What Is Bail For?

Appearance, Danger, and Ability to Pay in Maryland*

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Abstract

Courts may detain dangerous defendants outright, yet most held pretrial are there for failing to post money bail. Reformers argue bail should secure appearance alone, with danger handled through detention hearings, not the amount (Gouldin, 2016). Most reforms curtail or replace money bail; they do not restrict what it may target. Maryland's 2017 reform confines the amount to appearance and ability to pay while keeping broad detention authority. At the operational cutoff release rose about 10 points and held on default fell about 13; by the mature post-reform period both roughly doubled and amounts fell over 70 percent. Predicted rearrest risk moved off the amount and onto held-without-bail orders. Failure to appear and rearrest did not detectably rise, and the evidence gives little support that lower amounts raised nonappearance. It met the ability-to-pay limit bluntly, lowering amounts, not making any amount more postable, and changed detention's form more than its duration.

Keywords: pretrial detention; money bail; bail reform; ability to pay; regression discontinuity

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1 Introduction

On any given day, roughly 450,000 people sit in U.S. jails awaiting trial, about two-thirds of the local jail population (Sawyer & Wagner, 2022). Few are held under a formal order of detention. Rather, a court granted them release but set money bail, a price they must pay to leave jail before trial. This is the structural contradiction of money bail: a decision that grants release produces detention whenever the amount exceeds what the defendant can pay (Mayson, 2020).

Unaffordable bail produces detention in two ways. The first is deliberate: a court sets a high amount to hold a defendant it considers dangerous, using money bail in place of a formal detention order (Gouldin, 2016; Mayson, 2020). The second is incidental: a court means to release a defendant but sets the amount above their means, often without knowing what they can pay. Either way, detention results from nonpayment rather than from a formal order denying release.

In 47 of 50 states, laws governing pretrial procedure permit a court to set bail amounts based both on appearance and public-safety risk (Petis, 2024). Recent reforms aimed at reducing detention-through-nonpayment have mostly left that breadth in place, instead working *around* the amount rather than changing the rules for setting it. States have targeted four levers, often in combination: abolishing money bail (Griffin, DuPont, Stemen, Olson, & Ward, 2024; Sims, 2025), narrowing the offenses for which it may be set (Rempel, Lu, & Monaghan, 2026), expanding nonmonetary detention (Anderson Golub, Redcross, & Valentine, 2019; Griffin et al., 2024)[1], and adding procedural protections at bail-setting against detention for inability to pay (Lacoe, Skog, & Bird, 2024).[2] None of them touches how the amount itself may be set.

One line of bail scholarship argues that the amount should do only one job: secure the defendant's return to court (Foote, 1965; Gouldin, 2016). On this view the amount should not be raised to address danger or read from a charge schedule, uses that sit in tension with longstanding bail doctrine (Section 2). Barring these uses does not remove the considerations

[1] N.M. Const. art. II, § 13 (amended 2016).

[2] *Valdez-Jimenez v. Eighth Judicial District Court*, 136 Nev. Adv. Op. 20, 460 P.3d 976 (2020).

behind them: some defendants are more likely to be rearrested, and some charges are more serious. What the rule changes is how the amount may be set in response. That leaves two empirical questions. How much had a defendant's risk of rearrest and the seriousness of the charge added to the bail amount before the reform? And once the rule bars those uses, is detention reduced, or recast as an explicit order denying release? This paper takes up both.

Maryland barred exactly these uses in 2017. Rule 4-216.1 told courts to set bail only to secure appearance, to keep it within the defendant's ability to pay, and not to set it primarily based on the current charge. The reform changed only the rules for setting the amount, not the set of eligible cases or the rest of the pretrial process. Comparing bail and detention before and after therefore isolates that change. Prior to the reform most pretrial detention came from nonpayment (86 percent), not from a formal order denying release. A rule confining the amount therefore reaches most of those detained. Maryland is also unusual: it is the only active money-bail system that pairs an appearance-only rule for the amount with broad authority to deny release (Petis, 2024) — the pairing this study exploits.

The rule's three prohibitions imply three testable predictions about what bail was doing prior to the reform. If defendants had been detained for inability to pay, confining the amount to their means should weaken how well the amount predicts who posts. If courts had been using the amount to detain on danger rather than ordering detention outright, barring that use should remove the amount's association with predicted violent rearrest — and, where the concern remains, move detention onto an explicit held-without-bail order. And if courts had been setting the amount by the charge, barring that should make the charge less predictive of the amount.

We bring these predictions to administrative court records on roughly 179,000 Maryland bail decisions. We estimate a regression discontinuity in time at the reform date. A difference-in-differences against neighboring Virginia, which made no comparable change, nets out trends shared by both states. We test the three predictions and measure how the reform changed release, bail amounts, posting, detention, failure to appear, and rearrest.

The study contributes to two literatures. Research on bail reform estimates the effects of pretrial detention and evaluates reforms to release eligibility, detention hearings, or the use of money bail (Dobbie, Goldin, & Yang, 2018; Gouldin, 2016; Heaton, Mayson, & Stevenson, 2017; Mayson, 2020; Stevenson, 2018). We study a different margin: a rule that left money bail in place but changed the rules for setting the amount. Work on front-line court decision-making studies how judges decide under uncertainty (Johnson, 2006; Kurlychek & Johnson, 2019; Steffensmeier, Ulmer, & Kramer, 1998; Ulmer & Johnson, 2017). Rule 4-216.1 rewrites those rules without changing who decides or how. Bushway and Forst (2013) call this the authority to write the rules, as distinct from the discretion that applies them. We trace how commissioners and judges set the bail amount, and whether inability to pay, danger, and charge severity reappeared elsewhere in the pretrial decision.

2 Background

2.1 What bail amounts could target

Money bail emerged in the nineteenth century as a tool to assure appearance, and by the mid-twentieth century empirical work documented that it was producing wealth-based detention: the Manhattan Bail Project found that many defendants with strong community ties were jailed because they could not afford bail (Ares, Rankin, & Sturz, 1963). Reformers pressed the point that monetary bail should be limited to securing appearance (Foote, 1965), a position with constitutional roots: in *Stack v. Boyle* (1951) the Supreme Court held that bail set higher than an amount reasonably calculated to assure appearance is excessive, and that the amount must be fixed for each defendant individually rather than by the charge. The American Bar Association's 1968 Standards on Pretrial Release (American Bar Association, 1968) and the 1966 federal Bail Reform Act[3] carried the same appearance-centered view. The 1966 Act addressed affordability chiefly by substituting non-financial release for money where appearance could be assured without it. The federal posture then admitted safety as a ground for detention—but only through procedure. The 1984 Bail Reform Act[4] authorized detention for dangerousness,

[3] Bail Reform Act of 1966, Pub. L. No. 89-465, 80 Stat. 214 (1966).
[4] Bail Reform Act of 1984, Pub. L. No. 98-473, 98 Stat. 1976 (codified as amended at 18 U.S.C. §§ 3141–3150).

and *United States v. Salerno* (1987)[5] upheld it on the strength of its procedural safeguards: a court could detain for dangerousness only after an adversary hearing, on written findings and stated reasons, subject to immediate review (Goldkamp, 1985).[6]

The modern ability-to-pay requirement is distinct from the older idea that defendants likely to appear should be released without requiring money. Earlier doctrine recognized that detaining an indigent defendant through money bail, without meaningful consideration of alternatives, raised due-process and equal-protection concerns.[7] In the mid-2010s, that principle was tested in a wave of procedural litigation over secured-bail schedules. Civil-rights litigants and the Department of Justice challenged systems that set money bail without individualized inquiry into ability to pay or nonfinancial alternatives, and federal courts treated those systems as constitutionally defective when they converted poverty into detention without adequate process.[8] The defect was not only distributive but procedural: an unaffordable condition detains without the procedural safeguards that made formal preventive detention constitutionally tolerable in *Salerno* (Mayson, 2020).

Pretrial detention carries costs. It raises recidivism, an effect concentrated among low-risk defendants (Sacks & Ackerman, 2014). Studies using random judge assignment estimate causal effects: detention raises conviction probability, mostly through guilty pleas, and lowers later employment (Didwania, 2020; Dobbie et al., 2018; Heaton et al., 2017; Stevenson, 2018). Black defendants face higher bail amounts than risk-equivalent White defendants (Arnold, Dobbie, & Yang, 2018). Volume is high, so small changes in how bail is set affect tens of thousands of defendants.

Against these costs, the benefit the amount is kept for — securing appearance — is itself uncertain. The evidence that a financial condition deters nonappearance is limited. One prosecutorial policy stopped seeking money bail for lower-level cases, and failure to appear and

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[5] *United States v. Salerno*, 481 U.S. 739 (1987).

[6] The federal Act followed the states rather than leading them: starting with the District of Columbia in 1970, thirty-four states had authorized detention on public-safety grounds before 1984 (Gouldin, 2016).

[7] *Pugh v. Rainwater*, 572 F.2d 1053 (5th Cir. 1978).

[8] *Walker v. City of Calhoun*, 901 F.3d 1245 (11th Cir. 2018); *O'Donnell v. Harris County*, 251 F. Supp. 3d 1052 (S.D. Tex. 2017), aff'd as modified, 892 F.3d 147 (5th Cir. 2018).

rearrest did not rise (Ouss & Stevenson, 2023). Field experiments find that non-financial measures, such as court-date reminders, reduce nonappearance at a fraction of the cost (Fishbane, Ouss, & Shah, 2020). The question is therefore not only whether money bail detains the poor, but whether the amount secures appearance, the one use the Maryland rule keeps it for.

2.2 Front-line decision-making

Bail-setting is a routinized, discretionary practice carried out at high volume by judicial officers deciding under uncertainty about defendant risk. Focal-concerns theory (Steffensmeier et al., 1998) provides the dominant criminological framework: judges weigh blameworthiness, dangerousness, and practical constraints, including jail space, processing time, and victim concerns, when making release decisions. Where information is incomplete or contested, judges develop heuristics, anchoring on charge severity or defaulting to local bail schedules (Albonetti, 1991; Englich, Mussweiler, & Strack, 2006). Bail schedules operate as going rates: they attach a typical amount to a charge type rather than to the individual (Sudnow, 1965). These routines are embedded in local court communities with their own norms and going rates (Johnson, 2006; Ulmer & Johnson, 2017). A commissioner sets one amount, weighing the danger a case is thought to pose, the going rate for its charge, and the constraints of a high-volume docket.

This framing motivates the analysis that follows. A rule that bars the danger concern from the bail amount need not remove the concern. If commissioners continue to weigh it, the focal-concerns account predicts they will act on it through the one channel the rule leaves open for that concern: holding a defendant without bail. The same logic implies that the reform's effect must be read across the decision sequence, not at a single stage. A commissioner's initial decision can be revised at bail review, and a shift at one point can be absorbed or undone at the next (Kurlychek & Johnson, 2019).

2.3 The reform landscape and prior evidence

Recent reforms have changed the case pool or the release decision, not what the amount may be set for. Appendix Table 31 maps Maryland and fourteen peer-state reforms across five architectural dimensions (Petis, 2024). New Jersey narrowed monetary bail eligibility and paired it with risk-assessment-based release; Illinois eliminated money bail under the Pretrial Fairness Act and reserved custody for dangerousness or willful flight; New Mexico expanded formal preventive-detention authority by constitutional amendment; and California and Nevada added procedure before an unaffordable amount may detain, after *In re Humphrey* (2021) and *Valdez-Jimenez* (2020).[9]

New York and Maryland both confine the bail amount to securing appearance while keeping money bail in active use, but their recent reforms took different routes. New York narrowed the set of charges for which a court may set money bail, leaving the amount-setting rule itself untouched, while Maryland left the eligible pool alone and restricted the purpose of the amount, keeping its preexisting detention authority in place.

Across reforms that restricted money bail, detention fell without a measured rise in crime. New Jersey's 2017 reform raised release without conditions by 9.2 percentage points and cut detention after a hearing by 14.6, with modest effects on failure to appear (Anderson Golub et al., 2019). Philadelphia's prosecutor-led no-money-bail directive raised release on recognizance about 11 points for eligible cases, with no detected increase in detention, failure to appear, or recidivism (Ouss & Stevenson, 2023). New York's reform reduced recidivism for the affected misdemeanor and nonviolent-felony pool, with the largest reductions among low-risk defendants and increases for a high-risk subgroup (Ropac & Rempel, 2023); the New York City reductions persisted over fifty months while effects elsewhere were null (Ropac, 2025). Where detention fell, adjacent mechanisms often absorbed part of the change. In Illinois, detention fell from roughly 33% to 9% across twenty-two counties, but expanded supervision

[9]The distinction is between a factor and a floor. Roughly forty of the fifty states already direct courts to weigh a defendant's ability to pay or financial resources, but only as one factor among many (Petis, 2024). That language did not stop an unaffordable amount from producing detention. The reforms here add an enforceable floor: individualized findings and a clear-and-convincing showing before an unaffordable amount may detain.

offset much of the jail-population decline (Griffin et al., 2024). In California, detention fell in San Francisco after *In re Humphrey* (Lacoe et al., 2024), but no statewide decline materialized, as judges shifted into findings-based detention (Virani, Campos-Bui, & Wallace, 2024). No study isolates a restriction on what the amount may be set for; this paper fills that gap.

3 Institutional Setting

3.1 Maryland's pretrial process

The pretrial process in Maryland follows a standard sequence. After arrest, a police officer prepares a charging document and brings the defendant to the District Court for an initial appearance hearing. The initial appearance is conducted by a District Court Commissioner, a non-attorney appointee who staffs the court continuously, including evenings, weekends, and holidays. Commissioners typically hear arrestees within hours of booking. At the initial appearance, the commissioner chooses among four statuses. Three allow release. Release on personal recognizance carries no financial condition; release on an unsecured personal bond sets an amount the defendant owes only if they fail to appear but need not post to be released; and held on default of bond sets a bail amount the defendant must post to obtain release. The fourth, held without bail, sets no amount and denies release outright.

Defendants whose initial appearance ends in held on default or held without bail are entitled to a bail review hearing before a District Court judge, typically within a few days. The bail review is a more deliberative proceeding than the initial appearance. Defense counsel is present, the judge has a fuller record of the defendant's history and circumstances, and the proceeding is not constrained by immediate post-arrest time pressure. The reviewing judge may affirm the commissioner's decision, modify the bail amount, move the defendant to a different release status, or impose additional nonfinancial conditions.

A District Court judge may also hold a defendant without bail under the general release standard, when no condition of release would reasonably assure appearance or safety (Rule

4-216). Criminal Procedure §5-202 adds a separate constraint at the commissioner stage: for defined categories of charges and circumstances, a commissioner may not authorize release at all and must hold the defendant for a judge.[10]

3.2 The 2017 reform: a three-stage transition

The reform emerged through a sequence of institutional signals over the eight months before formal adoption. Its proximate trigger came from within state government, against the backdrop of the national ability-to-pay litigation described in Section 2.1. In August 2016, five members of the Maryland House of Delegates asked the state Attorney General whether bail set without regard to ability to pay violated due process and equal protection. The Attorney General answered on October 11, 2016 with an advice letter setting out the legal rationale: money bail, as applied, produced detention through conditions defendants could not afford, and the law required an individualized inquiry into ability to pay. At the Attorney General's referral, the Rules Committee took up those concerns as the immediate impetus for proposed Rule 4-216.1 (Standing Committee on Rules of Practice and Procedure, 2016).

About two weeks later, on October 26, 2016, the Chief Judge of the District Court turned the Attorney General's opinion into operating guidance for judges and commissioners. The Letter of Advice instructed judicial officers to begin from release on recognizance, to use the least onerous conditions necessary, to use money only to assure appearance rather than public safety, and not to impose conditions the defendant could not afford. If a defendant posed a reasonable likelihood of danger, the Letter directed that the appropriate response was nonrelease rather than unaffordable bail. The Letter framed these instructions as cautionary advice under existing law rather than as a new rule. The Court of Appeals adopted Rule 4-216.1 on February 16, 2017 and the rule became effective on July 1, 2017. [11]

[10]The categories include escape, drug-kingpin charges, crimes of violence with qualifying prior convictions, certain new offenses committed while on pretrial release, protective-order violations, firearm offenses with qualifying prior convictions, and registered-sex-offender cases. For most categories a judge may later authorize release on suitable bail or other conditions, and several carry a rebuttable presumption of flight and danger at that stage.

[11]The rule otherwise retains a list of consideration factors that govern the overall release decision: the nature and circumstances of the offense, prior appearance record, employment and community ties, and danger to victims or the community.

The rule left adjacent features of Maryland's pretrial system untouched. It did not narrow the set of charges eligible for monetary bail, the lever eligibility-focused reforms pulled elsewhere. It built no statewide pretrial-services infrastructure and required no validated risk assessment.

4 Data and empirical strategy

4.1 Data and outcomes

We use administrative court records from the Maryland Judiciary's Case Search system, covering filings, charges, hearings, bail decisions, and dispositions for District Court jurisdictions statewide. The analysis sample is District Court criminal cases filed between January 1, 2016 and December 31, 2018 that received a bail decision at an initial appearance before a commissioner (Section 3.1) within 24 hours of arrest (Md. Rule 4-213). We exclude defendants held for extradition, whose custody is not set by the commissioner's discretionary decision, leaving 179,117 cases. Records are linked across cases by probabilistic record linkage (Appendix A.1).[12]

The primary outcome is realized pretrial status three days after the initial appearance. Three days captures the commissioner's decision and any change at the bail review that follows it (Section 3.1): the review comes immediately or at the court's next session (Rule 4-216.1(a)), plus a day for a weekend or holiday.

Among cases held on default, we observe the bail amount and whether the defendant posted. We measure posting status within one, three, five, or seven days. We measure detention two ways: any detention at the three-day endpoint, and days held from the initial appearance to the first observed release, capped at 365. In three counties (Baltimore City, Prince George's, and Montgomery) Circuit-side release is unobserved for many transferred cases (30.4% of cases transfer at some point), so the days-detained outcome restricts to the 160,766 fully observ-

[12]The dataset is documented in the Maryland Crime Research and Innovation Center's 2023 final report (Maryland Crime Research and Innovation Center, 2023).

able cases (89.8%); the decision-share, bail-amount, and any-detention outcomes use the full sample (Appendix B). Downstream outcomes are failure to appear during the pretrial period, rearrest within 12 months by offense type, and case resolution within a fixed 365-day window — whether the case closed and its disposition — with cases still open at that horizon coded unresolved.

Covariates include charge characteristics (felony, violent, firearm-or-weapon, and a detention-eligible indicator carrying a statutory presumption against release; Appendix A.2), demographics (age, race, sex), prior arrests, charges, and convictions over a complete three-year lookback, the initial-appearance court, and address-linked census-tract measures (we use tract median household income as an ability-to-pay proxy). We also construct frozen pre-period risk scores: a gradient-boosted model trained on released pre-October-2016 cases predicts three 12-month outcomes (any, violent, and failure to appear; AUC 0.69–0.71), then frozen and applied to every case to give a common defendant ordering across periods (Appendix A.3.1). The score discriminates similarly for Black and White defendants and for men and women, so it is no more accurate for any one group (Appendix Table 11). A defendant's charges alone predict the outcomes weakly (AUC 0.57–0.59); criminal history is the strongest contributor and lifts the full model to AUC 0.69–0.71 (Appendix Table 12), so the score is not a restatement of charge severity. The scores stay calibrated on released cases in the post-reform windows (Appendix Figure 2). The Maryland–Virginia comparison uses Virginia Pre-Trial Services Agency release-status data for 2014–2018 (Appendix A.5).

4.2 Regression discontinuity in time

Our primary design is a regression discontinuity in time at the October 2016 Letter of Advice (Hausman & Rapson, 2018). Of the reform's three signals (Section 3.2), the Letter was the first operational instruction to commissioners and judges, and the decision series breaks there rather than at the later adoption or effective dates. The running variable is each case's initial-appearance date relative to the cutoff, which we place at October 25, 2016, the day before the Letter issued: appearances through October 25 are pre-Letter, those from October 26 onward post-Letter. Panel A of Figure 1 makes this visible: held on default tracks near 50% through October 2016, drops in November, and drifts to the low twenties by late 2017, with release rising in mirror image and held without bail more than quadrupling, from about 5.0% to 20.7%.[13] We fit local-linear regressions on each side with a triangular kernel and 90-day bandwidth (rdrobust; Calonico, Cattaneo, & Titiunik, 2014), with robust, bias-corrected standard errors clustered by court-county. Section 5.6 reports sensitivity to the data-driven CCT bandwidth and the endpoint window.

A causal reading rests on three assumptions. First, no sorting across the cutoff: the Letter was an internal directive issued without public notice, so defendants, counsel, and police had little scope to time an appearance around it, and a density test shows no discontinuity in appearances at either reference date ($p = 0.18$ at October 25, $p = 0.72$ at July 1; Appendix C.1). Second, continuity of potential outcomes: predetermined covariates — charge attributes, defendant demographics (race, sex, age), pending caseload, and the frozen risk scores — are balanced at the primary 90-day bandwidth (all $p \geq 0.14$), with firearm-or-weapon, violent, and predicted-any-rearrest indicators turning marginal only at the narrowest windows (Appendix C.2). Third, no coincident Maryland change — which we cannot test directly, but the placebo cutoffs (Section 5.6), the absence of any known contemporaneous Maryland directive, and the Maryland–Virginia comparison below all bear on it.

4.3 Long-window and cross-state comparisons

Decisions kept changing between the Letter and the rule's July 1, 2017 effective date (Figure 1, Panel A), so we add a "long-window" comparison: the 90 days before October 25, 2016 against the 90 days after July 1, 2017, dropping the transition. The same local-linear specification applies. This contrast captures the mature post-Rule regime, but we treat it as descriptive: a

[13] The Attorney General's October 11 opinion precedes the cutoff by two weeks; it was addressed to the Rules Committee rather than to the officers who set bail (Section 3.2). At weekly resolution, decision shares drift a few points in the two weeks between the opinion and the Letter and then break sharply at the Letter. Excluding those two weeks in a donut specification leaves the decision estimates unchanged or larger, while dating the cutoff to the opinion itself yields no discontinuity on any decision margin (Appendix C.3).

causal reading would require continuity and no other Maryland change across both reference dates and the discarded months.

To absorb region-wide shocks the within-state design cannot, we compare Maryland to Virginia, which enacted no comparable rule in 2016–2018. The design is a case-level difference-in-differences of each decision outcome on a Maryland-post-October-25 indicator, with court, month, and charge-severity fixed effects, court-county clustered standard errors, and an event-study counterpart that tests parallel pre-trends (Figure 6; Appendix A.5). The comparison absorbs shocks common to the two states, such as regional crime trends or economic conditions; it cannot rule out a Maryland-specific shock coincident with the Letter, the third RDiT assumption above. Because Virginia's release-status data are coarser and differently defined (Appendix A.5), we read this comparison as a sign-and-magnitude check on the discontinuity rather than an independent design of equal standing.

4.4 Tests of the three prohibitions

Rule 4-216.1's three prohibitions on the bail amount, subsections (e)(1)(A)–(C), each imply a distinct, observable change in how bail is set, and they need not move together. We take each in turn. Each compares a feature of how bail is set — how strongly the amount predicts posting, how heavily the decision weights predicted risk, how much the charge fixes the amount — before and after the reform, and asks whether it moved as the prohibition predicts. We read these as descriptive comparisons, like the long-window contrast: evidence that bail-setting changed in the predicted direction, not causal estimates of the reform's effect on these relationships.

The *ability-to-pay* test asks whether, among cases held on default of bail, the bail amount became a weaker predictor of who posts within three days. Before the reform the two were tightly linked: a higher amount put release out of reach for more defendants, so who was released depended on who could pay. Confining the amount to a defendant's means should weaken that link. We regress three-day posting on the log amount interacted with a post-reform indicator, among held-on-default cases; the coefficient on the amount is the posting–bail slope, and the test is whether it flattens after the reform.

The *disentangling* test asks whether public safety risk moved off the bail amount and onto the held-without-bail decision. The prediction is asymmetric across the two risks. Predicted rearrest should lose its association with the bail decision and bail amount and gain one with held without bail: before the reform a defendant judged dangerous could be detained through a high amount, and the rule forecloses that channel, leaving held without bail the route that remains. Flight is different, and it splits across the two margins. The decision to set a money bail should lean more on flight, since securing appearance is the only purpose the rule still allows it to serve. The amount carries no signed prediction: a bail amount deters nonappearance only insofar as losing it would hurt, and how much it hurts depends on the defendant's means, so the deterring amount differs from person to person. Because the ability-to-pay limit ties the amount to what a defendant can post, the amount reflects means rather than flight risk.

We measure how strongly each risk drives each decision by regressing the decision — the money-bail amount and the held-without-bail order — on the standardized rearrest (both any and violent) and flight scores, each interacted with the reform, so every loading is net of the other axis. The rearrest loadings should fall for the amount and rise for held without bail. We add a case's full set of specific charge codes, so the shift is estimated within charges rather than from a change in the charge mix.

The *charge-neutrality* test asks whether a case's charges still fix the bail amount on their own, beyond the defendant's predicted risk. We measure the extra variation in the log amount that a case's charges explain on top of the frozen risk scores — their incremental adjusted R^2 — before and after the reform. Because the schedule ban came with the Rule, not the Letter, this share should fall at the Rule but not at the Letter.

Netting out predicted risk matters for two reasons. A serious charge can legitimately raise the amount by raising the defendant's risk; what the rule bars is a schedule that ties an amount to the charge directly, so removing risk isolates the schedule. It also guards against a confound: serious charges carry higher predicted risk, so to the extent the rule's separate ban on danger pulls that risk off the amount, a raw fall in the charges' explanatory power could come from the risk leaving rather than from the schedule loosening. We also recompute the share on within-period ranks of the amount, so the comparison is not an artifact of amounts shrinking.

Standard errors cluster by court-county for the ability-to-pay and disentangling regressions, adding week for the posting regression; the charge-neutrality interval is a court-county cluster bootstrap of the pre-to-post change. Appendix A.6 states the three estimating equations.

5 The reform's effects

Table 1: Summary statistics, two 90-day comparison windows

		Local window		Long window	
	Pre	Post	Δ	Post	Δ
A. Sample composition					
N	15,044	12,977	–	15,685	–
B. Decision outcomes					
% on recognizance	39.3%	47.9%	8.6 pp***	51.5%	12.3 pp***
% unsecured bond	5.9%	7.0%	1.2 pp***	8.6%	2.8 pp***
% held on default	48.2%	33.3%	-14.9 pp***	23.2%	-24.9 pp***
% held w/o bail	6.5%	11.7%	5.2 pp***	16.5%	10.0 pp***
C. Bail and posting (held on default)					
Mean bail amount	$37.0K	$20.3K	−$16.7K***	$10.6K	−$26.4K***
% posted within 3 days	51.6%	60.5%	8.9 pp***	59.8%	8.2 pp***
D. Detention					
Mean days detained	24.25	18.44	−5.81***	20.31	−3.94***
% detained >3 days	23.5%	19.5%	-4.0 pp***	20.2%	-3.3 pp***
% detained >30 days	15.1%	12.6%	-2.5 pp***	13.7%	-1.4 pp**
E. Downstream outcomes					
FTA (pretrial)	10.1%	10.4%	0.3 pp	10.6%	0.5 pp
12-mo any rearrest	38.2%	38.8%	0.6 pp	37.6%	-0.7 pp
12-mo violent rearrest	12.7%	13.4%	0.7 pp	12.0%	-0.7 pp
F. Defendant demographics					
Age at filing	33.02	32.81	−0.21	33.35	0.33*
% female	21.1%	21.1%	0.1 pp	20.3%	-0.8 pp
% Black	54.9%	58.8%	3.9 pp***	55.6%	0.7 pp
% White	42.2%	38.3%	-3.9 pp***	40.7%	-1.6 pp**
G. Charge characteristics					
% felony	30.3%	30.7%	0.5 pp	29.7%	-0.5 pp
% violent	30.4%	30.2%	-0.3 pp	29.2%	-1.2 pp*
% firearm or weapon	9.0%	10.2%	1.2 pp***	8.8%	-0.2 pp
% §5-202 trigger	6.5%	6.6%	0.1 pp	7.2%	0.7 pp*

Notes: Continuous variables reported as means; binary variables as percentages. Δ columns report post-pre difference with significance markers: * $p < .05$, ** $p < .01$, *** $p < .001$ (Welch t-test for continuous variables, two-proportion z-test for binary). The Pre column is the 90 days before October 25, 2016, common to both windows. Local Post: the 90 days following October 25, 2016. Long Post: the 90 days following July 1, 2017. Decision outcomes (Panel B) and bail amounts (Panel C) are measured three days after the initial appearance. All panels condition on the analysis sample (*N* in Panel A). Panel C further restricts to cases held on default of bond at three days. Panel D excludes cases transferred to Circuit Court in the three counties without observed Circuit-side release events.

Table 2: Local-linear RDiT estimates of the reform's effect.

	October cutoff	Long window
A. Decisions		
Released	10.2*	19.9***
Held on default	-13.1**	-26.2***
Held without bail	2.6	6.0**
B. Bail and detention		
Bail $/case	-$8.4k*	-$15.2k***
Days detained — combined	-1.1	-6.1
District-only	-2.5	-2.9
Circuit-transferred	0.8	-12.5
C. Safety and case resolution		
Failure to appear	1.0	0.8
Any rearrest, 1 yr	4.2	0.1
Violent rearrest, 1 yr	0.8	-1.1
Case closed, 1 yr	3.5	3.0
N (effective)	27,290	29,727

Note. Both columns pooled; 90-day bandwidth. Charge, risk, and demographic breakdowns in Table 3. Decisions, safety, and case-resolution shares in percentage points; bail in dollars per case; detention in days. Days-detained rows use the detention-measurement sample (Section 4.1). N is the effective (kernel-weighted) sample. $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$ on robust CCT, court-county-clustered SEs.

Table 1 describes the analysis sample across the two 90-day windows the rest of this section compares. Before the reform, in the 90 days before October 25, 2016, it comprised 15,044 commissioner cases. At three days, the most common status was held on default of bond (48.2%), followed by release on recognizance (39.3%); 5.9% received an unsecured bond and 6.5% were held without bail. Among the cases held on default, the mean bail amount was about $37,000 and 51.6% posted within three days; 23.5% were still detained after three days, and the mean case was detained 24 days. Failure to appear occurred in 10.1% of cases, and 38.2% were rearrested within twelve months. The defendants were 33 years old on average, 21.1% female, 54.9% Black and 42.2% White; 30.3% faced a felony, 30.4% a violent offense, and 9.0% a firearm or weapon. The remaining columns report the two post-reform windows — the 90 days after the October 2016 cutoff and after the July 2017 effective date — whose changes the rest of this section estimates.

5.1 Decisions and bail amounts

Table 2 reports the reform's estimated effect on each outcome — the discontinuity at the October 25 cutoff and the long pre-reform-to-post-Rule contrast. At the cutoff, the share of cases released on recognizance or unsecured personal bond rose by 10.2 percentage points (95% CI $[0.7, 19.6]$). The share held on default of bond fell by 13.1 percentage points ($[-23.0, -3.3]$). The share held without bail did not move (+2.6 percentage points, $[-1.1, 6.3]$). Over the long window, release rose by 19.9 percentage points ($[9.6, 30.2]$), held on default fell by 26.2 ($[-36.2, -16.2]$), and held without bail rose by 6.0 ($[2.1, 9.8]$).

Bail set per case fell by \$8,401 ([−\$15,339, −\$1,462], excluding zero) at the October cutoff, 45.9% of the \$18,300 pre-reform per-case mean; over the long window the drop reached \$15,159 ([−\$21,294, −\$9,024]), or 82.9% of that mean. Among cases still held on default of bond, the mean bail amount fell by more than 70%, in two steps (Panel B of Figure 1): from about \$40,000 through October 2016 to about \$20,000 at the October 25 cutoff, then to about \$11,000 at the July 2017 effective date, where it stays through 2018.

5.2 Safety and case resolution

The failure-to-appear rate rose by 1.0 percentage points ($[-0.9, 3.0]$) at the October cutoff and by 0.8 ($[-1.1, 2.8]$) over the long window, both intervals including zero. Violent rearrest within twelve months was likewise flat (+0.8, $[-1.4, 3.0]$ at the October cutoff; -1.1, $[-4.0, 1.8]$ over the long window). Any rearrest within twelve months has the only non-trivial point estimate among the safety outcomes: it rose by +4.2 at the October cutoff ($[-0.9, 9.4]$, $p = .11$), but the interval includes zero and the estimate does not persist, centering on zero over the long window (+0.1, $[-4.7, 4.8]$). This null does not arise from confining release to low-risk cases; Section 5.5 characterizes whom the reform released.

A case's chance of closing within a year held near its pre-reform level of 84.3% (+3.5 percentage points, 95% CI $[-0.9, 7.8]$ at the October cutoff; +3.0, $[-0.9, 6.9]$ over the long window). The mix of dispositions was also unchanged: guilty, not-guilty, probation-before-judgment, stet, and other are all small and none reaches significance, at the cutoff or over the long window (supplement Table 32).

5.3 Heterogeneity by severity, risk, and demographics

Table 3 shows the money-bail decline split unevenly by charge severity. For misdemeanor-nonviolent cases the decline went to release, and held without bail barely moved (-0.1 percentage points). For felony-violent cases most of the decline went the other way: held without bail rose $+31.0$ points, far more than release. The more serious the charge, the larger the share of the money-bail decline absorbed by held without bail rather than release.

The same sorting appears in the magnitudes and in predicted risk. The bail-amount decline is largest for felony-violent cases ($-$\$52,593) and smallest for misdemeanor-nonviolent ($-$\$4,342). Predicted violent rearrest orders the held-without-bail rise the same way (Table 3, Panel B): held without bail rose $+13.1$ percentage points in the top risk tercile against -1.6 in the bottom. Safety estimates are null across subgroups, with the lone exception of a higher failure-to-appear estimate in the top tercile (Table 3); the intervals are otherwise wide.

The release gains also varied by race and sex. Over the long window, release rose +22.8 percentage points for White defendants against +15.6 for Black, and pretrial detention fell correspondingly less for Black defendants — largely because Black defendants are more often charged with a felony or a firearm offense and carry higher predicted rearrest, the cases the reform moved least toward release (Table 3). The gap is not significant at the October cutoff or once charge, predicted risk, and court are controlled (Appendix D.1). Money bail fell by more in dollars for Black defendants ($-$\$17,081 versus $-$\$12,962), but from higher pre-reform amounts, so the proportional cut was larger for White defendants (94.4% versus 84.3%); by sex, amounts fell more for men than women ($-$\$16,482 versus $-$\$10,047).

Table 3: Heterogeneity in the reform's effects, by charge, predicted risk, and demographics.

		Decisions				Safety (12-mo)			
	Share	Released	Held on default	Held w/o bail	Bail ($1,000s)	FTA	Viol. rearrest	Any rearrest	N
A. By charge type									
§5-202	7%	16.6	-18.4*	1.7	-$22.9	-0.5	1.5	8.9	2,049
		(21.6)	(55.5)	(22.8)	($28.1)	(5.5)	(18.2)	(47.4)	
Felony, violent	10%	10.7	-41.9*	31.0*	-$52.6*	1.7	-4.1	-1.4	2,953
		(7.4)	(75.1)	(17.2)	($72.1)	(0.6)	(15.1)	(30.9)	
Felony, nonviolent	17%	23.7*	-33.5*	9.2*	-$25.8*	-0.8	0.6	5.5	5,154
		(22.7)	(71.8)	(5.3)	($33.0)	(6.0)	(9.2)	(37.0)	
Misd., violent	18%	15.5	-26.3*	10.5*	-$5.7*	0.6	-0.1	1.1	5,441
		(39.5)	(57.8)	(2.6)	($11.4)	(6.5)	(17.3)	(32.1)	
Misd., nonviolent	48%	19.4*	-19.6*	-0.1	-$4.3*	0.5	-1.5	-3.7	14,130
		(64.3)	(33.1)	(2.3)	($5.0)	(15.9)	(10.9)	(42.2)	
B. By predicted violent rearrest									
Bottom tercile	25%	25.0*	-24.0*	-1.6	-$9.1*	0.4	1.5	1.1	7,437
		(60.1)	(37.4)	(2.3)	($11.3)	(10.3)	(4.9)	(23.4)	
Middle tercile	32%	17.6*	-21.8*	4.0*	-$17.4*	-3.4	0.1	0.1	9,530
		(44.6)	(50.8)	(4.4)	($21.1)	(9.7)	(9.2)	(33.3)	
Top tercile	43%	16.8*	-30.1*	13.1*	-$16.9*	4.3*	-2.8	1.1	12,760
		(34.2)	(56.8)	(8.7)	($22.5)	(10.5)	(19.7)	(51.0)	
C. By race and sex									
Black male	46%	13.4*	-23.8*	10.2*	-$18.3*	2.0	-4.2	0.9	13,646
		(39.2)	(53.1)	(7.5)	($23.8)	(9.5)	(15.6)	(42.2)	
Black female	9%	26.9*	-25.8*	-2.4	-$10.2*	2.4	3.7	9.9	2,765
		(56.3)	(41.8)	(1.7)	($11.2)	(12.0)	(11.8)	(29.0)	
White male	31%	24.9*	-27.0*	1.8	-$14.0*	2.0	1.2	-0.7	9,129
		(42.9)	(51.1)	(5.6)	($17.9)	(9.6)	(10.5)	(37.3)	
White female	11%	25.2*	-28.5*	3.0	-$10.6*	-9.6*	2.0	-6.4	3,202
		(53.8)	(43.1)	(2.8)	($8.6)	(13.3)	(8.8)	(38.5)	

Note. Long-window RDiT estimates within each subgroup, with the pre-reform baseline in parentheses below each estimate. *Share* is the subgroup's share of the analysis sample. Decisions and safety outcomes in percentage points, bail in thousands of dollars per case; failure to appear and rearrest are measured over twelve months. Risk terciles are of the frozen pre-reform predicted-violent-rearrest score. * marks a 95% interval excluding zero, court-county clustered SEs. N is the effective (kernel-weighted) sample for each subgroup. The October-cutoff estimates appear in Appendix Table 33.

5.4 What drove the detention change

Across the full sample, the probability of any pretrial detention fell by 10.5 percentage points at the October cutoff and 20.2 over the long window, as the held-on-default share gave way to release. On the detention-measurement sample (Section 4.1), days detained fell by 1.1 days ($[-10.8, 8.6]$) at the October cutoff and by 6.1 days ($[-15.0, 2.8]$) over the long window; neither interval excludes zero. Panel C of Figure 1 shows monthly mean days detained in a band of roughly 14 to 22 days, with post-reform months in the lower half.

Total days barely moved, but the reform reallocated the money-bail cases sharply — some to release, some to held without bail, the rest to a lower money bail. Most pre-reform detention came from money-bail nonpayment, so these cases carry the detention change, and across them three channels pull against each other: cases routed to release spend fewer days, cases routed to held without bail spend more, and the cases left on money bail are ambiguous, since amounts fell but whether their detention fell, held, or rose depends on which cases stayed.

We take the cases set money bail before the reform and ask what the reform would have done with each one — which decision it would reach, and how long the defendant would be detained — then compare that to what the case actually experienced. We predict each case's reform outcome from how similar cases, in their charges, risk scores, prior record, and demographics, were treated once the rule was in force.

We use two prediction models, both fit on post-Rule cases. A decision model predicts, for each case, the probability of release, money bail, and held without bail; a duration model predicts the days a case would be detained under each of those decisions, $\hat{E}[\text{days} \mid d, X]$. A case's predicted reform detention is the days under each decision, weighted by that decision's probability, $\sum_d \hat{P}(d \mid X)\, \hat{E}[\text{days} \mid d, X]$. Only the reform side is predicted: each case's money-bail amount and days served are observed, so the detention comparison sets observed money-bail days against predicted reform days, by predicted decision.

Two diagnostics support the simulation. First, both models are calibrated: the decision model's predicted probabilities match observed decision frequencies, and the duration model's predicted days match observed days. Second, the reform shift the model implies — toward release and toward held without bail — reproduces the direction and rough magnitude of the release and held-without-bail discontinuities in Table 2, estimated with no model. Both diagnostics are in Appendix A.3.2.

The simulation rests on one substantive assumption: that decisions in both regimes are driven by the characteristics we observe. The observable case mix does shift modestly across the pre-reform and post-Rule windows — toward fewer violent charges and fewer Circuit transfers (Appendix C.2) — but the simulation conditions on those features case by case. What remains is selection on unobservables: case features commissioners and judges acted on but

Table 4: Predicted reform decisions for cases set money bail before the reform.

			Mean days detained	
Predicted reform decision	Share	Amount	Money bail	Reform
Released	50.9%	\$25,000	23	2
Still on money bail	27.9%	\$40,000	30	21
Held without bail	21.2%	\$82,000	43	63

Note. Cases held on money bail before the reform, weighted by their predicted probability of each decision under the post-reform regime (Appendix A.3.2). Share is the predicted allocation across the three reform decisions. Bail amount and days under money bail are observed; days under the reform are predicted from a post-reform model. Amounts are arithmetic means.

our data does not record, on which cases alike in everything we observe could still differ.

Table 4 reports the predicted allocation: 50.9% of the money-bail cases released, 27.9% set a lower money bail, and 21.2% held without bail. The released cases had the lowest observed amounts, about \$25,000, and the shortest observed detention, 23 days; the held-without-bail cases had the highest, about \$82,000 and 43 days.

The released cases fall from 23 observed days to 2 predicted, and the cases left on money bail fall from 30 to 21 days as amounts drop — the ambiguous channel resolves downward. The held-without-bail cases run the other way, rising from 43 to 63 days, and that rise offsets part of the two declines.

The held-without-bail cases are where money bail had most resembled detention — the highest amounts, the longest stays. For them the reform makes that detention explicit, holding by order rather than by an amount set beyond reach.

5.5 Whom the reform moved, and whom it could have

The reform's detention decline comes from the defendants it moved from detention to release. We characterize that group and benchmark it against the cases that stayed detained, by predicted risk and current charge, separately for felonies and misdemeanors (Table 5). Because the reform changes who is released, the marginal releases are not observed directly: no defendant is seen under both regimes. We recover their profile with the counterfactual of Section 5.4.

Table 5: Whom the reform moved, and whom it could have: predicted risk and current charge of the released and detained, long window.

	All released	Marginal released	Detained, lowest risk	Detained, no vio/ firearm	All detained
Panel A. Felony					
N	1,179	537	537	537	3,480
Predicted rearrest	0.37	0.39	0.18	0.26	0.42
Predicted violent rearrest	0.10	0.11	0.08	0.06	0.15
Predicted failure to appear	0.10	0.09	0.03	0.06	0.08
Violent charge (%)	15	15	77	0	44
Gun/firearm charge (%)	3	1	23	0	23
§5-202 detention-eligible (%)	4	3	6	3	12
Any drug charge (%)	39	46	15	59	34
Held without bail (%)	0	—	67	23	57
Secured bail (median)	—	—	\$7,750	\$7,500	\$7,500
Panel B. Misdemeanor					
N	7,095	1,472	1,472	1,472	3,738
Predicted rearrest	0.37	0.39	0.22	0.41	0.43
Predicted violent rearrest	0.12	0.13	0.09	0.12	0.16
Predicted failure to appear	0.16	0.17	0.10	0.18	0.17
Violent charge (%)	19	22	54	0	40
Gun/firearm charge (%)	3	6	13	0	9
§5-202 detention-eligible (%)	4	7	5	16	10
Any drug charge (%)	34	34	14	31	18
Held without bail (%)	0	—	37	31	38
Secured bail (median)	—	—	\$3,500	\$3,000	\$3,000

Notes. Long window (pre-reform vs. post-Rule). Release is on recognizance or unsecured bond; detained is the complement. *Marginal released* are the defendants the reform moved from detention to release (Section 5.4). *Detained, lowest risk* are the lowest-predicted-rearrest detained, matched in size to the marginal releases; *Detained, no vio/firearm* applies the same within cases carrying no violent, weapon, or firearm charge. Predicted scores are frozen pre-reform risk models. Secured bail is the median amount among defendants held on default with a positive bail amount. §5-202 detention-eligible flags cases a commissioner may not release (Md. CP §5-202, the broad commissioner-no-release set); gun/firearm is a current firearm charge.

For each pre-reform case, the decision model predicts the probability of release under the old rules and under the new rules, and the rise between the two measures how far the reform moves that case toward release. Each defendant also carries a predicted risk — a model's estimated probability that the defendant fails to appear, is rearrested, or is rearrested for a violent offense. Averaging those predicted risks across the pre-reform cases, and weighting each case by its release-probability rise, gives the risk profile of the defendants the reform moves from detention to release.[14]

[14] Release is a single yes-or-no margin, so the same average can be recovered a second way, with no model at all, from the increase in the release rate alone—provided the reform moved essentially no one in the other direction, from release into detention. The two estimates agree (Appendix A.4), despite resting on different assumptions, and the one assumption the model-free version needs holds in the data: almost no defendant's predicted release probability falls under the reform.

The marginal releases — defendants held on default before the reform and released after it — are not a low-risk group: their predicted rearrest and violent-rearrest risk sit near the full sample's and above the already-released, with failure-to-appear risk matched. The absence of any detectable rise in failure to appear or rearrest (Section 5.2) therefore does not come from releasing only the safest cases.

Set against who stayed detained, though, the releases were not the lowest-risk cases available. Compared with an equal number of the lowest-predicted-risk detained (Table 5, "Detained, lowest risk"), the pattern differs by charge. Among felonies, the detained score below the releases on predicted rearrest (0.18 against 0.39) — whether ranked by charge severity or predicted risk, and even with no violent or weapon charge (0.26 against 0.39). Among misdemeanors, the detained are lower-risk only when the release set extends to violent charges; with violent and weapon charges set aside, they are no lower in risk than the released (0.41 against 0.39). Either way, the lower-risk detained are mostly held not without bail but on a secured bail they did not post, a median of $7,500 for felonies and $3,000 for misdemeanors. Section 7 takes up what this implies for targeting detention.

5.6 Robustness

The effects survive the design checks reported in Appendix C. The decision-share estimates move by a percentage point or less between the fixed and data-driven bandwidths, and by less than 0.2 points across the 3-, 7-, and 14-day measurement endpoints. Placebo cutoffs in stable pre- and post-reform periods return a null at every one (0 of 20 significant); the effect appears at the true October 25 cutoff alone. And a Maryland–Virginia difference-in-differences, which nets out shocks common to the region, preserves the signs and significance of the three decision effects.

Section 6 turns from the reform's effects to how it changed bail-setting, testing whether courts stopped using the amount to detain those who could not pay, to price danger, and to apply a charge schedule.

6 Testing the three substantive predictions

Section 5 showed the reform moved cases off money bail. This section turns to how bail had been set, testing the three predictions the rule's prohibitions imply: on ability to pay, on disentangling appearance from danger, and on charge-neutrality.

Ability to pay. Section 4.4 predicted that the ability-to-pay limit would make the bail amount a weaker predictor of who posts. The reform met the limit bluntly: it lowered and downgraded money bail rather than making any given amount more payable, and the share held on default and still detained at three days dropped by about half over the long window (from 20.0%; supplement Table 35), through judicial release and lower amounts.

A defendant held on bail can leave detention two ways: by posting the bail or by a judicial downgrade to recognizance or an unsecured bond. We regress each route — whether the case posted, and whether it was downgraded — on a post-reform indicator. Posting rose 3.0 percentage points over the long window (Table 6), but the rise was composition, not improved ability to pay: holding the amount fixed, posting did not change (0.5 points, not significant). And the amount predicts posting in no simple lower-is-easier direction — posting was highest in the middle of the distribution, near 63%, and lowest at both ends, among the smallest amounts as well as the largest (Table 6).

Judicial downgrades rose more, and unlike posting the rise was not composition: holding the amount fixed, downgrades rose 3.4 points (6.0 unadjusted), concentrated at the high amounts — among cases set \$50,000 or more they went from 2 to 8%, a fourfold increase (Table 6). Where the amount was highest and posting lowest, judges released more defendants outright.

About one in four cases held on default still did not post within three days, even at the lower amounts (Table 34). These non-posters faced the same \$5,000 median bail as posters, down from \$15,000, and no heavier charges; what separated them was predicted risk — failure-to-appear risk at the 65th percentile against the 46th. Non-posting alone cannot distinguish

Table 6: How money-bail cases left detention, by bail amount.

	Posted money bail		Downgraded by judge		
Bail amount	Pre	Post	Pre	Post	N (pre/post)
$0–2,500	45%	39%	17%	21%	460/671
$2,500–5,000	59%	59%	11%	14%	1,419/940
$5,000–10,000	63%	64%	7%	12%	1,737/1,110
$10,000–25,000	63%	66%	6%	7%	1,544/699
$25,000–50,000	55%	56%	5%	9%	861/268
$50,000+	37%	44%	2%	8%	1,735/144
All amounts	54%	57%	7%	13%	7,756/3,832
Reform effect (Oct / long)	$+5.1^{***}$ / $+3.0^{*}$		$+5.2^{***}$ / $+6.0^{***}$		

Sample: cases set a money bail at the initial appearance (held on default of bond). *Posted money bail* = released within three days with the money-bail decision still standing. *Downgraded by judge* = the bail-review decision was switched to release on recognizance or an unsecured personal bond. Pre-reform is the 90 days before October 25, 2016; post-Rule the 90 days after July 1, 2017. The reform-effect row regresses each outcome on a post-reform indicator (percentage points; court-and-week clustered standard errors); cells pair the October-cutoff and long-window estimates. $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$.

whether they could not afford the amount or did not post for other reasons. The discussion returns to this group.

Disentangling Section 4.4 predicted that correlates of future rearrest would move off the money-bail amount and onto an explicit held-without-bail order, and that flight would split — loading the money-bail decision but not the amount.

Table 7 shows that before the reform, a one-standard-deviation increase in predicted violent rearrest raised the chance of a held-without-bail order by 1.1 percentage points. The reform raised that effect by a further 3.9 points and cut the same risk's effect on the money-bail decision by 4.0, holding flight risk fixed. Predicted any-rearrest risk moved the decision the same way (4.3 and 5.1 points), so the reform moved general recidivism risk, not violence alone, onto held without bail. These estimates are net of the case's specific charge codes and of tract income, so the shift is neither a reshuffling of charges nor a proxy for who can pay; it holds across weaker control sets as well (Appendix D.2). It is clearest over the long window; at the Letter the same shift is smaller (1.4 and 2.2 points).

Among cases still given money bail, the bail amount had been set higher for defendants at

Table 7: Disentangling: predicted-risk loadings on the pretrial decision and the bail amount.

	Held without bail (pp / SD)		Held on default (pp / SD)		Bail amount (% / SD)	
	pre	Δ	pre	Δ	pre	Δ
Predicted violent rearrest	1.1**	3.9***	4.1***	−4.0***	7.2**	−5.1*
Predicted any rearrest	1.2*	4.3***	7.9***	−5.1***	6.6**	−4.1
Predicted failure to appear	0.4	−1.1	2.9**	−0.3	−4.4	−4.0

Note. Each row is a frozen pre-period predicted-risk score; entries are the association between a one-standard-deviation increase in that score and the named outcome. For each outcome, *pre* is the pre-reform level and Δ is the change at the reform (long window, pre-reform vs. post-Rule). Held without bail and held on default are in percentage points; the bail amount is in percent (log points), conditional on a case held on default with a positive amount. A single fully-controlled specification: frozen pre-period risk, demographics, and charge attributes, tract median household income, and the full set of specific charge codes a case carries, each interacted with Post, with court and month fixed effects; predicted violent rearrest and predicted failure to appear enter jointly, so each loading is net of the other. The 9,577 amount observations are the income-observed cases held on default; the 26,434 decision observations are all income-observed cases. Pre-reform shares: held without bail 6%, held on default 50%. Stars from court-county-clustered inference (24 courts). The A/B/+income/+charge-code robustness ladder, for both the Letter and post-Rule windows, is in Appendix D.2.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

higher predicted violent rearrest: a one-standard-deviation increase raised it by 7.2% before the reform (Table 7). The reform cut that gradient to 2.2%, a significant reduction.

So far the analysis has used the predicted risk scores. The final check uses a realized outcome instead: whether the reform's decision shift predicts defendants' actual later violence. For each case we take the decision shift behind the money-bail counterfactual (Section 5.4): how far the post-reform rule moves the case's predicted decision probability from the pre-reform rule's. We relate that shift to realized violent rearrest, measured among defendants released within three days, within charge, since rearrest is observed only out of custody. The further the reform shifted a case toward held without bail, the higher its later violent rearrest (2.3 points per standard deviation of the shift); the further toward money bail, the lower (2.8 fewer). The shift does not predict failure to appear (Table 8). Among released defendants, then, the reform's shift toward held without bail predicts higher realized violent rearrest, not only higher predicted risk.

Predicted flight risk played a different role from predicted violent rearrest. The decision to

Table 8: Realized outcomes and the rule-induced decision shift (within-charge)

	Change in decision probability (post − pre)	
Realized outcome	Held without bail	Money bail
Violent rearrest	+2.3*** (0.5)	-2.8*** (0.6)
Any rearrest	+1.3 (0.8)	-4.5*** (0.9)
Failure to appear	-1.1*** (0.3)	-0.9 (0.5)

Each cell is the coefficient b (percentage points; standard errors in parentheses) from a regression of the realized outcome on the standardized, within-charge rule-induced decision shift $z(\Delta\hat{d}_i)$, where $\Delta\hat{d}_i = \hat{d}_{\text{post}}(X_i) - \hat{d}_{\text{pre}}(X_i)$ is the change in a case's predicted three-day decision from the pre- to the post-reform rule. A more positive money-bail value means a case retained more on money bail; a more negative value, a case pushed off it. Outcomes are observed only among defendants released by the three-day endpoint (twelve-month rearrest by type and pretrial failure to appear). Controls: age, sex, race, pending cases, court and month fixed effects. $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$.

set a money bail did load on flight: a one-standard-deviation increase raised it by 2.9 percentage points before the reform — about as much as predicted violent rearrest raised it (4.1 points) — and the reform left that loading in place. The bail *amount* did not: it was no higher for defendants at higher predicted flight (−4.4%, not significant), even as it was set 7.2% higher for a standard deviation of predicted violent rearrest. The dollar figure — the part of money bail the rule reserves for securing appearance — had been set by predicted violent rearrest, not by predicted flight. The rule had predicted that, once danger was barred, the money-bail decision would lean more on flight. It did not: flight already loaded on that decision before the reform, and the amount had carried no flight loading to begin with.

Charge-neutrality Section 4.4 predicted that under the charge-neutrality limit a case's specific charges would fix the bail amount less. Table 9 shows that, net of predicted risk, a case's charges explained 22.7% of the variation in amounts before the reform and 16.0% after — a fall of 6.7 points (95% CI $[-9.5, -4.0]$), about 29.4% of their pre-reform explanatory power. The charge still fixes a meaningful share, so the limit loosened the schedule rather than removing it.

A fall in the charge's explanatory power could come from sources other than de-scheduling, and the table separates three of them. The first is the charge's overlap with predicted risk: a

Table 9: Charge-neutrality: adjusted R^2 of the bail amount on a case's charges

	Pre	Post	Change (95% CI)
Long (pre-reform → post-Rule)			
Net of predicted risk	23	16	-6.7 $[-9.5, -4.0]$
Net of predicted risk, on ranks	20	15	-5.5 $[-8.2, -2.5]$
Raw	37	28	-8.8 $[-11.9, -5.0]$
October 2016 (placebo)			
Net of predicted risk	23	26	$+2.7$ $[-4.8, +6.2]$
Net of predicted risk, on ranks	20	20	-0.7 $[-5.8, +2.4]$
Raw	37	39	$+2.1$ $[-6.1, +6.3]$

Note. Each entry is an adjusted R^2 in percent, from a regression of the bail amount on indicators for every charge a case carries, among cases set a money bail. "Net of predicted risk" is the rise in adjusted R^2 when the charge indicators are added to a regression on the frozen flight, violent-rearrest, and any-rearrest scores; it captures what the charge fixes beyond individualized risk, so a fall is de-scheduling rather than danger leaving the amount. "On ranks" recomputes the same incremental statistic on each period's ranks of the amount, so the post-reform compression of dollar amounts cannot drive the fall. "Raw" regresses the log amount on the charges alone, omitting the risk scores. The long window contrasts the 90 days before October 25, 2016 with the 90 days after July 1, 2017; October 2016 is a placebo, since the charge-schedule prohibition is a Rule provision absent from the Letter. Confidence intervals from a court-cluster bootstrap ($B = 1{,}000$).

more serious charge carries more predicted risk, so part of what the raw share captures is risk rather than the schedule. The raw row strips out nothing, and even there the share falls, from 36.9% to 28.1% — risk leaving the amount accounts for part of the decline, not all of it. The second is compression: amounts fell by more than 70%, leaving less dollar variation for any predictor to explain. The on-ranks row recomputes the net-of-risk share on within-period ranks of the amount, which compression leaves untouched, and the share falls by a comparable amount (20.2% to 14.6%). The third is timing: the charge-schedule provision was written into the Rule, not the Letter, so a fall that predated the Rule would point to a general trend. It does not — at the October placebo the net-of-risk share does not move (23.0% to 25.7%, the interval $[-4.8, +6.2]$ spanning zero). One source lies off the table: cases might carry fewer charges after the reform. They do not — 2.8 per case before against 2.6 after — so the charge explains less within a stable docket.

7 Discussion

Rule 4-216.1 barred three uses of the bail amount: holding a defendant for nonpayment, setting the amount by danger, and setting it by the charge. We asked two questions of that change — how much a defendant's risk of rearrest and the seriousness of the charge had added to the amount before the reform, and whether, once the rule barred those uses, detention fell or was recast. Over the long window release rose 19.9 percentage points and the share held on default of bond fell 26.2; amounts among those still held on default fell by more than 70 percent. Detention itself fell little: the defendants who left money bail did not all go free, and some were held without bail instead. Detention was recast, not reduced.

We detect no increase in failure to appear or rearrest, and case resolution was unchanged — the pattern other money-bail reforms show when release expands (Section 2.3). These flat safety rates are not an artifact of releasing only the safest cases: the defendants the reform moved to release were not low-risk, carrying about-average predicted rearrest risk, higher than the already-released group's (Section 5.5).

The disentangling critique held that money bail conflates two judgments the law treats separately — whether a defendant will return to court, and whether the defendant is dangerous (Gouldin, 2016; Mayson, 2020). At the level of individual decisions, we show the conflation was real and could be undone. Before the reform, higher predicted violent rearrest had raised both the chance of a money-bail order and the amount set; the reform moved that risk onto an explicit held-without-bail order instead (Section 6). The cases it shifted toward that order were rearrested for violence at a higher rate, so the reform picked out defendants by realized violence, not only by a model's prediction. Focal-concerns theory anticipates this: barring one tool from a concern leaves the concern in place, and officials act on it through the tools that remain (Steffensmeier et al., 1998). The concern was public safety, and the tool that remained was the power to deny release outright.

A money-bail decision has two parts — whether to set one, and how much — and they answered to different concerns. The decision to set a money bail loaded on predicted flight, the consideration the rule still allows. The amount did not: it was no higher for defendants at higher predicted flight, even though it was set higher for predicted violent rearrest and largely fixed by the charge (Section 6). The part of money bail meant to secure appearance, then, had been doing the work of danger and the charge — so confining the amount to appearance confined it to a job it had not been doing.

Detention did not fall because the reform reallocated it rather than removed it. The cases that left money bail split three ways: some to release, who spent fewer days in jail; some to held without bail, who spent more; and the rest to a lower money bail (Section 5.4). The release and held-without-bail movements roughly offset, and total detention held near its pre-reform level. Making detention explicit did not make it lighter. By Starger's measure of unjustified pretrial detention — defendants jailed before trial whose charges are all later dropped (Starger, 2020) — the burden grew: the rate of such detentions barely moved, from 16 to 18 per 1,000 filed cases, but mean days held among them nearly doubled, and detention-days per 1,000 filed cases rose 2.1-fold. Held-without-bail orders detain longer than unpaid money bail did, so recasting detention as an explicit order lengthened it.

What would have reduced detention is a limit not on how the amount is set but on which cases money bail may hold. A rough calculation makes the contrast concrete. Of the defendants still detained three days after the initial appearance before the reform, about a third were misdemeanor, nonviolent cases held on money bail; releasing them — the posture Illinois took toward low-level charges — would have lowered the detained share of cases from about 56 to 39 percent.[15] Maryland's purpose restriction did less: it left the detained population near its pre-reform size. But a low-level ban reaches only low-level cases. Most detention sits in felony and violent cases, which neither a low-level ban nor a limit on the amount's purpose touches.

The cases that carry most of the detention burden can be reached only by narrowing the grounds for detention itself. Illinois did both: it released low-level cases and replaced money bail with a narrow, danger-based detention hearing (Griffin et al., 2024). Maryland's experience

[15] Back-of-the-envelope: of the roughly 8,200 cases detained at three days in the pre-reform window, about 2,500 (a third) were misdemeanor, nonviolent cases held on money bail. The count includes only cases such a ban would release and assumes no offsetting change in other decisions.

adds that an appearance-only rule for the amount depends on the detention authority beside it. Maryland could confine the amount and see amounts fall because commissioners kept a broad power to deny release, which absorbed the cases the amount could no longer reach. New York limits its amount to appearance too, but its authority to detain is narrow, and its 2019 reform changed which charges money bail covers rather than how the amount is set; on the cases that stayed eligible, amounts rose and posting fell (Rempel et al., 2026). And the detention that remained was not sorted by risk: among the lower-risk defendants who stayed detained, most were held not without bail but on a secured amount they did not post (Section 5.5), so who stays in jail still turns partly on ability to pay.

The amount the reform preserved is kept for the one purpose a price does least to secure. After the reform, failure to appear did not rise even as amounts among the held-on-default fell sharply, and the defendants most likely to miss court were largely released rather than held (Section 6). That a financial condition does little to secure appearance is consistent with evidence from other settings, where removing money bail for eligible cases left nonappearance unchanged (Ouss & Stevenson, 2023) and nonfinancial measures such as court-date reminders reduced it at a fraction of the cost (Fishbane et al., 2020). The purpose the amount is kept for, then, is one that nonfinancial tools serve more cheaply.

7.1 Limitations

Several limitations bound these conclusions. The first is identification. The October 2016 cutoff supports a causal reading only under the continuity assumptions of Section 4.2: at the primary bandwidth the covariates show no discontinuity, the density test detects no sorting, and placebo cutoffs return null. The long-window contrast and the three prediction tests are weaker designs — they compare periods nine months apart rather than estimating a discontinuity — so we read them as descriptive, in the direction each prohibition predicts, not as causal estimates. The Maryland–Virginia comparison differences out shocks common to the region but does not substitute for continuity at a single cutoff.

A second limitation is measurement. We observe ability to pay only through the median income of a defendant's census tract, not the defendant's own resources, so an amount set to a defendant's true means would not register in our charge-conditioned measure (Section 6). We do not observe detention duration for cases transferred to Circuit Court in three counties, and we restrict the duration outcome to fully observable cases (Section 4.1, Appendix B). Failure to appear and rearrest are observed only among released defendants, so those outcomes condition on release.

A third limitation concerns the risk scores. They are pre-reform predictions of rearrest, estimated before the reform and held fixed; they give a stable ordering of defendants but are not a measure of dangerousness. They predict arrest, which reflects enforcement as much as conduct, and they omit considerations a court may weigh. The cases the reform shifted toward held without bail did show higher realized violence (Section 6), but the score remains a prediction, and our account of how bail was set rests on that prediction rather than on danger itself.

7.2 Conclusion

Rule 4-216.1 shows that Maryland's money bail had done more than secure appearance: it had priced danger and applied a charge schedule, and a rule can separate those uses without raising failure to appear or rearrest. But a jurisdiction can do so only where a separate authority absorbs the cases the amount once held — in Maryland, the power to deny release outright — and even then detention does not fall so much as change form, into longer, explicit holds. As jurisdictions weigh whether to limit, narrow, or abolish money bail, Maryland's experience locates the binding choice not in how the amount is set but in who may be detained at all. The question the reform leaves open is not what the bail amount should be, but whether a money amount should decide who is held before trial.

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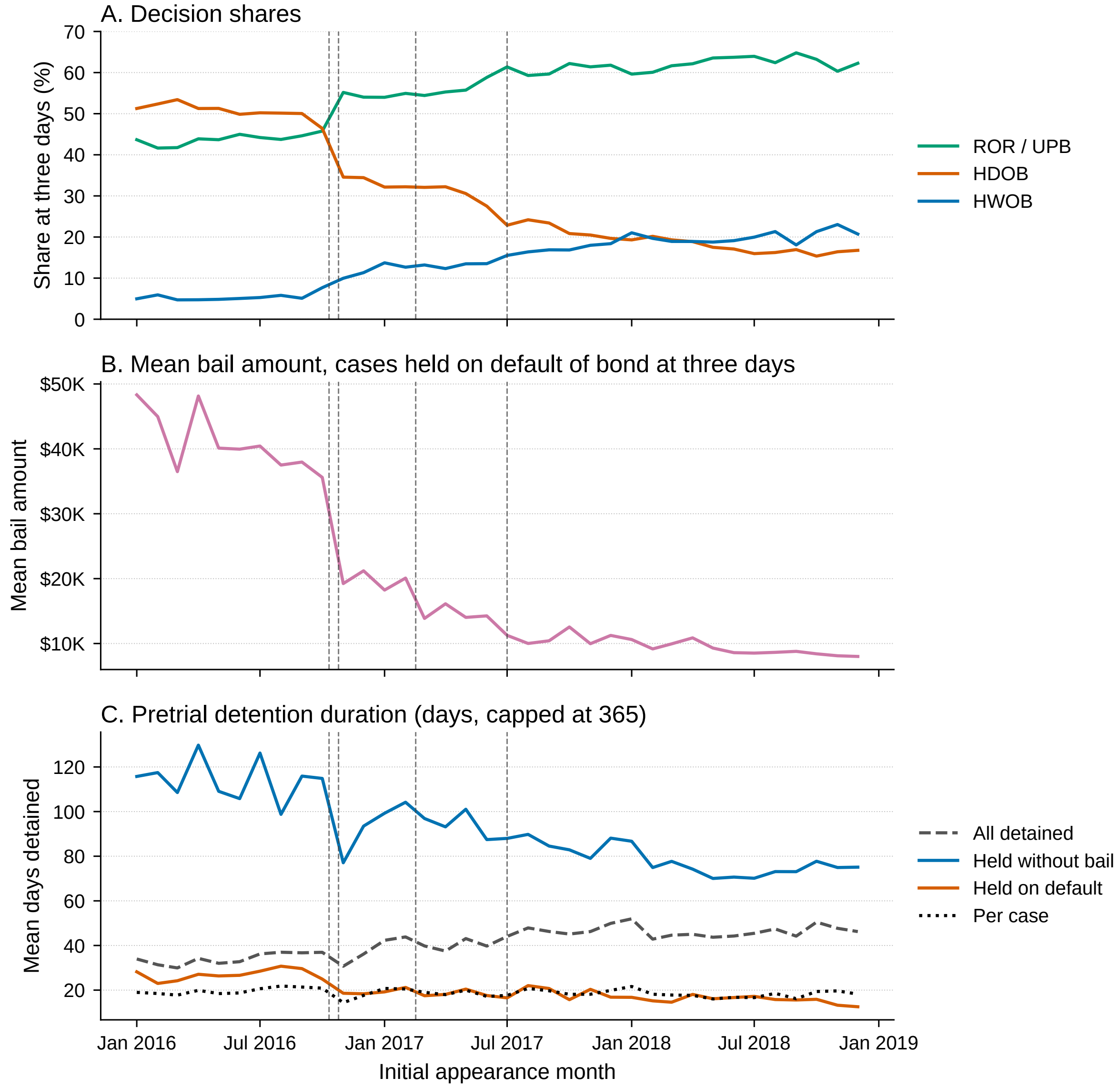

Figure 1: Monthly headline outcomes, January 2016–December 2018, at the three-day endpoint. Panel A: share released on recognizance or unsecured personal bond, held on default of bond, and held without bail. Panel B: mean bail amount among held-on-default cases. Panel C: mean pretrial days detained (capped at 365), detention-measurement sample (Section 4.1), shown four ways — per case (counting a zero for released cases), conditional on being held on default of bond, conditional on being held without bail, and conditional on any detention (either type) at the endpoint. Dashed vertical lines: October 11, 2016 Attorney General opinion; October 26, 2016 Letter of Advice; Rule 4-216.1 adoption (February 16, 2017) and effective date (July 1, 2017).

Appendix A Methodological appendix

This appendix documents the construction and estimation procedures summarized in Section 4. It is intended for online publication.

A.1 Record linkage

Records are linked within and across the two data collections using probabilistic record linkage, which assigns each record to a person identifier from name, date of birth, and address fields. Linkage uses a supervised model with match-type-specific acceptance thresholds: pairs agreeing exactly on first name, last name, and date of birth are accepted at a high threshold, while inexact pairs are accepted at a lower threshold tuned to recover data-entry variants — typographical and nickname differences — without merging distinct people. A cluster-level age-range constraint blocks implausible merges. The resulting person identifier links a defendant's cases over time and joins the primary and conviction-history collections; it also supports the cross-case detention-eligibility indicator in Appendix A.2. Full construction and quality-assurance procedures are documented in the Maryland Crime Research and Innovation Center's 2023 report (Maryland Crime Research and Innovation Center, 2023).

A.2 Construction of the detention-eligibility indicator

The detention-eligibility indicator flags whether any charge in a case falls under one of the seven categories in Maryland's pretrial-detention statute (Criminal Procedure §5-202): crimes of violence with prior violent convictions; drug-kingpin charges; new charges committed while on pretrial release for a qualifying earlier offense; firearm possession by prohibited persons; sex-offender-registration violations; escape; and protective-order violations. Each reflects a legislative judgment that the underlying conduct warrants categorical attention at the pretrial stage, and for each the statute creates a presumption against release that a judicial officer may rebut. The indicator is constructed by hierarchical matching on the CJIS charge code, the statute citation, and the charge description, in that order of priority, against a coded list of qualifying offenses for each category. Two categories require information beyond the index charge. The (d) trigger — a new offense committed while on pretrial release for a qualifying earlier offense — uses the person identifier from Appendix A.1 to determine whether a qualifying earlier case was pending at the time of the index offense. The (c) and (f) triggers — a prior crime-of-violence or firearm-disqualifying conviction — use the linked conviction history, counting convictions (not arrests or dropped charges) within the statutory lookback. Coding details and counts are in the MCRIC report (Maryland Crime Research and Innovation Center, 2023).

A.3 Prediction models

The paper uses gradient-boosted prediction models for two purposes: a frozen risk score that gives a fixed ordering of defendants across the reform (Section 4.2), and the counterfactual decision, duration, and amount models behind the money-bail-fate simulation (Section 5.4). All are gradient-boosted trees fit by cross-validation grouped on the defendant identifier — so a defendant never appears in both training and prediction — with hyperparameters tuned by an inner search within the training folds.

A.3.1 Frozen risk score

The score is a set of gradient-boosted decision-tree models (LightGBM) predicting three twelve-month outcomes — any arrest, violent arrest, and failure to appear — trained on pre-reform cases filed before October 25, 2016 and released at the initial appearance on recognizance or unsecured personal bond, where the outcomes are observable. Features are defendant demographics (age, sex, race), time-windowed counts of prior arrests, charges, and convictions over 180-day, one-year, three-year, and longer lookbacks, and address-level census measures; the feature set contains no court, commissioner, or current-decision variables, so the score does not encode the decision it is later used to study.

Predictions are out of fold under five-fold cross-validation clustered on the defendant, so a defendant's cases never span training and test. Hyperparameters (number of trees, learning rate,

Table 10: Frozen risk-score performance, by predicted outcome.

Predicted outcome	Base rate (%)	AUC (gradient boosting) [95% CI]	AUC (lasso) baseline	Calibration error
Any arrest	38.8%	0.709 [0.706, 0.712]	0.686	2.2
Violent arrest	12.4%	0.686 [0.682, 0.691]	0.672	1.2
Failure to appear	9.4%	0.706 [0.702, 0.709]	0.640	4.9

Out-of-fold predictions from the frozen pre-reform model, evaluated on the analysis sample ($N = 191{,}931$ cases) where the score is used; 5-fold cross-validation clustered on the defendant. *AUC* is the area under the ROC curve; the 95% CI is a defendant-clustered bootstrap (500 resamples). The *lasso* column is an L_1-penalized logistic baseline on the same features and folds. *Calibration error* is the mean absolute gap between predicted and observed rates across score deciles (percentage points; lower is better).

number of leaves, tree depth) are tuned by randomized search within an inner three-fold split of each training fold. Once trained, the model is frozen and scored on every case, including detained defendants, who receive a counterfactual risk estimate; the same frozen score, not a re-fit model, enters every analysis, giving one fixed ordering of defendants on both sides of the reform.

Table 10 reports out-of-fold performance on the analysis sample. Discrimination is modest — areas under the ROC curve of 0.69–0.71 — consistent with the broad pretrial literature on actuarial risk; the scores are well calibrated, with mean absolute miscalibration under five percentage points across score deciles, and stay calibrated on released cases in the post-reform windows (Figure 2), the failure-to-appear model running slightly high. An L_1-penalized logistic baseline on the same features and folds is close behind on the arrest outcomes and well behind on failure to appear, where the tree model's nonlinearities help most. Most of the discrimination comes from features beyond the charge. Charge attributes alone discriminate weakly (0.57–0.59 for the arrest outcomes, 0.68 for failure to appear), rising to 0.69–0.71 once criminal history and neighborhood measures are added, so the score is not a restatement of charge severity. By family, prior record is the strongest single predictor of the later outcome; neighborhood, like the charge, discriminates weakly on its own (Table 12).

Discrimination is similar across demographic subgroups (Table 11): the areas under the ROC curve fall within a narrow band across Black and White defendants and across men and women, so the score is not markedly more accurate for any one group.

Table 11: Frozen risk-score AUC within demographic subgroups.

Predicted outcome	Overall	Black	White	Male	Female
Any arrest	0.709	0.707	0.707	0.706	0.712
Violent arrest	0.686	0.675	0.683	0.679	0.691
Failure to appear	0.706	0.721	0.684	0.711	0.686

Area under the ROC curve of the frozen pre-reform full-feature model, out-of-fold, within each subgroup of the analysis sample. Discrimination is similar across race and sex; the score is not markedly more or less accurate for any one group.

Table 12: Risk-score feature ablation: single-group AUC by feature family.

Predicted outcome	Charge	Prior record	Neighborhood	Demographics	Full
Any arrest	0.57	0.70	0.55	0.66	0.72
Violent arrest	0.56	0.65	0.53	0.61	0.66
Failure to appear	0.62	0.60	0.53	0.59	0.66

Out-of-fold AUC of the risk model trained on one feature family at a time, against the full feature set, on the training sample (pre-reform released cases). *Prior record* = time-windowed counts of prior arrests, charges, and convictions; *Neighborhood* = address-level census deciles. The charge alone predicts the later outcome weakly; prior record and neighborhood carry the signal — the score is not a restatement of charge severity.

A.3.2 Counterfactual models for the money-bail cases

The money-bail-fate simulation (Section 5.4) rests on three models, each fit separately on pre-reform and post-reform cases. A decision model predicts the probability of each three-day decision — release, money bail, or held without bail — as separate gradient-boosted classifiers. A duration model predicts days detained, capped at 365, under each decision, with the decision entered as a feature so it can be set counterfactually; it is fit on the detention-measurement sample (Section 4.1). An amount model predicts the log bail amount among money-bail cases. Each model uses charge attributes, criminal-history counts, demographics, and the frozen risk scores as features.

All three models are fit on the same windows as the long-window RDiT: the 90 days before October 25, 2016 and the 90 days after July 1, 2017. Each model's predictions are out of fold within its own period, and each period's model is applied out of sample to the other period's cases.

Table 13 reports the decision model's out-of-fold discrimination by window: it predicts

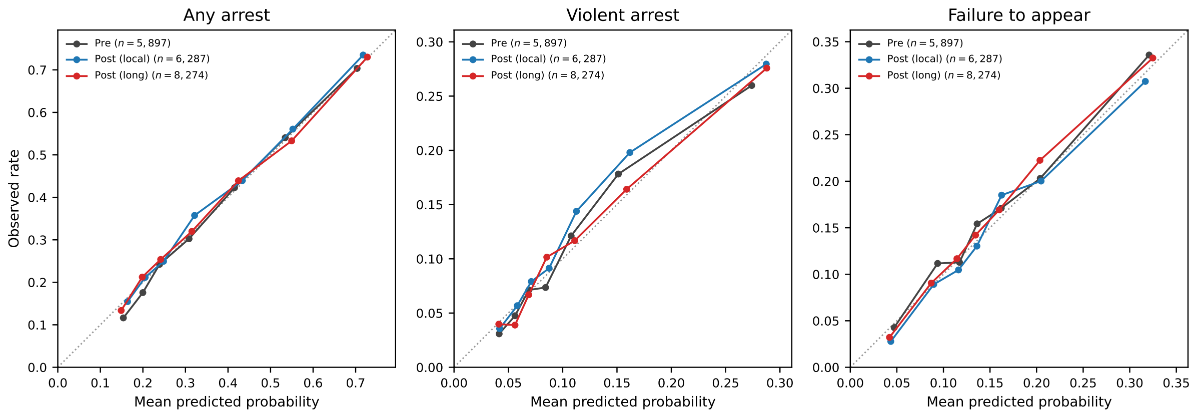

Figure 2: Risk-score calibration by predicted outcome and window. Out-of-fold predicted probability (binned into score quantiles) against the observed rate, computed on released cases — where twelve-month outcomes are observed — in each of the three RDiT windows (pre; 90 days after the October 2016 cutoff; 90 days after the July 2017 effective date). The dotted line is perfect calibration; the frozen pre-reform model stays calibrated in the post-reform windows.

each three-day decision well, best held without bail, and is well calibrated. Trained on one feature family at a time (Table 14), charge attributes predict the decision far better than any other family: alone they reach areas under the ROC curve of 0.68–0.79, against only 0.54–0.56 for the frozen risk scores, even though the charge barely predicts who is later rearrested. The full feature set reaches 0.72–0.84. Across the windows charge attributes predict the money-bail decision less well, and predicted violent rearrest predicts held without bail somewhat more; this is the disentangling result, read in the inputs to the decision.

The duration model orders detention length well (Table 15), though its level R^2 is modest because days are right-skewed and capped at one year. By family (Table 16) the decision dominates: which decision a case receives sets how long it is held, the more so after the reform as held-without-bail orders grow. Charge attributes add the within-decision ordering, and the predicted risk adds little.

The amount model predicts the log bail amount a case would receive if held on default with money bail (the counterfactual amount in the money-bail-fate simulation, Section 5.4). It is a LightGBM regressor fit by randomized search under the same 5-fold defendant-clustered cross-validation, on held-on-default cases with a positive initial amount, and scored on every paper-sample case. Out-of-fold it orders the observed amount well before the rule (Spearman 0.65)

Table 13: Decision-model performance, by predicted decision and window.

Predicted decision / window	Base rate (%)	AUC [95% CI]	Calibration error	N
Release				
Pre	43.8%	0.805 [0.797, 0.811]	1.5	14,618
Post (local)	54.4%	0.781 [0.774, 0.790]	1.6	13,022
Post (long)	60.0%	0.789 [0.781, 0.796]	1.6	15,473
Money bail				
Pre	50.2%	0.749 [0.741, 0.757]	1.3	14,618
Post (local)	34.1%	0.694 [0.684, 0.704]	1.0	13,022
Post (long)	23.6%	0.696 [0.686, 0.706]	1.4	15,473
Held without bail				
Pre	5.8%	0.830 [0.814, 0.845]	0.8	14,618
Post (local)	11.2%	0.845 [0.834, 0.855]	1.0	13,022
Post (long)	16.3%	0.850 [0.843, 0.859]	1.0	15,473

Out-of-fold one-vs-rest discrimination for the three-day decision predictor behind the money-bail-fate simulation (Section 5.4), fit separately on each window (pre; 90 days after the October 2016 cutoff; 90 days after the July 2017 effective date). AUC 95% CI from a defendant-clustered bootstrap; calibration error is the mean absolute predicted-minus-observed gap across deciles (percentage points).

and less well after it (0.54); Table 17 reports performance by period. The ablation in Table 18 shows the ordering rests almost entirely on charge: the charge family alone reaches a Spearman of 0.61 against 0.66 for the full model before the rule, with prior record, neighborhood, and the predicted risk score each adding little on their own. After the rule the charge family weakens to 0.51, leaving the amount less schedule-like.

The main text takes the cases actually held on money bail before the reform and applies the post-reform decision model: case i receives $\hat{p}_{\text{post}}(d \mid X_i)$, its probability of each decision under the new regime. We never classify — case i contributes $\hat{p}_{\text{post}}(d \mid X_i)$ to decision d — and the fate shares in Table 4 are these probabilities averaged over the money-bail cases. Bail amount and days under money bail are observed for these cases; days under the reform come from the post-reform duration model. The mirror direction applies the pre-reform models to the post-reform cases, asking of the cases the prior regime would have money-bailed where they actually went. It gives the same picture (Table 19), and its amount column uses the pre-reform amount model with a smearing correction.

The amount model is fit on log bail, so the exponentiated prediction estimates the geometric mean. We recover the arithmetic mean by Duan's smearing estimator, multiplying by

Table 14: Decision-model feature ablation: single-group AUC by feature family.

Predicted decision / window	Charge	Prior record	Neighborhood	Predicted risk	Full
Release					
Pre	0.78	0.62	0.55	0.57	0.81
Post (local)	0.75	0.60	0.54	0.55	0.78
Post (long)	0.76	0.62	0.56	0.57	0.80
Money bail					
Pre	0.72	0.59	0.54	0.56	0.76
Post (local)	0.67	0.55	0.55	0.53	0.70
Post (long)	0.66	0.55	0.56	0.52	0.71
Held without bail					
Pre	0.74	0.61	0.55	0.54	0.82
Post (local)	0.81	0.62	0.59	0.55	0.85
Post (long)	0.81	0.65	0.60	0.58	0.86

Out-of-fold AUC of the three-day decision predictor trained on one feature family at a time, against the full feature set, by window. *Predicted risk* is the frozen risk scores the model uses as features. The decision tracks the charge far above any other family — and far above the predicted risk, which alone barely separates the decisions. Across the windows the charge's grip on the money-bail decision loosens while held without bail tracks predicted risk a little more, mirroring the disentangling result.

Table 15: Days-detained (duration) model performance, by period.

Period	Mean days detained	R^2	Spearman [95% CI]	N
Pre	20.3	0.232	0.724 [0.719, 0.729]	24,729
Post	20.3	0.235	0.736 [0.731, 0.742]	24,729

Out-of-fold fit of the days-detained model (capped at 365) behind the money-bail-fate simulation (Section 5.4), evaluated at each case's observed decision. Days is right-skewed and capped, so we report the rank ordering (Spearman, 95% defendant-clustered bootstrap CI) alongside R^2; the model orders detention length well even where the level R^2 is modest.

$\frac{1}{n}\sum_j \exp(\hat{\varepsilon}_j)$, the mean exponentiated residual on the training cases.

Two diagnostics support the counterfactual. Figure 3 plots reliability for each model. Within each period (solid curves), predicted probabilities and values track observed frequencies and means along the diagonal, which establishes calibration. The cross-period curves (dashed) apply each regime's model to the other regime's cases. They deviate from the diagonal in the direction of the reform's effect on each outcome — release higher, money bail and amounts lower — and that deviation, not model error, is the counterfactual the comparison measures. The second diagnostic is aggregate: the decision shift the comparison implies reproduces the direction and rough magnitude of the long-window discontinuity estimates (Table 2). The im-

Table 16: Days-detained model feature ablation: single-group Spearman by feature family.

Period	Decision	Charge	Prior record	Neighborhood	Predicted risk	Full
Pre	0.58	0.35	0.27	0.11	0.16	0.65
Post	0.74	0.39	0.28	0.13	0.13	0.75

Out-of-fold Spearman rank correlation between observed and predicted days when the model is trained on one feature family at a time, against the full set. *Decision* is the counterfactual decision one-hots; *Predicted risk* the frozen risk scores. The decision dominates — which decision a case receives sets how long it is held — while charge, prior record, and predicted risk add the within-decision ordering.

Table 17: Bail-amount model performance, by period.

Period	Mean amount	R^2 (log)	Spearman [95% CI]	N
Pre	$43,371	0.459	0.654 [0.640, 0.669]	7,746
Post	$9,571	0.324	0.538 [0.513, 0.562]	3,828

Out-of-fold fit of the bail-amount model behind the money-bail-fate simulation (Section 5.4), measured on the held-on-default money-bail cases where the amount is observed. The model targets the log amount, so we report log-scale R^2 and the rank ordering (Spearman, 95% defendant-clustered bootstrap CI); N is the out-of-fold money-bail count in each window.

plied shift is +16.1 percentage points for release against the discontinuity's +19.9, -27.1 for held on default against -26.2, and +10.9 for held without bail against +6.0. The held-without-bail share runs somewhat above the local estimate, as expected when a window-average effect is compared to a discontinuity.

A.4 Whom the reform moved: recovery and a model-free check

Section 5.5 recovers the predicted-risk profile of the marginal releases by the probability weight of Appendix A.3.2: each pre-reform case i is weighted by $w_i = \hat{p}_{\text{post}}(\text{rel} \mid X_i) - \hat{p}_{\text{pre}}(\text{rel} \mid X_i)$, the reform-induced rise in its predicted release probability from the Section 5.4 decision model, and the frozen scores are averaged as $\hat{\mu}_M = \sum_i w_i X_i / \sum_i w_i$. Because release is a single margin, the same mean is also point-identified without a model; we give that identity here and compare the two.

Table 18: Bail-amount model feature ablation: single-group Spearman by feature family.

Period	Charge	Prior record	Neighborhood	Predicted risk	Full
Pre	0.61	0.24	0.23	0.13	0.66
Post	0.51	0.21	0.21	0.17	0.55

Out-of-fold Spearman rank correlation between the observed and predicted log bail amount when the model is trained on one feature family at a time, against the full set, among the held-on-default money-bail cases. *Predicted risk* is the frozen pre-period risk score. Charge and prior record carry most of the amount's ordering; the predicted-risk and neighborhood families add little on their own.

Table 19: Mirror direction: the cases the prior regime would have held on money bail.

		Mean days detained		Counterfactual
Realized destination	Share	Realized	Counterfactual	bail amount
Released	51%	1	22	\$23,000
Still on money bail	28%	21	29	\$38,000
Held without bail	21%	88	43	\$113,000

Note. Post-reform cases weighted by their predicted probability of money bail under the prior regime, grouped by the decision they actually received. The counterfactual columns are the days and the (smeared) bail amount the prior regime would have imposed; realized days are actual pretrial days detained.

Model-free identity. Let release denote a recognizance or unsecured-bond decision and r the share released in a window. Partition the window's cases into always-released (AR), movers (M, detained before and released after), and always-detained (AD). Under no defiers — the reform moves no one from release into detention (monotonicity) — the pre-reform released are exactly AR and the post-reform released are $AR \cup M$. For any characteristic X with group mean μ, under the assumption that the always-released keep their pre-reform composition ($\mu^{AR}_{\text{post}} = \mu_{\text{pre,rel}}$),

$$\mu_M = \frac{r_{\text{post}}\,\mu_{\text{post,rel}} - r_{\text{pre}}\,\mu_{\text{pre,rel}}}{r_{\text{post}} - r_{\text{pre}}}.$$

Identification rests on (i) monotonicity, (ii) always-taker stability, and (iii) no cross-window drift in X among the always-released. We report court-county-clustered bootstrap intervals.

The two estimates agree. Table 20 reports the probability-weighted and model-free recoveries side by side. They agree on every score over both windows, with overlapping intervals,

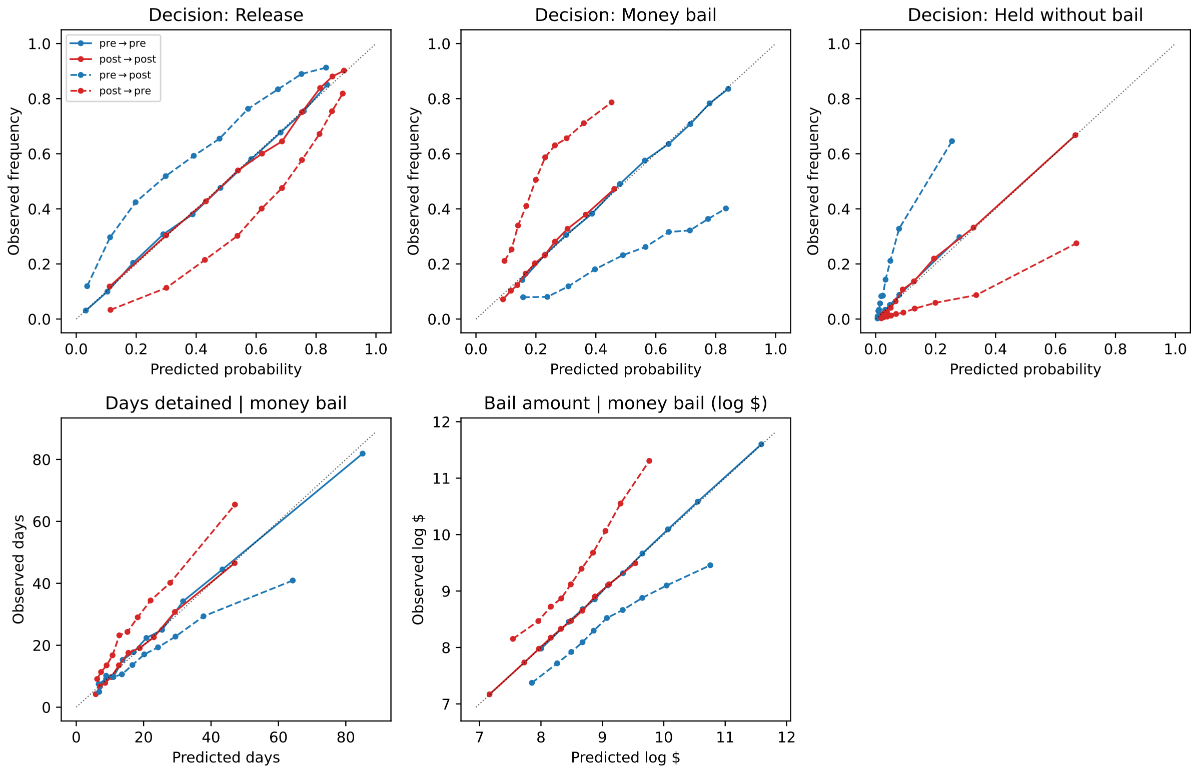

Figure 3: Calibration of the counterfactual prediction models. Each panel plots predicted against observed by decile; the dotted line is perfect calibration. Solid curves are in-period (each model on its own period, out of fold) and lie on the diagonal. Dashed curves are cross-period: the pre model on post cases (blue) and the post model on pre cases (red). Their deviation from the diagonal is the reform's effect on that outcome, not miscalibration. Top row: the three-day decision model (release, money bail, held without bail). Bottom row: days detained and log bail amount among money-bail cases. All three models use the 90-day windows around the two cutoffs.

despite resting on different assumptions — the probability weight on the decision model transporting across regimes, the identity on (i)–(iii). Monotonicity (i) holds in the data: only 1.3% of cases see their predicted release probability fall under the reform (5.5% in the narrower October window), so the held-without-bail expansion drew from the detained pool, not from defendants who would otherwise have been released. The model-predicted release increase (16.1 percentage points) is close to the observed (13.1).

Benchmark detained groups. The marginal-detained group is not a structural object and carries no identification claim. It is the $n_M = (r_{\text{post}} - r_{\text{pre}})N_{\text{post}}$ detained cases with the lowest predicted rearrest — a group the same size as the marginal releases — and answers whether a same-size detained group scores lower on risk than those released. The no-violent-or-firearm

Table 20: Marginal-release predicted-risk profile: probability-weighted versus model-free, long window.

Predicted score	Probability-weighted	Model-free identity	Already released
Failure to appear	0.141 [0.133, 0.150]	0.146 [0.127, 0.159]	0.155
Any rearrest	0.414 [0.396, 0.429]	0.395 [0.343, 0.439]	0.372
Violent rearrest	0.140 [0.129, 0.149]	0.127 [0.115, 0.138]	0.115

Note. Mean frozen predicted risk among the marginal releases, recovered two ways, with 95% court-county-clustered bootstrap intervals; the already-released column is the observed mean among pre-reform released cases.

variant applies the same construction within cases carrying no violent, weapon, or firearm charge, holding charge severity fixed. Both are descriptive comparisons, not estimates of a causal margin.

A.5 Maryland–Virginia comparison design

The cross-state check uses Virginia as a control state to absorb region-wide shocks the within-state design cannot. Virginia did not enact a comparable bail rule in 2016–2018. We pool case-level records and fit a difference-in-differences regression of each decision outcome on a Maryland-post-October-25, 2016 indicator, with court, month, and charge-severity fixed effects and standard errors clustered at the court level, over the same $\pm$90-day window as the within-state design. Virginia's pretrial records (Virginia Pre-Trial Services Agency, January 2014 through December 2018) measure release status at a coarser granularity than Maryland's three-day endpoint; to approximate that endpoint, the primary specification restricts Virginia to cases with a bail hearing within ninety days of arrest, and sensitivities at sixty and thirty days are reported. The event-study version replaces the post indicator with month-relative-to-cutoff dummies, omitting month -1; its pre-period coefficients test the parallel-trends assumption (Figure 6).

A.6 Estimating equations for the three predictions

The three tests share a scaffold: the long-window pre/post comparison, the frozen pre-period risk scores, and court-county clustered inference (with week added for the posting and realized-outcome regressions). Tests (A) and (B) also use a cell partition (felony $\times$ violent $\times$ detention-eligible). The charge-neutrality statistic in (C) is an adjusted R^2 whose pre-to-post change is bootstrapped over court clusters.

(A) ATP-neutrality. The posting-gradient regression is

$$\text{posted}_{3d} = \beta_0 + \beta_1\,\text{Post} + \beta_2 \log\text{bail} + \beta_3\,(\log\text{bail} \times \text{Post}) + \varepsilon, \tag{1}$$

and the prohibition predicts the post-reform slope $\beta_2 + \beta_3$ moves toward zero ($\beta_3 > 0$). We report a raw form and one adding cell $\times$ Post, month, and court fixed effects, risk $\times$ Post controls, and demographics; the test uses every money-bail case and does not rely on an income proxy. As a supplement we add tract income and its interaction with Post. Fewer than 0.2% of these cases are disposed within the three-day window, so a release reflects posting rather than case resolution.

(B) Disentangling. The first (routing) regression is

$$D_i = \pi_0 + \pi_1\text{Post} + \pi_2 R_i^{\text{fta}} + \pi_3(R_i^{\text{fta}} \times \text{Post}) + \pi_4 R_i^{\text{viol}} + \pi_5(R_i^{\text{viol}} \times \text{Post}) + \varepsilon_i, \tag{2}$$

where D is the held-on-default indicator, the held-without-bail indicator, or the log bail amount (predicted signs $\pi_5 < 0$ for the financial instruments, $\pi_5 > 0$ for held without bail); it carries court, month, and cell fixed effects and age, sex, and race controls. Charge composition could produce the shift, so we add an indicator for every specific charge code a case carries; the change in routing is then identified within charge profiles, not from a shift in the charge mix. A non-parametric companion stratifies cases by terciles of the two risks and reports the pre-to-post change in each decision's share within each cell, separately for felony and misdemeanor charges.

The same routing regression, fit on the log bail amount among cases set a money bail, shows the violence loading on the amount falling toward zero. Over the long window, a one-standard-deviation rise in predicted violent rearrest raised the conditional amount by 7.2% before the reform and only 2.2% after, a significant reduction, so predicted violent rearrest moved off the amount as well as the decision. The flight loading on the amount is small and not significant (-4.0%), so predicted flight did not predict the amount more sharply after the reform either. These amount margins are estimated on the smaller money-bail subsample and are reported here; the main ladder (Table 7) carries the decision margins.

(C) Charge-neutrality. We regress the log bail amount, among cases set a money bail, on an indicator for every specific charge a case carries,

$$\log \text{bail}_i = \alpha + \sum_c \beta_c \mathbf{1}[\text{case } i \text{ carries charge } c] + \varepsilon_i, \tag{3}$$

fit separately by period. The adjusted R^2 of this regression is the share of the variation in amounts a case's charge profile explains. Its raw value conflates two prohibitions, however: charge severity correlates with predicted rearrest, and the disentangling prohibition has removed that risk from the amount, so part of any pre-to-post fall would follow from the risk leaving rather than from de-scheduling. We therefore report the charge's *incremental* adjusted R^2 — the rise in adjusted R^2 when the charge indicators are added to a regression of the log amount on the frozen flight, violent-rearrest, and any-rearrest scores — which nets out the predicted-risk channel and isolates what the charge fixes beyond individualized risk. Table 9 reports the net-of-risk, on-ranks, and raw statistics for the long window and the October placebo. We measure the charge at the grade-pure specific-charge-code level, finer than the felony-by-violent-by-detention-eligible cell partition used elsewhere, and keep codes that recur at least thirty times in each period. Entering a case's charges jointly handles multi-charge cases without choosing which charge an amount belongs to: a minor charge filed alongside a felony is not credited with the felony's amount, because the felony's own indicator absorbs it. The statistic is unchanged when a period's log amounts all shift or rescale together, so the across-period comparison does not require a common dollar scale. The rank variant, computed on each period's ranks of the amounts, drops even that monotone-scale assumption. We bootstrap the pre-to-post change over court clusters ($B = 1{,}000$) and report charges per case by period as a composition check. The table itself appears with the result in Section 6.

Appendix B Detention-days measurement

B.1 Construction procedure

Pretrial detention days are constructed from release-event records in the Maryland Judiciary's Case Search system. For each defendant detained at the initial appearance, we identify the first observed release event — a commit-and-release event code, a bond posting record, or a disposition that resolves the detention — and compute days held as the gap between the initial appearance and that release event, capped at 365 days. Cases that do not have an observed release event in our data but do have an observed disposition receive the disposition date as the de facto release date, capped at 365. Cases with neither an observed release nor a disposition within the observation window are not measurable.

B.2 Per-county coverage

Table 21 reports, for each Maryland court county, the number of cases detained at the initial appearance in our canonical sample, the share that transfer to Circuit Court at some point, the share with an observed release event, and the share with an observed release among transferred cases specifically. The last column is the diagnostic. In most counties between 65 and 85 percent of transferred-detained cases have an observed release. In Baltimore City the share is 53%; in Prince George's and Montgomery it is 68%. These three counties also account for the largest absolute volumes of transferred-detained cases (9,099, 2,462, and 1,900 respectively). The detention-days analysis restricts to the cases where release is reliably observable: it drops the transferred subset from these three counties and keeps everything else.

Table 21: Per-county detention coverage on the canonical sample. "Detained" = detained at the initial appearance; "Transferred" = case linked to a Circuit Court case at any point. Bold rows: the three counties from which transferred cases are excluded for detention-days analyses.

County	N detained	% transferred	% with observed release	% with release | transferred
Baltimore City	**15,259**	**55.3**	**69.7**	**48.9**
Caroline	679	55.1	80.7	74.3
Cecil	2,527	54.3	89.4	87.7
Baltimore	17,121	53.6	88.4	84.4
Queen Anne	1,055	51.5	82.0	75.9
Harford	2,673	50.9	77.4	70.8
Kent	323	48.6	66.9	61.1
Frederick	2,239	48.1	84.0	73.3
Washington	2,404	47.6	84.9	76.8
Talbot	734	45.8	87.9	83.9
Anne Arundel	6,699	45.5	87.8	81.2
Allegany	2,029	45.0	89.2	85.1
Carroll	1,783	37.1	89.3	84.9
Wicomico	3,339	36.7	82.4	67.0
Charles	2,542	36.3	84.6	71.9
Howard	3,558	35.3	91.7	82.3
Dorchester	1,299	32.1	81.4	62.6
Worcester	2,072	31.2	80.5	75.7
Somerset	787	27.6	86.5	74.2
Garrett	531	26.7	86.1	79.6
Saint Mary	1,449	26.0	79.8	52.0
Montgomery	**6,070**	**25.7**	**88.4**	**60.3**
Calvert	1,663	24.1	89.4	70.3
Prince George	11,168	21.6	90.1	67.2
All counties	**90,003**	**42.4**	**84.1**	**71.2**

B.3 Dispositions of transfer cases with no observed release

A natural question about the excluded subset is whether those cases were truly detained for long periods or whether they were released at some point but our data missed the release event. One way to probe this is to look at the final disposition. If the cases that show no observed release were nonetheless prosecuted to a guilty disposition, they were most likely released at some point — typically at sentencing — because the defendant would generally have appeared at trial.

Table 22 shows disposition outcomes for four nested groups. Most cases resolve within a year: 19.8% of the diagnostic subset — detained, transferred to Circuit, with no observed

Table 22: Disposition outcomes for nested groups in the canonical sample. "% no disposition" is the share of cases with no charge resolved within 365 days of the initial appearance; the guilty, not-guilty, PBJ, and other columns are shares of cases *with* a disposition that had at least one charge resolved the named way within 365 days (not mutually exclusive — a case can carry several charges resolved different ways).

			Of cases with a dispositio		
Group	N	% no disposition	% guilty	% not guilty	% PBJ
All canonical	179,117	15.3	31.0	68.6	9.2
Detained at initial appearance	90,003	16.1	39.9	71.6	6.2
Detained, transferred to Circuit	38,193	23.7	58.1	71.0	6.5
Detained, transferred, no observed release	11,010	19.8	65.3	66.8	1.3

release — have no disposition within 365 days, comparable to the 16.1% among detained cases generally and 15.3% in the full sample. These cases were prosecuted, not lost to the record system. Among cases with a disposition, the guilty share rises across the nested groups, from 31% in the full sample to 40% among detained cases and 65% in the diagnostic subset; the shares are not mutually exclusive, since a case can carry several charges resolved different ways. A guilty disposition generally follows the defendant's appearance for a plea or trial, so the high and rising guilty share fits these cases having been released at some point — typically at sentencing — rather than held for the full observation window. What our District Court data miss for them is the release event, not the case.

B.4 Days-detained distribution among transferred cases that do have observed releases

A second question: among the transferred cases that do have an observed release, are they representative of the underlying detained population, or are they a selected subset (e.g., concentrated in short-detention cases where the release happened before the transfer to Circuit Court)? Table 23 compares the days-detained distribution between non-transferred detained cases and transferred-detained cases with an observed release. The median is the same in both groups (one day). The mean and the upper tail differ substantially: the transferred-with-release group has a longer right tail (90th percentile 122 days, against 48 days for non-transferred).

Table 23: Days-detained distribution among cases detained at the initial appearance with an observed release event, by transfer status. Days are capped at 365.

Group	N	Mean	Median	P25	P75	P90
Non-transferred	48,492	20.6	1	0	28	58
Transferred (observed release only)	27,183	35.7	1	0	15	148

This is consistent with the transferred-with-release group containing some cases where the defendant was held through the trial or sentencing and the release event happens at the Circuit Court but is recorded back in our District Court extract — i.e., a long-detention subset where coverage happens to be available. Including these cases would skew the days-detained distribution; dropping the entire transferred subset (rather than only the no-observed-release cases) is the cleaner restriction.

Appendix C Identification and robustness

Section 4.2 states three assumptions for a causal reading of the RDiT: no sorting of cases across the cutoff, continuity of case characteristics, and no other change at the same date. This appendix tests each, then reports the design's robustness to the bandwidth, the measurement endpoint, and a Maryland–Virginia cross-state comparison.

C.1 No sorting: density of the running variable

If cases sorted across the cutoff, the density of the running variable — the count of initial appearances by day relative to the cutoff — would jump at zero. It does not. Figure 4 plots the weekly appearance counts within 90 days of each cutoff; the bars run smoothly across the threshold. The local-polynomial density test of Cattaneo, Jansson, and Ma (2018) is consistent with this: no discontinuity detected at either cutoff (Table 24). This is expected, because the Letter of Advice was an unannounced internal directive, unlikely to have been known to defendants, attorneys, or arresting officers (the directive instructed those officers but was not publicly announced).

Table 24: Manipulation (density) test of the running variable. T is the local-polynomial density-test statistic of Cattaneo et al. (2018); p is its jackknife p-value. A small p would indicate a density discontinuity (sorting) at the cutoff.

Cutoff	Window	N	T	p
October 25, 2016 (Letter of Advice)	±180d	58,245	1.329	0.184
July 1, 2017 (Rule effective)	±180d	59,581	-0.353	0.724

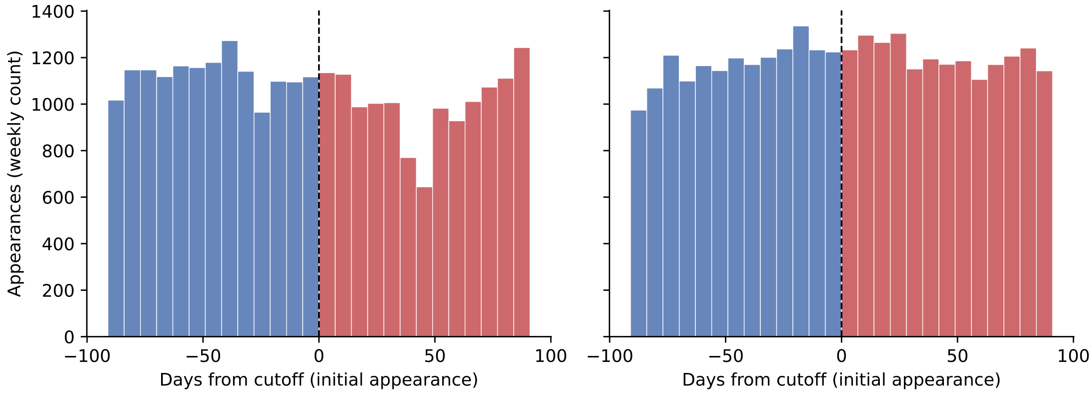

Figure 4: Density of the running variable around each cutoff. Weekly counts of initial appearances within 90 days of the cutoff; pre-cutoff weeks in blue, post-cutoff in red; dashed line marks the cutoff. Left panel: October 25, 2016 (Letter of Advice). Right panel: July 1, 2017 (Rule effective date).

C.2 Continuity of potential outcomes: covariate balance

We re-run the local-linear RDiT on each covariate at the same October 25, 2016 cutoff and the same family of bandwidths used for the main outcome analysis, on the same running variable (the initial-appearance date). The covariates span charge mix (felony, violent, firearm-or-weapon, detention-eligible), defendant demographics (Black, White, female, age at filing), pending caseload, the frozen pre-period risk scores (predicted violent rearrest, any rearrest, and failure to appear), the filing-to-appearance lag (whether the appearance occurred within three days of filing, which distinguishes fresh arrests from later warrant service), and a sample-construction variable (whether the case transferred to Circuit Court, which the detention-days outcome conditions on per Section 4.1). Estimates are local-linear with a triangular kernel and HC1 robust standard errors; significance markers are † $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$.

At the primary 90-day bandwidth, no covariate shows a discontinuity at conventional levels

(Table 25): every estimate is small and all have $p \geq 0.14$. Demographics and the risk scores are among the best balanced — the Black, White, female, and age estimates sit within a fraction of their scale of zero ($p \geq 0.57$) and the three predicted-risk scores likewise ($p \geq 0.14$) — so the higher post-window Black share in the summary statistics (Table 1) is long-window drift, not a jump at the cutoff. Discontinuities appear only at the narrowest, lowest-power windows — the firearm-or-weapon, violent, and predicted-any-rearrest covariates are imbalanced at 30 and 60 days — and settle back toward zero at the 90- and 180-day bandwidths, the pattern expected from charge-mix noise in thin windows rather than a discontinuity in case composition.

The long-window column is a different object: it compares the pre-reform and post-Rule windows nine months apart, where continuity at a single cutoff need not hold. Several covariates do shift over that span — the case mix drifts toward fewer Black defendants, fewer violent charges, lower predicted any-rearrest, and fewer Circuit transfers — which is why the long-window results are paired with the channel decomposition that nets such shifts out.

As a further check, any residual case-mix movement is immaterial to the headline decision and bail effects: decomposing those changes attributes only 1.4% of the held-on-default shift and 3.2% of the bail-amount shift to observable case mix. The Maryland–Virginia difference-in-differences (Section 5.6) differences out state-invariant shifts in composition, and the close match between the within-Maryland and cross-state estimates on release, held on default, and any-detention indicates that composition is unlikely to be driving the result.

C.3 Timing: the opinion and the Letter

Figure 5 plots the decision shares and the mean bail amount at weekly resolution from September 2016 through February 2017. Bins are seven-day periods anchored at the October 26, 2016 Letter of Advice, so no bin straddles the cutoff. Decision shares drift a few percentage points in the two weeks between the October 11 Attorney General opinion and the Letter, then break sharply in the first post-Letter week. Any pre-Letter response to the opinion sits in the estimation pre-window, which attenuates the Letter discontinuity rather than inflating it.

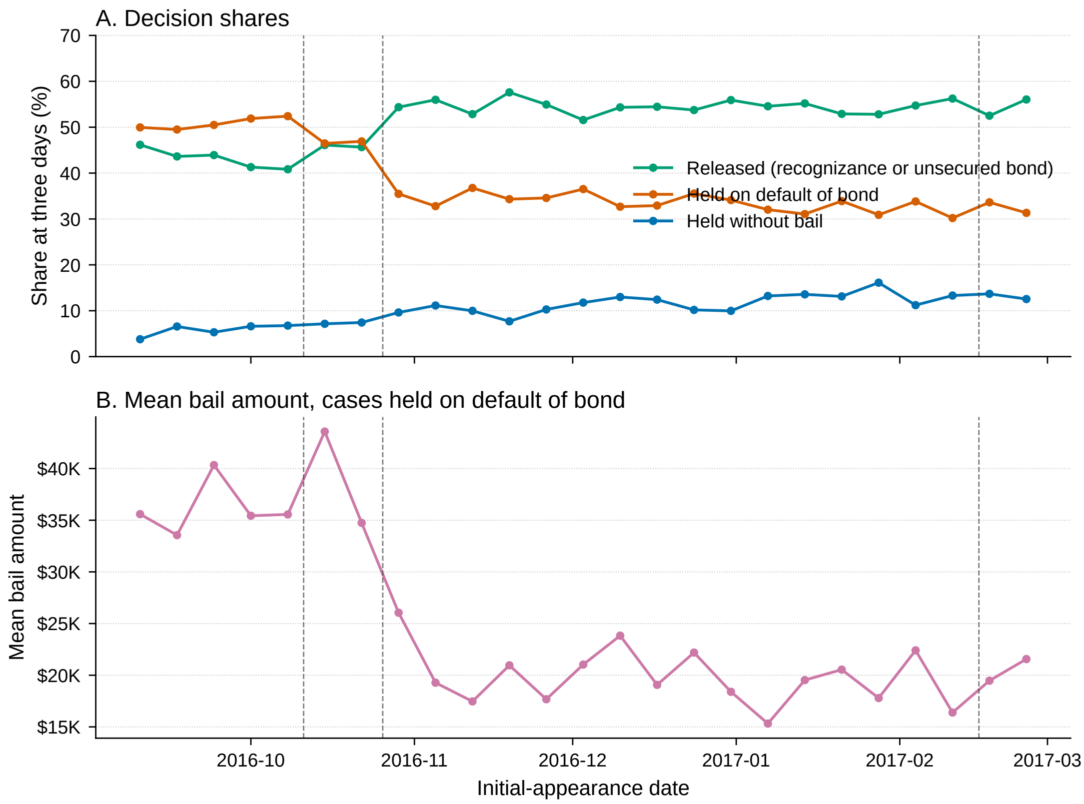

Figure 5: Weekly decision shares and bail amounts around the reform signals, September 2016–February 2017. Panel A: share released on recognizance or unsecured personal bond, held on default of bond, and held without bail at the three-day endpoint. Panel B: mean bail amount among held-on-default cases. Points are seven-day bins anchored at the October 26, 2016 Letter of Advice (so no bin straddles it), plotted at bin midpoints. Dashed vertical lines: October 11, 2016 Attorney General opinion; October 26, 2016 Letter of Advice; February 16, 2017 adoption of Rule 4-216.1.

The drift in those two weeks invites a donut check: drop the 2,212 cases heard after the October 11 opinion but before the October 25 cutoff, then re-estimate. Dropping them strengthens the decision estimates rather than weakening them. At the primary 90-day bandwidth, release rises +18.4 percentage points, against +10.2 in the full sample, and the share held on default falls -20.2 (Table 26). Dropping the days nearest the cutoff under a fixed window makes the local-linear fit extrapolate from points farther out, so the bandwidth-reoptimized estimate is the more reliable one. It is smaller but moves the same way: release +13.7 percentage points and held on default -17.0. The bail amount is the exception. It weakens to $-\$5{,}658$ ($p = .35$), because amounts ran high among the excluded cases, so removing them narrows the pre-to-post gap. The decision pattern is the same under both bandwidths: the excluded days had begun to move toward the post-reform pattern, so keeping them shrank the measured jump.

Moving the cutoff the other way locates the break. Dating it to October 10, so that the October 11 opinion is the first post-treatment day, attenuates every estimate toward zero. Release is +4.9 percentage points ($p = .39$), the share held on default -6.2 ($p = .26$), and any detention -5.0 ($p = .37$). The opinion went to the Rules Committee, not to commissioners as operational guidance, so no discontinuity appears on that date. The operational break is the Letter.

C.4 No contemporaneous shock: placebo cutoffs

If the October-25, 2016 discontinuity reflected a recurring seasonal or secular pattern rather than the Letter of Advice, the same RDiT run at dates where no reform occurred would also show effects. It does not. Table 27 re-estimates the five headline decision outcomes at the true cutoff and at four placebo cutoffs — one in the stable pre-reform period and three in the stable post-Rule period, avoiding the October-2016-to-July-2017 transition. The effects appear at the true cutoff alone; every estimate at the placebo cutoffs is null (0 of 20 significant).

C.5 Bandwidth sensitivity

The decision-share estimates move by about a percentage point or less between the fixed 90-day bandwidth and the Calonico–Cattaneo–Titiunik MSE-optimal bandwidth, with signs and significance preserved (Table 28). The bail-amount and detention estimates are sensitive to bandwidth in level but not in sign: the data-driven bandwidth tightens the bail-amount estimate from $-\$8{,}401$ ($p = .02$) to $-\$10{,}252$ ($p = .002$), and the detention-duration estimate moves from -1.1 to -5.4 days, with a confidence interval including zero under both.

C.6 Endpoint window

The three-day estimates stack the commissioner's initial call and any bail-review modification within three days (Section 4.1); comparing them to the commissioner stage alone separates the two. Each of the three main effects is larger at the commissioner stage, meaning bail-review judges walked back some of the commissioner shifts. Held on default of bond falls by 14.1 percentage points at the commissioner stage and 13.1 by three days; held without bail rises by 5.1 at the commissioner stage and 2.6 by three days, bail review absorbing about half of the commissioner-stage increase; and the mean bail amount drops by $17,682 at the commissioner stage and $8,401 by three days. Estimates measured at 7 and 14 days after the initial appearance differ from the three-day figures by less than 0.2 percentage points (Table 29).

C.7 Cross-state: Maryland–Virginia difference-in-differences

To rule out a contemporaneous shock common to the region, we compare Maryland to Virginia, which enacted no comparable bail rule in 2016–2018. Within $\pm$90 days of the October 25, 2016 cutoff (Table 30, "VA hearing $\leq$ 90d" column), the three decision estimates retain their signs and significance: release rises by 10.4 percentage points (SE 1.3), held on default of bond falls by 14.8 (SE 1.7), and held without bail rises by 4.5 (SE 0.8). The cross-state estimates stay within about two percentage points of the within-state estimates for release and held on default; the held-without-bail estimate is somewhat larger than the within-state discontinuity (+4.5 versus +2.6 percentage points); Virginia's own held-without-bail share rose only slightly over the window, so differencing it out leaves most of Maryland's increase intact. The event study shows parallel pre-cutoff trends in months -9 through -1 and a widening gap through month $+24$ (Figure 6).

Appendix D Supplementary results

This online supplement collects secondary tables and figures referenced in the main text.

D.1 Reform effects by race

Section 5.3 reports that the reform's release gains were smaller for Black defendants over the long window. Table 37 asks whether that gap is differential treatment or differential composi-

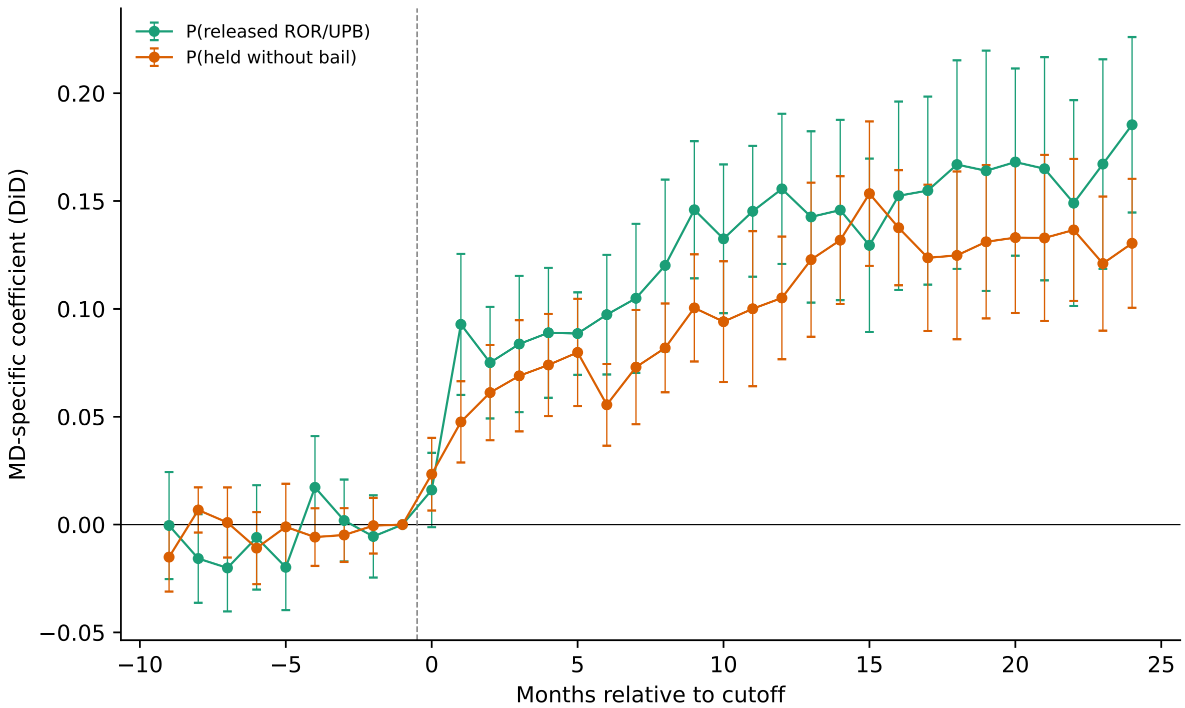

Figure 6: Maryland-Virginia DiD event study by month relative to the October 25, 2016 cutoff. Series are the Maryland-specific coefficients for release on recognizance or unsecured personal bond and for held without bail (reference month $k = -1$). Vertical line at month 0; shaded bands are 95% CIs.

tion, fitting each headline decision outcome with the running-variable polynomial fully interacted by a Black indicator, with and without controls. At the October cutoff, where identification is cleanest, the Black–White differential is small and insignificant for every outcome (all $p \geq 0.66$). Over the long window the release gap is significant before adjustment (-7.2 points, $p = 0.05$) but narrows to 5.6 points and loses significance once charge, predicted risk, and court are controlled, and the post window is itself less Black and lower-risk (Appendix C.2). The gap therefore follows from the over-representation of Black defendants in the felony, firearm, and higher-predicted-risk cases the reform moved least toward release, rather than from the reform treating otherwise-similar defendants differently. That composition is upstream of the reform, and the risk scores encode prior enforcement as much as conduct, so an even-handed rule still produced uneven gains.

D.2 Disentangling: robustness across control sets

Table 7 reports the fully-controlled specification. Tables 38 and 39 show the same predicted-risk loadings across the full control ladder — from no controls to the fully-controlled specification — for the Letter (October) cutoff and the post-Rule long window. The danger results — predicted violent rearrest and any rearrest moving off the money-bail decision and the bail amount, and onto held without bail — hold under every control set. The flight results depend more on the controls: the money-bail decision's loading on predicted flight is small without charge controls and larger within specific charges, while the bail amount carries no positive flight loading under any specification.

Table 25: Covariate balance. Left columns: local-linear RDiT estimates at the October 25, 2016 cutoff across bandwidths. Right column: the pre-reform versus post-Rule long-window contrast. Cells give the estimate (top) and robust SE (bottom, parentheses); binary and probability covariates are scaled to percentage points (originals are shares or predicted probabilities in $[0, 1]$), while age (years) and pending cases (count) are in natural units. Primary bandwidth is 90 days.

	October 25 cutoff				Long window
	30d (sens.)	60d (sens.)	90d (primary)	180d (sens.)	pre vs post
Felony	1.71	-0.32	-0.31	0.30	-2.17†
	(2.10)	(1.49)	(1.21)	(0.86)	(1.18)
Violent	-4.39*	-2.67†	-1.03	-0.16	-2.77*
	(2.10)	(1.48)	(1.21)	(0.86)	(1.18)
Firearm or weapon	-2.69*	-1.96*	-1.09	0.33	-0.69
	(1.36)	(0.96)	(0.78)	(0.54)	(0.76)
Detention-eligible	1.30	0.56	0.52	0.12	1.01
	(1.06)	(0.77)	(0.64)	(0.46)	(0.64)
Black	-1.18	0.13	0.24	2.68**	-2.81*
	(2.23)	(1.58)	(1.29)	(0.92)	(1.28)
White	1.50	0.06	-0.26	-2.72**	2.38†
	(2.21)	(1.56)	(1.28)	(0.91)	(1.26)
Female	-1.31	-0.50	-0.61	-0.45	-0.67
	(1.87)	(1.32)	(1.08)	(0.76)	(1.06)
Age at filing (years)	-0.46	0.08	0.15	-0.16	0.25
	(0.53)	(0.37)	(0.30)	(0.22)	(0.30)
Pending cases (count)	0.066	0.026	0.018	-0.010	-0.010
	(0.054)	(0.040)	(0.032)	(0.022)	(0.026)
Predicted violent rearrest	0.68	0.33	0.22	0.20	-0.46†
	(0.43)	(0.31)	(0.25)	(0.18)	(0.25)
Predicted any rearrest	3.00**	1.62*	0.77	0.30	-1.79***
	(0.91)	(0.64)	(0.53)	(0.37)	(0.51)
Predicted failure to appear	0.72†	0.32	0.11	-0.04	-0.05
	(0.42)	(0.30)	(0.25)	(0.18)	(0.24)
Initial hearing within 3 days of filing	-1.59	-0.52	-0.79	0.15	2.08*
	(1.72)	(1.22)	(1.00)	(0.71)	(0.95)
Transferred to Circuit Court	3.56†	-0.35	-0.53	-0.10	-4.36***
	(2.12)	(1.49)	(1.22)	(0.86)	(1.17)

Table 26: Donut RDiT: headline estimates dropping the inter-signal days (October 11–24, 2016)

	Baseline (full sample)		Donut (drop Oct 11–24)	
	Estimate	95% CI	Estimate	95% CI
October 2016 cutoff (90-day bandwidth, primary)				
Release	$+10.2^{*}$	$[+0.7, +19.6]$	$+18.4^{**}$	$[+7.4, +29.5]$
Held on default of bond	-13.1^{**}	$[-23.0, -3.3]$	-20.2^{***}	$[-31.6, -8.8]$
Held without bail	$+2.6$	$[-1.1, +6.3]$	$+1.2$	$[-3.9, +6.4]$
Any detention	-10.5^{*}	$[-19.8, -1.2]$	-19.0^{***}	$[-29.9, -8.0]$
Bail amount ($)	$-8{,}401^{*}$	$[-15{,}339, -1{,}462]$	$-5{,}658$	$[-17{,}513, +6{,}196]$
October 2016 cutoff (CCT-optimal bandwidth)				
Release	$+11.1^{*}$	$[+0.8, +21.4]$	$+13.7^{*}$	$[+2.5, +24.9]$
Held on default of bond	-13.8^{**}	$[-24.2, -3.3]$	-17.0^{**}	$[-28.0, -5.9]$
Held without bail	$+2.4$	$[-0.3, +5.1]$	$+2.9^{*}$	$[+0.0, +5.9]$
Any detention	-11.5^{*}	$[-21.6, -1.3]$	-14.1^{*}	$[-25.3, -2.9]$
Bail amount ($)	$-10{,}252^{**}$	$[-16{,}783, -3{,}721]$	$-8{,}889^{*}$	$[-16{,}178, -1{,}600]$
Long window (90-day bandwidth)				
Release	$+19.9^{***}$	$[+9.6, +30.2]$	$+28.2^{***}$	$[+16.4, +40.0]$
Held on default of bond	-26.2^{***}	$[-36.2, -16.2]$	-33.2^{***}	$[-44.8, -21.7]$
Held without bail	$+6.0^{**}$	$[+2.1, +9.8]$	$+4.6$	$[-0.7, +9.8]$
Any detention	-20.2^{***}	$[-30.3, -10.1]$	-28.7^{***}	$[-40.3, -17.1]$
Bail amount ($)	$-15{,}159^{***}$	$[-21{,}294, -9{,}024]$	$-12{,}417^{*}$	$[-23{,}820, -1{,}014]$

Note. Each cell is a local-linear RDiT estimate with robust bias-corrected confidence intervals, court-county clustered. Decision outcomes are in percentage points; bail amount is in dollars. The donut columns drop the 2,212 cases initially heard between the October 11, 2016 Attorney General opinion and the October 25 cutoff, the days after the opinion but before the October 26 Letter of Advice, when guidance had not yet reached commissioners. The primary specification is the 90-day bandwidth at the October cutoff; the CCT panel re-optimizes the bandwidth after the cases are dropped, and the long window contrasts the 90 days before October 25, 2016 with the 90 days after July 1, 2017. $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$.

Table 27: Placebo-cutoff falsification. Each cell is the local-linear RDiT estimate of the named outcome at the named cutoff (final endpoint, 90-day bandwidth, robust CCT). Decision shares in percentage points; bail in dollars. Significance: $^{\dagger}\ p < .10$, $^{*}\ p < .05$, $^{**}\ p < .01$.

Cutoff	Release	Held-default	Held-no-bail	Any detention	Bail ($)
Oct 25, 2016 (true)	10.2^{*}	-13.1^{**}	2.6	-10.5^{*}	$-8{,}401^{*}$
Apr 25, 2016	0.7	-1.2	0.4	-0.7	2,403
Oct 25, 2017	-1.5	0.8	0.7	1.4	-909
Apr 25, 2018	-0.9	0.7	0.1	0.7	-51
Oct 25, 2018	0.8	2.6	-3.6	-0.9	255

Table 28: Bandwidth sensitivity. Fixed 90-day vs. CCT MSE-optimal bandwidth, October 25, 2016 RDiT. Units as in Table 2. *, **, *** denote $p < .05, .01, .001$ on robust CCT SEs.

	Fixed BW (90 d)	CCT-optimal BW
Released	10.17*	11.10*
Held on default	-13.14**	-13.75**
Held w/o bail	2.63	2.38
Any detention	-10.51*	-11.47*
Bail $/case	-$8,401*	-$10,252**
Days detained/case	-1.12	-5.44
FTA	1.01	0.72
1yr rearrest	4.25	1.75
Pretrial rearrest	2.27	0.32

Table 29: Endpoint-window sensitivity. Commissioner stage and realized status three, seven, and fourteen days after the initial appearance. October 25, 2016 RDiT, fixed 90-day bandwidth. Detention duration and safety are stage-independent and shown only at three days. Decisions and safety in percentage points, bail in dollars per case; significance stars from robust CCT court-county-clustered inference.

	Commissioner	Primary (3 d)	7 d realized	14 d realized
Released	9.07	10.17*	10.11*	9.97*
Held on default	-14.13**	-13.14**	-13.15**	-13.15**
Held w/o bail	5.06*	2.63	2.69	2.80
Any detention	-9.07	-10.51*	-10.46*	-10.35*
Bail $/case	-$17,682**	-$8,401*	—	—

Table 30: Maryland–Virginia difference-in-differences at the October 25, 2016 cutoff.

	Main	VA hearing $\leq$ 90d	VA hearing $\leq$ 60d	VA hearing $\leq$ 30d
Released	8.93***	10.39***	10.53***	10.64***
	(1.28)	(1.30)	(1.33)	(1.51)
Held on default	-14.10***	-14.76***	-14.79***	-14.06***
	(1.69)	(1.71)	(1.74)	(1.87)
Held w/o bail	5.28***	4.49***	4.38***	3.52***
	(0.82)	(0.84)	(0.88)	(0.94)

Note. The main column pools all VA District-Court cases over 2016–2018; the VA-hearing $\leq$90d/60d/30d columns restrict VA to cases with a bail hearing within that many days of arrest, tightening toward MD's three-day endpoint. Court-county clustered SEs in parentheses. Decisions in percentage points. $^{***}p < .001$.

Table 31: State pretrial reforms since 2013: five-axis mapping

State (reform year)	Mechanism	Bail-amount purpose	Detention authority	New infrastructure	Charge-pool change
Maryland (2017)	**Court rule (4-216.1)**	**Appearance only**	**Unchanged**	**None**	**None**
New Jersey (2014, 2017)	Const. amend. + CJRA	Appearance only[a]	Expanded (PD enumerated)	Statewide PSA + services	Detention pool enumerated
Illinois (2021, 2023)	Statute (PFA + SAFE-T)	n/a (cash bail eliminated)	Expanded (PD enumerated)	Partial[b]	Cash bail eliminated; pool enumerated
New Mexico (2016)	Const. amend. + Rule 5-409	Appearance + safety[c]	Expanded (felony PD)	Statewide PSA + services	Detention pool enumerated
New York (2019; amended 2020–23)	Statute (CPL amendments)	Appearance only[d]	Unchanged	None	Bail pool narrowed
California (*In re Humphrey* 2021)	Court decision	Appearance + safety	Unchanged	None	None
Nevada (*Valdez-Jimenez* 2020)	Court decision	Appearance + safety	Unchanged	None	None
Colorado (2013)	Statute (HB 13-1236)	Appearance + safety	Unchanged	Partial	None
Missouri (2019)	Court rule (33)	Appearance + safety	Unchanged	None	None
Connecticut (2017)	Statute (PA 17-145)	Appearance + safety	Unchanged	None	None
Texas (2021, 2025)	**Statute + const. amend.**	**Appearance + safety**	**Expanded**	**Data only[e]**	**Detention pool expanded**
North Carolina (2023, 2025)	**Statute**	**Appearance + safety**	**Expanded**	**None**	**Detention pool expanded**
Georgia (2024)	**Statute (SB 63)**	**Appearance + safety**	**Unchanged**	**None**	**Bail pool expanded[f]**
Ohio (2020, 2022, 2023)	**Mixed (rule/amend./statute)**	**Appearance + safety[g]**	**Unchanged**	**None**	**None**
Alaska (2016, 2017, 2019)	Statute (curtailed)	Appearance + safety	Unchanged	Partial (curtailed)	None

Notes: Columns code the reform's legal instrument (*mechanism*); the post-reform purpose monetary conditions may serve (*bail-amount purpose*); whether it expanded formal preventive detention (*detention authority*); whether it built statewide pretrial infrastructure (*new infrastructure*); and whether it changed the pool of cases eligible for bail or detention (*charge-pool change*). Bold: Maryland and post-2020 reforms that expanded bail or detention reach.

[a] N.J. Stat. §2A:162-15 limits monetary conditions to appearance; cash bail largely displaced by recognizance + supervision.

[b] Statewide detention-hearing infrastructure built; county pretrial-services rollout uneven.

[c] Bail may address safety but cannot be set to detain except via Rule 5-409.

[d] CPL 510.10 makes appearance the sole bail purpose — a long-standing provision, not a 2019 innovation.

[e] Damon Allen Act built a statewide criminal-history data system (Public Safety Report System), no pretrial-services apparatus.

[f] Inverse direction: SB 63 expanded the offenses for which money bail is *required*, restoring cash bail as detention.

[g] Post-rollback: 2020 Crim.R. 46 narrowed bail to appearance; 2022 Issue 1 + 2023 R.C. 2937.011 reversed it, adding safety.

Sources: R Street Institute 2024 50-state pretrial-features coding; statute and case-law mapping cited per row.

Table 32: Local linear RD estimates of reform impact on case-resolution outcomes (CCT robust SE)

Outcome	Oct 25, 2016 cutoff			Long-window (pre-reform to po		
	Pre (%)	Post (%)	RD (p) (pp)	Pre (%)	Post (%)	RD (p) (pp)
Case closed within 365 days	84.3	83.8	3.49 (0.117)	84.3	84.4	2.98 (0.13
Guilty disposition	26.9	26.2	2.26 (0.659)	26.9	26.7	0.41 (0.92
Not-guilty disposition	58.5	57.7	1.12 (0.675)	58.5	58.6	0.57 (0.86
Other disposition (incl. nolle pros, dismissal)	7.8	9.1	0.86 (0.839)	7.8	10.9	0.65 (0.86
Probation before judgment (PBJ)	7.8	7.5	-1.21 (0.475)	7.8	8.3	2.64 (0.32
Stet	14.5	15.1	0.52 (0.806)	14.5	15.0	1.83 (0.47
Effective n		27,290			29,727	

Cluster-robust CCT bias-corrected SE at the court-county level. Significance stars from robust p-values: $^{*}p < 0.05$, $^{**}p <$ 0 0.001. Sample restricted to cases with an initial hearing within 3 days of filing and a non-missing commissioner decision. Effec number of cases within the 90-day bandwidth used by the local-linear fit.

Table 33: Heterogeneity in the reform's effects at the October 25, 2016 cutoff.

		Decisions				Safety (12-mo)			
	Share	Released	Held on default	Held w/o bail	Bail ($1,000s)	FTA	Viol. rearrest	Any rearrest	*N*
A. By charge type									
§5-202	7%	9.1	-23.9**	12.5*	-$14.0	1.6	8.6	10.1	1,789
		(21.6)	(55.5)	(22.8)	($28.1)	(5.5)	(18.2)	(47.4)	
Felony, violent	10%	2.8	-14.9	11.9*	-$30.1**	0.5	-1.4	-2.6	2,789
		(7.4)	(75.1)	(17.2)	($72.1)	(0.6)	(15.1)	(30.9)	
Felony, nonviolent	17%	15.3*	-15.8*	0.3	-$12.2	-0.7	1.7	4.8	4,788
		(22.7)	(71.8)	(5.3)	($33.0)	(6.0)	(9.2)	(37.0)	
Misd., violent	18%	6.7	-13.7	6.9**	-$2.9	1.4	-3.0	1.8	5,090
		(39.5)	(57.8)	(2.6)	($11.4)	(6.5)	(17.3)	(32.1)	
Misd., nonviolent	49%	10.5	-9.6	-1.1	-$2.8	0.9	1.8	5.1	12,834
		(64.3)	(33.1)	(2.3)	($5.0)	(15.9)	(10.9)	(42.2)	
B. By predicted violent rearrest									
Bottom tercile	25%	10.7*	-10.4	-0.8	-$4.7	1.3	-1.2	-0.1	6,483
		(60.1)	(37.4)	(2.3)	($11.3)	(10.3)	(4.9)	(23.4)	
Middle tercile	32%	12.6**	-12.5*	-0.6	-$11.7**	1.0	4.4*	7.6*	8,908
		(44.6)	(50.8)	(4.4)	($21.1)	(9.7)	(9.2)	(33.3)	
Top tercile	42%	9.6	-16.0**	6.3*	-$8.4*	0.9	-2.0	2.0	11,899
		(34.2)	(56.8)	(8.7)	($22.5)	(10.5)	(19.7)	(51.0)	
C. By race and sex									
Black male	47%	11.6*	-14.8**	3.4	-$10.1	2.7	-0.0	4.1	12,778
		(39.2)	(53.1)	(7.5)	($23.8)	(9.5)	(15.6)	(42.2)	
Black female	9%	12.1*	-13.0*	0.4	-$5.4	-1.8	-3.0	5.9	2,626
		(56.3)	(41.8)	(1.7)	($11.2)	(12.0)	(11.8)	(29.0)	
White male	29%	6.1	-8.0	0.5	-$7.3*	0.3	3.8	7.1*	8,113
		(42.9)	(51.1)	(5.6)	($17.9)	(9.6)	(10.5)	(37.3)	
White female	11%	12.3	-15.1	2.9	-$8.3*	-3.3	-0.7	-3.8	2,964
		(53.8)	(43.1)	(2.8)	($8.6)	(13.3)	(8.8)	(38.5)	

Note. October 25, 2016 cutoff RDiT estimates within each subgroup, with the pre-reform baseline in parentheses below each estimate; the October-cutoff companion to the long-window Table 3. *Share* is the subgroup's share of the analysis sample. Decisions and safety outcomes in percentage points, bail in thousands of dollars per case; failure to appear and rearrest are measured over twelve months. Risk terciles are of the frozen pre-reform predicted-violent-rearrest score. Significance stars from robust CCT court-county-clustered inference. *N* is the effective (kernel-weighted) sample.

Table 34: Money-bail cases by posting outcome, pre vs post

	Pre-reform		Post-reform		Oct. post (first dose)	
	Posted	Not posted	Posted	Not posted	Posted	Not posted
Cases	4,691	3,031	2,657	1,154	3,276	1,362
Bail and charge						
Median bail	\$7,500	\$15,000	\$5,000	\$5,000	\$5,000	\$7,500
Felony (%)	39	46	41	36	39	40
Violent (%)	38	38	30	22	35	30
§5-202 trigger (%)	5	8	5	8	5	9
Predicted risk (within-pool percentile)						
Failure to appear	45	56	46	65	47	62
Violent rearrest	46	59	44	58	47	59
Demographics						
Defendant Black (%)	55	56	61	56	62	60
Mean age	32	33	32	34	32	33
Neighborhood (census tract)						
Median household income	\$60,061	\$56,786	\$60,777	\$55,780	\$58,679	\$55,339
Per-capita income	\$29,003	\$27,760	\$28,850	\$27,646	\$28,506	\$27,586
Poverty rate (%)	12	13	12	13	13	14
Unemployment (%)	7	7	7	8	8	8
Owner-occupied (%)	61	58	60	59	57	57
Black (%)	29	30	37	33	39	42

Money-bail-set pool (a financial condition was imposed at the initial appearance). “Posted” = released within three days. Risk percentiles are within-pool ranks of the frozen pre-period FTA and violent-rearrest scores. Tract rows are census-tract characteristics of the defendant’s address (available for 88% of cases), and describe the neighborhood rather than the individual. Post-reform posters and non-posters face the same median bail; the non-posters are distinguished mainly by predicted risk, with a modest tilt toward poorer, more renter-heavy, and more heavily Black tracts.

Table 35: Nonpayment detention by charge group: held on default and not released within three days, share of all cases

		October 2016 cutoff		Long window	
	Pre-mean (%)	Estimate	95% CI	Estimate	95% CI
All cases	20	-6.6^{***}	$[-10.0, -3.2]$	-11.3^{***}	$[-14.8, -7.8]$
Felony, violent	38	-15.4^{*}	$[-28.2, -2.7]$	-24.7^{***}	$[-39.2, -10.1]$
Felony, nonviolent	27	-11.7^{***}	$[-18.0, -5.5]$	-18.1^{***}	$[-24.3, -11.8]$
Misdemeanor, violent	17	$+0.1$	$[-6.2, 6.3]$	-3.2	$[-9.5, 3.1]$
Misdemeanor, nonviolent	13	-4.4^{*}	$[-8.7, -0.2]$	-8.7^{***}	$[-12.2, -5.3]$
Detention-eligible (§5-202)	28	-12.1	$[-25.6, 1.5]$	-10.2	$[-24.4, 4.0]$

Note. The outcome is an indicator for being held on default of bond at the three-day endpoint and not released within three days of the initial appearance, over all cases in the analysis sample (no conditioning on the money-bail pool). Local-linear RDiT, 90-day bandwidth, robust bias-corrected estimates in percentage points, court-county clustered. Pre-mean is the local pre-cutoff level. Charge groups follow the five-type partition; the four felony/misdemeanor cells exclude detention-eligible cases. $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$.

Table 36: Predicted-risk profile of the marginal releases

	Long window			October window		
	All	Released	Marginal [95% CI]	All	Released	Marginal [95% CI]
Mean predicted risk (%)						
Failure to appear	14.0	15.5	14.1 [13.3, 15.0]	13.9	15.5	13.9 [13.2, 14.7]
Any rearrest	40.0	37.2	41.4 [39.6, 42.9]	40.5	37.2	41.5 [39.2, 43.0]
Violent rearrest	13.5	11.5	14.0 [12.9, 14.9]	13.6	11.5	14.1 [12.8, 15.1]
Share by all-defendant violent-rearrest quintile (%)						
Q1 (lowest)	20.0	28.0	17.8 [13.7, 22.8]	20.0	28.0	17.5 [13.2, 22.9]
Q2	20.0	22.3	19.8 [18.7, 21.2]	20.0	22.3	20.0 [18.9, 21.6]
Q3	20.0	18.9	20.2 [18.3, 21.5]	20.0	18.9	20.5 [18.8, 21.7]
Q4	20.0	17.6	20.8 [19.3, 21.8]	20.0	17.6	20.2 [18.2, 21.4]
Q5 (highest)	20.0	13.2	21.4 [18.2, 24.6]	20.0	13.2	21.9 [18.1, 25.2]

The marginal releases are defendants held on default of an unaffordable amount before the reform who are released on recognizance or unsecured bond after it. Individuals are not observed under both regimes, so the marginal group's predicted-risk profile is recovered by the probability weight of Appendix A.3.2: each pre-reform case is weighted by the reform-induced rise in its predicted release probability, and the frozen scores are averaged with those weights. A model-free identity gives the same profile and the two agree (Appendix A.4). "All" is the full initial-appearance population in the window; "Released" is the pre-reform released pool. Risk scores are frozen pre-reform predictions on a 0–100 scale. Brackets are 95% court-county-clustered bootstrap intervals (2,000 draws). The long window contrasts the 90 days before the October 2016 cutoff with the 90 days after the July 2017 effective date; the October window uses the 90 days after the October 2016 cutoff, limiting case-mix drift. The marginal releases sit near the average across all defendants on all three scores and modestly above the already-released on rearrest and violent rearrest; they are not the highest-risk cases, which moved to held without bail rather than release.

Table 37: Reform effects by race: Black-versus-White differential.

	White effect	Black effect	Black − White: No controls	Black − White: + Controls
Panel A. October 25, 2016 cutoff (regression discontinuity)				
Release (ROR/UPB)	+10.8	+11.3	+0.5	+0.7
	(3.2)	(2.1)	(2.7)	(2.6)
Held on default	-13.1	-14.0	-0.9	-1.0
	(3.7)	(2.4)	(2.8)	(2.5)
Held without bail	+1.9	+2.5	+0.7	+0.7
	(1.3)	(1.1)	(1.5)	(1.7)
Any detention	-11.3	-11.5	-0.2	-0.4
	(3.2)	(2.2)	(2.6)	(2.5)
Log bail amount	-35.4	-32.9	+2.4	-4.0
	(12.5)	(9.7)	(12.6)	(9.9)
Panel B. Pre-reform to post-Rule long window (descriptive)				
Release (ROR/UPB)	+22.8	+15.6	-7.2*	-5.6
	(4.0)	(2.6)	(3.6)	(3.5)
Held on default	-28.7	-24.5	+4.2	+3.4
	(4.3)	(2.1)	(4.3)	(3.9)
Held without bail	+5.8	+8.6	+2.8	+2.1
	(1.8)	(1.7)	(1.9)	(1.8)
Any detention	-22.9	-15.9	+7.1†	+5.5
	(4.0)	(2.6)	(3.6)	(3.4)
Log bail amount	-71.8	-67.1	+4.8	+6.7
	(17.7)	(15.8)	(14.1)	(10.4)

Pooled Black-and-White sample. Each outcome is fit by local-linear RDiT with the running-variable polynomial fully interacted with a Black indicator; the "Black − White" columns report the post-by-Black coefficient (the differential reform effect), without and with controls for charge attributes, the frozen risk scores, and court-county fixed effects. Decision outcomes in percentage points; log bail amount (among cases set a money bail) in log points; standard errors, clustered by court-county, in parentheses. The per-group effects differ slightly from the separate-subgroup figures in Table 3 because this specification pools the two groups in one interacted regression. †, *, **, *** denote $p < .10, .05, .01, .001$.

Table 38: Disentangling robustness ladder, Letter (October 2016) cutoff: predicted-risk loadings by control set.

	A	B	+inc	+chg	full
Held without bail (pp / SD)					
Violent rearrest	3.8*** / 2.9***	2.0*** / 0.9	1.7*** / 1.4**	1.9*** / 0.9	1.6*** / 1.4*
Any rearrest	4.2*** / 2.6***	1.9*** / 1.1*	1.6*** / 1.5**	1.9*** / 1.0	1.7*** / 1.4**
Failure to appear	−2.9*** / −3.3***	−0.5 / −0.0	−0.5 / −0.2	−0.1 / −0.1	0.1 / −0.2
Held on default (pp / SD)					
Violent rearrest	8.9*** / −3.9***	4.2*** / −2.1**	4.6*** / −2.5**	3.3*** / −1.8*	3.7*** / −2.2**
Any rearrest	13.7*** / −3.7**	8.5*** / −2.2*	9.2*** / −2.7*	7.0*** / −1.7	7.6*** / −2.2
Failure to appear	−7.3*** / 3.2**	1.5 / −0.1	1.0 / 0.3	3.5*** / −0.2	3.0*** / 0.3
Bail amount (% / SD)					
Violent rearrest	20.2*** / −6.2*	6.3* / −3.1	6.4* / −4.5*	6.4** / −1.9	6.9** / −3.4
Any rearrest	29.2*** / −10.0**	8.5** / −7.0*	8.0** / −6.6*	5.9* / −5.1	6.1* / −5.3
Failure to appear	−47.6*** / 4.6*	−14.9*** / −3.6	−14.3** / −4.5	−6.1* / −3.0	−5.7 / −3.5

Note. Each cell is pre-reform level / change at reform, per standard deviation of the frozen pre-period predicted-risk score (Letter (October 2016) cutoff). Control sets: *A*, risk axes only; *B*, + demographics, charge attributes, and court and month fixed effects; *+inc*, + tract median household income; *+chg*, + the full set of specific charge codes; *full*, + income and charge codes together (the main-text specification, Table 7). Predicted violent rearrest and predicted failure to appear enter jointly. Held-without-bail and held-on-default in percentage points; the bail amount in percent (log points), conditional on a case held on default with a positive amount. Court-county-clustered inference. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 39: Disentangling robustness ladder, post-Rule (long) window: predicted-risk loadings by control set.

	A	B	+inc	+chg	full
Held without bail (pp / SD)					
Violent rearrest	3.6*** / 6.5***	1.6*** / 3.6***	1.4*** / 3.9***	1.4** / 3.7***	1.1** / 3.9***
Any rearrest	4.0*** / 6.2***	1.5** / 3.5***	1.2** / 4.1***	1.5** / 3.8***	1.2* / 4.3***
Failure to appear	−2.9*** / −5.0***	−0.4 / −0.2	−0.3 / −0.7	0.2 / −0.5	0.4 / −1.1
Held on default (pp / SD)					
Violent rearrest	9.2*** / −7.4***	4.6*** / −4.4***	4.9*** / −4.3***	3.7*** / −4.0***	4.1*** / −4.0***
Any rearrest	13.9*** / −7.9***	9.0*** / −5.6***	9.6*** / −5.7***	7.3*** / −5.2***	7.9*** / −5.1***
Failure to appear	−7.2*** / 5.3***	1.3 / −0.0	0.8 / 0.1	3.4*** / −0.4	2.9** / −0.3
Bail amount (% / SD)					
Violent rearrest	21.1*** / −8.7**	7.1* / −6.1*	7.2* / −6.4*	6.7** / −4.2*	7.2** / −5.1*
Any rearrest	30.0*** / −13.0***	9.4** / −7.9**	8.8** / −7.9**	6.5** / −3.8	6.6** / −4.1
Failure to appear	−47.8*** / 9.2***	−15.0*** / −1.4	−14.5** / −2.6	−5.0* / −2.7	−4.4 / −4.0

Note. Each cell is pre-reform level / change at reform, per standard deviation of the frozen pre-period predicted-risk score (post-Rule (long) window). Control sets: *A*, risk axes only; *B*, + demographics, charge attributes, and court and month fixed effects; *+inc*, + tract median household income; *+chg*, + the full set of specific charge codes; *full*, + income and charge codes together (the main-text specification, Table 7). Predicted violent rearrest and predicted failure to appear enter jointly. Held-without-bail and held-on-default in percentage points; the bail amount in percent (log points), conditional on a case held on default with a positive amount. Court-county-clustered inference. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.