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

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October 12, 2026

Abstract

Pretrial detention accounts for a growing share of jail incarceration, yet the large literature on bail reform focuses almost entirely on monetary bail and low-level offenders, leaving reforms that target violent offenders largely unexamined. This paper studies the repeal of the Presumptive Denial of Bail statute in Virginia, which, prior to its repeal, made defendants charged with specified violent offenses presumed ineligible for bail. Using administrative court records from the Virginia Court System and a difference-in-differences design comparing defendants charged with treated and control offenses, I estimate the effects of repeal on case outcomes, pretrial misconduct, and recidivism. I find that the repeal increased pretrial release among treated defendants by 6.4 percent, and lowered conviction and incarceration rates by 11.4 percent and 11.7 percent, respectively. I argue this improvement in case outcomes is driven by a reduction in the coercive pressure detention places on defendants to accept unfavorable plea deals. Although bail laws are designed to prevent pretrial misconduct and recidivism, I find that increasing access to bail does not increase failure to appear for court hearings or recidivism. These findings suggest that restoring judicial discretion over pretrial detention, even for violent charges, improves case outcomes without compromising court appearance or public safety.

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

JEL classification codes: K42, K14, K41

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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. Importantly, the rise in pretrial detention is a key driver of the rapid growth in jail costs nationwide. 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 some 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 rebutting.[1] This channel to pretrial

[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].

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. Understanding this distinct channel matters because, unlike monetary bail, charge-based detention cannot be circumvented by wealth, making it a binding constraint for defendants regardless of financial resources, and because it is typically applied to serious offenses, its effects on public safety carry especially high policy implications.

This paper examines Virginia's repeal of its Presumptive Denial of Bail (I hereby term it as PDoB) statute, which eliminated rebuttable presumptions against bail for defendants charged with mostly violent offenses. Before the repeal, defendants charged with any of these offenses faced a statutory presumption against bail, where courts were directed to treat them as ineligible for release subject to rebuttal. The repeal removed this restriction, allowing defendants to have a standard bail determination, in which release decisions are based on individualized assessments of the defendant's flight risk and threat to public safety. This repeal generated variation in the strength of the barrier to pretrial release faced by defendants charged with the specified offenses: because they no longer had to rebut a presumption against their release, their likelihood of pretrial release should increase. Using criminal case records web-scraped from the Virginia Court System, I exploit this variation by comparing outcomes for defendants whose charges would have triggered presumptions against bail prior to the repeal (eligible defendants) to those whose charges would not have (ineligible defendants), before and after the repeal.

I begin by estimating the impact of the repeal on pretrial release, which is the direct consequence of the policy. The results show that eliminating charge-based presumptions expanded pretrial release for eligible defendants by 5.2 percentage points. In terms of the method of release, I find that defendants are 6.7 percentage points more likely to be released on recognizance, 3.7 percentage points more likely to be released on summons, 24.7 percentage points more likely to be released on bond, and finally 28.7 percentage points less likely to be released on money bail. These findings indicate that once the barrier to pretrial release was removed, judges became more likely to release defendants through less restrictive options — such as summons or recognizance — rather than more restrictive ones like money bail.

Next, I investigate whether the reform impacted defendants' case outcomes. I find that the repeal decreased guilty pleas by 7.2 percentage points, consistent with pretrial release increasing bargaining power during plea negotiations, thereby reducing pressure to plead guilty. I find that conviction rates decrease by 8.1 percentage points, reflecting the channel of a decrease in the pressure to plead guilty mechanically reducing conviction rates. I also find that incarceration rates decrease by 7.8 percentage points, as custodial

sentences are based on being convicted.

Finally, I examine whether the reform impacted pretrial misconduct, mainly failure to appear in court (FTA), and subsequent criminal justice contact. Because PDoB applied to defendants charged with 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 presumptions against bail should increase failure to appear in court or lead to new arrests upon release. I find little evidence of flight risk. Failure-to-appear rates in court are unchanged in my preferred specification. In terms of recidivism, I find no impact on short-term recidivism (reoffense within six months of release, which is likely to cover the pretrial period) or long-term recidivism. I also estimate effects on recidivism separately by crime type and find no strong evidence of an increase in reoffense in any category, including violent felonies.

I examine whether the effects of repealing PDoB differed by race. Black defendants experienced a larger increase in overall pretrial release than White defendants, yet the reductions in conviction, guilty plea, and incarceration rates were larger for White defendants than for Black defendants.

I subject my main results to multiple robustness checks. First, I verify whether other policy changes occurred around the repeal 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 repealed. 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 they are parallel and move smoothly through the threshold. This also helps to test whether offenses are charged the same way before and after the policy. I present time trends which show no indication of bulking around the policy reform date.

Thirdly, I test whether the policy led to cases being upcharged (the charge was amended from a less severe to a more severe offense) or downcharged (the charge was amended from a more severe to a less severe offense) and find no statistically significant effects.

Fourth, I test the sensitivity of my results to the COVID-19 pandemic, which occurred a year before the policy was repealed. Due to health-risk concerns, jails all over the country were more likely to release defendants, which could be driving my results. To test for this, I exclude the COVID-19 pandemic era from my main analysis, and I find that the results are similar to the main estimates. In other words, I do not find evidence that the treated group is less likely to be released than the control group due to the

nature of the crime to preserve public safety.

Related Literature

This paper contributes to several strands of the existing literature. First, it supplements the literature on the impacts of pretrial detention on defendants' outcomes by using quasi-experimental variation in judges' 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 uses this same method to examine the impact of pretrial detention on household insolvency and finds that when the household head is detained pretrial, it increases household financial distress. My results mirror the findings in this strand of the literature about the negative impacts of pretrial detention and find that this policy in Virginia also leads to positive outcomes for defendants, reflecting the generalizability of my findings.

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 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 the COVID-19 pandemic and finds that it 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. My paper provides new evidence on defendants who commit more serious crimes than is common in this literature. I find similar effects on case outcomes but no strong evidence that release of violent offenders significantly worsens public safety outcomes.

Finally, this paper complements the existing research evaluating the impact of PDoB in Virginia. The only existing evaluation is a report by the Virginia Criminal Sentenc-

ing Commission (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, 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. Second, I show that restoring judicial discretion over bail, so that judges decide case by case whether to release defendants charged with violent offenses, does not increase pretrial misconduct or recidivism. This is consistent with judges using that discretion to release defendants who pose a lower risk of misconduct. Third, 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 not studied. Fourth, I contribute to the literature on racial disparities in pretrial decision-making by examining whether the effects of bail reform differ by race. I show that although Black defendants experienced a larger increase in pretrial release than White defendants, this difference did not translate into statistically significant racial differences in case outcomes. I also find no evidence that this larger expansion in release increased Black defendants' relative likelihood of pretrial misconduct or recidivism. More broadly, these findings provide evidence that restrictive pretrial release standards for Black defendants may be unnecessary, as expanding release did not produce detectable increases in public-safety risk.

Outline

The rest of this paper is organized as follows. Section 2 provides institutional background on Virginia's bail system and the subsequent policy change. 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 benefits and costs of the policy reform. Section 7 dicsusses mechanisms and policy implications whiles Section 8 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, does not recidivate before trial, and fulfills all other conditions associated with release, 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, 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 the bail decision. 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 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 presents 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

[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."

[3]Most magistrate offices in Virginia are open 24 hours a day, 7 days a week, with offices in more rural cities or counties available on call.

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.[5] 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). 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 to demonstrate that pretrial release was appropriate. Over the following two and a half decades, the scope of these presumptions expanded considerably through 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 individul factors when making bail determinations, similar to

[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.

[5] My data provides the aggregate number of defendants released on bond and does not specify whether the bond is secured or unsecured.

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.[6] 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).[7] 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.[8]

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

[6]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.

[7]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.

[8]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.

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.].[9] I sourced district criminal court cases for all defendants whose cases were heard from 2010 to 2024 comprising about 3.8 million observations. The data are at the person-hearing-charge level and include information on the defendant (name, date of birth[10], 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 and final disposition).

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 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. A defendant is convicted if any of the charges within his case has a guilty verdict and not convicted if otherwise

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

[10]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.

(case is dropped, individual is acquitted, prosecution does not pursue the case, etc.). A defendant is regarded as having pled guilty if any of the charges within his case resulted in a guilty plea.[11] 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 offense dates to identify individuals who re-offended following their first hearing. Because the court records do not distinguish whether a subsequent offense precedes or follows case disposition, this measure captures recidivism generally and cannot separately identify pretrial versus post-trial reoffending. 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 to account for differences in court practices and other circuit-level factors that may affect the outcomes.

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.[12] 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.[13] 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

[11] The hearing-plea variable contains a substantial number of missing values, particularly for cases resolved through summonses and traffic citations, for which a formal guilty plea may not be required or recorded. I therefore code missing plea values as indicating no observed guilty plea and define the case-level measure as the maximum across all charges within the case to preserve a consistent estimation sample across outcomes. This coding may introduce measurement error if some charges with missing plea information in fact resulted in guilty pleas. Appendix Table A1 provides a robustness check that restricts the analysis to cases with non-missing plea information. The estimated effect of the reform on guilty pleas remains negative in this restricted sample, although the magnitude is smaller than in the main specification. Thus, the negative relationship is also present among cases for which plea information is observed, although differences in magnitude should be interpreted cautiously given the change in sample composition.

[12] 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.

[13] Appendix Table A2 outlines the full list of clauses and specifies which of them require past conviction conditions.

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, 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.[14]

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.[15] Eligible and non-eligible defendants differ substantially in the type of release they receive. Non-eligible defendants are released on summons most 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 are released on summons only 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

[14]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 A12–A14. The direction of the effects on release types and case outcomes matches the main results, with the release effect having a higher magnitude compared to the main results. Case outcomes also have lower magnitudes, although the direction is the same. Lastly, pretrial misconduct decreases while short-term recidivism increases. The slight differences in the estimates could be due to the different samples in this robustness check and in the main results, as offenses that do not require criminal history are less severe on average than offenses that do.

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

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).

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 much lower rate (70.9 percent versus 43.1 percent), similarly with incarceration rates (66.8 percent versus 4.6 percent), although the gap is much wider. This may signify that non-eligible defendants, when convicted, have their cases resolved through non-custodial sentencing like fines, etc. Eligible defendants plead guilty at a slightly higher rate (34.2 percent versus 32.5 percent) than non-eligible defendants.

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. Overall recidivism rates for the eligible group are also slightly higher prior to the repeal (42.6 percent versus 36.7 percent). In the short run (0–6 months from the first hearing), eligible defendants have higher recidivism rates than non-eligible defendants (9.9 percent versus 7.6 percent), which may reflect differences in the composition of the two groups, including the greater number and severity of offenses among eligible defendants and their higher prevalence of felony charges. By the 1–2 year horizon, however,

this gap largely disappears, with recidivism rates of 7.2 percent among eligible defendants and 7.0 percent among non-eligible defendants.

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.

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 defendant i with charge c at time t, $Eligible_c = 1$ if any charged offense within the defendant'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:

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

where γ_c denotes offense-severity-by-eligibility status fixed effects that absorb time-invariant differences in outcomes across offense-severity-by-eligibility cells, subsuming the main effect of $Eligible_c$.[16] μ_i denotes fixed effects for the number of charges within

[16] I use offense-severity-by-eligibility-status fixed effects rather than offense-code fixed effects because estimating a separate fixed effect for each of the 2,900+ offense codes is too slow and resource-intensive given the large number of observations. Since a defendant's specific charge determines PDoB eligibility,

each offense-severity-by-eligibility cell for each defendant, which I include as a control for case complexity. 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 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. Standard errors are clustered at the level of broad offense category[17] to account for correlation in outcomes among defendants charged under the same offense category. The identifying assumption is that, absent the reform, outcomes for eligible and ineligible defendants would have evolved in parallel.

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 period, $k = -1$, is April–June 2021, the quarter immediately preceding the reform (treatment). 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 quarter before treatment.

5 Results

In this section, I present my main findings, beginning with the impact of repealing PDoB on pretrial release, and then I show the consequent impacts on defendant outcomes, pretrial misconduct and recidivism. I explore heterogeneity by defendant race and also show some robustness checks to increase confidence in the difference-in-differences estimates.

5.1 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

offense-code fixed effects are a finer-grained version of offense-severity-by-eligibility-status fixed effects, which group the same charges into coarser cells by severity and eligibility status while preserving the same identifying comparison. For outcomes with smaller samples, where full offense-code fixed effects were feasible to estimate, I obtain similar results to my preferred specification; for larger outcomes, this was computationally infeasible to verify directly, though I expect similar results.

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

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, while bond goes up by 25.9 percentage points, recognizance goes up by 7.4 percentage points, and summonses go up by 3.3 percentage points. 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 may have shifted release to less restrictive types such as recognizance or summons.

In Column 3, I present estimates from equation 2 where I replace the *Eligible* and *Post* with offense and time 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, while bond increases by 25.1 percentage points.

Finally, in Column 4, I present my preferred estimates from equation 2, incorporating defendant characteristic controls. These findings remain similar to Columns 2 and 3. PDoB repeal 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, and this is counteracted by increases in bond of 24.7 percentage points, increases in recognizance of 6.7 percentage points, and increases in summons of 3.7 percentage points.

Figure A1 plots event-study estimates of the effect of the repeal of PDoB on pretrial release. Pre-reform trends in overall release rates are parallel: the estimated coefficients are close to zero in every pre-period except quarter $k = -14$; however, this is a singular deviation, and there is no evidence of a continued trend of differences between treatment

and control groups. Coefficients do jump within the region labeled as the quarters in which COVID-19 occurred, which may reflect jails releasing both pretrial defendants and convicted individuals in a bid to reduce the spread of the virus, but these estimates are not statistically significant, indicating no differential release propensity between eligible and ineligible defendants even during the pandemic. As a result, the absence of any pre-reform trend in the gap between eligible and ineligible defendants supports the parallel trends assumption. Post-reform coefficients, however, do not show a statistically significant trend of an increase in release rates of the eligible group compared to the non-eligible group. The first post-reform estimates are positive yet marginally significant, while the rest of the estimates are not statistically significant, although they indicate an increasing trend in pretrial release post-repeal. The event-study graph shows that the dynamic impact of the policy on pretrial release is weaker than the aggregate effect across the entire time frame.

5.2 Case Outcomes

After establishing that PDoB repeal affected pretrial release, I turn my attention to its effect on outcomes that are downstream of initial detainment. Overall, I find that the repeal 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, and incarceration rates fall.

Table 3 presents the effect of the reform on case outcomes. Columns 1–3 report estimates for convictions; Columns 4–6 report estimates for guilty pleas, while Columns 7–9 report estimates for incarceration rates. Across columns, I show results from different specifications, similar to Table 2, with Columns 3, 6, and 9 providing evidence from the most robust specification. Columns 1, 4, and 7 report estimates of the baseline difference-in-differences specification in Equation 1. As shown in Column 1, the estimated effect on convictions is an 11.1-percentage-point decrease, equivalent to a 15.7 percent decline relative to the mean, and is statistically significant at the 1 percent level. Column 4 shows a 6.5-percentage-point decrease in the likelihood of pleading guilty, equivalent to a 19.0 percent decline relative to the mean, also significant at the 1 percent level. Column 7 shows an 8.0-percentage-point decrease in the likelihood of incarceration, equivalent to a 12.0 percent decline relative to the mean, significant at the 1 percent level.

Columns 2, 5, and 8 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 9.8 percentage points, equivalent to

13.8 percent relative to the mean, and the estimate is statistically significant at the 1 percent level. Column 5 shows a 6.5-percentage-point decrease in guilty pleas, equivalent to 19.0 percent relative to the mean, also significant at the 1 percent level. Column 8 shows a 7.7-percentage-point decrease in the likelihood of incarceration, equivalent to 11.5 percent relative to the mean, and this estimate is likewise statistically significant at the 1 percent level.

Columns 3, 6, and 9 report the preferred estimates from Equation 2, including controls for defendant characteristics. The main findings remain broadly consistent across all outcomes. As shown in Column 3, convictions decrease by an estimated 8.1 percentage points, equivalent to 11.4 percent relative to the mean. Column 6 shows that guilty pleas decrease by 7.2 percentage points, equivalent to 21.1 percent relative to the mean, and the estimate remains statistically significant at the 1 percent level. Column 9 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.

The decline in guilty pleas is consistent with a bargaining-power mechanism where pretrial detention increases the probability that a defendant accepts an unfavorable plea rather than contesting the charge. A detained defendant faces pressure to accept a plea offer simply to end their confinement, cannot easily consult with counsel, gather evidence, or maintain the employment and community ties that might otherwise support a stronger defense, and as a result bargains from a weaker position than a released defendant would. By expanding pretrial release, the repeal of PDoB may have allowed more defendants to participate in plea negotiations without the immediate pressure of confinement, reducing the incentive to accept an unfavorable plea offer; hence the decline in guilty pleas.

The estimated declines in convictions, guilty pleas, and incarceration following the repeal are larger in magnitude than the corresponding increase in pretrial release. This may be because the case-outcome estimates capture the reduced-form effect of the policy repeal itself, rather than an effect operating specifically through pretrial release. The larger effects therefore suggest that the repeal may have altered other aspects of the pretrial and case-resolution process. Consistent with the hypothesis that pretrial release reduces pressure to plead guilty — thereby allowing some defendants to contest their cases rather than resolve them through a plea — I find increases in acquittals and nolle prosequi dispositions (Appendix Table A3). This pattern suggests that some cases that would otherwise have ended in guilty pleas instead proceeded to outcomes more favorable to defendants. The same mechanism can also help explain the decline in convictions, as fewer guilty pleas and more non-conviction dispositions would reduce the overall probability of conviction. Finally, because incarceration generally follows conviction, the substantial decline in incarceration is consistent with the corresponding reduction in conviction rates.

Figure A2 presents event-study estimates of the impact of the repeal of PDoB on conviction, incarceration, and guilty-plea rates. For conviction, pre-reform coefficients are close to zero, except before and during the COVID-19 window; however, the overall estimates show no systematic trend, supporting the parallel trends assumption. Post-reform, conviction coefficients are negative and significant in the first post-reform quarter, although the effect is not sustained over time but reappears in later quarters. However, they indicate a modest declining trend in conviction rates that is immediate but declines in potency in later periods. For incarceration, pre-reform coefficients decline sharply around the COVID-19 period, which plausibly reflects a pandemic-era shift in sentencing practice where judges may have substituted away from incarceration toward alternative forms of punishment in response to public health concerns about the spread of the virus. Outside of the COVID-19 period, however, the incarceration pre-trend is closer to flat. The post-reform trend starts flat, although there is an immediate modest decline in incarceration rates, with the decline re-emerging as statistically significant in later quarters. For guilty plea rates, although there seems to be some moderate differences between the groups earlier in the pre-reform period, those differences fade and are no longer statistically significant in the quarters immediately before the reform. After the reform, the coefficients become negative and grow into a strong, statistically significant decline. The event study plot shows that immediately after the policy, case outcomes improve, but this effect is not sustained over time.[18]

5.3 Pretrial Misconduct & Recidivism

A potential concern associated with increased pretrial release is that it may lead to more 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 4 presents difference-in-differences estimates for three outcomes. Columns 1–3 report estimates for failure to appear, Columns 4–6 report estimates for recidivism within six months of the defendant's bail hearing, and Columns 7–9 report estimates for recidivism between one and two years after the defendant's bail hearing.

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 (45.8 percent relative to the mean), statistically significant at the 1 percent level.

[18] I present robustness checks where I estimated the main difference-in-differences effect excluding the COVID-19 period and the estimates are very similar to the main effect. These results are discussed in Section 5.5

Column 4 shows essentially no change in recidivism within six months, with an estimated increase of 0.1 percentage points that is not statistically significant. Similarly, Column 7 shows an estimated 0.1-percentage-point increase in recidivism between one and two years, which is also not statistically significant.

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 to 0.1 percentage points and is no longer statistically significant. Column 5 shows a 0.3-percentage-point increase in recidivism within six months, which is not statistically significant. In Column 8, recidivism between one and two years decreases by 0.1 percentage points and is also not statistically significant.

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. Column 6 shows a 0.1-percentage-point increase in recidivism within six months, which is also not statistically significant. Thus, the estimates provide no evidence that the reform affected recidivism during the first six months following the first observed hearing. In Column 9, recidivism between one and two years decreases by 0.1 percentage points and remains statistically insignificant.[19]

Figure A3 presents the event-study estimates for failure to appear in court, recidivism within six months, and recidivism within one to two years. For failure to appear, the estimates remain close to zero throughout both the pre- and post-reform periods, with no discernible change following the repeal. This pattern is consistent with the main difference-in-differences estimate and provides no evidence that the reform affected failure-to-appear rates. The estimates for recidivism within six months are very close to zero during the pre-policy window; however, there seems to be an upward trend in the later quarters after the policy repeal, indicating a public safety risk associated with this policy that does not show up in the main difference-in-differences estimates. The post-reform estimates continue to fluctuate around zero, and their confidence intervals are generally wide, providing no clear evidence of a change in short-term recidivism following the repeal. For recidivism within one to two years, the pre-reform estimates also show some variation, but there is no sustained difference between the treated and control groups immediately before the reform. The post-reform estimates are similarly centered

[19]Appendix Table A4 disaggregates misconduct and recidivism by offense type, which helps explain why the aggregate effects in Table 4 are largely null. Failure to appear declines significantly for felony, violent, and property offenders, and rises only slightly for misdemeanor defendants (who dominate the sample), explaining the near-zero aggregate effect. Recidivism shows almost no effect once disaggregated, aside from weak, marginally significant increases for violent offenses at 6 months and property offenses at 1–2 years.

around zero and do not show a consistent upward pattern. Taken together, the event-study results provide no evidence that repealing PDoB increased longer-run recidivism among affected defendants; however, there seems to be a modest increase in short-term recidivism in later years after the policy was repealed. Appendix Table A4 shows that this potentially increase in short-term recidivism may be driven by defendants who are charged with violent crimes although the estimate is marginally significant at the 10% level.

5.4 Heterogeneity by Race

Tables 5 - 7 examine whether the effects of repealing 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 5 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 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 6 shows that the downstream effects on case outcomes of the reform, while present for both racial groups, were larger in magnitude for White defendants. Conviction rates declined for both groups, with a 10.1 percentage point reduction among White defendants compared with a 4.3 percentage point reduction among Black defendants. Guilty pleas also decreased more for whites than for blacks. White defendants became 8.4 percentage points less likely to enter a guilty plea following the repeal of PDoB, compared with a 4.0 percentage point decline among Black defendants. Incarceration rates also declined for both groups, with White defendants becoming 8.2 percentage points less likely to receive a custodial sentence compared with a 6.3 percentage point reduction among Black defendants.

Table 7 reports heterogeneous effects on pretrial misconduct and recidivism. For both Black and White defendants, the point estimates are close to zero and not statistically

significant across all three outcomes. Failure to appear shows no meaningful effect for either group, with a small increase for White defendants (0.1 percentage points) and a similarly small decrease for Black defendants (0.1 percentage points). The recidivism estimates follow a comparable pattern: for recidivism within six months, the point estimate is positive for Black defendants (0.4 percentage points) but negative for White defendants (0.1 percentage points), and for recidivism one to two years after case disposition, the estimate is negative for both groups but larger in magnitude for Black defendants (0.4 percentage points) than for White defendants (0.1 percentage points). Overall, there is no evidence that the reform affected pretrial misconduct or recidivism for either racial group consistent with the main effect.

Racial disparities pervade the criminal justice system, providing strong motivation for examining whether this reform's effects differ by race. Black Americans are overrepresented among individuals who come into contact with the pretrial system Enders, Pecorino, and Souto, 2019; Pettit and Western, 2004; Bayer and Charles, 2018, and racial disparities also operate along the intensive margin: prior work shows that Black defendants are less likely to be released pretrial and face more restrictive release conditions than White defendants Anderson, 2024. These patterns suggest the reform may disproportionately benefit Black defendants. Consistent with this, Black defendants experienced a larger increase in pretrial release than White defendants (5.8 percentage points versus 4.6). However, this larger release gain did not translate into correspondingly larger gains in case outcomes: point estimates for reductions in conviction, guilty pleas, and incarceration are all larger in magnitude for White defendants than for Black defendants, despite Black defendants' larger increase in release. Because the reform had no statistically significant effect on failure to appear or recidivism in the full sample, I find no evidence that its impact on pretrial misconduct or recidivism varies by defendant race either. These results suggest that some form of racial disparity exists in downstream case processing for Black defendants: despite experiencing a larger increase in pretrial release, they do not see correspondingly larger benefits in case outcomes.

5.5 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 paragraphs.

Threats to Identification.—I check whether other policy changes coincided with the repeal of PDoB. Virginia decriminalized simple marijuana possession in July 2020 and legalized recreational marijuana in July 2021. Because several offenses in the treated

group are drug offenses, and the offenses initially subject to presumptions against bail were predominantly drug offenses, changes in marijuana enforcement or charging could differentially affect the sentencing and conviction of treated defendants. To rule out this confound, I identify defendants whose treatment status could be affected by either marijuana policy and re-estimate the main specification without them. This group makes up 0.68 percent of the treated group. The results are very similar to those from the main sample (Appendix Tables A5–A7).

The COVID-19 pandemic began about a year before the repeal of the PDoB and could have affected both pretrial release and case outcomes. Courts may have released defendants at unusually high rates to limit the spread of the virus in jails, judges may have imposed non-confining sentences for the same reason, and law enforcement may have prioritized arrests for felonies and jailable misdemeanors over lesser offenses to reduce jail intake. Because these responses could have affected the treated and control groups differently, I drop the period when the pandemic's effects were most acute (March 2020 to March 2021) and re-estimate the main specification. The estimates are very similar to the main results (Appendix Tables A8–A10), indicating that pandemic-era changes in release or enforcement practices do not drive my findings.

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 A4, the time trends of eligible and ineligible offenses trend similarly between the two groups and move smoothly through the policy repeal date (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 create indicators that represent whether an offense was upcharged or downcharged and estimate the impact of the policy repeal on them. I define a charge as upcharged if it is amended from an offense in the control category to an offense in the treated category. I define a charge as downcharged if it is amended from an offense in the treated category to an offense in the control category. I test whether the policy led to cases being upcharged or downcharged using the preferred specification in Equation 2 and include controls. I 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 (using data from the National Incident-Based Reporting System) and find no statistically significant effects. These results are presented in Appendix Table A11.

6 Discussion

This section considers the mechanisms that may explain my findings and discusses the fiscal benefits associated with repealing PDoB.

6.1 Potential Mechanisms

Increased pretrial release may shape case outcomes through three related channels: greater bargaining power in plea negotiations, more opportunity to prepare a defense while the case is pending, and the avoidance of potentially prejudicial courtroom signals associated with detention. A separate incapacitation channel links release to higher recidivism over a longer horizon.

The first channel concerns defendants' bargaining power during plea negotiations. Although my empirical analysis does not separately identify this mechanism, the decline in guilty pleas is consistent with released defendants facing less pressure to accept a plea offer while their cases are pending. The lower conviction rate among released defendants therefore appears to be driven primarily by changes in plea behavior rather than by a lower likelihood of conviction at trial. Released defendants face less pressure to plead guilty because they are not required to remain in detention while their case is pending, which may give them greater leverage in plea negotiations. This pattern is intuitive because a guilty plea is an admission of guilt that is very likely to lead to a conviction without the need for a trial. Because a guilty plea generally makes a conviction more likely, a shift away from guilty pleas can mechanically reduce the overall conviction rate.

The second channel concerns defendants' ability to prepare and present a defense. Unlike detained defendants, released defendants can more easily meet with counsel, help gather evidence and identify witnesses, and maintain the employment and family ties that may support their case. These practical advantages may help defendants evaluate plea offers, negotiate more favorable resolutions, or contest the charges when appropriate. They may also help explain the decline in incarceration rates. By allowing defendants to investigate their cases, present mitigating evidence, and work more closely with counsel, pretrial release may improve their ability to secure reduced charges, an acquittal, or a sentence that does not involve incarceration. While my estimates do not distinguish among these pathways, the pattern is consistent with release improving defendants' capacity to

obtain case outcomes that avoid incarceration.

A third possible channel operates through courtroom appearance. Detained defendants may be more likely to appear in jail clothing or under visible restraints, which can signal dangerousness or guilt to courtroom decision-makers Street et al., 2025. This suggests that defendants shown in prison attire receive less favorable assessments and are more likely to be judged guilty than otherwise identical defendants shown in civilian clothing.

Pretrial release could affect future crime through two channels that work in opposite directions. Release reduces exposure to the criminogenic effects of detention, such as harsh conditions and negative peer effects, which would lower recidivism. But release also reduces incapacitation: defendants who would have been detained spend more time in the community while their cases are pending, giving them more opportunity to commit offenses that lead to a new arrest. Because these channels offset each other, the net effect of the repeal is theoretically ambiguous. My main results show no significant effect on recidivism, consistent with the two channels roughly canceling out. The heterogeneity analysis by offense type Appendix Table A4 shows modest increases in recidivism for violent offenses at six months and for property offenses at one to two years. This suggests that in some subsamples the loss of incapacitation outweighs any benefit from avoiding detention, so the policy carries some risk of recidivism, although that risk is minimal.

6.2 Fiscal Savings from Reduced Pretrial Detention

To assess the broader fiscal implications of the PDoB repeal, I quantify the short-term fiscal savings for jails generated by the resulting reduction in pretrial detention. A comprehensive cost-benefit analysis of the reform — one that would also weigh offsetting costs, such as increased pretrial supervision expenditures or any public-safety costs associated with earlier release — is beyond the scope of this paper. Instead, I provide a back-of-the-envelope calculation of the fiscal savings attributable to reduced detention, valued at Virginia's average jail operating cost per inmate-day. I measure the annual savings for Virginia jails using the following equation:

$$\text{Benefit} = \Delta Pr(\text{Released}) \times N_{\text{eligible}} \times \overline{D} \times C \tag{4}$$

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.

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 ($\Delta Pr(\text{Released}) = 0.052$). 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.

For the counterfactual 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 in October 2017 and followed through case disposition (or through December 31, 2018 for cases still pending at that point)—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. 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).

Combining these estimates yields

$$\text{Annual Benefit} = 0.052 \times 7{,}657 \times 58 \times \$107.09 = \$2{,}472{,}998 \tag{5}$$

$$\text{Total Benefit (2010–2024)} = 0.052 \times 114{,}851 \times 58 \times \$107.09 = \$37{,}094{,}971 \tag{6}$$

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 savings of roughly $2.47 million.

Several caveats apply to this calculation. 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.I rely on this external estimate because my data lack release dates, which prevents me from computing average detention length directly and checking it against this benchmark — so I cannot determine whether $\overline{D}$ overstates or understates the true value for my sample, and the resulting fiscal-savings estimate could be biased in either direction. 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 2024); using costs from the year 2021 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 again makes the benefits a conservative estimate.

7 Conclusion

I provide new evidence on how the 2021 reform of Virginia's rebuttable presumptions against bail affected defendant case outcomes, pretrial misconduct, and future offending. I find that pretrial release rose among defendants who, prior to the reform, would have been subject to a presumption against bail — and therefore less likely to be released before their case was resolved. I present suggestive evidence that this increase in pretrial release improved case outcomes for these previously presumption-eligible defendants. Specifically, I find that defendants are less likely to be incarcerated after the reform, and I argue this operates through a decline in guilty pleas: defendants who are not detained pretrial face less pressure to plead guilty, and since guilty pleas mechanically raise conviction rates, fewer guilty pleas translate into a decline in conviction rates. Lower conviction rates in turn mechanically reduce incarceration, since incarceration follows from a conviction carrying a sentence of jail or prison confinement.

A natural concern is that bail exists precisely to prevent pretrial misconduct — failure to appear and pretrial recidivism — so an increase in release could be counterproductive or could itself raise misconduct. I do not find strong evidence of this (I use "counterintuitive" cautiously: post-reform defendants still go through a standard bail determination process, so release continues to be granted to the least risky defendants). I find no evidence that failure-to-appear rates increase. I also find no statistically significant evidence that recidivism following defendants' trials increases.

My results provide important implications for bail reform. First, my paper suggests that in the absence of the reform in Virginia, there would have been a significant number of defendants who, because they would have been detained due to these presumptions, would have experienced negative criminal case outcomes like a higher likelihood of being convicted and/or incarcerated. Eliminating these presumptions appears to improve defendants' case outcomes.

Second, my paper shows that assigning restrictive bail measures rather than releasing a defendant might not provide the gains in reducing pretrial misconduct as is deemed prudent in the judicial system. Although the demographic of offenders the presumptive

denial of bail policy targets is mainly violent offenders, my paper shows that expanding release to these defendants does not lead to adverse pretrial misconduct effects. Failure to appear in court does not worsen, and future recidivism in the short term also shows no effect.

Finally, these findings point to implications for asymmetric penalties in errors magistrates make in setting bail. Magistrates could be setting bail too low such that defendants can fulfill the requirements of bail, get released, and recidivate or fail to appear in court (Type II error). On the other hand, magistrates could be setting bail too high, leading to high levels of detention (Type I error). The costs of Type II errors are more visible than for Type I errors since it is more difficult to determine whether a defendant who was detained due to a Type I error would have appeared for all court dates or not committed a crime once out in society. My results show that magistrates may have committed Type I errors frequently, where they detained defendants who would not have been a threat to society and would have appeared in court.

Overall, the PDoB repeal improved case outcomes for defendants without increasing pretrial misconduct or recidivism, in either the short or long run. These results suggest that expanding pretrial release did not come at the cost of public safety: defendants released under the reformed standard were no more likely to fail to appear or reoffend than defendants released under the prior, more restrictive regime. Taken together, these findings indicate that bail reform can improve case outcomes for defendants without introducing offsetting risks to public safety.

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8 Figures and tables

8.1 Figures

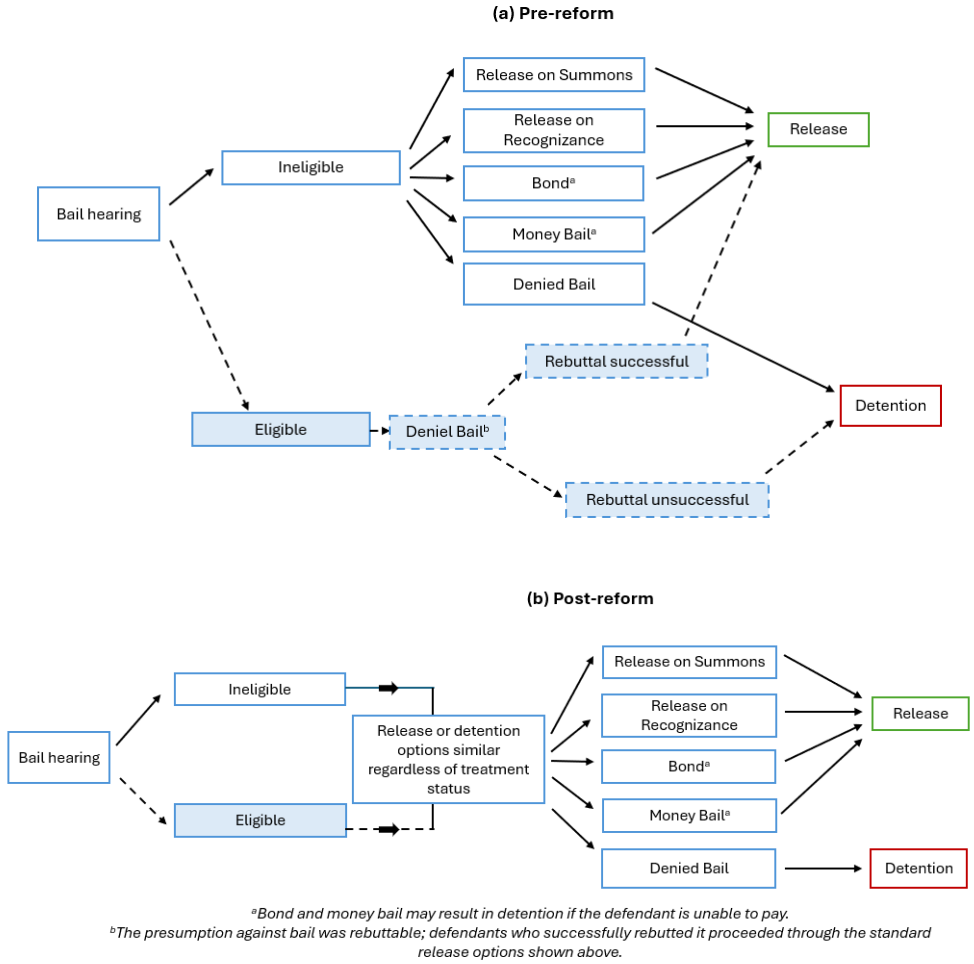


Figure 1: The Pretrial Process in Virginia before and after the Bail Reform

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.

8.2 Tables

Table 1: Pre-repeal Descriptive Statistics by Eligibility (Treatment) Status

	Eligible		Ineligible	
	Mean	Obs.	Mean	Obs.
	(1)	(2)	(3)	(4)
Panel A: Pretrial Release Types				
Released (agg.)	0.812	95,225	0.982	3,102,265
Released on Bail	0.315	95,225	0.028	3,102,265
Released on Bond	0.008	95,225	0.001	3,102,265
Released on Recognizance	0.439	95,225	0.041	3,102,265
Released on Summons	0.284	95,225	0.923	3,102,265
Panel B: Defendant Characteristics				
Female	0.231	95,225	0.390	3,102,265
Black	0.299	93,715	0.261	3,009,640
Hispanic	0.011	93,715	0.039	3,009,640
White	0.668	93,715	0.661	3,009,640
Median Household Income in zip code	69,599	75,205	76,710	2,534,921
Public Defender	0.119	95,225	0.023	3,102,265
Panel C: Charge Characteristics				
Felony Offense	0.247	95,225	0.028	3,102,265
Any DUI offense	0.701	95,225	0.001	3,102,265
Any drug offense	0.031	95,225	0.028	3,102,265
Any violent crime	0.117	95,225	0.011	3,102,265
Any property crime	0.071	95,225	0.025	3,102,265
Number of offenses	2.123	95,225	1.176	3,102,265
Panel D: Case Outcomes				
Conviction	0.709	95,225	0.431	3,102,265
Guilty plea	0.342	95,225	0.120	3,102,265
Any incarceration	0.668	95,225	0.046	3,102,265
Panel E: Pretrial Misconduct & Recidivism				
Failure to appear in court	0.024	95,225	0.011	3,102,265
Recidivism	0.426	95,225	0.367	3,102,265
Recidivism in 0-6 months	0.099	95,225	0.076	3,102,265
Recidivism 1-2 years	0.072	95,225	0.070	3,102,265

Notes: This table presents descriptive 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 repealing 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 repealing PDoB on Case Outcomes

	Conviction			Guilty Plea			Incarceration		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Eligible × *Post 07/21*	-0.111***	-0.098***	-0.081***	-0.065***	-0.065***	-0.072***	-0.080***	-0.077***	-0.078***
	(0.025)	(0.017)	(0.019)	(0.016)	(0.011)	(0.011)	(0.023)	(0.019)	(0.020)
Fixed effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes	No	No	Yes
Mean dep. var.	0.709	0.709	0.709	0.342	0.342	0.342	0.668	0.668	0.668
Observations	3,851,082	3,851,075	3,705,897	3,851,082	3,851,075	3,705,897	3,851,082	3,851,075	3,705,897

Notes: This table presents the impact of the policy reform on case outcomes. Columns 1, 4, and 7 report estimates of β_1 from Equation 1. Columns 2, 5, and 8 report estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Columns 3, 6, and 9 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. "Incarceration" 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 4: Difference-in-Differences Estimates of the Effect of repealing PDoB on Pretrial Misconduct and Recidivism

	Failure to appear			Recidivism within 6 months			Recidivism from 1 - 2 years		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Eligible × *Post 07/21*	0.011***	0.001	0.000	0.001	0.003	0.001	0.001	-0.001	-0.001
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.005)	(0.004)	(0.004)
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	0.099	0.099	0.099	0.072	0.072	0.072
Observations	3,851,082	3,851,075	3,705,897	3,851,082	3,851,075	3,705,897	3,851,082	3,851,075	3,705,897

Notes: This table presents the impact of the policy reform on pretrial misconduct and recidivism. Column 1, 4, and 7 report estimates of β_1 from Equation 1. Column 2, 5, and 8 report estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Columns 3, 6 and 9 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. "Failure to Appear" equals one if the defendant misses any scheduled court date. "Recidivism within 6 Months" equals one if the defendant is arrested within 6 months of the hearing date of the first observed case. "Recidivism 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 5: Difference-in-Differences Estimates of the Effect of repealing 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 6: Difference-in-Differences Estimates of the Effect of repealing PDoB on Case Outcomes, by Defendant Race

	Conviction	Guilty Plea	Incarceration
	(1)	(2)	(3)
Panel A. Black, non-Hispanic			
Eligible × *Post 07/21*	-0.043*	-0.040**	-0.063***
	(0.023)	(0.017)	(0.022)
Mean dep. var.	0.703	0.799	0.526
Observations	984,372	984,372	984,372
Panel B. White, non-Hispanic			
Eligible × *Post 07/21*	-0.101***	-0.084***	-0.082***
	(0.019)	(0.010)	(0.020)
Mean dep. var.	0.861	0.823	0.728
Observations	2,459,456	2,459,456	2,459,456
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 1-3 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. "Incarceration" 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 7: Difference-in-Differences Estimates of the Effect of repealing PDoB on Pretrial Misconduct and Recidivism, by Defendant Race

	Failure to appear	Recidivism within 6 months	Recidivism from 1 - 2 years
	(1)	(2)	(3)
Panel A. Black, non-Hispanic			
Eligible × *Post 07/21*	-0.001	0.004	-0.004
	(0.007)	(0.005)	(0.004)
Mean dep. var.	0.023	0.111	0.083
Observations	984,372	984,372	984,372
Panel B. White, non-Hispanic			
Eligible × *Post 07/21*	0.001	-0.001	-0.001
	(0.002)	(0.003)	(0.004)
Mean dep. var.	0.024	0.095	0.068
Observations	2,459,456	2,459,456	2,459,456
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 1-3 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. "Recidivism within 6 Months" equals one if the defendant is arrested within 6 months of the hearing date of the first observed case. "Recidivism 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

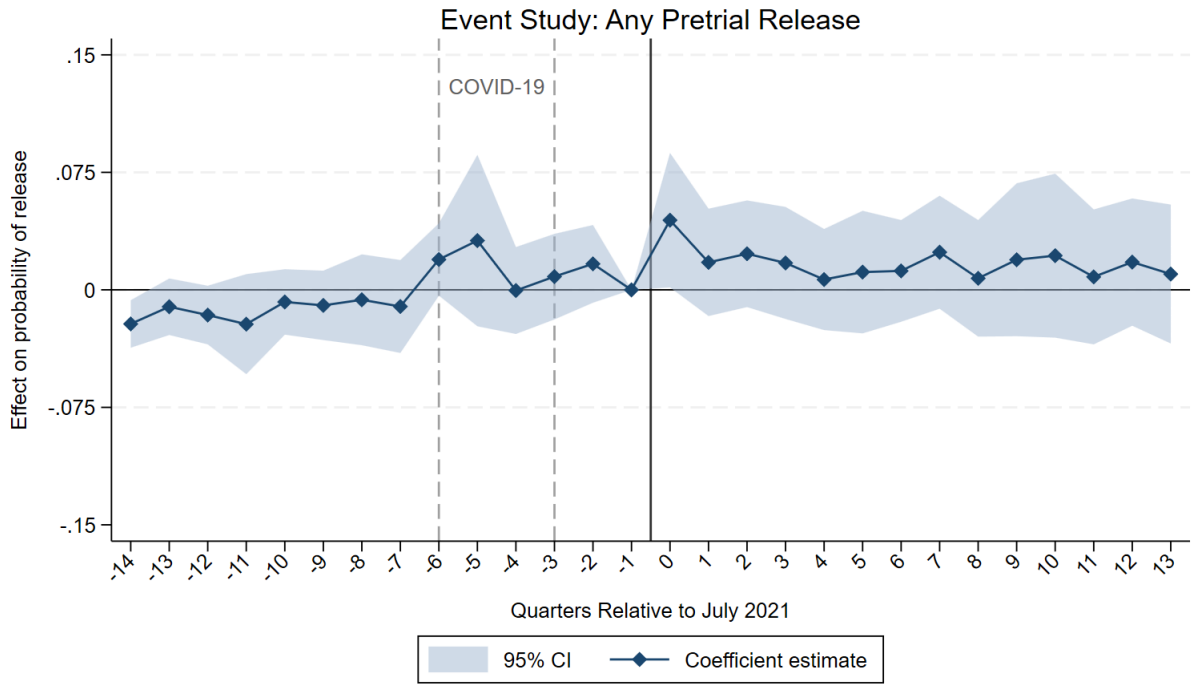


Figure A1: Dynamic Impact of the repeal of PDoB on Pretrial Release

Notes: Figure A1 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). The shaded region in light blue represents 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 line marks the omitted period, while the dashed vertical lines denote the COVID-19 period (January to December 2020).

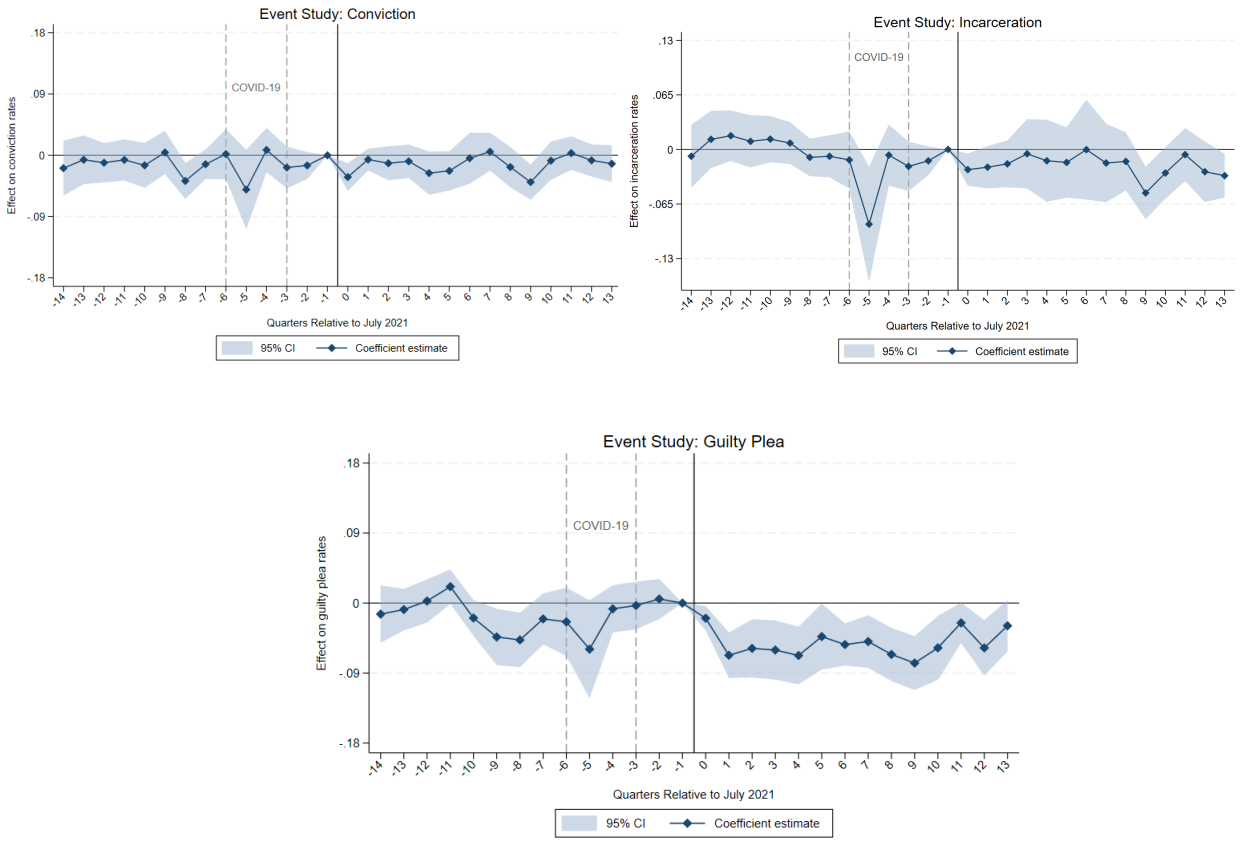


Figure A2: Dynamic Impact of the repeal of PDoB on Case Outcomes

Notes: Figure A2 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). The shaded region in light blue represents 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 line marks the omitted period, while the dashed vertical lines denote the COVID-19 period (January to December 2020).

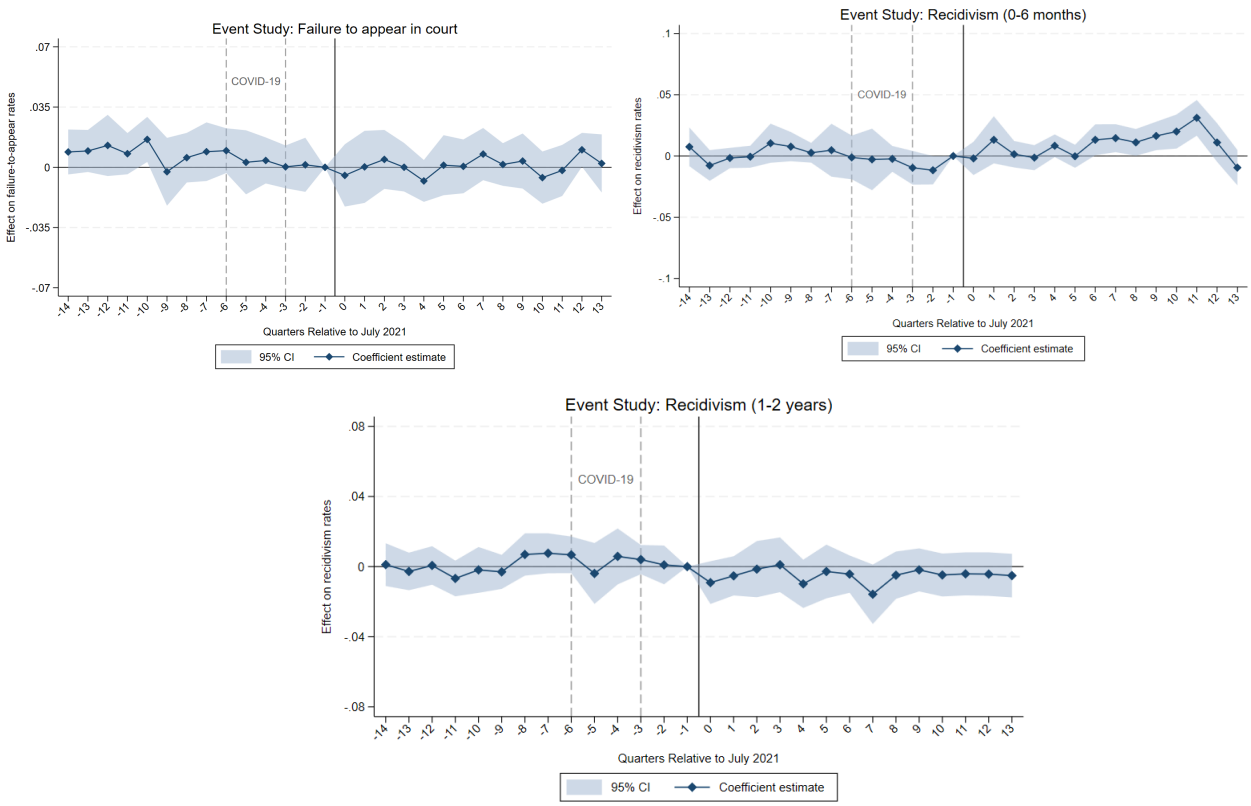
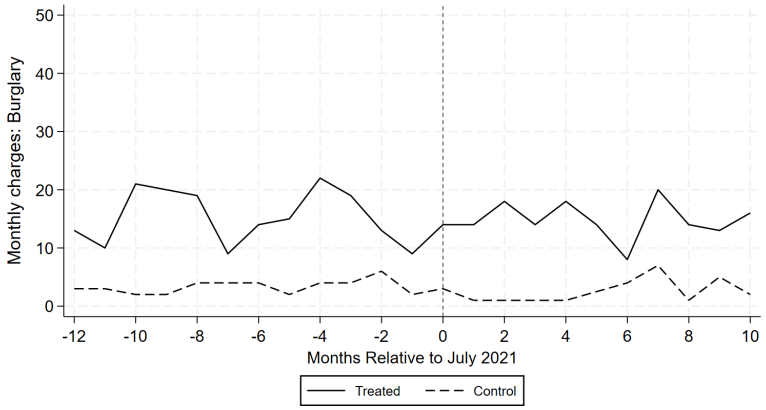
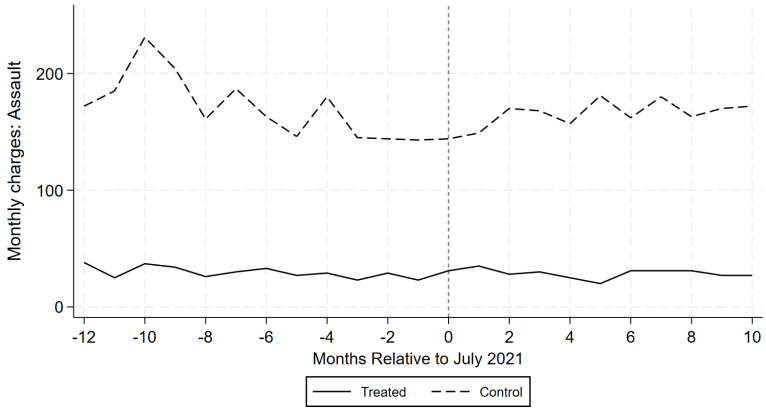


Figure A3: Dynamic Impact of the repeal of PDoB on Pretrial Misconduct and Recidivism

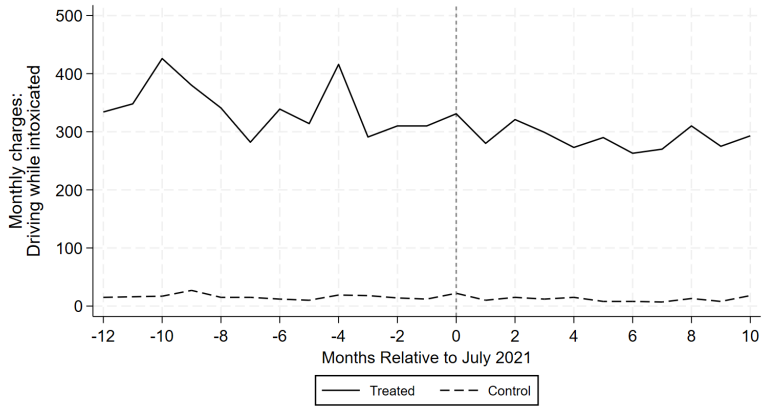
Notes: Figure A3 plots difference-in-differences estimates from Equation 3, using quarterly leads and lags to estimate effects relative to the quarter immediately preceding treatment (April to June). The shaded region in light blue represents 95 percent confidence intervals. The treated group consists 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 line marks the omitted period, while the dashed vertical lines denote the COVID-19 period (January to December 2020).



(a) Burglary



(b) Assault



(c) Driving while intoxicated (DWI)

Figure A4: 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: Difference-in-Differences Estimates of the Effect of PDoB Repeal on Guilty Pleas (Non-Missing Plea Sample)

	Guilty Plea		
	(1)	(2)	(3)
Eligible x Post 07/21	-0.023**	-0.030***	-0.020**
	(0.010)	(0.009)	(0.008)
Fixed effects	No	Yes	Yes
Offense controls	No	Yes	Yes
Controls	No	No	Yes
Mean dep. var.	0.819	0.819	0.819
Observations	606,987	606,983	578,121

Notes: This table presents the impact of the policy reform on guilty pleas using the restricted sample of cases with non-missing plea information. Column 1 reports estimates of β_1 from Equation 1. Column 2 reports estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Column 3 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. "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 A2: 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 (e.g., 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 sentence requirement	
5	(references clause 1, 2 above)	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 or execution of sentence of appeal of sentence	[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 A3: Difference-in-Differences Estimates of the Effect of Repealing PDoB on Case Resolution Outcomes

	Case Dismissed	Acquittal	Nolle Prosequi
	(1)	(2)	(3)
Eligible × *Post 07/21*	-0.032***	0.005*	0.016*
	(0.010)	(0.003)	(0.009)
Fixed effects	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Mean dep. var.	0.034	0.013	0.155
Observations	3,705,897	3,705,897	3,705,897

Notes: This table presents the impact of the policy reform on case resolution outcomes. Columns 1–3 report estimates of ϕ_1 from Equation 2, including offense-by-treatment-status and year-month fixed effects and controls for defendant race and gender, public defender representation, and judicial circuit. "Eligible" refers to the treated group and "ineligible" to the control group. "Case Dismissed" equals one if the case is dismissed. "Acquittal" equals one if the defendant is acquitted on any charge. "Nolle Prosequi" equals one if any charge in the case is resolved through nolle prosequi. 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 repealing PDoB on Pretrial Misconduct and Recidivism, by Offense Type

	Felony	Misdemeanor	Violent	Property
	(1)	(2)	(3)	(4)
Panel A. Failure to Appear				
Eligible × *Post 07/21*	-0.019***	0.004*	-0.010**	-0.054**
	(0.006)	(0.002)	(0.002)	(0.007)
Mean dep. var.	0.018	0.025	0.009	0.021
Observations	137,103	3,568,789	54,912	100,136
Panel B. Recidivism within 6 Months				
Eligible × *Post 07/21*	0.005	0.003	0.013*	0.004
	(0.007)	(0.003)	(0.005)	(0.005)
Mean dep. var.	0.101	0.099	0.091	0.132
Observations	137,103	3,568,789	54,912	100,136
Panel C. Recidivism from 1–2 Years				
Eligible × *Post 07/21*	0.003	0.001	0.004	0.010*
	(0.006)	(0.003)	(0.006)	(0.003)
Mean dep. var.	0.083	0.068	0.073	0.103
Observations	137,103	3,568,789	54,912	100,136
Fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: This table presents the impact of the policy reform on pretrial misconduct and recidivism, broken down by offense type. Panel A presents results for failure to appear; Panel B presents results for rearrest within 6 months of the hearing date of the first observed case; Panel C presents results for rearrest 1 to 2 years after the hearing date of the first observed case. Columns 1–4 report the coefficient on *Eligible* × *Post 07/21*, controlling for defendant race and gender and public defender representation. "Eligible" refers to the treated group and "ineligible" to the control group. "Felony" and "Misdemeanor" restrict the sample to defendants whose offense was classified as a felony or misdemeanor, respectively; "Violent" and "Property" restrict the sample to defendants charged with a violent or property offense, respectively. Mean dep. var. reports the pre-repeal mean of the outcome for the eligible (treated) group within each offense-type subsample. Standard errors are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A5: Difference-in-Differences Estimates of the Effect of PDoB repeal 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 A6: Difference-in-Differences Estimates of the Effect of PDoB repeal on Case Outcomes (excluding marijuana-related offenses)

	Conviction			Guilty Plea			Incarceration		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Eligible x Post 07/21	-0.109***	-0.099***	-0.082***	-0.065***	-0.066***	-0.073***	-0.078***	-0.077***	-0.078***
	(0.024)	(0.018)	(0.019)	(0.016)	(0.011)	(0.011)	(0.023)	(0.019)	(0.020)
Fixed effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes	No	No	Yes
Mean dep. var.	0.713	0.713	0.713	0.344	0.344	0.344	0.672	0.672	0.672
Observations	3,849,693	3,849,686	3,704,542	3,849,693	3,849,686	3,704,542	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. Columns 1, 4, and 7 report estimates of β_1 from Equation 1. Columns 2, 5, and 8 report estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Columns 3, 6, and 9 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. "Incarceration" 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 A7: Difference-in-Differences Estimates of the Effect of PDoB repeal on Pretrial Misconduct and Recidivism (excluding marijuana-related offenses)

	Failure to Appear			Recidivism (0-6 months)			Recidivism (1-2 years)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Eligible x Post 07/21	0.011***	0.001	0.000	0.001	0.003	0.002	0.002	-0.000	-0.001
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.005)	(0.004)	(0.004)
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	0.100	0.100	0.100	0.072	0.072	0.072
Observations	3,849,693	3,849,686	3,704,542	3,849,693	3,849,686	3,704,542	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, 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. "Recidivism within 6 Months" equals one if the defendant is arrested within 6 months of the hearing date of the first observed case. "Recidivism 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 A8: Difference-in-Differences Estimates of the Effect of PDoB repeal on Pretrial Release (excluding the COVID-19 period)

	Eligible Mean	DID results		
	(1)	(2)	(3)	(4)
Panel A. Pretrial Release (Aggregate)				
Any Pretrial Release	0.810	0.052***	0.052***	0.053***
	[0.392]	(0.020)	(0.017)	(0.017)
Panel B. Pretrial Release Types				
Money Bail	0.319	-0.290***	-0.291***	-0.290***
	[0.466]	(0.014)	(0.014)	(0.015)
Bond	0.008	0.258***	0.251***	0.247***
	[0.091]	(0.011)	(0.011)	(0.012)
Recognizance	0.433	0.079***	0.073***	0.072***
	[0.495]	(0.020)	(0.017)	(0.016)
Summons	0.288	0.030***	0.028**	0.033***
	[0.453]	(0.011)	(0.011)	(0.011)
Offense-type-by-treatment-status fixed effects	—	No	Yes	Yes
Year-month fixed effects	—	No	Yes	Yes
Controls	—	No	No	Yes
Observations	90,267	3,737,484	3,737,477	3,599,279

Notes: This table presents the impact of the policy reform on pretrial release, excluding hearings that took place during the COVID-19 period (March 2020 to March 2021). 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 A9: Difference-in-Differences Estimates of the Effect of PDoB repeal on Case Outcomes (excluding the COVID-19 period)

	Conviction			Guilty Plea			Incarceration		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Eligible x Post 07/21	-0.113***	-0.099***	-0.082***	-0.066***	-0.065***	-0.073***	-0.081***	-0.079***	-0.079***
	(0.026)	(0.018)	(0.019)	(0.016)	(0.011)	(0.011)	(0.024)	(0.019)	(0.020)
Fixed effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes	No	No	Yes
Mean dep. var.	0.711	0.711	0.711	0.343	0.343	0.343	0.670	0.670	0.670
Observations	3,737,484	3,737,477	3,599,279	3,737,484	3,737,477	3,599,279	3,737,484	3,737,477	3,599,279

Notes: This table presents the impact of the policy reform on case outcomes, excluding hearings that took place during the COVID-19 period (March 2020 to March 2021). Columns 1, 4, and 7 report estimates of β_1 from Equation 1. Columns 2, 5, and 8 report estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Columns 3, 6, and 9 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. "Incarceration" 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 repeal on Pretrial Misconduct and Recidivism (excluding the COVID-19 period)

	Failure to Appear			Recidivism (0-6 months)			Recidivism (1-2 years)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Eligible x Post 07/21	0.011***	0.001	0.001	-0.001	0.003	0.001	0.001	-0.000	-0.001
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.005)	(0.004)	(0.004)
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.	0.023	0.023	0.023	0.102	0.102	0.102	0.072	0.072	0.072
Observations	3,737,484	3,737,477	3,599,279	3,737,484	3,737,477	3,599,279	3,737,484	3,737,477	3,599,279

Notes: This table presents the impact of the policy reform on pretrial misconduct and recidivism, excluding hearings that took place during the COVID-19 period (March 2020 to March 2021). Columns 1, 4, and 7 report estimates of β_1 from Equation 1. Columns 2, 5, and 8 report estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Columns 3, 6, and 9 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. "Failure to Appear" equals one if the defendant misses any scheduled court date. "Recidivism within 6 Months" equals one if the defendant is arrested within 6 months of the hearing date of the first observed case. "Recidivism 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: 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 A12: 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 A13: Difference-in-Differences Estimates Using Statutory Clauses Not Requiring Prior Criminal History: Case Outcomes

	Conviction			Guilty Plea			Incarceration		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Eligible × *Post 07/21*	-0.035	-0.067***	-0.061***	0.005	-0.019*	-0.029***	-0.014	-0.055***	-0.062***
	(0.023)	(0.014)	(0.016)	(0.014)	(0.011)	(0.011)	(0.021)	(0.017)	(0.017)
Fixed effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes	No	No	Yes
Mean dep. var.	0.112	0.112	0.112	0.050	0.050	0.050	0.101	0.101	0.101
Observations	3,739,706	3,739,701	3,596,645	3,739,706	3,739,701	3,596,645	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. Columns 1, 4, and 7 report estimates of β_1 from Equation 1. Columns 2, 5, and 8 report estimates of ϕ_1 from Equation 2, which includes offense-by-treatment-status and year-month fixed effects. Columns 3, 6, and 9 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. "Incarceration" 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			Recidivism (0-6 months)			Recidivism (1-2 years)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Eligible × *Post 07/21*	0.004	-0.019***	-0.021***	0.036***	0.026***	0.023**	0.000	-0.005	-0.007
	(0.003)	(0.007)	(0.007)	(0.010)	(0.009)	(0.009)	(0.008)	(0.007)	(0.007)
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	0.062	0.062	0.062	0.065	0.065	0.065
Observations	3,739,706	3,739,701	3,596,645	3,739,706	3,739,701	3,596,645	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, 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. "Recidivism within 6 Months" equals one if the defendant is arrested within 6 months of the hearing date of the first observed case. "Recidivism 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 released 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 and not convicted if otherwise.

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) and not a guilty plea if otherwise

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.

Recidivism: An indicator equal to one if a defendant commits a new offense after the first observed hearing date for the index case. The timing of recidivism is determined by the date on which the subsequent offense was committed, rather than the date on which the defendant was arrested for that offense. The measure captures new offenses committed both before and after disposition of the index case and includes qualifying offenses regardless of the county, jurisdiction, or location in which the subsequent offense occurred.