Moreover, among those who were released, there was a shift away from money bail.

Column 1 presents the pre-reform mean of PDoB eligible defendants with standard deviations in brackets. As shown in Column 2, when I estimate the baseline difference-in-differences model from equation 1, I find that pretrial release increases by 5.0 percentage points; this is a 6.2 percent change relative to the mean. I also find evidence of compositional shifts in bail types amongst those who are released. Money bail goes down by an estimated 28.6 percentage points (90.8% relative to the pre-reform mean), while bond goes up by 25.9 percentage points, recognizance goes up by 7.4 percentage points (16.9 percent), and summonses go up by 3.3 percentage points (11.6 percent). This shift away from monetary bail is consistent with how the presumption against bail distorted release conditions prior to the reform. Before the repeal, PDoB treated eligible defendants as high-risk by statute, so when release did occur, judicial officers had a strong incentive to attach the most risk-minimizing conditions available—typically monetary bail—as a hedge against the flight risk. Once the reform removed the presumption and returned these defendants to the standard individualized bail process, judges could condition release on the defendant's actual risk profile rather than defaulting to the most restrictive mode of detention. This mechanically shifts the composition of release types toward recognizance and unsecured bond.

In Column 3, I present estimates from equation 2 where I replace the treatment indicators with more detailed fixed effects. I begin by excluding the defendant characteristic covariates, but add those later on (to gauge the sensitivity of the model). Notably, the results in Column 3 remain qualitatively similar to before. Pretrial release is estimated to increase by 5.1 percentage points (6.3 percent relative to the mean). There are also shifts away from money bail towards bond: money bail declines by 28.7 percentage points (91.1 percent), while bond increases by 25.1 percentage points (3,137.5 percent).

Finally, in Column 4, I present my preferred estimates from equation 2, incorporating defendant characteristic controls. The main finding remains the same. PDoB revocation

is estimated to increase pretrial release by 5.2 percentage points (6.4 percent relative to the mean). Again, it is also found to affect the composition of bail types among releasees. Money bail decreases by 28.7 percentage points (91.1 percent), and this is counteracted by increases in bond (24.7 percentage points, or 3,087.5 percent), increases in recognizance (6.7 percentage points, or 15.3 percent), and increases in summons (3.7 percentage points, or 13.0 percent).

5.3 Case & Sentencing Outcomes

After establishing that PDoB revocation affected pretrial release, I turn my attention to its effect on outcomes that are downstream of initial detainment. Overall, I find that the revocation of PDoB goes on to impact defendants' case dispositions and sentencing. It does so in a way that is favorable to defendants. Convictions, guilty pleas, sentence length, and incarceration rates fall.

Table 3 presents the effect of the reform on conviction rates and guilty pleas. Columns 1-3 report estimates for conviction, while Columns 4-6 report estimates for guilty pleas. Across columns, I show results from different specifications, with Columns 3 and 6 providing evidence from the most robust specification. Columns 1 and 4 report estimates of the baseline difference-in-differences specification in Equation 1. As shown in Column 1, the estimated effect on convictions is a marginally significant decrease of 6.3 percentage points, equivalent to 7.7 percent relative to the mean of 0.818. Column 4 shows a 2.3-percentage-point decrease in guilty pleas, equivalent to 2.8 percent relative to the mean, which is statistically significant at the 5 percent level.

Columns 2 and 5 report estimates from the fixed-effects specification in Equation 2, before the inclusion of defendant-characteristic covariates used later to assess the model's sensitivity. The results remain qualitatively similar to the baseline estimates. As shown in Column 2, convictions decrease by an estimated 4.1 percentage points, equivalent to 5.0 percent relative to the mean. Column 5 shows a slightly larger decrease in guilty pleas of 3.0 percentage points, equivalent to 3.7 percent relative to the mean, which is statistically significant at the 1 percent level.

Columns 3 and 6 report the preferred estimates from Equation 2, including controls for defendant characteristics. The main findings remain broadly consistent, although the estimated effect on convictions attenuates further and is no longer statistically significant. As shown in Column 3, convictions decrease by an estimated 3.2 percentage points, equivalent to 3.9 percent relative to the mean. Because PDoB-eligible offenses are more serious, including violent and specified felony charges, conviction outcomes in this group may depend more heavily on evidentiary strength and prosecutorial priorities than on

pretrial detention status. This differs from lower-level offenses, for which pretrial detention may exert greater pressure on defendants to plead guilty. The attenuation following the inclusion of demographic and case-level controls suggests that part of the variation initially attributed to the reform may instead reflect underlying differences in case composition. Column 6 shows that guilty pleas decrease by 2.0 percentage points, equivalent to 2.4 percent relative to the mean, and the estimate remains statistically significant.

Table 4 presents the estimated effect of the reform on sentencing outcomes. Columns 1–3 report estimates for sentence length, while Columns 4–6 report estimates for the likelihood of incarceration.

Columns 1 and 4 report estimates from the baseline difference-in-differences specification in Equation 1. As shown in Column 1, sentence length decreases by an estimated 18.0 days, equivalent to 10.9 percent relative to the mean of 166.3 days, although the estimate is not statistically significant. Column 4 shows an 8.0-percentage-point decrease in the likelihood of incarceration, equivalent to 12.0 percent relative to the mean, which is statistically significant at the 1 percent level.

Columns 2 and 5 report estimates from the fixed-effects specification in Equation 2, before the inclusion of defendant-characteristic controls. The results remain qualitatively similar to the baseline estimates. In Column 2, sentence length decreases by an estimated 15.9 days, equivalent to 9.6 percent relative to the mean, but the estimate remains statistically insignificant. Column 5 shows a 7.7-percentage-point decrease in the likelihood of incarceration, equivalent to 11.5 percent relative to the mean, and the estimate remains statistically significant.

Columns 3 and 6 report the preferred estimates from Equation 2, including controls for defendant characteristics. As shown in Column 3, sentence length decreases by an estimated 17.4 days, equivalent to 10.5 percent relative to the mean, although the effect remains statistically insignificant. Column 6 shows a 7.8-percentage-point decrease in the likelihood of incarceration, equivalent to 11.7 percent relative to the mean, and the estimate remains highly statistically significant.

5.4 Pretrial Misconduct & Recidivism

A potential concern associated with increased pretrial release is that it may lead to greater pretrial misconduct or recidivism, particularly when measures such as electronic monitoring or intensive supervision are not implemented to deter misconduct during the pretrial period. This concern may be especially salient in the present setting because the policy affects defendants charged with violent or other specified felony offenses. Table 5 presents difference-in-differences estimates for three outcomes. Columns 1–3 report esti-

mates for failure to appear, Columns 4–6 report estimates for rearrest within six months of release, and Columns 7–9 report estimates for rearrest between one and two years after release.

Columns 1, 4, and 7 report estimates from the baseline difference-in-differences specification in Equation 1. As shown in Column 1, failure to appear increases by 1.1 percentage points, equivalent to 45.8 percent relative to the mean of 0.024, and the estimate is statistically significant at the 1 percent level. Column 4 shows that rearrest within six months of release decreases by 4.5 percentage points, equivalent to 25.0 percent relative to the mean of 0.180. This estimate is statistically significant at the 10 percent level. In contrast, Column 7 shows that rearrest between one and two years after release increases by 2.7 percentage points, equivalent to 15.3 percent relative to the mean of 0.176, and the estimate is statistically significant at the 5 percent level.

Columns 2, 5, and 8 report estimates from the fixed-effects specification in Equation 2, before the inclusion of defendant-characteristic controls. As shown in Column 2, the estimated effect on failure to appear falls substantially to 0.1 percentage points, equivalent to approximately 4.2 percent relative to the mean, and is no longer statistically significant. Column 5 shows that rearrest within six months remains lower by an estimated 3.8 percentage points, equivalent to 21.1 percent relative to the mean, although the estimate is not statistically significant. In Column 8, rearrest between one and two years after release increases by 2.6 percentage points, equivalent to 14.8 percent relative to the mean, and remains statistically significant at the 5 percent level.

Columns 3, 6, and 9 report the preferred estimates from Equation 2, including fixed effects and controls for defendant characteristics. As shown in Column 3, the estimated effect on failure to appear is essentially zero and is not statistically significant. Because pretrial release was not entirely absent among PDoB-eligible defendants before the reform, the policy increased release at the margin rather than producing a shift from universal detention to release. The null effect may therefore indicate that the marginal defendants released because of the reform were, on average, no more likely to fail to appear than eligible defendants who were already being released before the reform. This interpretation is consistent with individualized bail determinations identifying defendants with similar risks of pretrial nonappearance both before the reform and at the margin following its implementation.

Column 6 shows that rearrest within six months of release decreases by 3.8 percentage points, equivalent to 21.1 percent relative to the mean of 0.180. However, the estimate remains statistically insignificant. Thus, although the point estimate suggests a reduction in short-run rearrest, the estimates do not provide sufficiently precise evidence that the

reform reduced rearrest during the first six months following release.

In Column 9, rearrest between one and two years after release increases by 2.5 percentage points, equivalent to 14.2 percent relative to the mean of 0.176, and the estimate remains statistically significant at the 5 percent level. The stability of this estimate across Columns 7, 8, and 9 indicates that the longer-run increase in rearrest is robust to the inclusion of fixed effects and defendant-characteristic controls. One possible interpretation is that pending charges and pretrial supervision temporarily constrain the behavior of marginally released defendants, with underlying differences in risk becoming more apparent after case disposition and the removal of those constraints. Evaluating the broader policy implications of this pattern requires weighing the benefits of reduced pretrial detention against the costs associated with the estimated longer-run increase in rearrest, a normative trade-off that cannot be resolved by these estimates alone.

Figures 3 through 5 present event-study estimates for the primary outcomes, with quarterly leads and lags measured relative to the quarter immediately preceding the reform (April–June 2021). The event-study estimates provide generally supportive, though not uniformly conclusive, evidence for the parallel-trends assumption. Across most outcomes—including pretrial release, conviction, guilty pleas, failure to appear, and rearrest between one and two years—the pretreatment coefficients fluctuate around zero without a clear upward or downward pattern, and their confidence intervals generally appear to include zero. The pretreatment estimates for incarceration and rearrest within six months are more volatile, with a few relatively large quarter-specific deviations, but these movements do not form a persistent trend approaching the reform date. Sentence length presents the greatest concern because the pretreatment coefficients are comparatively imprecise and remain mostly positive, although they also do not display a clear systematic trend immediately before treatment.

5.5 Heterogeneity by Race and Gender

Tables 6 - 8 examine whether the effects of revoking the presumptions against bail differ between Black and White non-Hispanic defendants. Overall, the reform increased pretrial release for both groups, although the downstream consequences differed across race.

Table 6 presents heterogeneous effects on pretrial release outcomes. The repeal of PDoB increased the likelihood of any pretrial release by 5.8 percentage points for Black defendants compared with 4.6 percentage points for White defendants, suggesting that Black defendants experienced a slightly larger expansion in overall access to pretrial release. Despite this, the changes in the composition of release types were somewhat larger

for White defendants. Among Black defendants, release on recognizance increased by 6.0 percentage points, release on bond increased by 23.9 percentage points, and release on money bail declined by 26.6 percentage points. The corresponding changes for White defendants were 6.8, 25.0, and 29.6 percentage points, respectively. Thus, while Black defendants experienced the larger increase in overall release, White defendants experienced somewhat larger shifts away from monetary bail toward less restrictive release conditions.

Table 7 shows that the downstream effects of the reform were concentrated among White defendants. There is no statistically significant effect on conviction rates or sentence length for either racial group. However, White defendants became 3.0 percentage points less likely to enter a guilty plea following the repeal of PDoB, whereas there is no corresponding effect for Black defendants. Likewise, incarceration rates declined for both groups, but the reduction was larger for White defendants. Eligible White defendants became 8.2 percentage points less likely to receive a custodial sentence compared with a 6.3 percentage point reduction among Black defendants.

Table 8 reports heterogeneous effects on pretrial misconduct and recidivism. For Black defendants, the reform had no statistically significant effect on failure to appear, short-term rearrest, or longer-term rearrest, suggesting that expanded access to pretrial release neither increased nor decreased subsequent misconduct for this group. In contrast, White defendants experienced a 5.4 percentage point reduction in rearrest within six months, alongside a 2.8 percentage point increase in rearrest one to two years after case disposition. Similar to the main results, there is no evidence that the reform affected failure to appear among White defendants.

These heterogeneity analyses suggest that the repeal of PDoB affected Black and White defendants differently. While Black defendants experienced a slightly larger increase in pretrial release, the downstream legal benefits were more evident for White defendants. White defendants were less likely to plead guilty and experienced a larger reduction in incarceration, whereas Black defendants experienced a more modest decline in incarceration with no corresponding reduction in guilty pleas. Similarly, the effects on recidivism were concentrated among White defendants, while the reform had no detectable effect on failure to appear or rearrest outcomes for Black defendants.

Overall, these results paint a consistent picture of the effects of repealing PDoB. Eliminating the charge-based presumption expanded pretrial release and shifted its composition away from monetary bail toward recognizance and unsecured bond, consistent with judges regaining discretion to condition release on individualized risk rather than defaulting to the most restrictive option. This expansion translated into better case and sentencing outcomes — lower rates of conviction, guilty pleas, and incarceration — with

no evidence of increased short-run pretrial misconduct. The one exception, a modest increase in longer-run rearrest emerging only after case disposition, suggests that any risk among the marginal releasee was constrained during the pretrial period rather than realized immediately upon release. Notably, my aggregate release estimate closely mirrors the Virginia Sentencing Commission's own evaluation (Virginia Criminal Sentencing Commission, 2024): my DiD estimate implies a roughly 5 percentage-point increase in pretrial release (Table 2, Panel A), close to the Commission's reported 3.8 percentage-point increase. Given differences in sample, control group, and estimation approach, this convergence lends independent support to the conclusion that repealing PDoB meaningfully expanded pretrial release.

5.6 Robustness

I subject the main results to a series of robustness checks. I first examine whether other contemporaneous factors threaten the identification strategy, before turning to analyses using alternative samples of defendants, for reasons explained in the ensuing subsections.

Threats to Identification.– I verify whether other policy changes occurred around the revocation of PDoB. Notably, Virginia decriminalized simple marijuana possession in July 2020 and legalized recreational marijuana in July 2021, which cover some of the offenses the reform impacted. To address the possibility that contemporaneous shifts in marijuana enforcement or charging behavior could confound the estimated effect of bail reform, I identify defendants whose treatment status could be affected by either marijuana policy and re-estimate the main specification excluding this group. First of all, only 143 of 3,909 flagged narcotic/marijuana defendants (0.15 percent of the treated group) have an offense enumerated in both the presumption statute. Second, I find that the results are very similar to the main sample. These results can be found in Appendix Tables A2 – A5

A key threat to identification would arise if prosecutors or law enforcement altered charging behavior in anticipation of or in response to the reform, since any such change would confound the estimated treatment effect with a shift in the composition of who is charged, rather than a change in outcomes for a fixed population of defendants. To assess this, I identify the modal offense categories within the treated group and plot monthly charge volumes for eligible and ineligible defendants within these categories, providing exploratory evidence on whether charging patterns shifted discontinuously around the reform. As shown in Appendix Figure A1, the time trends of eligible and ineligible offenses trend similarly between the two groups and move smoothly through the threshold (July 2021).

A related concern is that prosecutors could have manipulated defendants' treatment

status not by altering how many charges were filed, but by upcharging or downcharging a given offense to shift a defendant into or out of PDoB eligibility. I test whether the policy led to cases being upcharged or downcharged and find no statistically significant effects. Additionally, I test whether the policy repeal led to a significant change in the volume of charges and volume of arrests and find no statistically significant effects. These results are presented in Appendix Table A6.

Alternative Samples.–I test the sensitivity of my main results using alternative samples for different reasons. First, I exclude defendants released on summons. Summons releases are largely associated with traffic infractions and are typically issued by the arresting officer rather than determined through a magistrate or judicial bail hearing, though some treated defendants are released this way as well; excluding this group allows me to assess whether my results are sensitive to the inclusion of cases that may not have gone through the standard bail determination process. Additionally, since this exclusion removes traffic infractions, it allows me to narrow the sample to felonies and misdemeanors, which helps to increase comparability between the treated and control groups. Appendix Tables A7 – A10 present the results on all the outcomes using this summons-excluded sample, and the results are similar to the main findings, with the main exception being that guilty pleas increase for this presumably more serious offense sample; however, that does not translate into any statistically significant impact on conviction rates.

Next, I restrict the sample to the clauses within the legislation that do not require criminal history. Because my treatment classification does not account for prior convictions, as discussed in section 3, a natural concern is that some defendants coded as treated may not actually have met the full statutory criteria for presumptive denial of bail, introducing measurement error into the treatment variable. To address this directly, I restrict the sample to the ten clauses that do not require prior convictions, isolating variation driven purely by the current charge and eliminating any ambiguity about whether a defendant's treatment status was correctly assigned. These results are included in Appendix Tables A11 – A14. These findings are similar to the main findings. Where I find significant estimates, the magnitude is higher than in the main results, suggesting that using the charge alone for treatment assignment introduced downward bias in my estimates.

As a final robustness check, I restrict the sample to defendants who have non-missing observations for all outcome variables to ensure that the results for all outcomes are based on the same sample of defendants. The results presented in Appendix Tables A15 – A18 are similar to the main results. If I am to take this as the main results, then it would mean that the repeal of PDoB led to an increase in pretrial relase by 2.4 percentage points, a decrease in conviction by 0.6 percentage points, a decrease in guilty pleas by 1.7 percentage pints, decreased sentence length by 24.1 days, decreased short term recidivism

by 1.6 percentage points, decreased long term recidivism by 2.5 percentage points and had no impact on failure to appear.

5.7 Instrumental Variables Design

To get a sense of the actual impact of pretrial release on defendants' outcomes, I present instrumental variable (IV) estimates using the policy change as an instrument for pretrial release. The IV specifications are listed below:

$$PR_{ict} = \alpha + \pi_1(Eligible \times Post)_{ct} + \gamma_{ct} + \mu_c + \lambda_t + X_i\theta + \epsilon_{ict} \tag{4}$$

$$Y_{ict} = \alpha + \psi_1\widehat{\text{PR}}_{ct} + \gamma_{ct} + \mu_c + \lambda_t + \theta X_i + \epsilon_{ict} \tag{5}$$

Equation 4 is the first stage equation which augments Equation 2 by replacing Y_ict with PR_{ict}, which represents Pretrial Release. Therefore, Equation 4 estimates the impact of the policy reform on pretrial release. Equation 5 is the second stage equation which augments Equation 2 by replacing $\pi_1(Eligible \times Post)_{ct}$ with $\psi_1\widehat{\text{PR}}_{ct}$ where $\psi_1\widehat{\text{PR}}_{ct}$ represents the estimate of Pretrial Release derived from Equation 4. Therefore, Equation 5 estimates the impact of pretrial release on defendant outcomes.

These IV results are shown in Table 9. The first-stage results reported in Panel A indicate that instrument strength varies substantially across outcomes. Except for conviction ($F = 10.2$) and guilty plea ($F = 19.2$), the first-stage F-statistics fall well below the conventional threshold of 10 for reliable identification. As a result, the corresponding 2SLS estimates in Panel B for sentence length, incarceration, FTA, and rearrest should be interpreted with caution, as weak instruments can produce biased and unstable Local Average Treatment Effects (LATE).

As a robustness check, I re-estimate the IV models on a balanced sample of defendants with non-missing observations across all outcomes, with the exception of sentence length, which is necessarily restricted to incarcerated defendants. These results are shown in Table A19. The first-stage F-statistics are higher and more stable across outcomes in this specification ($F = 14.47$ for all outcomes except sentence length), comfortably clearing the conventional threshold of 10 typically used to indicate a reasonably strong instrument; sentence length remains the exception ($F = 1.87$), continuing to reflect a weak first stage in the smaller, incarceration-conditional sample. Despite this improvement in instrument strength, several of the 2SLS point estimates in Panel B exceed what is mechanically plausible for binary outcomes, suggesting that even with a stronger first stage, the LATE estimates remain imprecise and should be interpreted with caution rather than taken as

reliable point estimates of effect magnitude.

6 Benefit-Cost Analysis

To translate the estimated increase in pretrial release into a fiscal benefit, I calculate the person-days of pretrial detention avoided due to the repeal, valued at Virginia's average jail operating cost per inmate-day. The benefit is given by:

$$\text{Benefit} = \Delta Pr(\text{Released}) \times N_{\text{eligible}} \times \overline{D} \times C \tag{6}$$

where $\Delta Pr(\text{Released})$ is the estimated increase in the probability of pretrial release, N_{eligible} is the number of PDoB-eligible defendants, $\overline{D}$ is the average number of additional days a defendant would have spent in detention absent the reform, and C is the average operating cost per inmate-day.

Release-rate increase. I use the DiD estimate from Table 2 (Panel A, column 4), which indicates that the repeal increased the probability of any pretrial release among eligible defendants by 5.2 percentage points ($\Delta Pr(\text{Released}) = 0.052$).

Eligible population. The treated (PDoB-eligible) sample comprises 114,851 defendant-observations across the full 2010–2024 sample period, drawn directly from my own court-records data. Annualizing over this 15-year period yields approximately 7,657 eligible defendants per year.

Detention duration avoided. I draw on BenYishay, 2024, who uses Virginia Pretrial Data Project microdata to estimate the causal effect of presumptions against bail on detention length, controlling for offense category, locality, court type, demographics, and criminal history. Among all defendants charged with a jailable offense—including those never released, for whom duration is imputed as the time between the initial contact event and case disposition—defendants subject to a presumption against bail were held for an average of 85 days, compared to 27 days for otherwise similar defendants not subject to a presumption, a difference of 58 days attributable to the presumption itself. I use this 58-day estimate as $\overline{D}$, the counterfactual reduction in detention length for defendants affected by the repeal.

Per-diem cost. I value each avoided detention-day at Virginia's average jail operating cost per inmate-day in FY2021 ($107.09), the fiscal year in which the reform took effect, as

reported by the Virginia Compensation Board's Annual Jail Revenues and Expenditures Report (Virginia Compensation Board, 2022).

Calculation. Combining these estimates:

$$\text{Annual Benefit} = 0.052 \times 7{,}657 \times 58 \times \$107.09 \approx \$2{,}472{,}998 \tag{7}$$

$$\text{Total Benefit (2010–2024)} = 0.052 \times 114{,}851 \times 58 \times \$107.09 \approx \$37{,}094{,}971 \tag{8}$$

This implies an average fiscal saving of approximately \$6,211 per defendant released as a result of the reform ($58 \times \$107.09$), and an aggregate annual saving to Virginia localities and the Commonwealth of roughly \$2.47 million.

Limitations. Several caveats apply. First, the 58-day estimate is drawn from a distinct sample (October 2017 defendants subject to presumptions against bail under the pre-repeal statute) rather than from my own post-reform data, and should be understood as an external benchmark rather than a design-based estimate specific to this study's sample. Second, I apply a single FY2021 per-diem cost across the full sample period, though jail operating costs rose substantially in subsequent years (to \$151.38 by FY2024); using FY2021 costs throughout yields a conservative (lower-bound) estimate of fiscal savings. Third, this calculation captures only direct jail operating costs and does not incorporate other potential benefits (e.g., avoided earnings losses, the private value of liberty to defendants) or offsetting costs (e.g., any increase in pretrial supervision expenditures), which I discuss qualitatively above.

7 Conclusion

This paper examines the causal effects of Virginia's 2021 repeal of its Presumptive Denial of Bail statute, using a difference-in-differences design that compares defendants charged with offenses subject to the presumption against those charged with unaffected offenses, before and after the reform. Unlike much of the existing literature on pretrial detention, which relies on quasi-random variation in judges' release tendencies to identify effects at the margin of an individual judge's discretion, this setting isolates variation at the extensive margin of eligibility itself: defendants who were categorically denied individualized consideration for release, rather than defendants near the margin of a particular judge's leniency. This distinction matters substantively, since the PDoB-eligible population is composed of defendants charged with violent and serious offenses, a group

largely absent from a bail-reform literature concentrated on lower-level and nonviolent charges.

The results of this paper show that eliminating charge-based presumptions increased pretrial release by 5.2 percentage points, shifted release away from monetary bail, and reduced guilty pleas and incarceration without significantly affecting convictions. The reform did not increase failures to appear or short-run rearrest, although longer-run rearrest rose modestly after case disposition. The results also show that although Black defendants experienced larger increases in release, improvement in case outcomes was greater among White defendants. When these findings are considered alongside the benefit-cost analysis in Section 6, the repeal appears to have delivered both fiscal and legal benefits. In particular, it generated an estimated $2.47 million in annual jail operating savings while improving case outcomes for defendants previously subject to charge-based restrictions on release.

These findings suggest that removing charge-based presumptions can reduce reliance on pretrial detention and monetary bail while improving some case and sentencing outcomes without compromising short-term public safety. However, the unequal downstream benefits across racial groups and the modest increase in longer-run rearrest indicate that release reform alone may be insufficient. Policymakers should pair such reforms with attention to racial disparities in plea bargaining and sentencing, as well as continued support and supervision after case disposition.

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8 Figures and tables

8.1 Figures

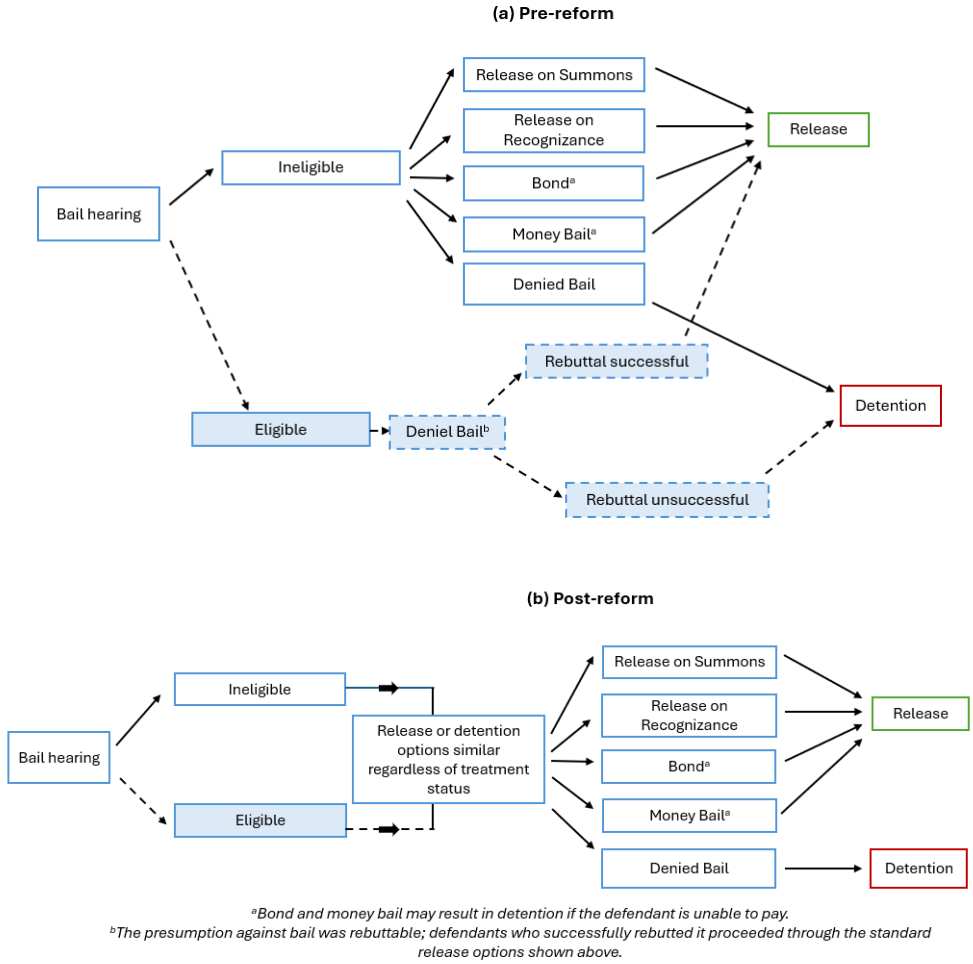


Figure 1: The Pretrial Process in Virginia before and after the Bail Reform

Notes: "Eligible" refers to defendants whose first case includes a charge subject to Virginia's Presumptive Denial of Bail (PDoB) statute prior to its repeal; "Ineligible" refers to defendants whose charges were not subject to the presumption. Panel (a) of Figure 1 shows that prior to the bail reform, cases subject to a presumption against bail were presumptively detained unless the defendant successfully rebutted the presumption, while ineligible cases had four release options: release on summons, release on recognizance, release on bond, and release on money bail, with a chance of detention if the defendant was assigned to bond or money bail and was unable to pay. However, panel (b) of Figure 1 shows that after the reform, eligible and ineligible cases were subject to the same release or detention options, with the outcome depending on the judge's determination.

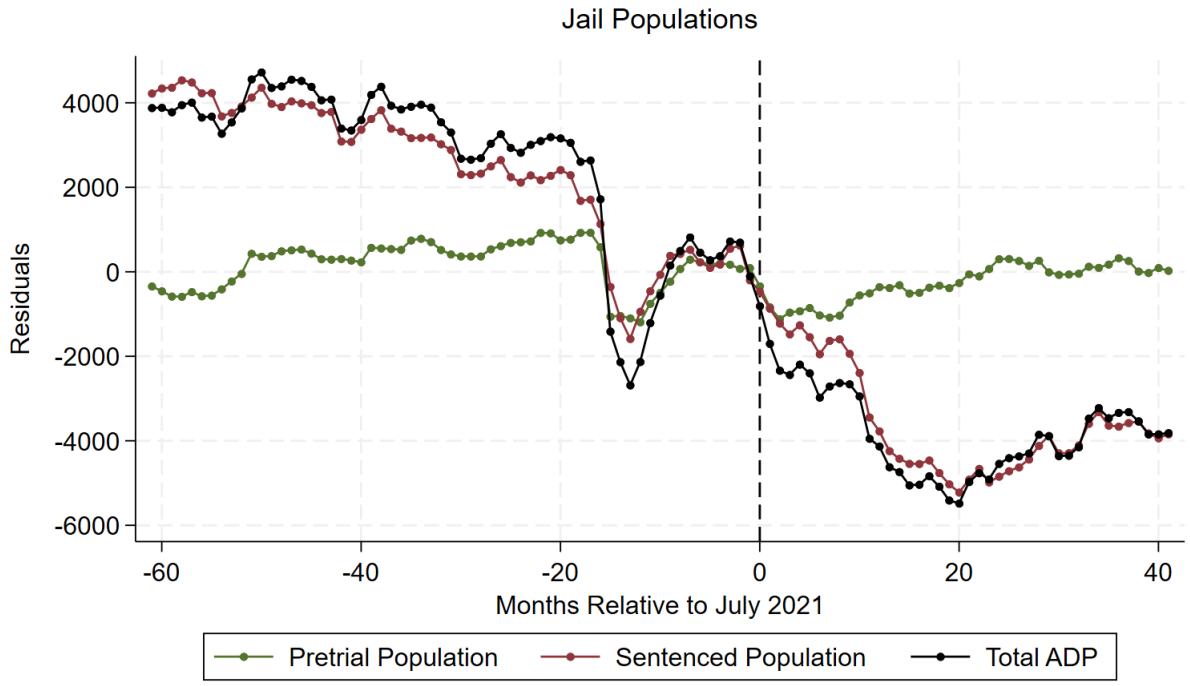


Figure 2: Trends of Jail Populations In Virginia

Notes: Each point in the figure represents the seasonally-adjusted monthly jail population aggregated across all Virginia jails, after removing month-of-year fixed effects. The vertical dashed line marks July 2021, the implementation date of the repeal of presumptions against bail. Data for Figure 2 are drawn from the Virginia Compensation Board's Local Inmate Data System (LIDS) Average Daily Population (ADP) Reports for the years 2016–2024 [Virginia State Compensation Board, 2025]. The reports provide monthly jail population counts for all Virginia jails, categorized by conviction status and jail type. Federal jail populations and out-of-state inmates are excluded from the analysis. The reports provide monthly counts of the total pretrial population for each jail. I constructed the total average daily population as the summation of the local-responsible (convicted population with 1 year or less sentences and which also contains the pretrial population) and the state-responsible (convicted population with more than 1 year sentences). I then construct the total convicted population as the average daily population minus the pretrial population.

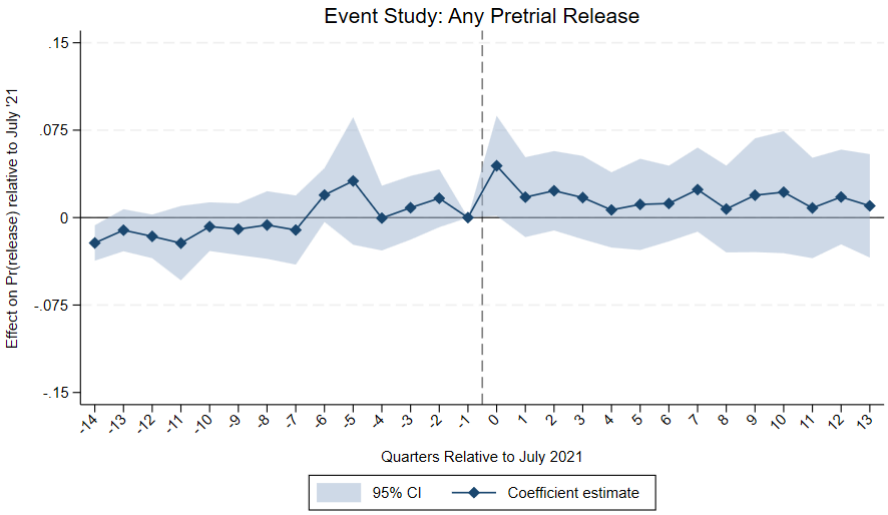


Figure 3: Dynamic Impact of PDoB revocation on Pretrial Release

Notes: Figure 3 plots difference-in-differences estimates from Equation 3, using quarterly leads and lags to estimate effects relative to the quarter immediately preceding treatment (April to June). Vertical solid bars represent 95 percent confidence intervals. The treated group consists of individuals facing at least one charge that would have triggered a presumption against bail under the pre-reform regime; the control group consists of individuals whose charges would not have triggered such a presumption. The vertical dashed line marks the omitted period.

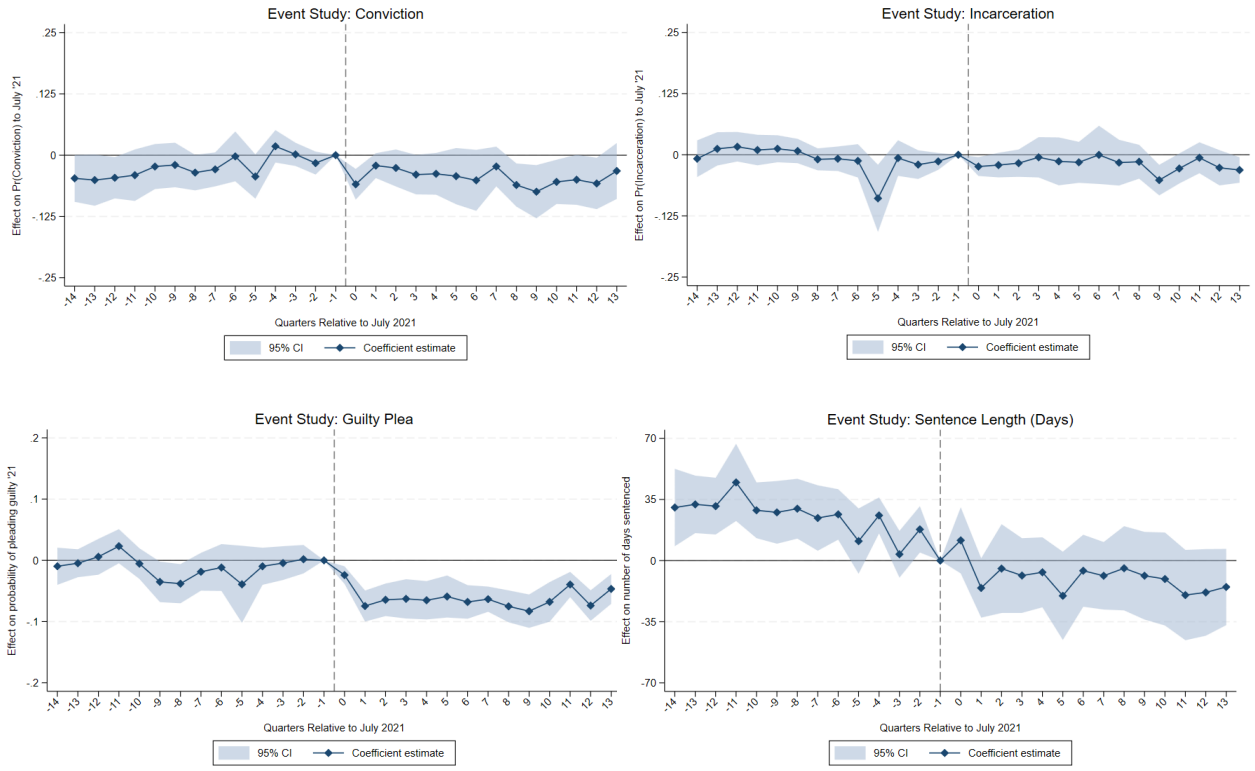


Figure 4: Dynamic Impact of PDoB revocation on Case Outcomes

Notes: Figure 4 plots difference-in-differences estimates from Equation 3, using quarterly leads and lags to estimate effects relative to the quarter immediately preceding treatment (April to June). Vertical solid bars represent 95 percent confidence intervals. The treated group consists of individuals facing at least one charge that would have triggered a presumption against bail under the pre-reform regime; the control group consists of individuals whose charges would not have triggered such a presumption. The vertical dashed line marks the omitted period.

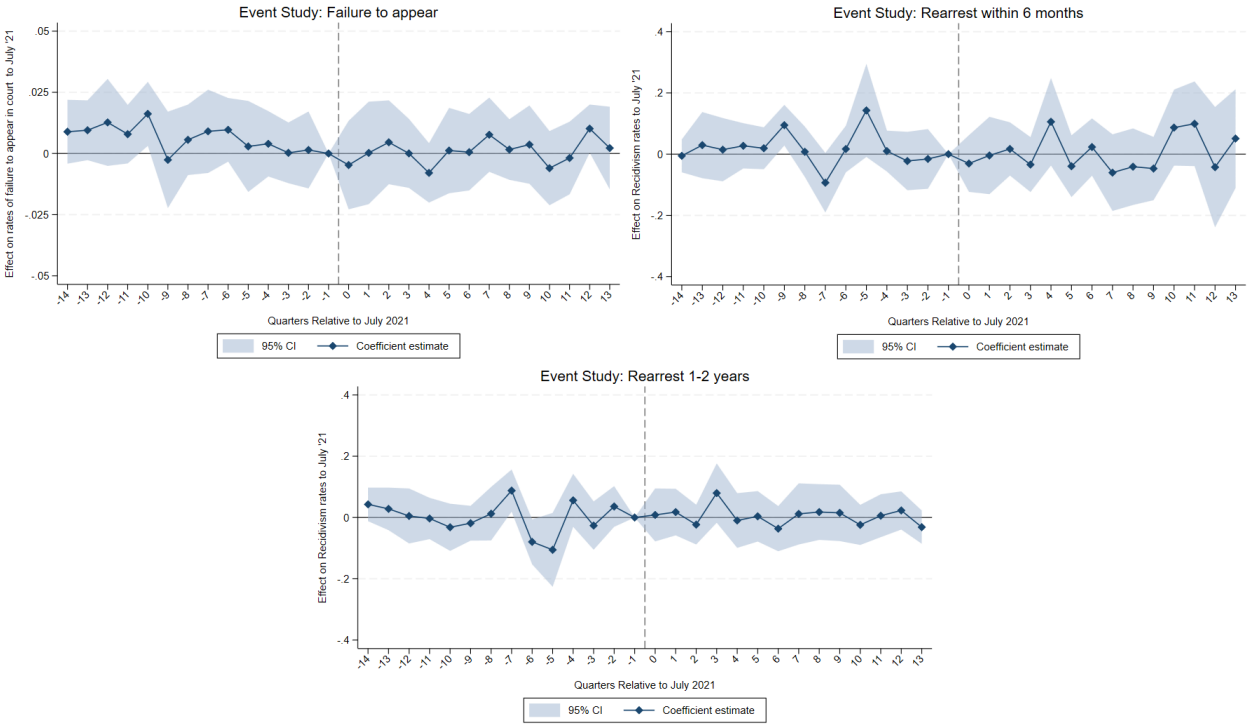


Figure 5: Dynamic Impact of PDoB revocation on Pretrial Misconduct and Recidivism

Notes: Figure 5 plots difference-in-differences estimates from Equation 3, using quarterly leads and lags to estimate effects relative to the quarter immediately preceding treatment (April to June). Vertical solid bars represent 95 percent confidence intervals. The treated group consists of individuals facing at least one charge that would have triggered a presumption against bail under the pre-reform regime; the control group consists of individuals whose charges would not have triggered such a presumption. The vertical dashed line marks the omitted period.

8.2 Tables

Table 1: Descriptive Statistics by Eligibility (Treatment) Status prior to PDoB repeal

	Full Sample		Eligible		Ineligible	
	Mean	Obs.	Mean	Obs.	Mean	Obs.
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Pretrial Release Types						
Released (agg.)	0.977	3197490	0.812	95225	0.982	3102265
Released on Bail	0.036	3197490	0.315	95225	0.028	3102265
Released on Bond	0.001	3197490	0.008	95225	0.001	3102265
Relased on Recognizance	0.053	3197490	0.439	95225	0.041	3102265
Released on Summons	0.904	3197490	0.284	95225	0.923	3102265
Panel B: Defendant Characteristics						
Female	0.385	3197490	0.231	95225	0.390	3102265
Black	0.262	3103355	0.299	93715	0.261	3009640
Hispanic	0.038	3103355	0.011	93715	0.039	3009640
White	0.661	3103355	0.668	93715	0.661	3009640
Median Household Income in zip code	76505	2610126	69599	75205	76710	2534921
Public Defender	0.026	3197490	0.119	95225	0.023	3102265
Panel C: Charge Characteristics						
Felony Offense	0.035	3197490	0.247	95225	0.028	3102265
Any DUI offense	0.022	3197490	0.701	95225	0.001	3102265
Any drug offense	0.028	3197490	0.031	95225	0.028	3102265
Any violent crime	0.014	3197490	0.117	95225	0.011	3102265
Any property crime	0.026	3197490	0.071	95225	0.025	3102265
Number of offenses	1.204	3197490	2.123	95225	1.176	3102265
Panel D: Case Outcomes						
Convicted	0.890	1578445	0.818	82538	0.893	1495907
Guilty plea	0.771	524300	0.819	39718	0.767	484582
Sentence length (in days)	147.35	205783	166.28	63634	138.87	142149
Any incarceration	0.064	3197490	0.668	95225	0.046	3102265
Panel E: Pretrial Misconduct & Recidivism						
Failure to appear in court	0.011	3197490	0.024	95225	0.011	3102265
Rearrest in 0-6 months	0.226	109668	0.180	24320	0.239	85348
Rearrest 1-2 years	0.175	109668	0.176	24320	0.174	85348

Notes: This table presents descriptive statistics for both the full sample of defendants and statistics by treatment status. "Eligible" refers to the treated group and "ineligible" to the control group. Data for all variables except the median household income in the zip code are derived from court records from the Virginia Court System from 2010 to 2024. The Median Household Income in Zip Code (in the past 12 months in 2024 inflation-adjusted dollars) was derived from the US Census Bureau, which is available from 2011 to 2024.

Table 2: Difference-in-Differences Estimates of the Effect of Revoking PDoB on Pretrial Release

	Eligible Mean	DID results		
	(1)	(2)	(3)	(4)
Panel A. Pretrial Release (Aggregate)				
Any Pretrial Release	0.812	0.050**	0.051***	0.052***
	[0.391]	(0.019)	(0.017)	(0.017)
Panel B. Pretrial Release Types				
Money Bail	0.315	-0.286***	-0.287***	-0.287***
	[0.465]	(0.014)	(0.014)	(0.015)
Bond	0.008	0.259***	0.251***	0.247***
	[0.088]	(0.011)	(0.011)	(0.012)
Recognizance	0.439	0.074***	0.067***	0.067***
	[0.496]	(0.020)	(0.017)	(0.016)
Summons	0.284	0.033***	0.032***	0.037***
	[0.451]	(0.011)	(0.011)	(0.011)
Fixed effects	—	No	Yes	Yes
Controls	—	No	No	Yes
Observations	95,225	3,851,082	3,851,075	3,705,897

Notes: This table presents the impact of the policy reform on pretrial release. Panel A presents results for aggregate pretrial release; Panel B presents results by release type. Column 1 reports the pre-reform mean for the treated group, with standard deviations in brackets. Column 2 reports estimates of β_1 from Equation 1. Column 3 reports estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Column 4 adds controls for defendant race and gender, public defender representation, and judicial circuit. "Eligible" refers to the treated group and "ineligible" to the control group. "Any Pretrial Release" equals one if the defendant was released under any of the four types listed below: "Money Bail" (release with financial conditions), "Bond" (release on secured or unsecured bond or otherwise), "Recognizance" (release on own recognizance), and "Summons" (release on summons). Data are from Virginia court records, 2010–2024. Standard errors, clustered at the offense level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3: Difference-in-Differences Estimates of the Effect of Revoking PDoB on Case Outcomes

	Conviction			Guilty Plea		
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible × *Post 07/21*	-0.063*	-0.041*	-0.032	-0.023**	-0.030***	-0.020**
	(0.035)	(0.023)	(0.024)	(0.010)	(0.009)	(0.008)
Fixed effects	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes
Mean dep. var.	0.818	0.818	0.818	0.819	0.819	0.819
Observations	1,902,158	1,902,152	1,823,681	606,987	606,983	578,121

Notes: This table presents the impact of the policy reform on case outcomes. Columns 1 and 4 report estimates of β_1 from Equation 1. Columns 2 and 5 report estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Columns 3 and 6 add controls for defendant race and gender, public defender representation, and judicial circuit. "Eligible" refers to the treated group and "ineligible" to the control group. "Conviction" equals one if convicted on any charge. "Guilty Plea" equals one if the defendant pled guilty to any charge. Data are from Virginia court records, 2010–2024. Standard errors, clustered at the offense level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: Difference-in-Differences Estimates of the Effect of Revoking PDoB on Sentencing Outcomes

	Sentence length (Days)			Incarcerated		
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible × *Post 07/21*	-18.043	-15.938	-17.369	-0.080***	-0.077***	-0.078***
	(19.656)	(14.859)	(13.851)	(0.023)	(0.019)	(0.020)
Fixed effects	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes
Mean dep. var.	166.283	166.283	166.283	0.668	0.668	0.668
Observations	235,882	235,878	231,213	3,851,082	3,851,075	3,705,897

Notes: This table presents the impact of the policy reform on sentencing outcomes. Columns 1 and 4 report estimates of β_1 from Equation 1. Column 2 and 5 report estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Columns 3 and 6 add controls for defendant race and gender, public defender representation, and judicial circuit. "Eligible" refers to the treated group and "ineligible" to the control group. "Sentence Length" measures days of incarceration following conviction. "Incarcerated" equals one if the defendant received any custodial sentence. Data are from Virginia court records, 2010–2024. Standard errors, clustered at the offense level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: Difference-in-Differences Estimates of the Effect of Revoking PDoB on Pretrial Misconduct and Recidivism

	Failure to appear			Rearrest within 6 months			Rearrest from 1 - 2 years		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Eligible × *Post 07/21*	0.011***	0.001	0.000	-0.045*	-0.038	-0.038	0.027**	0.026**	0.025**
	(0.003)	(0.003)	(0.003)	(0.026)	(0.023)	(0.023)	(0.011)	(0.011)	(0.010)
Fixed effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes	No	No	Yes
Mean dep. var.	0.024	0.024	0.024	0.180	0.180	0.180	0.176	0.176	0.176
Observations	3,851,082	3,851,075	3,705,897	118,113	118,108	116,987	118,113	118,108	116,987

Notes: This table presents the impact of the policy reform on pretrial misconduct and recidivism. Column 1, 4 and 7 reports estimates of β_1 from Equation 1. Column 2, 5 and 8 reports estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Column 3, 6 and 9 adds controls for defendant race and gender, public defender representation, and judicial circuit. "Eligible" refers to the treated group and "ineligible" to the control group. "Failure to Appear" equals one if the defendant misses any scheduled court date. "Rearrest within 6 Months" equals one if the defendant is arrested within 6 months of the hearing date of the first observed case. "Rearrest from 1–2 Years" equals one if arrested 1 to 2 years after the hearing date of the first observed case. Data are from Virginia court records, 2010–2024. Standard errors, clustered at the offense level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6: Difference-in-Differences Estimates of the Effect of Revoking PDoB on Pretrial Release, by Defendant Race

	Any Pretrial Release	Recognizance	Money Bail	Bond
	(1)	(2)	(3)	(4)
Panel A. Black, non-Hispanic				
Eligible × *Post 07/21*	0.058**	0.060**	-0.266***	0.239***
	(0.025)	(0.028)	(0.023)	(0.022)
Mean dep. var.	0.720	0.360	0.314	0.008
Observations	984,372	984,372	984,372	984,372
Panel B. White, non-Hispanic				
Eligible × *Post 07/21*	0.046***	0.068***	-0.296***	0.250***
	(0.013)	(0.012)	(0.012)	(0.008)
Mean dep. var.	0.850	0.473	0.317	0.008
Observations	2,459,456	2,459,456	2,459,456	2,459,456
Fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: This table presents the impact of the policy reform on pretrial release broken down by defendant race. Columns 1-4 present estimates of ϕ_1 from Equation 2 including controls (defendant race and gender, public defender representation, and judicial circuit). "Eligible" refers to the treated group and "ineligible" to the control group. "Any Pretrial Release" equals one if the defendant was released under any of the four types listed below: "Money Bail" (release with financial conditions), "Bond" (release on secured or unsecured bond or otherwise), "Recognizance" (release on own recognizance), and "Summons" (release on summons). Data are from Virginia court records, 2010–2024. Standard errors, clustered at the offense level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7: Difference-in-Differences Estimates of the Effect of Revoking PDoB on Case and Sentencing Outcomes, by Defendant Race

	Conviction	Guilty Plea	Sentence length (Days)	Incarcerated
	(1)	(2)	(3)	(4)
Panel A. Black, non-Hispanic				
Eligible × Post 07/21	-0.041	0.002	-16.487	-0.063***
	(0.030)	(0.019)	(15.411)	(0.022)
Mean dep. var.	0.703	0.799	192.087	0.526
Observations	600,020	156,393	87,203	984,372
Panel B. White, non-Hispanic				
Eligible × Post 07/21	-0.031	-0.030***	-17.396	-0.082***
	(0.021)	(0.006)	(12.557)	(0.020)
Mean dep. var.	0.861	0.823	160.174	0.728
Observations	1,091,511	366,320	134,854	2,459,456
Fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: This table presents the impact of the policy reform on pretrial release broken down by defendant race. Columns 1-4 present estimates of ϕ_1 from Equation 2 including controls (defendant race and gender, public defender representation, and judicial circuit). "Eligible" refers to the treated group and "ineligible" to the control group. "Conviction" equals one if convicted on any charge. "Guilty Plea" equals one if the defendant pled guilty to any charge. "Sentence Length" measures days of incarceration following conviction. "Incarcerated" equals one if the defendant received any custodial sentence. Data are from Virginia court records, 2010–2024. Standard errors, clustered at the offense level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 8: Difference-in-Differences Estimates of the Effect of Revoking PDoB on Pretrial Misconduct and Recidivism, by Defendant Race

	Failure to appear	Rearrest within 6 months	Rearrest from 1 - 2 years
	(1)	(2)	(3)
Panel A. Black, non-Hispanic			
Eligible × Post 07/21	-0.001	-0.009	0.016
	(0.007)	(0.039)	(0.014)
Mean dep. var.	0.023	0.188	0.171
Observations	984,372	55,046	55,046
Panel B. White, non-Hispanic			
Eligible × Post 07/21	0.001	-0.054***	0.028*
	(0.002)	(0.019)	(0.014)
Mean dep. var.	0.024	0.174	0.179
Observations	2,459,456	60,289	60,289
Fixed effects	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Notes: This table presents the impact of the policy reform on pretrial release broken down by defendant race. Columns 1-4 present estimates of ϕ_1 from Equation 2 including controls (defendant race and gender, public defender representation, and judicial circuit). "Eligible" refers to the treated group and "ineligible" to the control group. "Failure to Appear" equals one if the defendant misses any scheduled court date. "Rearrest within 6 Months" equals one if the defendant is arrested within 6 months of the hearing date of the first observed case. "Rearrest from 1–2 Years" equals one if arrested 1 to 2 years after the hearing date of the first observed case. Data are from Virginia court records, 2010–2024. Standard errors, clustered at the offense level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

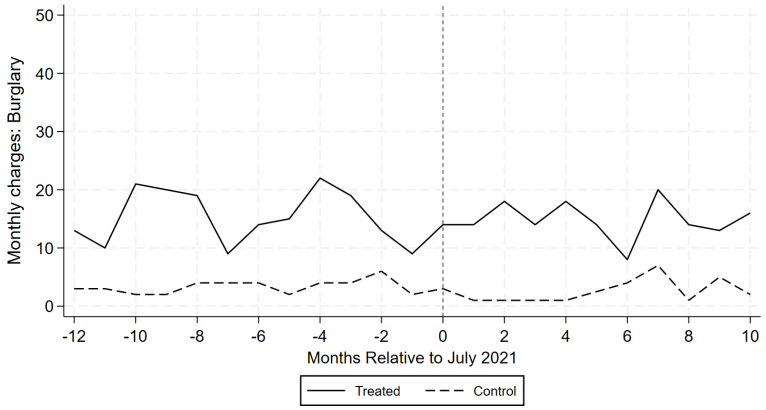
Table 9: IV Estimates of the Effect of Pretrial Release on Case and Recidivism Outcomes

	Case & Sentencing Outcomes				FTA & Recidivism		
	Convicted	Guilty Plea	Sentence Length	Incarcerated	FTA	Rearrest within 6 mths.	Rearrest from 1 -2 years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: First stage results							
Estimate	0.042***	0.029***	0.024	0.052***	0.052***	0.035*	0.035*
	(0.013)	(0.007)	(0.019)	(0.017)	(0.017)	(0.020)	(0.020)
F-statistic	10.2	19.2	1.6	8.9	8.9	2.9	2.9
Panel B: 2SLS results							
Pretrial Release	-0.757*	-0.702**	-713.96**	-1.509***	0.007	-1.086	0.709
	(0.444)	(0.292)	(346.66)	(0.502)	(0.053)	(0.886)	(0.580)
Mean dep. var.	0.818	0.819	166.283	0.668	0.024	0.180	0.176
Observations	1,823,681	578,121	231,213	3,705,897	3,705,897	116,987	116,987
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

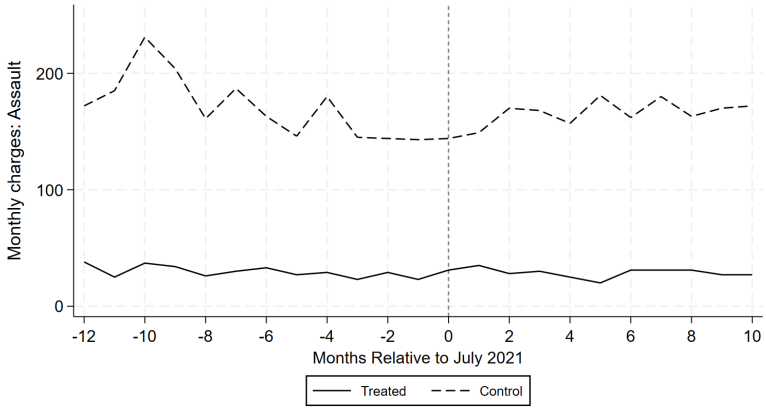
Notes: Notes: This table reports IV estimates of the effect of pretrial release on defendant outcomes, using *Eligible* × *Post 07/21* as an instrument. Panel A reports first-stage estimates of the effect of PDoB revocation on pretrial release; the estimate and F-statistic vary by outcome sample size. Panel B reports IV estimates of pretrial release on outcomes. All regressions include offense-type-by-treatment-status and year-month fixed effects. Controls include defendant race and gender, public defender representation, and judicial circuit. "Conviction" equals one if convicted on any charge. "Guilty Plea" equals one if the defendant pled guilty to any charge. "Sentence Length" measures days of incarceration following conviction. "Incarcerated" equals one if the defendant received any custodial sentence. "Failure to Appear" equals one if the defendant misses any scheduled court date. "Rearrest within 6 Months" equals one if the defendant is arrested within 6 months of the hearing date of the first observed case. "Rearrest from 1–2 Years" equals one if arrested 1 to 2 years after the hearing date of the first observed case. Data are from Virginia court records, 2010–2024. Standard errors, clustered at the offense level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

A. Appendix: Supplementary Figures and Tables

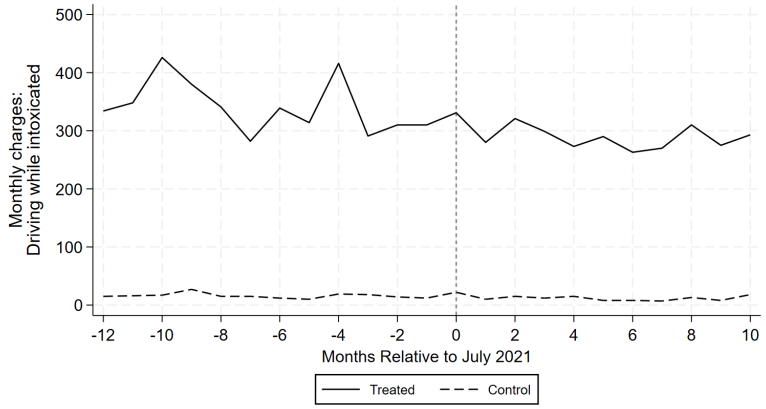
A.1. Supplementary Figures



(a) Burglary



(b) Assault



(c) Driving while intoxicated (DWI)

Figure A1: Trends in monthly charges by treatment status and offense

Notes: This figure plots monthly charge volume for treated and control offense categories. The vertical line marks July 2021, the implementation date of Virginia's presumption against bail (PDoB) repeal. Data are from Virginia court records, 2010–2024.

A.2. Supplementary Tables

Table A1: Presumptive Denial of Bail (PDoB) Clauses Under Pre-Reform §19.2-120

Clause	Statute(s)	Offense	Prior Conviction Required[a]
1	§19.2-297.1	Acts of violence (ex. First and second-degree murder, voluntary manslaughter, Malicious felonious assault, criminal sexual assault etc.)	✓
2	–	Offense with maximum sentence of life or death	
3	§§18.2-248, 248.01, 255, 255.2	Manufacture/distribution/transport of Sched. I/II drugs	✓
4	§§18.2-308.1, 308.2, 308.4	Firearm possession with mandatory minimum requirement	
5	(refs. cl. 1, 2)	Any felony, if person has been convicted of two or more offenses from clause 1 or 2	✓
6	–	Felony committed on release pending prior felony trial/sentence/appeal	[b]
7	§18.2-67.5:2	Felony sexual assault	✓
8	§§18.2-374.1, 374.3	Child pornography / solicitation of a minor	
9	§§18.2-46.2, 46.3, 46.5, 46.7	Street gang, prostitution, sex trafficking	
10	§§18.2-36.1, 51.4, 266; 46.2-341.24	DUI-related manslaughter/maiming/DUI	✓
11	§§16.1-253.2, 18.2-60.4	Protective order violation	✓[c]
12	§18.2-57.2(B)	Assault/battery, family or household member	✓
13	§18.2-460(C)	Obstructing justice via threats to a witness	
14	§18.2-51.6	Strangulation of family or household member	
15	§§18.2-355, 356, 357, 357.1	Human trafficking / commercial sex trafficking	
16	§19.2-81.6 / §19.2-120.1	Arrest of illegal aliens	

Notes: Clauses 1–15 follow the enumeration in SB1266's bill text under pre-reform §19.2-120. Clause 16 refers to the separate presumption under §19.2-81.6 and §19.2-120.1, repealed by the same bill. A checkmark indicates the clause's applicability is conditioned on the defendant's prior record rather than solely on the offense currently charged.

[a] "Prior record" here includes both prior convictions and, for clause 11, a prior violation.

[b] Clause 6 conditions on a *pending* prior felony charge (release status), not a prior *conviction*, and is therefore excluded from the conviction-dependent count used in the drop-and-reestimate (Tables A5–A8).

[c] Clause 11 requires a prior *violation*, not a prior *conviction*; included here for completeness given its repeat-offense structure.

Table A2: Difference-in-Differences Estimates of the Effect of PDoB Revocation on Pretrial Release (excluding marijuana-related offenses)

	Eligible Mean	DID results		
	(1)	(2)	(3)	(4)
Panel A. Pretrial Release (Aggregate)				
Any Pretrial Release	0.813	0.049**	0.049***	0.050***
	[0.390]	(0.019)	(0.017)	(0.017)
Panel B. Pretrial Release Types				
Money Bail	0.315	-0.286***	-0.287***	-0.287***
	[0.464]	(0.014)	(0.014)	(0.015)
Bond	0.008	0.255***	0.246***	0.243***
	[0.088]	(0.010)	(0.011)	(0.012)
Recognizance	0.441	0.075***	0.069***	0.068***
	[0.497]	(0.021)	(0.017)	(0.017)
Summons	0.285	0.035***	0.033***	0.038***
	[0.452]	(0.010)	(0.012)	(0.012)
Offense-type-by-treatment-status fixed effects	—	No	Yes	Yes
Year-month fixed effects	—	No	Yes	Yes
Controls	—	No	No	Yes
Observations	94,639	3,849,693	3,849,686	3,704,542

Notes: This table presents the impact of the policy reform on pretrial release, excluding defendants charged with marijuana-related offenses. Panel A presents results for aggregate pretrial release; Panel B presents results by release type. Column 1 reports the pre-reform mean for the treated group, with standard deviations in brackets. Column 2 reports estimates of β_1 from Equation 1. Column 3 reports estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Column 4 adds controls for defendant race and gender, public defender representation, and judicial circuit. "Eligible" refers to the treated group and "ineligible" to the control group. "Any Pretrial Release" equals one if the defendant was released under any of the four types listed below: "Money Bail" (release with financial conditions), "Bond" (release on secured or unsecured bond or otherwise), "Recognizance" (release on own recognizance), and "Summons" (release on summons). Data are from Virginia court records, 2010–2024. Standard errors, clustered at the offense level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A3: Difference-in-Differences Estimates of the Effect of PDoB Revocation on Case Outcomes (excluding marijuana-related offenses)

	Conviction			Guilty Plea		
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible x Post 07/21	-0.060*	-0.038*	-0.030	-0.023**	-0.030***	-0.020**
	(0.033)	(0.022)	(0.023)	(0.010)	(0.009)	(0.008)
Fixed effects	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes
Mean dep. var.	0.822	0.822	0.822	0.819	0.819	0.819
Observations	1,901,136	1,901,130	1,822,679	606,904	606,900	578,039

Notes: This table presents the impact of the policy reform on case outcomes, excluding defendants charged with marijuana-related offenses. Column 1 and 4 report estimates of β_1 from Equation 1. Column 2 and 5 report estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Columns 3 and 6 add controls for defendant race and gender, public defender representation, and judicial circuit. "Eligible" refers to the treated group and "ineligible" to the control group. "Conviction" equals one if convicted on any charge. "Guilty Plea" equals one if the defendant pled guilty to any charge. Data are from Virginia court records, 2010–2024. Standard errors, clustered at the offense level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A4: Difference-in-Differences Estimates of the Effect of PDoB Revocation on Sentencing Outcomes (excluding marijuana-related offenses)

	Sentence length (Days)			Incarcerated		
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible x Post 07/21	-18.191	-16.024	-17.442	-0.078***	-0.077***	-0.078***
	(19.670)	(14.874)	(13.870)	(0.023)	(0.019)	(0.020)
Fixed effects	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes
Mean dep. var.	166.261	166.2614	166.2614	0.672	0.672	0.672
Observations	235,750	235,746	231,083	3,849,693	3,849,686	3,704,542

Notes: This table presents the impact of the policy reform on case outcomes, excluding defendants charged with marijuana-related offenses. Column 1 and 4 report estimates of β_1 from Equation 1. Column 2 and 5 report estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Columns 3 and 6 add controls for defendant race and gender, public defender representation, and judicial circuit. "Eligible" refers to the treated group and "ineligible" to the control group. "Sentence Length" measures days of incarceration following conviction. "Incarcerated" equals one if the defendant received any custodial sentence. Data are from Virginia court records, 2010–2024. Standard errors, clustered at the offense level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A5: Difference-in-Differences Estimates of the Effect of PDoB Revocation on Pretrial Misconduct and Recidivism (excluding marijuana-related offenses)

	Failure to Appear			Rearrest (0-6 months)			Rearrest (1-2 years)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Eligible x Post 07/21	0.011***	0.001	0.000	-0.046*	-0.038	-0.037	0.028**	0.027**	0.026**
	(0.003)	(0.003)	(0.003)	(0.025)	(0.024)	(0.023)	(0.011)	(0.011)	(0.010)
Offense-type-by-treatment-status fixed effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Year-month fixed effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes	No	No	Yes
Mean dep. var.	0.024	0.024	0.024	0.18	0.18	0.18	0.176	0.176	0.176
Observations	3,849,693	3,849,686	3,704,542	117,797	117,792	116,672	117,797	117,792	116,672

Notes: This table presents the impact of the policy reform on case outcomes, excluding defendants charged with marijuana-related offenses. Column 1, 4 and 7 reports estimates of β_1 from Equation 1. Column 2, 5 and 8 reports estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Column 3, 6 and 9 adds controls for defendant race and gender, public defender representation, and judicial circuit. "Eligible" refers to the treated group and "ineligible" to the control group. "Failure to Appear" equals one if the defendant misses any scheduled court date. "Rearrest within 6 Months" equals one if the defendant is arrested within 6 months of the hearing date of the first observed case. "Rearrest from 1–2 Years" equals one if arrested 1 to 2 years after the hearing date of the first observed case. Data are from Virginia court records, 2010–2024. Standard errors, clustered at the offense level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A6: Behavioral Responses in Arrests and Charges Around PDoB Repeal

	Case upcharged	Case downcharged	Log of charges	Log of arrests
	(1)	(2)	(3)	(4)
Eligible × *Post 07/21*	0.004	-0.002	0.207	-0.141
	(0.003)	(0.001)	(0.162)	(0.217)
Mean dep. var.	0.00004	0.004	8.151	8.578
Observations	3,851,075	3,851,075	384	325

Notes: This table presents estimates of the impact of the policy reform on the percentage of upcharged and downcharged cases, and the volume of charges and arrests. "Case upcharged" equals one if a charge within a case is amended by the court from "eligible" (treated) to "ineligible" (control). "Case downcharged" equals one if a charge within a case is amended by the court from "ineligible" (control) to "eligible" (treated). Log of charges represents monthly charges which are transformed by logging. Log of arrests represents monthly arrests which are transformed by logging. Columns 1-3 use data from Virginia court records, 2010–2024. Column 4 uses data from the National Incident-Based Reporting System (NIBRS). Standard errors are in parentheses. Standard errors are clustered at the offense level for columns 1-2. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A7: Difference-in-Differences Estimates of the Effect of PDoB Revocation on Pretrial Release (excluding summons-released defendats)

	Eligible Mean	DID results		
	(1)	(2)	(3)	(4)
Panel A. Pretrial Release (Aggregate)				
Any Pretrial Release	0.807	0.040	0.033*	0.041**
	[0.395]	(0.025)	(0.019)	(0.018)
Panel B. Pretrial Release Types				
Money Bail	0.32	-0.131***	-0.138***	-0.134***
	[0.467]	(0.024)	(0.023)	(0.024)
Bond	0.008	0.137***	0.135***	0.132***
	[0.089]	(0.018)	(0.017)	(0.017)
Recognizance	0.445	0.003	0.004	0.014
	[0.497]	(0.045)	(0.043)	(0.041)
Fixed effects	—	No	Yes	Yes
Controls	—	No	No	Yes
Observations	92,968	634,248	634,241	620,336

Notes: This table presents the impact of the policy reform on pretrial release, excluding defendants released on summons. Panel A presents results for aggregate pretrial release; Panel B presents results by release type. Column 1 reports the pre-reform mean for the treated group, with standard deviations in brackets. Column 2 reports estimates of β_1 from Equation 1. Column 3 reports estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Column 4 adds controls for defendant race and gender, public defender representation, and judicial circuit. "Eligible" refers to the treated group and "ineligible" to the control group. "Any Pretrial Release" equals one if the defendant was released under any of the four types listed below: "Money Bail" (release with financial conditions), "Bond" (release on secured or unsecured bond or otherwise), "Recognizance" (release on own recognizance), and "Summons" (release on summons). Data are from Virginia court records, 2010–2024. Standard errors, clustered at the offense level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A8: Difference-in-Differences Estimates of the Effect of PDoB Revocation on Case Outcomes (excluding summons-released defendats)

	Conviction			Guilty Plea		
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible × *Post 07/21*	0.038	0.047	0.055	0.054**	0.050**	0.056***
	(0.043)	(0.037)	(0.034)	(0.024)	(0.024)	(0.020)
Fixed effects	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes
Mean dep. var.	0.817	0.817	0.817	0.82	0.82	0.82
Observations	434,284	434,278	425,785	140,089	140,085	137,586

Notes: This table presents the impact of the policy reform on case outcomes, excluding defendants released on summons. Column 1 and 4 reports estimates of β_1 from Equation 1. Column 2 and 5 reports estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Column 3 and 6 adds controls for defendant race and gender, public defender representation, and judicial circuit. "Eligible" refers to the treated group and "ineligible" to the control group. "Conviction" equals one if convicted on any charge. "Guilty Plea" equals one if the defendant pled guilty to any charge. Data are from Virginia court records, 2010–2024. Standard errors, clustered at the offense level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A9: Difference-in-Differences Estimates of the Effect of PDoB Revocation on Sentencing Outcomes (excluding summons-released defendats)

	Sentence length (Days)			Incarcerated		
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible × *Post 07/21*	-26.853***	-25.194***	-29.262***	-0.022	-0.023	-0.021
	(10.151)	(9.396)	(8.155)	(0.042)	(0.033)	(0.032)
Fixed effects	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes
Mean dep. var.	166.579	166.579	166.579	0.666	0.666	0.666
Observations	192,963	192,957	189,884	634,248	634,241	620,336

Notes: This table presents the impact of the policy reform on case outcomes, excluding defendants released on summons. Column 1 and 4 reports estimates of β_1 from Equation 1. Column 2 and 5 reports estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Column 3 and 6 adds controls for defendant race and gender, public defender representation, and judicial circuit. "Eligible" refers to the treated group and "ineligible" to the control group. "Sentence Length" measures days of incarceration following conviction. "Incarcerated" equals one if the defendant received any custodial sentence. Data are from Virginia court records, 2010–2024. Standard errors, clustered at the offense level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A10: Difference-in-Differences Estimates of the Effect of PDoB Revocation on Pretrial Misconduct and Recidivism (excluding summons-released defendats)

	Failure to appear			Rearrest within 6 months			Rearrest from 1 - 2 years		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Eligible × *Post 07/21*	-0.008	-0.015	-0.015	-0.045*	-0.038	-0.038*	0.027**	0.026**	0.025**
	(0.011)	(0.010)	(0.011)	(0.026)	(0.023)	(0.023)	(0.011)	(0.011)	(0.010)
Fixed effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Controls	N	N	Y	N	N	Y	N	N	Y
Mean dep. var.	0.023	0.023	0.023	0.18	0.18	0.18	0.176	0.176	0.176
Observations	634,248	634,241	620,333	118,113	118,108	116,987	118,113	118,108	116,987

Notes: This table presents the impact of the policy reform on case outcomes, excluding defendants released on summons. Column 1, 4 and 7 reports estimates of β_1 from Equation 1. Column 2, 5 and 8 reports estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Column 3, 6 and 9 adds controls for defendant race and gender, public defender representation, and judicial circuit. "Eligible" refers to the treated group and "ineligible" to the control group. "Failure to Appear" equals one if the defendant misses any scheduled court date. "Rearrest within 6 Months" equals one if the defendant is arrested within 6 months of the hearing date of the first observed case. "Rearrest from 1–2 Years" equals one if arrested 1 to 2 years after the hearing date of the first observed case. Data are from Virginia court records, 2010–2024. Standard errors, clustered at the offense level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A11: Difference-in-Differences Estimates Using Statutory Clauses Not Requiring Prior Criminal History: Pretrial Release

	Eligible Mean	DID results		
	(1)	(2)	(3)	(4)
Panel A. Pretrial Release (Aggregate)				
Any Pretrial Release	0.354	0.180***	0.092*	0.095**
	[0.478]	(0.020)	(0.050)	(0.048)
Panel B. Pretrial Release Types				
Money Bail	0.220	-0.193***	-0.230***	-0.230***
	[0.415]	(0.041)	(0.031)	(0.033)
Bond	0.015	0.220***	0.203***	0.201***
	[0.121]	(0.027)	(0.026)	(0.026)
Recognizance	0.104	0.151***	0.086**	0.085**
	[0.306]	(0.022)	(0.043)	(0.043)
Summons	0.029	0.023**	0.019	0.024
	[0.167]	(0.009)	(0.018)	(0.016)
Fxed effects	—	No	Yes	Yes
Controls	—	No	No	Yes
Observations	2,436	3,739,706	3,739,701	3,596,645

Notes: This table presents the impact of the policy reform on pretrial release, excluding cases where either of the charges fall under those in the legislation that would require defendant criminal history as an extra requirement to be considered treated. Panel A presents results for aggregate pretrial release; Panel B presents results by release type. Column 1 reports the pre-reform mean for the treated group, with standard deviations in brackets. Column 2 reports estimates of β_1 from Equation 1. Column 3 reports estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Column 4 adds controls for defendant race and gender, public defender representation, and judicial circuit. "Eligible" refers to the treated group and "ineligible" to the control group. "Any Pretrial Release" equals one if the defendant was released under any of the four types listed below: "Money Bail" (release with financial conditions), "Bond" (release on secured or unsecured bond or otherwise), "Recognizance" (release on own recognizance), and "Summons" (release on summons). Data are from Virginia court records, 2010–2024. Standard errors, clustered at the offense level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A12: Difference-in-Differences Estimates Using Statutory Clauses Not Requiring Prior Criminal History: Case Outcomes

	Conviction			Guilty Plea		
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible × *Post 07/21*	-0.062**	-0.072***	-0.068***	0.111*	0.071	0.087*
	(0.029)	(0.020)	(0.020)	(0.065)	(0.046)	(0.046)
Fxed effects	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes
Mean dep. var.	0.216	0.216	0.216	0.752	0.752	0.752
Observations	1,805,781	1,805,775	1,729,137	561,588	561,582	533,603

Notes: This table presents the impact of the policy reform on case outcomes, excluding cases where either of the charges fall under those in the legislation that would require defendant criminal history as an extra requirement to be considered treated. Column 1 and 4 reports estimates of β_1 from Equation 1. Column 2 and 5 reports estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Column 3 and 6 adds controls for defendant race and gender, public defender representation, and judicial circuit. "Eligible" refers to the treated group and "ineligible" to the control group. "Conviction" equals one if convicted on any charge. "Guilty Plea" equals one if the defendant pled guilty to any charge. Data are from Virginia court records, 2010–2024. Standard errors, clustered at the offense level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A13: Difference-in-Differences Estimates Using Statutory Clauses Not Requiring Prior Criminal History: Sentencing Outcomes

	Sentence length (Days)			Incarcerated		
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible × *Post 07/21*	-19.275	0.310	3.257	-0.014	-0.055***	-0.062***
	(22.447)	(16.332)	(15.084)	(0.021)	(0.017)	(0.017)
Fixed effects	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes
Mean dep. var.	273.81	273.81	273.81	0.101	0.101	0.101
Observations	161,330	161,328	158,138	3,739,706	3,739,701	3,596,645

Notes: This table presents the impact of the policy reform on case outcomes, excluding cases where either of the charges fall under those in the legislation that would require defendant criminal history as an extra requirement to be considered treated. Column 1 and 4 reports estimates of β_1 from Equation 1. Column 2 and 5 reports estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Column 3 and 6 adds controls for defendant race and gender, public defender representation, and judicial circuit. "Eligible" refers to the treated group and "ineligible" to the control group. "Sentence Length" measures days of incarceration following conviction. "Incarcerated" equals one if the defendant received any custodial sentence. Data are from Virginia court records, 2010–2024. Standard errors, clustered at the offense level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A14: Difference-in-Differences Estimates Using Statutory Clauses Not Requiring Prior Criminal History: Pretrial Misconduct and Recidivism

	Failure to Appear			Rearrest (0-6 months)			Rearrest (1-2 years)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Eligible × *Post 07/21*	0.004	-0.019***	-0.021***	0.090**	0.102***	0.090***	-0.084**	-0.066**	-0.062**
	(0.003)	(0.007)	(0.007)	(0.040)	(0.033)	(0.031)	(0.033)	(0.029)	(0.029)
Fixed effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes	No	No	Yes
Mean dep. var.	0.007	0.007	0.007	0.192	0.192	0.192	0.202	0.202	0.202
Observations	3,739,706	3,739,701	3,596,645	92,626	92,621	91,797	92,626	92,621	91,797

Notes: This table presents the impact of the policy reform on case outcomes, excluding cases where either of the charges fall under those in the legislation that would require defendant criminal history as an extra requirement to be considered treated. Column 1, 4 and 7 reports estimates of β_1 from Equation 1. Column 2, 5 and 8 reports estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Column 3, 6 and 9 adds controls for defendant race and gender, public defender representation, and judicial circuit. "Eligible" refers to the treated group and "ineligible" to the control group. "Failure to Appear" equals one if the defendant misses any scheduled court date. "Rearrest within 6 Months" equals one if the defendant is arrested within 6 months of the hearing date of the first observed case. "Rearrest from 1–2 Years" equals one if arrested 1 to 2 years after the hearing date of the first observed case. Data are from Virginia court records, 2010–2024. Standard errors, clustered at the offense level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A15: Difference-in-Differences Estimates the Effect of Revoking PDoB on Pretrial Release (Balanced Sample)

	PDoB Eligible Mean	DID results		
	(1)	(2)	(3)	(4)
Panel A. Pretrial Release (Aggregate)				
Any Pretrial Release	0.937	0.022***	0.023***	0.024***
	[0.243]	(0.006)	(0.006)	(0.006)
Panel B. Pretrial Release Types				
Money Bail	0.394	-0.332***	-0.350***	-0.354***
	[0.489]	(0.026)	(0.026)	(0.025)
Bond	0.005	0.241***	0.240***	0.229***
	[0.071]	(0.015)	(0.015)	(0.016)
Recognizance	0.51	0.106***	0.094***	0.104***
	[0.5]	(0.016)	(0.011)	(0.012)
Summons	0.338	0.039***	0.041**	0.047***
	[0.473]	(0.013)	(0.016)	(0.016)
Offense-type-by-treatment-status fixed effects	—	No	Yes	Yes
Year-month fixed effects	—	No	Yes	Yes
Controls	—	No	No	Yes
Observations	15,762	217,868	217,863	209,776

Notes: This table presents the impact of the policy reform on pretrial release, using the balanced sample across all outcomes except sentence length, which is defined only for incarcerated defendants. Panel A presents results for aggregate pretrial release; Panel B presents results by release type. Column 1 reports the pre-reform mean for the treated group, with standard deviations in brackets. Column 2 reports estimates of β_1 from Equation 1. Column 3 reports estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Column 4 adds controls for defendant race and gender, public defender representation, and judicial circuit. "Eligible" refers to the treated group and "ineligible" to the control group. "Any Pretrial Release" equals one if the defendant was released under any of the four types listed below: "Money Bail" (release with financial conditions), "Bond" (release on secured or unsecured bond or otherwise), "Recognizance" (release on own recognizance), and "Summons" (release on summons). Data are from Virginia court records, 2010–2024. Standard errors, clustered at the offense level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A16: Difference-in-Differences Estimates the Effect of Revoking PDoB on Case Outcomes (Balanced Sample)

	Conviction			Guilty Plea		
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible x Post 07/21	-0.005	-0.006	-0.006**	-0.027***	-0.034***	-0.017**
	(0.004)	(0.004)	(0.003)	(0.010)	(0.009)	(0.008)
Fixed effects	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes
Mean dep. var.	0.983	0.983	0.983	0.821	0.821	0.821
Observations	217,868	217,863	209,776	217,868	217,863	209,776

Notes: This table presents the impact of the policy reform on case outcomes, using the balanced sample across all outcomes except sentence length, which is defined only for incarcerated defendants. Column 1 and 4 reports estimates of β_1 from Equation 1. Column 2 and 5 reports estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Column 3 and 6 adds controls for defendant race and gender, public defender representation, and judicial circuit. "Eligible" refers to the treated group and "ineligible" to the control group. "Conviction" equals one if convicted on any charge. "Guilty Plea" equals one if the defendant pled guilty to any charge. Data are from Virginia court records, 2010–2024. Standard errors, clustered at the offense level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A17: Difference-in-Differences Estimates the Effect of Revoking PDoB on Sentencing Outcomes (Balanced Sample)

	Sentence length (Days)			Incarcerated		
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible x Post 07/21	-28.249	-23.171	-24.182*	0.024	-0.013	-0.032
	(21.224)	(15.331)	(13.582)	(0.034)	(0.024)	(0.024)
Fixed Effects	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes
Mean dep. var.	0.062	0.062	0.062	0.947	0.947	0.947
Observations	52,539	52,534	51,725	217,868	217,863	209,776

Notes: This table presents the impact of the policy reform on sentencing outcomes, using the balanced sample across all outcomes except sentence length, which is defined only for incarcerated defendants. Column 1 and 4 reports estimates of β_1 from Equation 1. Column 2 and 5 reports estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Column 3 and 6 adds controls for defendant race and gender, public defender representation, and judicial circuit. "Eligible" refers to the treated group and "ineligible" to the control group. "Sentence Length" measures days of incarceration following conviction. "Incarcerated" equals one if the defendant received any custodial sentence. Data are from Virginia court records, 2010–2024. Standard errors, clustered at the offense level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A18: Difference-in-Differences Estimates the Effect of Revoking PDoB on Pretrial Misconduct and Recidivism (Balanced Sample)

	Failure to Appear			Rearrest (0-6 months)			Rearrest (1-2 years)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Eligible x Post 07/21	0.012	0.000	-0.000	0.006	-0.003	-0.016*	-0.035***	-0.027***	-0.025***
	(0.008)	(0.006)	(0.007)	(0.010)	(0.007)	(0.008)	(0.011)	(0.008)	(0.008)
Fixed effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes	No	No	Yes
Mean dep. var.	0.026	0.026	0.026	0.236	0.236	0.236	0.174	0.174	0.174
Observations	217,868	217,863	209,776	209,776	217,868	217,863	209,776	217,868	217,863

Notes: This table presents the impact of the policy reform on pretrial misconduct and recidivism, using the balanced sample across all outcomes except sentence length, which is defined only for incarcerated defendants. Column 1, 4 and 7 reports estimates of β_1 from Equation 1. Column 2, 5 and 8 reports estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Column 3, 6 and 9 adds controls for defendant race and gender, public defender representation, and judicial circuit. "Eligible" refers to the treated group and "ineligible" to the control group. "Failure to Appear" equals one if the defendant misses any scheduled court date. "Rearrest within 6 Months" equals one if the defendant is arrested within 6 months of the hearing date of the first observed case. "Rearrest from 1–2 Years" equals one if arrested 1 to 2 years after the hearing date of the first observed case. Data are from Virginia court records, 2010–2024. Standard errors, clustered at the offense level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A19: IV Estimates of the Effect of Pretrial Release on Case and Recidivism Outcomes (Balanced Sample)

	Case & Sentencing Outcomes				FTA & Recidivism		
	Convicted	Guilty Plea	Sentence Length	Incar-cerated	FTA	Rearrest within 6 months	Rearrest from 1-2 years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: First stage results							
Estimate	0.024***	0.024***	0.03	0.024***	0.024***	0.024***	0.024***
	(0.006)	(0.006)	(0.022)	(0.006)	(0.006)	(0.006)	(0.006)
F-statistic	14.47	14.47	1.87	14.47	14.47	14.47	14.47
Panel B: 2SLS results							
Pretrial Release	-0.265	-0.725*	-815.353**	-1.318	-0.006*	-0.665*	-1.061**
	(0.160)	(0.382)	(371.613)	(1.111)	(0.297)	(0.374)	(0.471)
Mean dep. Var.	0.983	0.821	141.678	0.947	0.026	0.236	0.174
Observations	209,776	209,776	51,725	209,776	209,776	209,776	209,776
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Notes: This table reports IV estimates of the effect of pretrial release on defendant outcomes, using the balanced sample of defendants with non-missing observations across all outcomes; the exception is sentence length, which is defined only for incarcerated defendants and is therefore missing for those not incarcerated. This estimation uses *Eligible* × *Post 07/21* as an instrument for pretrial release. Panel A reports first-stage estimates of the effect of PDoB revocation on pretrial release; the estimate and F-statistic vary by outcome sample size. Panel B reports IV estimates of pretrial release on outcomes. All regressions include offense-type-by-treatment-status and year-month fixed effects. Controls include defendant race and gender, public defender representation, and judicial circuit. "Conviction" equals one if convicted on any charge. "Guilty Plea" equals one if the defendant pled guilty to any charge. "Sentence Length" measures days of incarceration following conviction. "Incarcerated" equals one if the defendant received any custodial sentence. "Failure to Appear" equals one if the defendant misses any scheduled court date. "Rearrest within 6 Months" equals one if the defendant is arrested within 6 months of the hearing date of the first observed case. "Rearrest from 1–2 Years" equals one if arrested 1 to 2 years after the hearing date of the first observed case. Data are from Virginia court records, 2010–2024. Standard errors, clustered at the offense level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B. Data Appendix

Released on Recognizance: An indicator for whether the defendant was released on recognizance. Being released on recognizance means that the defendant signs a promise to appear in court for all future scheduled hearings and adhere to any conditions associated with release.

Released on Bond: An indicator for whether the defendant was released on bond. A bond is the financial promise that helps ensure the defendant complies with the bail conditions and appears in court. Bonds may be either secured or unsecured. A secured bond requires financial security, such as cash, property, or a surety provided by a bail bondsman. The defendant (or another person on the defendant's behalf) must provide this security to the court or obtain a surety bond from a licensed bail bondsman who guarantees the bond amount. On the other hand, for an unsecured bond the defendant signs an agreement promising to comply with the conditions of release and to pay the specified bond amount only if they fail to appear in court or otherwise violate the conditions of release.

Released on Bail: An indicator for whether the defendant was released on bail. In Virginia being released on bail means that the defendant has made bail on the charge meaning they have met all conditions set by the magistrate to secure release and have been relased from custody. These conditions include appearance at future court dates, avoiding contact with victims or suspects, drug tests etc.

Released on Summons: An indicator for whether the defendant was released on summons. An indicator of whether the defendant was released on summons. A summons is a legal order requiring the defendant to appear in court on a specified date without being taken into custody. Defendants released on summons are generally not subject to pretrial detention and are released based on their promise to appear in court as directed.

Public Defender: An indicator of whether the defendant was represented during court proceedings by a public defender, an attorney employed by or contracted with the government to provide legal representation to indigent defendants who cannot afford private counsel.

Felony Offense: An indicator of whether any charge associated with the defendant's case was classified as a felony.

DUI Offense: An indicator of whether the defendant's case included at least one charge for driving under the influence (DUI) of alcohol, drugs, or another intoxicating substance.

Drug Offense: An indicator of whether the defendant's case included at least one drug-

related offense, such as the possession, distribution, manufacture, or transportation of a controlled substance.

Violent Crime: An indicator of whether any charge associated with the defendant's case was classified as a violent crime. Violent crimes include murder and nonnegligent manslaughter, rape, robbery, and aggravated assault, which involve the use or threatened use of force against a person.

Property Crime: An indicator of whether any charge associated with the defendant's case was classified as a property crime. Property crimes include burglary, larceny-theft, motor vehicle theft, and arson, which involve the unlawful taking, destruction, or damage of property.

Convicted: An indicator of whether the defendant was convicted on at least one charge within the case. A charge is coded as convicted if its final disposition was Guilty or Guilty In Absentia. A charge is coded as not convicted if its final disposition was Not Guilty, Not Guilty by Reason of Insanity, Dismissed, Dismissed/Other, or Nolle Prosequi.

Guilty plea: An indicator of whether the defendant entered a guilty plea for any charge within the case. A charge is coded as a guilty plea if the plea entered at the hearing was Guilty or Nolo Contendere (No Contest). A charge is coded as not a guilty plea if the plea entered was Not Guilty.

Failure to appear in court: An indicator of whether the charge involved a failure to appear (FTA) or whether the defendant was tried in absentia. A charge is coded as a failure to appear if it was identified as an FTA-related offense based on the charge description or applicable Virginia code section. Charges involving a capias for failure to appear are also included. In addition, cases in which the defendant was Tried In Absentia are coded as failure to appear.

Rearrest: An indicator of whether the defendant was arrested again following the first hearing associated with the case. A case is coded as a rearrest if the defendant had any subsequent arrest date after the first observed hearing date for the case. This measure includes both pretrial rearrests (occurring before case disposition) and post-disposition rearrests (occurring after case disposition). Rearrests are counted regardless of the county, jurisdiction, or location in which the subsequent arrest occurred.