# The Effects of Citations in Lieu of Arrests: Evidence from Maryland

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September 2, 2026

**Abstract**

This paper examines the effect of replacing arrests with criminal citations on defendant outcomes and subsequent criminal charges. Using web-scraped Maryland court records, I implement a difference-in-discontinuities design that exploits a January 1, 2013 policy shock encouraging citations for low-level offenses. The policy increased the issuance of citations by 48.5% among citation-eligible defendants and 39.1% among citation-eligible first-time defendants. Reduced-form estimates show sizable decreases in pretrial detention for these groups, falling by 36.3% and 41.2%, respectively. Citation-eligible defendants are also 5.2% more likely to have their cases dismissed. However, reduced pretrial detention among eligible defendants is offset by increased pretrial detention among defendants charged with more serious offenses that are ineligible for citation, leaving aggregate pretrial detention unchanged. Finally, I find little evidence that the policy increased failure-to-appears or re-offending.

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# 1 Introduction

In 2023, local jails recorded 7.6 million admissions, with an average daily population of about 665,000 individuals (Zeng, 2025). Most jailed individuals (69%) are unconvicted, awaiting court on a current charge or held for other reasons, such as failing to appear in court. Jailing is also expensive, costing approximately $107 to house a single person per day (Virginia Compensation Board, 2022). Given the documented social costs of pretrial detention (Dobbie et al., 2018; Gupta et al., 2016; Leslie and Pope, 2017; Stevenson, 2018), policymakers have increasingly adopted reforms aimed at reducing detention, such as the elimination of cash bail and the use of pretrial risk assessment tools. However, such reforms operate only after an individual has already been arrested and booked at a jail. One method of reducing custodial contact altogether is a citation in lieu of arrest.

Unlike an arrest, a criminal citation formally charges an individual with a crime but allows them to remain in the community while awaiting court. This alternative avoids the time-intensive arrest process, from custody in a police cruiser to booking and possible pretrial detention. The issuance of criminal citations in lieu of arrest therefore may offer an attractive, cost-saving tool by routing fewer individuals through jail while still upholding the law. However, criminal citations may come with social costs if individuals change their perception of the probability of apprehension and conviction.

In this paper, I examine a policy shock in Maryland that increased the likelihood of a defendant receiving a criminal citation. On January 1, 2013, police officers statewide were instructed to issue criminal citations for low-level offenses carrying a maximum punishment of 90 days or less in jail. I exploit this sudden increase by employing a difference-in-discontinuities approach (Grembi et al., 2016). I leverage the granularity of web-scraped court records data to examine case-level outcomes for defendants with cases filed just before and after the cutoff date, relative to the same cutoff in prior years. The data also allow me to link individuals across time to examine re-offending.

I find that the policy increased the issuance of criminal citations by 24.7 percentage points, a 48.5% increase, among defendants eligible for a citation (i.e., individuals committing offenses with a maximum punishment of 90 days or less in jail). Downstream, defendants are 3.3 percentage points (36.3%) less likely to be detained pretrial and 3.3 percentage points (5.2%) more likely to have a dismissed case, suggesting that defendants improve bargaining by avoiding bail dockets. I find no statistically significant effect on failure-to-appears, with an estimated 0.8 percentage point (4.7%) increase. Taken at face value, the point estimates imply that the policy induced approximately one additional failure-to-appear for every four pretrial detentions avoided.

I also study outcomes for first-time defendants, who may be particularly sensitive to initial criminal justice contact and provide a cleaner sample for examining recidivism.[1] I find a 22.5 percentage point (39.1%) increase in citations among citation-eligible defendants. Again, I find the strongest downstream effect on pretrial detention, with a 2.1 percentage point (41.2%) reduction. Similar to all defendants, first-time defendants are 2.1 percentage points (3.1%) more likely to have their case dismissed, but the estimate is not statistically significant. Likewise, I estimate a meaningful but statistically insignificant 1.7 percentage point (10.6%) increase in failure-to-appears. I also find no statistically significant effect on recidivism, measured by a 1.1 percentage point (8.6%) decrease in the likelihood of having a subsequent case filed within one year of the initial offense.

However, the gains from eligible defendants are offset by changes in outcomes for defendants charged with offenses not targeted by the reform (i.e., citation-ineligible). Primarily, I show a 2.7 percentage point *increase* in pretrial detention for all defendants that are citation-ineligible. As a result, net pretrial detention across all defendants is unchanged. These findings suggest that, by reducing the inflow of low-level defendants into jail, the policy may have eased capacity constraints and shifted pretrial detention toward citation-ineligible defendants charged with more serious offenses. Furthermore, I find suggestive evidence of weaker case outcomes for first-time defendants outside the targeted population, with a 5.1 percentage point (11.6%) decrease in the likelihood of a case dismissal.

Taken together, the effects on eligible and ineligible defendants highlight both the

[1] As discussed in Section 5, I only observe an individual once when analyzing patterns of re-offending, as a re-offense can be used as a later observation and may bias estimates.

importance of identifying an appropriate counterfactual and the aggregate effects of the reform. Commonly, researchers employ a difference-in-differences design to identify causal effects of a policy shock by classifying eligible (or more intensely treated) individuals as "treated" and ineligible individuals as a control group. An important assumption of such designs is that the treatment event does not produce any spillover effects on the ineligible group (i.e., SUTVA). I show that a difference-in-differences framework using ineligible defendants as a counterfactual group would overstate the effect of the reform on outcomes like pretrial detention. Additionally, from a policy perspective, citations in lieu of arrest may appear to be an attractive tool for reducing jail populations, but the aggregate benefits depend on how judicial actors respond to the changed composition of defendants and cases in bail hearings.

My findings relate to work on the effects of early-stage criminal justice interventions in two dimensions: diversion and pretrial detention. Mueller-Smith and Schnepel (2021) show that diversion programs, which allow individuals to avoid a criminal record, decrease future offending and increase formal employment. Similarly, Agan et al. (2023) find that non-prosecution of certain misdemeanors substantially reduces subsequent criminal justice contact. By contrast, Hansen (2015) finds that individuals punished for driving under the influence are less likely to commit future DUIs, which suggests that less lenient early-stage interventions can strengthen deterrence and decrease re-offending. Studies on pretrial detention (Dobbie et al., 2018; Stevenson, 2018) find that being detained prior to trial increases the probability of conviction and worsens labor market outcomes, with limited or no evidence of reductions in future offending.[2] My setting is closer to the pretrial detention studies, since the policy reduces criminal justice contact by replacing custodial arrests and possible pretrial detention with criminal citations, rather than by declining to prosecute cases or convict defendants altogether.

More directly, my findings connect to research on bail reform. Ouss and Stevenson (2023) study a prosecutor-led reform that reduced reliance on cash bail and find limited impacts on failure-to-appears and pretrial crime. Albright (2025) examines the auto-

[2] See also Leslie and Pope (2017) and Gupta et al. (2016).

matic release of low-level offenders from pretrial detention and finds a small increase in failure-to-appears with minimal effects on pretrial crime. Sloan et al. (2025) evaluate the implementation of risk-assessment tools by judges and find a decrease in pretrial detention, with little effect on pretrial crime. My work complements these findings by exploring a setting that decreases pretrial detention by reducing arrests.

This study also fits into work on police discretion and its implications for public safety and equity. Gonçalves and Mello (2021) and Gonçalves and Mello (2023) document wide police discretion in speeding ticketing and show that this discretion in speeding ticketing yields few public safety benefits. Other papers have studied "de-policing" episodes, in which officers reduce enforcement in response to public scrutiny or deaths among fellow officers, and generally find limited effects on overall crime (Cheng and Long, 2022; Cho et al., 2024). I contribute to these findings by examining the effects of constraining officer discretion in arrest decisions. Last, I add to a small literature examining drivers of failure-to-appears (Fishbane et al., 2020; Owens and Sloan, 2023).

# 2 Background

## 2.1 Criminal procedure

Under the Becker (1968) model of crime, police officers are a key input into the probability of apprehension and conviction, and they influence the perceived threat of apprehension by making arrests. Officers have the authority to make warrantless arrests if they believe there is probable cause that an individual committed a criminal offense. If an officer initiates an arrest, the suspect is placed into custody, typically after a pat-down search for contraband, paraphernalia, or weapons. The officer then transports the suspect to a local jail or booking station for processing, which includes taking fingerprints and a mugshot, collecting information about the individual (including name, date of birth, and address), and searching for any outstanding warrants or criminal history. In Maryland specifically, the individual then has an initial appearance in front of a District Commissioner,[3] who

[3]District Commissioners in Maryland are officials who work out of local jails and do not need to be judges or lawyers. At the initial appearance, they review whether the officer's statement establishes

determines whether the suspect is released on recognizance or detained pretrial. If the suspect is held on bail by the District Commissioner and does not post bond, they appear in front of a judge within 24 to 48 hours for a bail hearing, and the judge has the ability to release the individual or change the bond amount and terms (Commission to Reform Maryland's Pretrial System, 2014).

An alternative to custodial arrest is the issuance of a criminal citation, which allows an individual to remain in the community while awaiting court. The defendant is issued a charging document listing the charges brought against them and specifying when they must appear in court. Unlike a civil citation, a criminal citation still carries the bite of a criminal charge, as the individual may ultimately face jail time rather than at most a fine. The officer collects basic identifying information about the defendant but does not transport them to a jail or booking station for full processing. Criminal citations take less time to complete than arrests, with Desmarais et al. (2023) finding that officers spent an average of approximately 28 minutes on a citation compared to about 171 minutes for a custodial arrest.[4] Thus, issuing citations may be an attractive cost-saving mechanism for police departments and court systems by limiting jail admissions and decreasing the workload of judges and other judicial actors. Police officers can then remain on duty patrolling their beat, rather than spending time processing an arrest at the station.

## 2.2 Policy background

On January 1, 2013, all law enforcement officers in Maryland were encouraged to issue criminal citations in lieu of arrest for certain crimes. This policy was implemented following several statewide shocks to the judicial system. On January 4, 2012, Maryland's highest court, in *DeWolfe v. Richmond*, ruled that the right to counsel began at the initial appearance in front of a District Commissioner (DeWolfe v. Richmond, 2012). This interpretation of the state's Public Defender Act suddenly extended the right to a public defender to the initial appearance, a service that had not previously been avail-

probable cause for each charge and decide on release conditions (DeWolfe v. Richmond, 2012).

[4]It is worth noting this study was conducted during 2020, so the COVID-19 pandemic may have increased the time of arrest for officers.

able in Maryland. After several stays delaying implementation of the decision, the state legislature compromised by amending the law to provide counsel for any bail hearing in front of a judge and increasing funding for the statewide Office of the Public Defender by $6.3 million to accommodate this right (Carroll, 2012; Maryland General Assembly, 2012). The bill was passed on May 22, 2012, and public defenders began representing defendants at bail hearings soon after.[5]

The same legislation aimed to limit the flow of individuals into local jails and the pool of potential clients for public defenders at bail hearings by encouraging officers to issue criminal citations in lieu of arrest. Specifically, the law stated that officers "must charge by citation" for:

(i) any misdemeanor or local ordinance violation that does not carry a penalty of imprisonment;

(ii) any misdemeanor or local ordinance violation for which the maximum penalty of imprisonment is 90 days or less; and

(iii) possession of marijuana

subject to several exceptions (See Appendix A.4). The statute further provided that an officer could proceed by citation only if the officer believed the defendant would comply, was not a threat to public safety, and was not subject to arrest for another criminal charge from the same incident. In other words, an individual charged only with eligible offenses would not be arrested and booked but rather issued a criminal citation. Meanwhile, the court proceedings and criminal record for such defendants would in theory remain the same. Notably, these citation provisions did not go into effect until January 1, 2013, seven months after the bill was passed.

Criminal citations are not unique to Maryland. In most states, statutes authorize officers to issue a criminal citation in lieu of custodial arrest for at least some offenses (National Conference of State Legislatures, 2019). However, the scope and structure of these citation statutes vary widely. Some states limit citations to traffic offenses or minor local ordinance violations, while others also allow citations for criminal misdemeanors,

[5]Because the interpretation of *DeWolfe v. Richmond* was based on statutory law, the legislature had the power to overturn this ruling by changing the language of the law. The legislature partially recognized the ruling by changing the language to provide public defenders at bail hearings.

and a smaller number permit citations even for certain low-level felonies. States also differ in whether certain crimes are "presumed" to be cited. Often, an officer has discretion to levy a citation, though in a few states, such as Maryland, officers are presumed to issue a citation for certain offenses. Additionally, a survey of police departments found that 86.8% of departments used criminal citations, though only 9.8% mandated officers "must use" citations for certain eligible crimes (International Association of Chiefs of Police, 2016). Thus, there is notable heterogeneity across states and cities in what crimes may be cited and whether there is a presumption of citation, and cross-state comparisons must be interpreted in light of each state's statutory framework.

## 2.3 Motivation

Allowing officers to charge by criminal citation in lieu of arrest may be a useful cost-saving measure for police departments and local courts. Jails are largely funded by taxpayers, with an average annual cost per inmate of roughly $47,000, primarily through payroll expenses (Henrichson et al., 2015).[6] Each additional person booked into jail requires staff attention for intake, supervision, and release. Jailed individuals also have a right to health care and receive treatment while in custody (Wilper et al., 2009), further raising the cost of detention. Fewer arrests could also reduce crowding on bail-hearing dockets, which are often brief and may last only one to three minutes per case (Stevenson, 2018). Each hearing typically involves at least a judge, a prosecutor, and, in Maryland, a public defender, all salaried by the local or state government.

There could also be important social costs and benefits associated with using citations in lieu of arrest. The issuance of a criminal citation may reduce the likelihood of re-offending if individuals are able to avoid the disruptive nature of a custodial arrest, including time in jail and its collateral consequences. However, a criminal citation might be less deterring than an arrest, which could change individuals' perceptions of the cost of future apprehension and alter their offending patterns. Therefore, the net effect of replacing arrests with citations on recidivism is theoretically ambiguous. An individual

[6] A Virginia report in 2021 finds a similar per-day cost of $107 (Virginia Compensation Board, 2022).

who receives a citation may also be more likely to fail to appear in court, either mechanically because they are not detained pretrial, or because they perceive a citation as less serious than an arrest. A failure to appear can potentially produce greater harm for the individual if they are subsequently arrested on a bench warrant, while also disrupting and delaying court proceedings.

# 3 Data

I use administrative court records drawn from the Maryland Judiciary Case Search, an online portal of Maryland court filings (Maryland Judiciary, nd). Specifically, I use the web-scraped records compiled by the "Client Legal Utility Engine" (CLUE). From 2016 to 2020, the CLUE portal, funded and hosted by the Maryland Volunteer Lawyers Service, continuously scraped new records and updated existing ones, with the goal of making case search data easier to use in practice by citizens, lawyers, and researchers.[7] The resulting dataset includes all court records that were publicly available at the time, covering criminal, civil, and traffic cases.

Outcomes are measured at the case level. Each case record includes information on the defendant, the type of case filed, the charges and associated Criminal Justice Information System (CJIS) codes, the court of origin, and other case characteristics. Each case is filed in a specific District Court and primarily classified into one of four case types: summons, warrant arrest, warrantless arrest, and citation. Warrantless arrests and citations correspond to cases initiated by police officers, while summonses and warrant arrests are initiated by the court. Because the paper focuses on whether individuals are arrested by officers or issued a citation, I restrict the analytical sample to cases initiated by officers.[8] There is one defendant per case. For each defendant, the data report full name, date of birth, address, race, gender, and other physical attributes. Events that occur while the case is active are also recorded, including bail hearings (with the hearing date and bail amount), related persons such as the arresting officer or any attorney of

[7]Per correspondence with the curator of the data and researchers in Maryland.

[8]In Appendix A.1, I show that the likelihood a case is filed by an officer rather than by the court is uncorrelated with treatment.

record, and any arrest warrants issued for failure to appear.

Importantly, a case contains all charges brought forward by an officer or court. Each charge has a plea, disposition, and an assigned CJIS code used to identify specific criminal statutes and offenses.[9] By linking CJIS codes to their respective maximum punishments in 2012, I am able to distinguish which offenses would have been eligible for a citation under the new law in 2013. A case containing any charge with a maximum punishment greater than 90 days in jail is ineligible for a criminal citation. Under the statute, some offenses, such as failure to comply with a peace order, were not eligible for citation despite carrying a maximum punishment of no more than 90 days in jail. In addition, possession of paraphernalia has a tiered penalty schedule in which the statutory maximum depends on offense history: a $500 fine for a first offense and up to two years of incarceration for subsequent offenses. I use the defendant's linked criminal history to determine whether a paraphernalia charge is a first offense and classify its eligibility accordingly.[10] I provide complete details on the classification of citation eligibility in Appendix A.4. Throughout the paper, I refer to cases in which all charges satisfy the statute's eligibility requirements as "citation-eligible."

The data allow me to examine several outcomes of interest. First, through the case type, I can observe if a defendant receives a criminal citation, allowing me to document the first-stage effect of the policy. I also examine bail records to determine whether defendants are released on recognizance or detained pretrial. Pretrial detention may be of particular interest given that a central objective of the policy was to reduce the number of defendants entering the bail process. Citations may alter the bargaining position of defendants, which can be captured in disposition outcomes such as the likelihood of a case dismissal. Last, I can examine any failure-to-appears through arrest warrants.

The breadth of defendant characteristics allows me to link individuals to cases across time. Specifically, individuals are linked by their first name, last name, and DOB. Detailed information on the linking process is provided in Appendix A.3. By doing so, I build a pool of first-time defendants, defined as individuals without a previous case filed

[9]For example, the CJIS code "1-0573" denotes possession of marijuana.
[10]See Appendix A.3 for further details on linking defendants across cases.

against them. This subsample is useful for two reasons. First, defendants with no prior criminal justice contact may respond differently to the policy than defendants with previous contact. Second, recidivism outcomes may be mechanically biased in a case-level regression discontinuity framework when repeat offenses re-enter the data as additional observations (Finlay et al., 2024). I return to this issue in Section 5.

Last, the CLUE dataset reflects the set of court records that were publicly available at the time of scraping and does not capture older cases that had been expunged prior. Appendix A.5 describes which cases were eligible for expungement under Maryland law at the time of scraping. To assess the extent to which expunged cases may affect my analysis, I match CLUE data with an alternative dataset, MD Case Explorer, which scraped the Maryland Judiciary Case Search several years after CLUE. I provide further discussion in Appendix A.5, and show my main results are similar using MD Case Explorer data in Appendix Table B7.

# 4 Methodology

## 4.1 Identification strategy

I exploit quasi-random variation in the likelihood of receiving a criminal citation generated by the granularity of case filing dates and the discrete policy shock on January 1, 2013. One natural approach is to estimate a regression discontinuity in time (RDiT) by comparing outcomes for individuals whose cases are filed just after January 1, 2013 to those with cases filed just before. The simple regression discontinuity is estimated by the following:

$$Y_{it} = \beta_0 + \beta_1 Post_{it} + \beta_2 Days_{it} + \beta_3 \big(Post_{it} \times Days_{it}\big) + \varepsilon_{it}. \tag{1}$$

where individuals are indexed by $i$ and the date of case filing is indexed by $t$. The running variable $Days_{it}$ is the number of calendar days between the filing date and January 1, 2013, normalized so that $Days_{it}$ equals zero on January 1, 2013. $Post_{it}$ is an indicator variable equal to one for values of $Days_{it} \geq 0$ and zero otherwise. The coefficient of interest, $\beta_1$, captures the intent-to-treat estimate of the policy shock. The underlying assumptions are that determinants of defendant outcomes evolve smoothly at the cutoff date, and that neither defendants nor officers can precisely manipulate the filing date near January 1. However, given the policy begins on the first day of the calendar year, outcomes may exhibit seasonal patterns even in the absence of reform, which may violate this continuity assumption. For example, defendants are more likely to be charged with drug offenses at the start of the calendar year, as illustrated in Appendix Figures B1 and B2.

I instead employ a difference-in-discontinuities approach, which combines elements of regression discontinuity and difference-in-differences designs. This design still leverages the granularity of case filing dates and the discrete policy shock but compares the discontinuity at January 1, 2013 to the discontinuities at January 1 in 2011 and 2012, when the policy was not in effect. I construct three separate RD subsamples consisting of cases filed near January 1 in 2011, 2012, and 2013. Within each subsample, I define a running variable equal to the number of days between the filing date and January 1 of that year, normalized to zero on January 1. The absolute value of the running variable is restricted to be at most 182 days so that each case can belong to only one RD subsample. The three subsamples are then combined, with an indicator variable to distinguish whether an observation belongs to the subsample around January 1, 2013 (hereafter the “2013 RD”) or to the subsamples around January 1 in 2011 and 2012 (hereafter the “placebo RD”). By differencing the discontinuity in the 2013 RD from the discontinuity in the placebo RD, I net out any common seasonal pattern at the turn of the year, under the assumption that similar movements around January 1 would have occurred in 2013 absent the policy reform.

My formal specification is as follows:

$$\begin{aligned} Y_{ict} = \beta_0 &+ \beta_1 Post_{ict} + \beta_2 Days_{ict} + \beta_3\big(Post_{ict} \times Days_{ict}\big) + \beta_4 2013RD_{ict} \\ &+ \beta_5\big(Post_{ict} \times 2013RD_{ict}\big) + \beta_6\big(2013RD_{ict} \times Days_{ict}\big) \\ &+ \beta_7\big(2013RD_{ict} \times Post_{ict} \times Days_{ict}\big) + X'_{ict}\theta + \gamma_c + \varepsilon_{ict} \end{aligned} \tag{2}$$

where defendants are again indexed by $i$ within District Court $c$, and the calendar day of case filing is indexed by $t$. Here, $Days_{ict}$ indicates the standardized value relative to January 1 within each RD sample (2011, 2012, or 2013), normalized to zero on January 1 of that year. $Post_{ict}$ now indicates if the case is filed on or after January 1 within the corresponding RD sample. The parameter of interest is $\beta_5$, which captures the difference between the discontinuity at January 1 in 2013 and the discontinuity in the placebo RD. Under this specification, I assume that the discontinuity at January 1 in 2013 would have been the same as the discontinuity at January 1 in the placebo RD absent the policy reform.

I allow for model flexibility in several ways. The interactions among $2013RD_{ict}$, $Post_{ict}$ and $Days_{ict}$ allow for differing slopes on either side of the cutoff and across RD subsamples. I also include court fixed effects, denoted as $\gamma_c$. I add a vector of covariates, $X_{ict}$, containing the following case characteristics: indicator variables for any property, drug, violent, or other crime type; the number of charges in the case; the defendant's age; and indicator variables for male and Black. Standard errors are clustered by the running variable, and I also present wild bootstrapped p-values, clustered by District Court.[11] Throughout the paper, I use the wild-cluster bootstrap p-values for statistical inference.

Across all specifications, the baseline estimates use a common bandwidth of 155 days on either side of the cutoff with uniform weighting. The maximum feasible bandwidth is 182 days, since larger windows would allow observations to appear in more than one RD sample. Because there is no existing MSE-optimal bandwidth selector for a difference-in-discontinuities design, I choose the baseline bandwidth using separate RDiT bandwidth calculations. Specifically, I estimate the MSE-optimal bandwidth (Calonico et al., 2020) around January 1 in 2011, 2012, and 2013 for both all defendants across all offenses and first-time defendants across all offenses, using an indicator variable for a citation issued as the outcome in each estimation. The average of these six optimal bandwidths is 154.5 days, so I use 155 days as the common baseline bandwidth across samples and outcomes. Section 5 provides sensitivity to alternative bandwidths.

[11] I provide bootstrapped p-values as there are only 12 District Courts in the sample.

## 4.2 Identification tests

Across all identification tests, I examine four groups: all defendants across all offenses, all defendants eligible for a citation, first-time defendants across all offenses, and first-time defendants eligible for a citation. I include the all-offense samples to test whether the overall composition of cases changes discontinuously at the policy threshold. Additionally, because the all-offense samples contain both citation-eligible and citation-ineligible defendants, I can directly test whether treatment changes the likelihood that a defendant is classified as citation-eligible.

I first examine whether there is any sorting of defendants across the cutoff date by plotting the density of cases. Any sorting would likely be driven by officers rather than defendants timing their offending to the calendar. Figure 1 shows, for each group, the density of the standardized running variable $Days_{ict}$, separately for observations in the 2013 RD and in the placebo RD. Across all groups, there is a higher density of cases in the tails (summer months) compared to around the cutoff (winter months), consistent with the seasonality of crime. I formally test for discontinuities in the density of case filings by collapsing the data to the number of cases filed per calendar day and estimating Equations (1) and (2) with daily case counts as the outcome.[12] This approach tests for a discrete change in filings at January 1 in 2013 and whether any such change differs from the placebo years. I do observe an increase in case filings in the 2013 RD within the subsamples of all defendants, but this trend is matched in the placebo RD. In all subsamples, the estimate using Equation (2)— i.e., the difference-in-discontinuities— is statistically insignificant.

I then examine whether there is any underlying relationship between treatment and observable case characteristics. Specifically, I re-estimate Equation (2) for each variable in the vector of covariates as an outcome, with the vector excluded from the regression. The estimates are reported in Table 1. I provide both a standard error clustered by the

[12]In the collapsed data, the unit of observation is day $\times$ RD year-of-interest (2011, 2012, 2013), with the running variable normalized to zero. I first estimate Equation (1) separately for the 2013 RD and the pooled placebo sample. In the placebo regression, I include an indicator for the RD year (2011 vs. 2012) to control for level differences. I then estimate Equation (2) on the pooled sample to recover the difference-in-discontinuities in daily filings at the cutoff, again using an indicator for each RD year.

running variable, presented in parentheses, and a wild bootstrapped p-value clustered by District Court, presented in braces. For each outcome, the value in brackets reports the mean of that variable within the 2013 RD before January 1 (i.e., $2013RD_{ict}$ =1 and $Post_{ict} = 0$).

Looking first at all defendants of all offenses, there is a marginally statistically significant 1.7% decrease in the age of the defendant, using the bootstrapped p-value for inference. There is also no detectable effect on the likelihood a defendant is charged only with citation-eligible offenses. Among citation-eligible defendants, there is a marginally significant 3.7% increase in the likelihood a defendant is Black and a statistically significant 2.3% decrease in the age of the defendant. For first-time defendants, there is only a statistically significant 3% decrease in the age for citation-eligible defendants. Furthermore, I use the case characteristic variables in Columns (1) through (8) of Table 1 to predict whether a defendant receives a citation. I then regress this predicted value as the outcome of Equation (2), again excluding the vector of covariates, to test whether the predicted value of citation is correlated with treatment, with the estimate provided in Column (9) of Table 1. Across all specifications, the estimated effect is small in magnitude and statistically insignificant.

Panels (a) and (c) of Figure 2 present the predicted likelihood of citation issuance for all and first-time citation-eligible defendants, distinguishing observations within the 2013 RD from those in the placebo RD. To maintain parity with Equation (2), the series plotted in Figure 2 is demeaned by District Court prior to binning. There is a small detectable change in the predicted likelihood of receiving a criminal citation at the turn of the calendar year. However, the predicted value is shown to *decrease*, opposite of the expected effect of the policy, and this occurs at both the start of 2013 and previous years. Thus, the difference-in-discontinuities approach should capture this idiosyncrasy at January 1 in all years.

In total, while there is some imbalance across subsamples, the effect sizes are not meaningfully large. Reassuringly, the predicted values of citation based on covariates are orthogonal to treatment. From both Panel A and Panel C of Table 1, defendants are not more likely to be classified as citation-eligible following the policy adoption. As an additional check, I use the observable case characteristics to predict the following downstream outcomes of interest: pretrial detention, case dismissal, failure-to-appear, and case filing within one year. Appendix Figure B3 presents estimates from Equation (2) separately for citation-eligible and citation-ineligible defendants, and each estimate is small and statistically insignificant. I therefore restrict my primary analysis to defendants charged with citation-eligible offenses in the next section, while also examining potential spillover effects among citation-ineligible defendants.

# 5 Results

I organize the results around three groups of analysis. I first examine citation-eligible defendants as these defendants were directly targeted by the reform. I separately report estimates for first-time defendants, both because this sample allows for a cleaner interpretation of recidivism and because first-time defendants may be especially responsive to initial criminal justice contact. I then analyze citation-ineligible defendants to test whether the policy generated spillover effects outside the targeted population. Finally, I report all-offense estimates as aggregate effects of the statewide policy and test whether the main results are sensitive to model specification.

## 5.1 Effects on citation-eligible defendants

I first examine the policy effects on defendants whose charges meet the eligibility requirements for a citation under the 2013 reform. Within this group, I divide my analysis between all defendants and first-time defendants, as first-time defendants may respond differently to the policy compared to defendants with prior criminal justice contact, and they could be more malleable to a policy reform than repeat offenders. Distinguishing first-time defendants is also necessary for interpreting recidivism outcomes. As Finlay et al. (2024) note, a short-run increase in recidivism can mechanically raise the share of repeat offenders on the treated side of a discontinuity, as re-offenses enter the data as additional case observations. This increase in the number of observations per person can bias case-level estimates of recidivism, so I restrict my analysis of recidivism to first-time defendants. As shown by the outcome means in Table 1, first-time defendants on average are younger, less likely to be Black, less likely to be male, and more likely to have citation-eligible cases.

Panels (b) and (d) of Figure 2 provide visual evidence of the effect on citation issuance at January 1, 2013 for all and first-time citation-eligible defendants, relative to the placebo RD. The outcome is demeaned by District Court prior to binning, with the overall outcome mean added back for level interpretation. There is a notable increase in citations issued at January 1, 2013, compared to a slight decrease at the same day in prior years. The policy appears to be more binding for first-time defendants, with more than 80% of such defendants receiving a citation after the policy shock. Additionally, there is a slight upward trend in citations issued prior to the implementation date, but this uptick is captured in the estimates by allowing the 2013 RD and the placebo RD to have differing slopes on either side of the cutoff.

Table 2 reports the resulting difference-in-discontinuities estimates. I present the results by incorporating court fixed effects and covariates (Columns (1) through (8) of Table 1) additively. Again, I report standard errors by clustering on the running variable, presented in parentheses, and a wild bootstrapped p-value clustered at District Court, presented in braces. I use the specification with court fixed effects and covariates (Columns (3) and (7) of Table 2) as my main specification. There is a statistically significant 24.7 percentage point increase in the likelihood of receiving a citation for all defendants, a 48.5% increase.[13] For first-time defendants, there is a statistically significant 22.5 percentage point increase, a 39.1% increase.

I next examine the effects on downstream outcomes, with visual evidence provided in Figure 3. I first test whether the policy had an effect on pretrial detention. Defendants eligible for a citation under the new policy were typically charged with low-level offenses such as marijuana possession or petty theft. Thus, it is plausible the marginal defendant

[13] Effect sizes are relative to the mean of the outcome in the 2013 RDiT prior to the policy adoption (i.e., $2013RD = 1$ and $Post = 0$).

to receive a citation because of the policy shock would have been released on recognizance had they been arrested. I find a statistically significant 3.3 percentage point (36.3%) reduction for all defendants and a 2.1 percentage point reduction (41.2%) for first-time defendants. Receiving a criminal citation may also improve bargaining for defendants if they become less likely to enter a guilty plea in order to be released from pretrial detention. For all defendants, there is a statistically significant 3.3 percentage point (5.2%) increase in the likelihood of having a case dismissal.[14] The effect size on first-time defendants is smaller, and across both samples, estimates are sensitive to the addition of court fixed effects. Additionally, the estimated effects on pretrial detention and case dismissal are substantially larger than the changes predicted by observable case characteristics (Appendix Figure B3).[15] Together, I find that defendant outcomes are improved through decreases in pretrial detention and increases in case dismissals.

The increase in criminal citations and subsequent decrease in pretrial detention, however, may increase failures to appear in court (FTAs) if defendants are not held in jail prior to trial. Additionally, individuals might perceive the ramifications of missing court less seriously when cited rather than arrested. I find a small and imprecise 0.8 percentage point increase in FTAs for all defendants and a 1.7 percentage point increase for first-time defendants. Taking the estimates at face value, the results imply that for every 1,000 eligible defendants charged, there were 8 additional failure-to-appears, relative to 33 fewer pretrial detentions, which equates to approximately one failure-to-appear for every four pretrial detentions avoided. The upper bound of the 95% confidence interval implies a 2.8 percentage point increase, or 28 additional failure-to-appears per 1,000 defendants, which remains below the estimated reduction of 33 pretrial detentions per 1,000 defendants. The trade-off is less favorable for first-time defendants, however, with 17 additional failure-to-appears compared to 21 fewer pretrial detentions per 1,000 defendants.

Last, I examine whether receiving a criminal citation affects re-offending patterns,

[14] This outcome is defined as equal to one if all charges within a case have one of the following dispositions: "Dismissed", "*Nolle prosequi*" (to not be prosecuted), or "Stet" (a postponement of any prosecution).

[15] For comparability with the predicted outcome estimates, I report the estimates from Columns (2) and (6), which include court fixed effects but exclude covariates.

measured by any subsequent case filing within one year.[16] This analysis is restricted to first-time defendants. There is a statistically insignificant 1.1 percentage point reduction in re-offending, an 8.6% decrease. I provide alternative methods of measuring recidivism in Appendix Table B1, by defining recidivism as any case filing within 3 months, 6 months, or 2 years, and each estimate is statistically insignificant.

## 5.2 Spillover and aggregate effects

I next turn my analysis to defendants charged with higher-severity offenses, defined by whether at least one charge within the case carries a maximum punishment above the citation-eligibility threshold. Table 3 reports estimates for "citation-ineligible" defendants, separately for all defendants and first-time defendants. For both defendant types, there is no detected effect on receiving a citation, though there is an economically meaningful 21.8% reduction for first-time defendants. However, I detect an increased likelihood of being detained pretrial. Including court fixed effects and covariates, there is a statistically significant 2.7 percentage point (5.3%) increase in pretrial detention for all defendants and a smaller, statistically insignificant 1.5 percentage point (3.4%) increase for first-time defendants. The increase in pretrial detention for ineligible defendants suggests that, in response to a changed composition of defendants in bail hearings, judicial actors detain a greater share of the ineligible population, producing a meaningful spillover effect on ineligible defendants.

Ineligible defendants also may experience differences in case outcomes if they are more likely to be detained pretrial. I find that first-time defendants are 2.4 percentage points (4.9%) less likely to have their case dismissed, though the estimate is imprecise. However, the estimate may be biased as I do not observe the disposition for any case elevated to Circuit Court in Prince George's County, which accounts for 21% of these defendants.[17]

[16] I use this outcome as I may be limited in analyzing the long-run effects on recidivism, as Maryland decriminalized marijuana in October 2014, 21 months after the policy adoption. Since drug offenses account for a large share of citation-eligible first-time defendants (40.2%), I may mechanically fail to detect an effect on re-offending in longer horizons if future drug offenses are no longer prosecuted.

[17] Specifically, a case originates in District Court and can elevate to Circuit Court, where it becomes a new case filing. The final disposition for the original (District Court) case is then captured in the Circuit Court case disposition, not the District Court case disposition. For all counties but Prince George's County, I can link District Court cases to their Circuit Court pair. See Appendix A.1 for more details.

Linking District Court cases to Circuit Court cases is particularly important for ineligible defendants, as 27% of District Court cases are elevated to Circuit Court among such defendants, compared to only 4% of citation-eligible cases. With cases filed in Prince George's County excluded altogether, I find first-time defendants are 5.1 percentage points (11.6%) less likely to have a case dismissed (Appendix Table B3),[18] suggestive of prosecutors or judges again offsetting gains experienced by eligible defendants. Last, I find increases in failure-to-appears and recidivism for first-time defendants, though neither estimate is statistically significant when using the bootstrapped p-value for inference.

I report the total effects of the policy on all defendants and first-time defendants of all offenses (eligible and ineligible) in Appendix Table B4. In total, there was a statewide increase in citations issued for all defendants and first-time defendants by 10.9 and 11.7 percentage points, respectively. As suggested previously, I detect no net effect on pretrial detention, consistent with judges subsequently holding more ineligible defendants on bail following the compositional change of defendants in bail hearings. However, I do find an aggregate increase in case dismissals for all defendants.

Taken together, the estimated effects on the ineligible group and total population may demonstrate the importance of model selection for estimation. In particular, one approach for estimating the effects of the policy could be a difference-in-differences (DiD) design using ineligible defendants as a counterfactual for eligible defendants. I illustrate how this approach would produce a biased estimate using pretrial detention as an outcome.

For comparability with Table 2, I restrict the analysis to only observations within the $2013RD$. I then regress the following:

$$PTD_{icm} = \alpha Eligible_{icm} + \beta(Eligible_{icm} \times Post_m) + \gamma_c + \delta_m + \epsilon_{icm}. \tag{3}$$

where defendants are again indexed by $i$ within District Court $c$, and the calendar month of case filing is indexed by $m$. $Eligible_{icm}$ denotes whether an individual is charged with citation-eligible offenses, and $Post_m$ indicates whether a case is filed on or after January 1, 2013. By interacting the two terms, $\beta$ captures the change in pretrial detention for

[18] I provide a similar table for citation-eligible defendants in Appendix Table B2.

eligible defendants, relative to the change for ineligible defendants, after the policy reform. District Court fixed effects are denoted by $\gamma_c$, and month fixed effects are denoted by $\delta_m$. Last, I present a standard error clustered by District Court and its bootstrapped p-value.

The model assumes that pretrial detention for eligible and ineligible defendants would have evolved in parallel trends absent the policy adoption. Figure 4a presents the dynamic DiD plot for all defendants, which suggests the two groups trended similarly before the policy adoption. The design also assumes no spillover effects on the untreated group under the stable unit treatment value assumption. As documented in Table 3, this assumption is unlikely to hold. Figure 4b displays the trends separately for citation-eligible and citation-ineligible defendants, showing that the two groups sharply split in opposite directions after the reform.

Using Equation (3), I estimate a statistically significant 6.4 percentage point decrease in pretrial detention for all defendants. I provide a comparable exercise for first-time defendants and estimate a statistically significant 6.1 percentage point decrease. In both cases, the difference-in-differences estimate is much larger in magnitude than the difference-in-discontinuities estimate provided in Table 2. This finding demonstrates that a difference-in-differences design using citation-ineligible defendants as a counterfactual group would produce biased estimates, as the stable unit treatment value assumption is shown to be violated.

## 5.3 Sensitivity

The results may be sensitive to model specification, including covariates and court fixed effects. As discussed previously, the results do fluctuate with the addition of court fixed effects for case dismissals, which could be attributed to a higher share of cases filed in lenient courts on the treated side of the cutoff. The results remain consistent, however, with the addition of covariates. I now test whether the results are sensitive to bandwidth selection, weighting schema, and other model choices. Throughout the robustness checks, I report estimates including court fixed effects and covariates, and I focus the discussion on citation-eligible defendants for brevity.

I first assess robustness to bandwidth choice. A unique feature of the model is that the maximum feasible bandwidth is 182 days, as larger bandwidths would mechanically cause observations to appear multiple times. I therefore report estimates using bandwidths of 180, 130, 105, and 80 days, relative to the main specification of 155 days. Figure 5 presents the estimates across bandwidths for all defendants and first-time defendants, with 95% confidence intervals using standard errors clustered by the running variable. Across both groups, the estimates appear stable to bandwidth selection. For first-time defendants, the estimated effect on having a case dismissed increases in magnitude and becomes marginally statistically significant at smaller bandwidths. Appendix Figures B4 and B5 provide analogous bandwidth sensitivity checks for the full sample of defendants and for citation-ineligible defendants, respectively, and the results remain robust.

I next test robustness to alternative kernel weights. Table 4 reports estimates using uniform, triangular, and Epanechnikov kernels. This exercise produces similar patterns to the bandwidth analysis, since down-weighting observations far from the cutoff is analogous to using a smaller effective bandwidth. Relative to uniform weighting, triangular and Epanechnikov kernels yield similar results across most outcomes for all defendants. For first-time defendants, the effect on case dismissal increases in magnitude using alternative weighting schemes but remains statistically insignificant. Again, I include kernel sensitivity checks for the full sample of defendants of all offenses in Appendix Table B5 and the sample of citation-ineligible defendants in Appendix Table B6. Altogether, I provide evidence that the results are robust to weighting choice.

I now provide additional robustness checks, with the estimates for all defendants provided in Appendix Figure B6 and first-time defendants in Appendix Figure B7. First, I show that the estimates remain stable to the addition of day-of-the-week fixed effects. I also estimate a one-day "donut" specification that drops observations for which $Days_{ict} = -1$ or 0. A concern may be that the running variable should be determined by the offense date, rather than case filing date. However, 14.8% of cases filed before 2013 are missing a recorded offense date, though 98.4% of cases with a reported offense date are filed within one day of the recorded offense date. A related concern is that cases associated with offenses committed around New Year's Eve may not be filed until after January 1, resulting in excess filings on the treated side of the cutoff. Thus, the one-day donut should address concerns about delays in case filing, particularly around January 1, and the results remain robust to this specification. Last, I also check whether the results are sensitive to how paraphernalia offenses are classified as citation-eligible. As a reminder, I use a defendant's prior paraphernalia charges to determine eligibility, as first-time paraphernalia charges are eligible for citation while subsequent charges are not. I show that treating all paraphernalia offenses as citation-eligible does not materially affect the results. In total, the estimates remain robust across these alternative model choices.

Across tables, I report standard errors clustered by the running variable alongside wild-cluster bootstrap p-values clustered by District Court. I further assess inference using a randomization exercise based on Equation (2). Specifically, I randomly assign one-third of defendants to a "treated" RD subsample and the remaining two-thirds to the placebo RD subsample. This reassignment preserves each defendant's actual position relative to the January 1 cutoff while randomizing membership in the treated RD group. I then re-estimate Equation (2) for each random assignment and compare the original estimate to the distribution of 999 placebo estimates. The permutation p-value is calculated as the share of placebo estimates whose absolute value is at least as large as the absolute value of the original estimate. Appendix Figure B8 presents the resulting permutation distributions separately for all defendants and first-time defendants. The original estimate is highlighted in blue, while placebo estimates are shown in gray, with estimates larger in magnitude than the original estimate shaded darker. The effects on pretrial detention and dismissal remain statistically significant for all defendants, and for first-time defendants, only the effect on pretrial detention is statistically significant.

# 6 Conclusion and discussion

I examine the effect of a Maryland policy that quasi-randomly increased the likelihood of receiving a criminal citation in lieu of arrest. Using a difference-in-discontinuities design, I find that the policy increased citation issuance among citation-eligible defendants by 48.5%, reduced their likelihood of pretrial detention by 36.3%, and increased the likelihood of case dismissal by 5.2%. Meanwhile, I do not detect an increase in failures to appear. The results are similar for first-time defendants, a group of particular policy interest that also allows for a cleaner analysis of recidivism. I find no detectable effect on recidivism, measured by whether a defendant has an additional case filed within one year.

The policy also generated meaningful spillover effects on defendants not directly targeted by the reform. Primarily, the reduction in pretrial detention among citation-eligible defendants was offset by an increase in detention among citation-ineligible defendants, leaving aggregate pretrial detention unchanged. The reform therefore appears to have changed who was detained pretrial rather than reducing overall detention. This spillover also has important implications for identification. A natural identification method may be a difference-in-differences design using ineligible defendants as a control group for eligible defendants. However, the reform itself increased detention among the ineligible group, which the DiD would classify as the unaffected counterfactual. As a result, such a design would substantially overstate the reduction in pretrial detention among defendants targeted by the reform.

These findings help clarify the welfare implications of citations in lieu of arrest. Reducing custodial arrests through citation issuance may be an attractive way to lower criminal justice costs, particularly through reductions in jail-related costs. However, the policy did not reduce aggregate pretrial detention, suggesting that savings from lower detention among eligible defendants were offset by greater detention among defendants charged with more serious offenses. At the same time, this reshuffling may generate efficiency gains by detaining a greater share of defendants charged with higher-severity offenses. More broadly, the results suggest that the effects of citation reform depend on both the behavior of targeted defendants and the responses of judicial actors.

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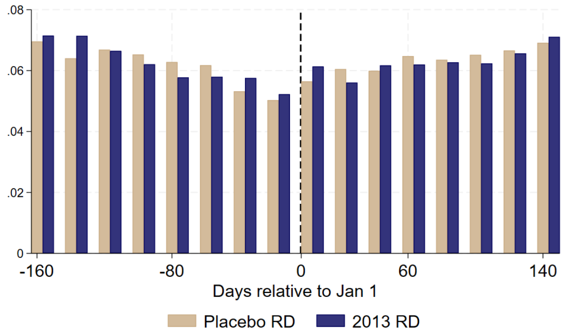
# 7 Figures and tables

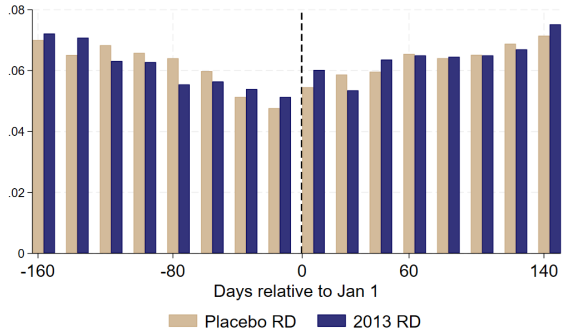
Figure 1: Density of case filings around January 1, by defendant sample

(a) All defendants



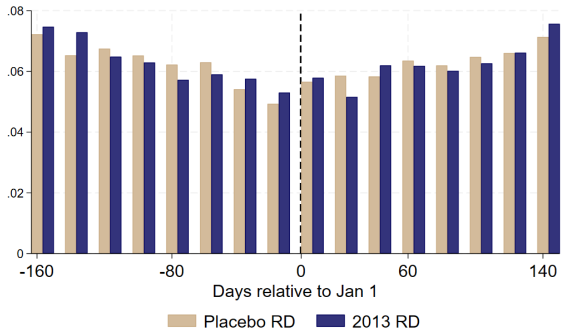

$$\hat{\beta}_{\text{2013RD}} = 29.22^{***}\ (11.79)$$
$$\hat{\beta}_{\text{PlaceboRD}} = 22.43^{***}\ (8.38)$$
$$\hat{\beta}_{\text{RD-DiD}} = 6.79\ (14.72)$$

(b) All defendants, citation-eligible



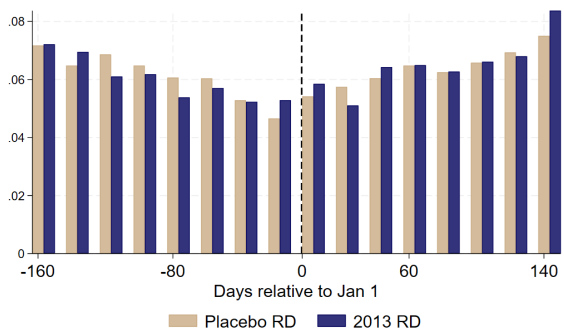

$$\hat{\beta}_{\text{2013RD}} = 14.42^{***}\ (5.40)$$
$$\hat{\beta}_{\text{PlaceboRD}} = 9.68^{**}\ (4.04)$$
$$\hat{\beta}_{\text{RD-DiD}} = 4.75\ (6.74)$$

(c) First-time defendants




$$\hat{\beta}_{\text{2013RD}} = 3.32\ (4.50)$$
$$\hat{\beta}_{\text{PlaceboRD}} = 6.33^{*}\ (3.29)$$
$$\hat{\beta}_{\text{RD-DiD}} = -3.01\ (5.57)$$

(d) First-time defendants, citation-eligible

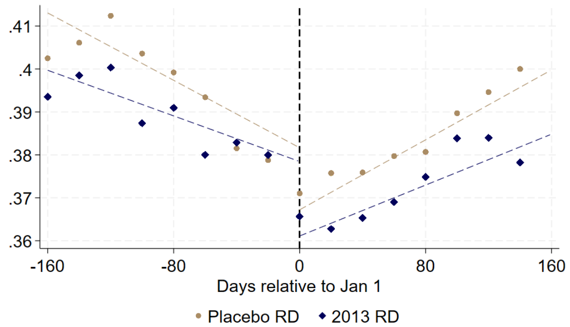



$$\hat{\beta}_{\text{2013RD}} = 3.07\ (2.89)$$
$$\hat{\beta}_{\text{PlaceboRD}} = 3.85^{*}\ (2.08)$$
$$\hat{\beta}_{\text{RD-DiD}} = -0.78\ (3.56)$$

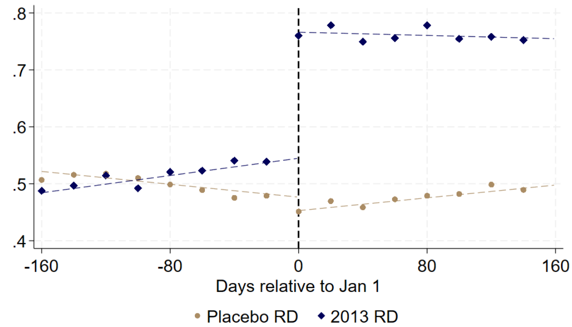
Notes: The unit of observation is a case filing from Maryland Judiciary Case Search records previously provided via the Client Legal Utility Engine (CLUE). Each bar shows the relative frequency of case filings in a 20-day bin of the running variable within a given RD window, either the January 1, 2013 RD or the pooled placebo RD around January 1, 2011 and January 1, 2012. Because the RD windows do not overlap, each case filing appears in at most one specification. Citation-eligible denotes cases consisting only of offenses eligible for citation under the 2013 policy change. First-time defendants appear once, at their first recorded case. $\hat{\beta}_{\text{2013RD}}$ and $\hat{\beta}_{\text{PlaceboRD}}$ report the estimated change in daily case filings using Equation (1), where the unit of observation is the running variable. $\hat{\beta}_{\text{RD-DiD}}$ reports the corresponding difference-in-discontinuities estimate using Equation (2). Statistical significance is denoted as * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 2: Predicted and actual citation issuance among citation-eligible defendants

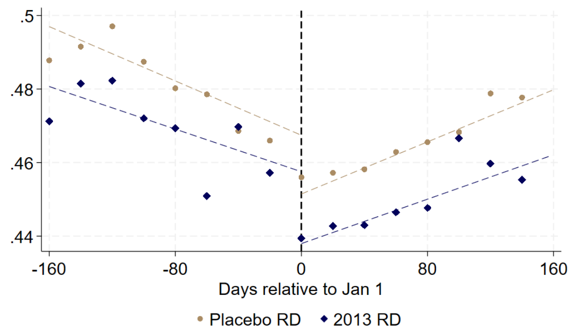
(a) All defendants, predicted value



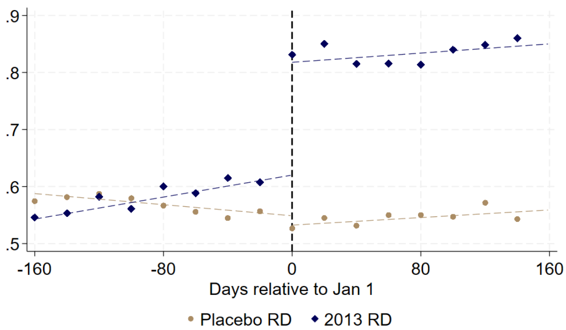

(b) All defendants, actual value




(c) First-time defendants, predicted value



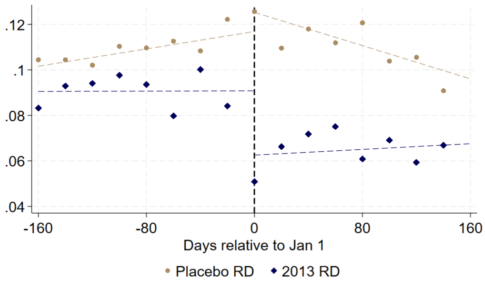

(d) First-time defendants, actual value



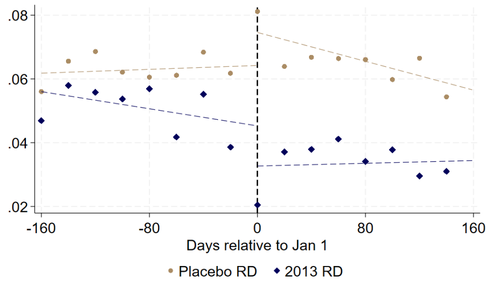

Notes: The unit of observation is a case filing from Maryland Judiciary Case Search records previously provided online via CLUE. Each subfigure plots outcomes demeaned by District Court in 20-day bins, with linear fits estimated separately on either side of the cutoff. The outcome mean is added back for interpretation. Panels (a) and (c) present values obtained by regressing an indicator for a citation issuance on crime-type indicators (property, drug, violent, other), indicators for male and Black, and continuous variables for the number of charges and age. Panels (b) and (d) present the actual values of citation issuance. Binned means are computed separately for the 2013 RD and the placebo RD (2011–2012).

Figure 3: Downstream outcomes for citation-eligible defendants

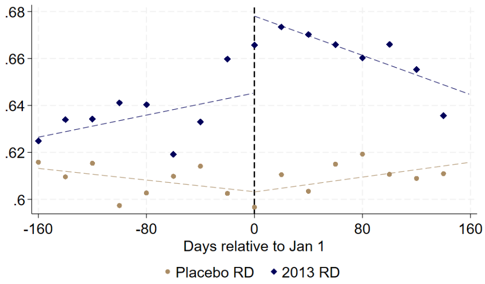
(a) Pretrial detention, all



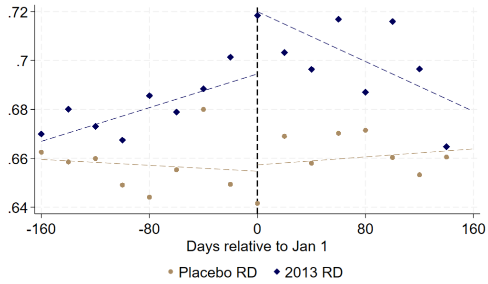

(b) Pretrial detention, first-time



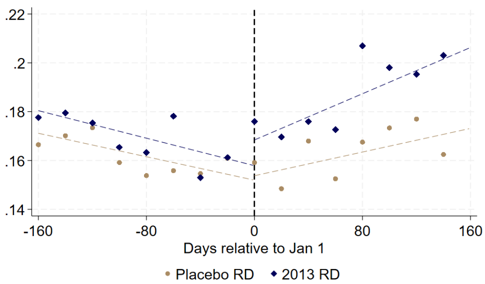

(c) Dismissed, all



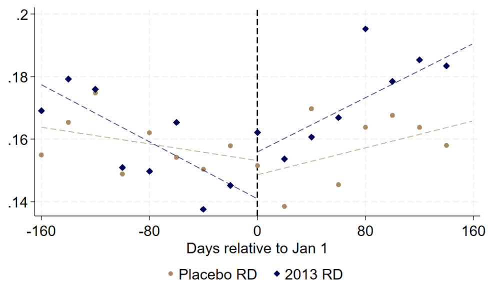

(d) Dismissed, first-time



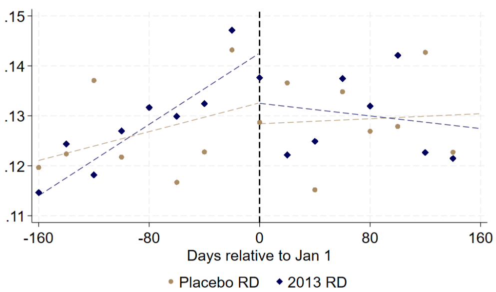

(e) Failure-to-appear, all




(f) Failure-to-appear, first-time



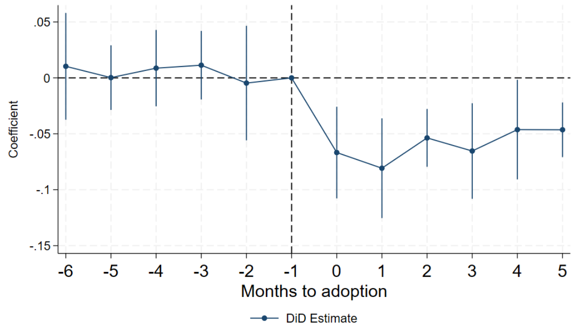

(g) Case filing $<$ 1Y, first-time




Notes: The unit of observation is a case filing from Maryland Judiciary Case Search records previously provided online via CLUE. See the notes to Figure 2 for plotting details. "All" denotes all defendants, while "first-time" denotes first-time defendants.

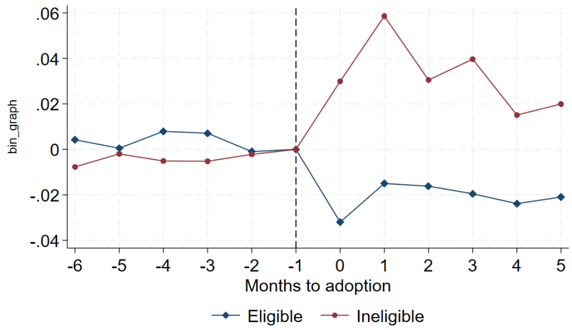
Figure 4: Pretrial detention among citation-eligible and citation-ineligible defendants

(a) All defendants, dynamic DiD

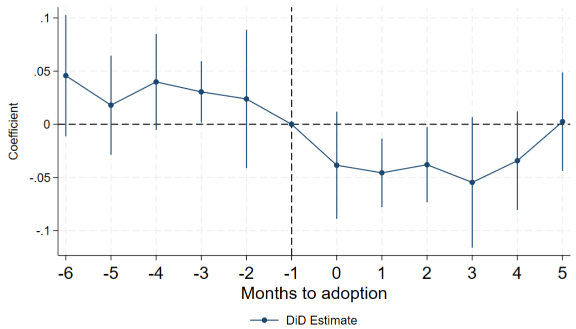



$\hat{\beta}_{\text{DiD}} = -0.064^{***}$ (0.005) [0.000]

(b) All defendants, raw trends

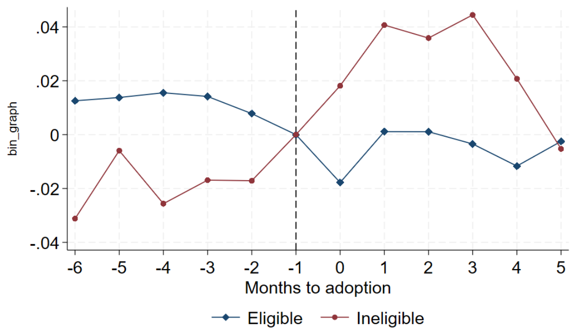



(c) First-time defendants, dynamic DiD




$\hat{\beta}_{\text{DiD}} = -0.061^{***}$ (0.008) [0.001]

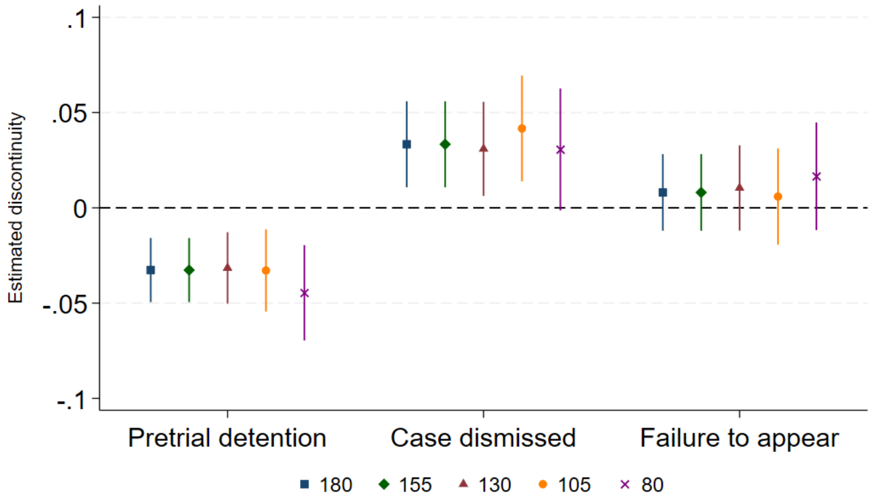
(d) First-time defendants, raw trends



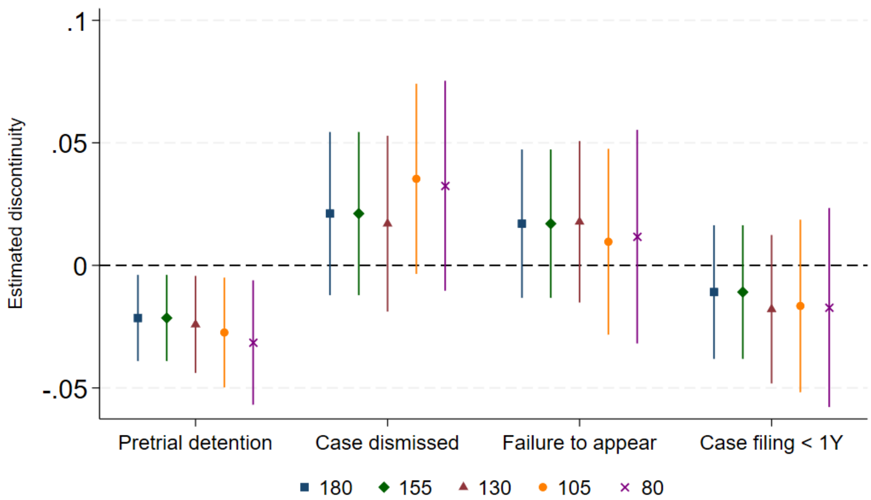

Notes: The unit of observation is a case filing from Maryland Judiciary Case Search records previously provided via the Client Legal Utility Engine (CLUE). Panels (a) and (c) present a dynamic difference-in-differences plot of Equation (3). $\hat{\beta}_{\text{DiD}}$ reports the difference-in-differences estimate using Equation (3). Panels (b) and (d) present the monthly average for pretrial detention by eligible and ineligible defendants, relative to the mean in the month prior to adoption. Statistical significance is denoted as * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Wild cluster bootstrapped p-values are presented in brackets.

Figure 5: Bandwidth sensitivity for citation-eligible defendants

(a) All




(b) First-time




Notes: The unit of observation is a case filing from Maryland Judiciary Case Search records previously provided online via CLUE. Each point reports the difference-in-discontinuities estimate from Equation (2) under the indicated bandwidth (in days) around January 1. Each estimate includes court fixed effects and covariates. Vertical lines denote 95% confidence intervals, with standard errors clustered by the standardized running variable (days relative to January 1). The baseline specification uses a 155-day bandwidth.

Table 1: Balance checks across defendant samples

| | Drug | Property | Violent | Other | Male | Black | Age | Charges | $\widehat{\text{Citation}}$ | Eligible | N |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
| **Panel A: All defendants, all offenses** | | | | | | | | | | | |
| Estimate | -0.017 | 0.007 | 0.005 | 0.014 | -0.005 | 0.013 | -0.562** | 0.007 | 0.003 | 0.015 | 265,209 |
| | (0.019) | (0.012) | (0.013) | (0.013) | (0.007) | (0.010) | (0.255) | (0.042) | (0.005) | (0.011) | |
| | {0.463} | {0.522} | {0.259} | {0.486} | {0.536} | {0.232} | {0.085} | {0.892} | {0.759} | {0.145} | |
| | [0.407] | [0.288] | [0.174] | [0.329] | [0.792] | [0.611] | [32.830] | [2.069] | [0.260] | [0.411] | |
| **Panel B: All defendants, citation-eligible offenses** | | | | | | | | | | | |
| Estimate | 0.006 | 0.010 | | -0.017 | -0.016 | 0.023* | -0.741* | -0.029** | -0.004 | | 109,131 |
| | (0.018) | (0.016) | | (0.019) | (0.010) | (0.014) | (0.432) | (0.013) | (0.007) | | |
| | {0.804} | {0.640} | | {0.668} | {0.173} | {0.082} | {0.027} | {0.123} | {0.825} | | |
| | [0.376] | [0.262] | | [0.403] | [0.803] | [0.614] | [32.494] | [1.209] | [0.389] | | |
| **Panel C: First-time defendants, all offenses** | | | | | | | | | | | |
| Estimate | -0.007 | 0.017 | -0.011 | 0.004 | -0.018 | 0.002 | -0.488 | 0.004 | 0.005 | 0.016 | 94,662 |
| | (0.020) | (0.016) | (0.017) | (0.018) | (0.013) | (0.016) | (0.430) | (0.057) | (0.009) | (0.019) | |
| | {0.897} | {0.294} | {0.503} | {0.838} | {0.191} | {0.940} | {0.105} | {0.976} | {0.757} | {0.446} | |
| | [0.349] | [0.274] | [0.180] | [0.373] | [0.727] | [0.524] | [30.086] | [1.951] | [0.348] | [0.503] | |
| **Panel D: First-time defendants, citation-eligible offenses** | | | | | | | | | | | |
| Estimate | 0.002 | 0.027 | | -0.029 | -0.032* | 0.002 | -0.893 | -0.015 | -0.005 | | 48,466 |
| | (0.023) | (0.020) | | (0.024) | (0.017) | (0.020) | (0.580) | (0.022) | (0.009) | | |
| | {0.932} | {0.205} | | {0.376} | {0.256} | {0.972} | {0.031} | {0.553} | {0.714} | | |
| | [0.402] | [0.210] | | [0.419] | [0.751] | [0.513] | [29.571] | [1.228] | [0.470] | | |

Notes: The unit of observation is a case filing from Maryland Judiciary Case Search records previously provided online via the Client Legal Utility Engine (CLUE). Each coefficient reports a difference-in-discontinuities estimate from Equation (2). Standard errors, clustered by the standardized running variable, are in parentheses. Wild bootstrap p-values, clustered by District Court, are in braces. Outcome means, shown in brackets, are computed for observations in the 2013 RD before January 1. "Eligible" denotes citation-eligible status. Statistical significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Difference-in-discontinuity estimates for citation-eligible defendants

| | All defendants | | | | First-time defendants | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | Mean | (5) | (6) | (7) | Mean |
| Citation | 0.234*** | 0.249*** | 0.247*** | 0.509 | 0.216*** | 0.223*** | 0.225*** | 0.576 |
| | (0.017) | (0.016) | (0.014) | | (0.024) | (0.022) | (0.019) | |
| | {0.000} | {0.000} | {0.000} | | {0.000} | {0.000} | {0.000} | |
| PTD | -0.032*** | -0.034*** | -0.033*** | 0.091 | -0.020** | -0.022** | -0.021** | 0.051 |
| | (0.008) | (0.008) | (0.009) | | (0.009) | (0.009) | (0.009) | |
| | {0.020} | {0.017} | {0.007} | | {0.047} | {0.020} | {0.030} | |
| Dismissed | 0.050*** | 0.034*** | 0.033*** | 0.635 | 0.047** | 0.021 | 0.021 | 0.679 |
| | (0.014) | (0.012) | (0.011) | | (0.019) | (0.017) | (0.017) | |
| | {0.000} | {0.050} | {0.037} | | {0.037} | {0.432} | {0.461} | |
| FTA | 0.003 | 0.010 | 0.008 | 0.169 | 0.013 | 0.020 | 0.017 | 0.160 |
| | (0.011) | (0.011) | (0.010) | | (0.016) | (0.016) | (0.015) | |
| | {0.707} | {0.225} | {0.340} | | {0.225} | {0.040} | {0.182} | |
| Case filing $<$ 1Y | — | — | — | — | -0.007 | -0.008 | -0.011 | 0.128 |
| | — | — | — | | (0.014) | (0.014) | (0.014) | |
| | — | — | — | | {0.618} | {0.611} | {0.410} | |
| N | 109,131 | 109,131 | 109,131 | | 48,466 | 48,466 | 48,466 | |
| Court FE | N | Y | Y | | N | Y | Y | |
| Covariates | N | N | Y | | N | N | Y | |

Notes: The unit of observation is a case filing for citation-eligible defendants from Maryland Judiciary Case Search records previously provided online via CLUE. Coefficients report difference-in-discontinuities estimates. Standard errors clustered by the standardized running variable are in parentheses. Wild bootstrap p-values clustered by District Court are in braces. Means are computed for pre-period observations in the 2013 RD. Covariates include columns 1–8 of Table 1. First-time defendants appear once, at their first recorded case.

Table 3: Spillover effects on ineligible defendants

| | All defendants | | | | First-time defendants | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | Mean | (5) | (6) | (7) | Mean |
| Citation | -0.003 | 0.003 | 0.005 | 0.069 | -0.021 | -0.016 | -0.019 | 0.087 |
| | (0.007) | (0.007) | (0.006) | | (0.013) | (0.014) | (0.012) | |
| | {0.795} | {0.782} | {0.645} | | {0.537} | {0.598} | {0.514} | |
| PTD | 0.036*** | 0.034** | 0.027** | 0.514 | 0.010 | 0.013 | 0.015 | 0.447 |
| | (0.013) | (0.013) | (0.012) | | (0.019) | (0.019) | (0.017) | |
| | {0.012} | {0.025} | {0.016} | | {0.718} | {0.665} | {0.515} | |
| Dismissed | 0.009 | 0.002 | 0.001 | 0.438 | -0.021 | -0.023 | -0.024 | 0.486 |
| | (0.012) | (0.012) | (0.011) | | (0.023) | (0.021) | (0.020) | |
| | {0.572} | {0.873} | {0.959} | | {0.506} | {0.514} | {0.455} | |
| FTA | -0.001 | 0.003 | 0.004 | 0.131 | 0.024* | 0.025* | 0.024* | 0.130 |
| | (0.008) | (0.008) | (0.007) | | (0.013) | (0.013) | (0.013) | |
| | {0.859} | {0.707} | {0.595} | | {0.192} | {0.145} | {0.128} | |
| Case filing $<$ 1Y — | — | — | — | 0.020 | 0.020 | 0.020 | 0.148 | |
| | — | — | — | | (0.015) | (0.015) | (0.014) | |
| | — | — | — | | {0.389} | {0.385} | {0.342} | |
| N | 156,078 | 156,078 | 156,078 | | 46,196 | 46,196 | 46,196 | |
| Court FE | N | Y | Y | | N | Y | Y | |
| Covariates | N | N | Y | | N | N | Y | |

Notes: The unit of observation is a case filing for citation-ineligible defendants from Maryland Judiciary Case Search records previously provided online via CLUE. Coefficients report difference-in-discontinuities estimates. Standard errors clustered by the standardized running variable are in parentheses. Wild bootstrap p-values clustered by District Court are in braces. Means are computed for pre-period observations in the 2013 RD. Covariates include columns 1–8 of Table 1. First-time defendants appear once, at their first recorded case.

Table 4: Difference-in-discontinuity results by kernel for citation-eligible defendants

| | All defendants | | | First-time defendants | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Citation | 0.247*** | 0.247*** | 0.246*** | 0.225*** | 0.243*** | 0.237*** |
| | (0.014) | (0.016) | (0.015) | (0.019) | (0.023) | (0.021) |
| | {0.000} | {0.000} | {0.000} | {0.000} | {0.000} | {0.000} |
| PTD | -0.033*** | -0.034*** | -0.032*** | -0.021** | -0.027*** | -0.024** |
| | (0.009) | (0.010) | (0.009) | (0.009) | (0.010) | (0.010) |
| | {0.007} | {0.006} | {0.008} | {0.030} | {0.003} | {0.008} |
| Dismissed | 0.033*** | 0.032*** | 0.033*** | 0.021 | 0.024 | 0.022 |
| | (0.011) | (0.012) | (0.012) | (0.017) | (0.017) | (0.017) |
| | {0.037} | {0.000} | {0.000} | {0.461} | {0.427} | {0.451} |
| FTA | 0.008 | 0.010 | 0.009 | 0.017 | 0.015 | 0.015 |
| | (0.010) | (0.011) | (0.011) | (0.015) | (0.017) | (0.016) |
| | {0.340} | {0.331} | {0.334} | {0.182} | {0.101} | {0.086} |
| Case filing $<$1Y | — | — | — | -0.011 | -0.015 | -0.015 |
| | — | — | — | (0.014) | (0.016) | (0.015) |
| | — | — | — | {0.410} | {0.348} | {0.348} |
| N | 109,131 | 109,131 | 109,131 | 48,466 | 48,466 | 48,466 |
| Weighting method | U | T | E | U | T | E |

Notes: The unit of observation is a case filing for citation-eligible defendants from Maryland Judiciary Case Search records previously provided online via CLUE. Coefficients report difference-in-discontinuities estimates. Standard errors clustered by the standardized running variable are in parentheses. Wild bootstrap p-values clustered by District Court are in braces. Statistical significance is denoted as * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Weight symbol "U" denotes uniform, "T" denotes triangular, and "E" denotes Epanechnikov.

# Appendix A Data

## A.1 Data overview

The data are drawn from Maryland Judiciary Case Search and were previously web-scraped and hosted by the Client Legal Utility Engine (CLUE), a project funded by Maryland Volunteer Lawyers Service.[19] The CLUE platform was designed to provide an accessible interface for clients and their attorneys to view existing court records, and to make these records available in a research-ready format.

The CLUE archive is organized into several datasets. During the period of scraping, the Maryland Judiciary was encouraging courts to transition to the Maryland Electronic Courts (MDEC) system. District Courts in Baltimore City, Baltimore County, Carroll County, Harford County, Howard County, Montgomery County, and Prince George's County had not yet transitioned to MDEC at the time of scraping. As a result, the data-generating process for these "non-MDEC" records differs slightly from that of the MDEC courts. In practice, however, the differences are minor, and all variables used in this study are measured consistently across the MDEC and non-MDEC samples.[20]

Circuit Court records are also split across several datasets. There is one Circuit Court dataset for MDEC cases, while Baltimore City, Montgomery County, and Prince George's County each have a separate Circuit Court file. The remaining non-MDEC Circuit Court records are included in a fourth dataset. All information related to a case is linked by a case number, which connects basic case information (such as the District Court of origin and document type) to the charges and dispositions, bail events, warrants, attorney filings, and other procedural events. Cases that originate in District Court but are later elevated to Circuit Court receive a new case number in the Circuit Court. Within the MDEC sample, a linkage file is used to match District Court and Circuit Court cases. In the non-MDEC sample, Circuit Court cases are already linked back

[19] The original portal was available at `https://cluesearch.org/`.

[20] For example, failure-to-appears are identified from outstanding warrants. In the MDEC sample, this is done using a "warrants" table where failure-to-appears are recorded as "Bench Warrant – Failure to Appear." In the non-MDEC data, failure-to-appears are inferred from an "event history" table by selecting events with type "WARI" (warrant issued) whose description contains "FTA."

to their corresponding District Court cases. One exception is Prince George's County, for which there is no reliable linkage between District Court and Circuit Court cases. Importantly, if a case is elevated to Circuit Court, the charges in the District Court case do not have the final sentencing updated. Instead, the final disposition is provided in the corresponding Circuit Court charges.

Cases may be filed by police officers or through the court system. If an officer initiates an arrest without a warrant, this case "type" is referred to as a "statement of charges" which I use interchangeably with "warrantless arrest." An officer can also elect to issue a criminal citation instead. Cases not initiated by police officers include warrants, which are carried out by police officers, as well as summonses. A summons may be thought of as a criminal citation issued by the court: rather than being arrested, the individual receives a statement of the charges and a date on which to appear in court. I limit the analytical sample to only cases initiated by police officers (warrantless arrests and citations). One identification concern may be the smoothness of officer-initiated cases across treatment. When using the sample of all cases, I find that the likelihood a case is filed by an officer is orthogonal to treatment, with a statistically insignificant 0.0059 (0.92%) increase in the likelihood of an officer-initiated case using the difference-in-discontinuities design and the full sample.

## A.2 Charge cleaning

Each case contains one or more charges. Within a charge, I observe the Criminal Justice Information System (CJIS) code, the associated statute code, the offense date, a description of the charge, the final plea, the ultimate disposition, and the date of disposition. Under the "MDEC" data, I can observe the date the plea was entered, though this is typically the day of disposition. If a case evolves from District Court to Circuit Court, I then use the Circuit Court charges and dispositions as the final verdict.

A fair share (7.9%) of cases contain charges that either lack a CJIS code or have the code replaced with "ConvertedCode." Per correspondence with a Maryland Courts Administrative Clerk, "ConvertedCode" is used as a placeholder for a CJIS code that is "no longer active or there was not a CJIS code assigned to the charge when originally filed." Particularly, this CJIS code (or lack thereof) was commonly used for local ordinance violations with no specific statewide statute to reference. Among criminal cases that contain a charge with a ConvertedCode filed before 2013, 90.7% are issued as criminal citations. These charges appear to be highly correlated with citation-eligible municipal infractions that lack a CJIS code. A text analysis of the charge descriptions shows that a large share of such charges (24%) were alcohol-related offenses, such as open container or public intoxication violations. Other types include, but are not limited to urinating in public, public disturbance, animal-related offenses, loitering, selling tobacco to minors, etc.

## A.3 Individual linkages and recidivism

I use defendant characteristics to link individuals across cases and time. In particular, I leverage that each case provides the last name, first name, and date of birth for the defendant. This linkage can be further enhanced by matching based on city of residence, skin color, gender, address, etc. For the purposes of this study, individuals are matched based on first name, last name, and DOB to minimize false linkages. For two distinct individuals to be incorrectly linked, they would need to share both a full name and a date of birth, which is assumed to be unlikely. For 3.1% of cases, there is no DOB provided for the defendant. Of this group, 95.6% are cases initiated by court, while the remaining 4.4% are initiated by officers (the subset used in the primary analysis). Therefore, while this missingness has minimal effect on my analytical sample, I may incorrectly link individuals to past or future offenses if the linked case is brought forward from District Court, rather than by an officer.

The name and DOB combination is used to construct a unique individual identifier, which is then used to match defendants to prior and subsequent cases. For cases with missing DOB, I assign the case to an existing individual only when the corresponding first-name and last-name combination is associated with a single observed DOB in the data. For example, if all observed cases for a given first and last name share the same

DOB, I assign a missing-DOB case with that same name to the corresponding individual; if the same first and last name is associated with multiple observed DOBs, I leave the missing-DOB case unmatched. Additionally, 1.9% of cases are either duplicates or are associated with multiple filings arising from the same incident, which the individual tracking allows me to identify. The sample of first-time defendants contains individuals with no linked criminal history since January 1995, the earliest data available in CLUE. Each first-time defendant appears once as an observation in the analysis, but subsequent and prior cases for these individuals are retained in the broader dataset for the purpose of measuring recidivism. I calculate future criminal involvement by flagging if an individual has a subsequent case filed against them, whether initiated by an officer or by court. I choose within one year of the individual's first case filing, so that all individuals in the analytical sample have the same time horizon for re-offending. I show robustness to using alternative measures in Appendix Table B1.

## A.4 Citation-eligible calculation

I link each CJIS code to its 2012 charging language and identify whether the maximum penalty is 90 days or less in jail. This mapping then can be used to determine whether a case would have been eligible for a citation under the new policy in 2013. Specifically, I note whether the maximum punishment for a charge within a case is 90 days in jail. If all charges in a case satisfy this criterion, I classify the case as citation-eligible. Notably, the statute excluded the following crimes, even though the maximum punishment was 90 days in jail or less: failure to comply with a peace order; failure to comply with a protective order; violations of conditions of pretrial or posttrial release in cases involving sexual offenses against minors; possession of an electronic control device (i.e., a taser) after certain prior convictions; violations of out-of-state domestic violence orders; and animal abuse or neglect. If a case contains these crimes, it is no longer eligible for classification.

There are two charge types that are influential in this classification. First, the maximum punishment for a paraphernalia offense varies depending on past paraphernalia offenses. From the charging language, the first offense of paraphernalia has a maximum punishment of $500, while a subsequent offense has a maximum punishment of 2 years in jail. I address this by using the individual linkages described in Appendix A.3. Specifically, I note whether the paraphernalia charge brought against the defendant is the first in their criminal history. If a defendant has no prior paraphernalia charges in their case history, they are deemed citation-eligible as long as all other charges in the case are also citation-eligible. Among defendants charged with a paraphernalia offense, 72% are classified as having a first-time paraphernalia offense. The remaining defendants are classified as ineligible for citation.

Secondly, I am unable to link the severity of punishment for any charge with a ConvertedCode CJIS type. As mentioned previously, these charge types are highly correlated with criminal citations and most likely are municipal infractions. Therefore, I assume the maximum punishment for a ConvertedCode is 90 days or less in jail. Additionally, the mean number of charges per cited case is 1.11, with the 99th percentile of 3. I then set the citation-eligible indicator equal to zero for observations containing ConvertedCode charges and more than three total charges, effectively as a way of winsorizing any outliers of cases with ConvertedCode charge types.

## A.5 Expungement

The CLUE data may not capture the full history of case filings if prior records were expunged before scraping began. At the time CLUE was scraped, a case could be expunged if the disposition was not guilty, *nolle prosequi*, dismissed, released without charge, or acquittal. Cases with a disposition of probation before judgment (similar to diversion) or *stet* (inactive case) required a three-year waiting period before becoming eligible for expungement. Beginning in 2017, Maryland also allowed petitions to expunge certain misdemeanor guilty convictions, subject to additional restrictions; in particular, petitions generally required that at least 10 years had elapsed since all sentence conditions were satisfied, and new convictions during the waiting period could render prior convictions ineligible.

Missing expunged cases would bias the difference-in-discontinuities estimates if expungement generated differential sample selection at the January 1 cutoff in 2013 relative to the placebo cutoffs. This could occur through a discontinuous change in the number of observed cases, or through changes in the composition of observed cases if expungement is correlated with outcomes such as dismissal, citation eligibility, or subsequent case filings. As an initial check, Figure 1 tests for discontinuities in the density of case filings around January 1. I find no detectable change in case filings at the 2013 cutoff relative to the placebo cutoffs, suggesting that there is not a large missing mass of cases around the policy threshold.

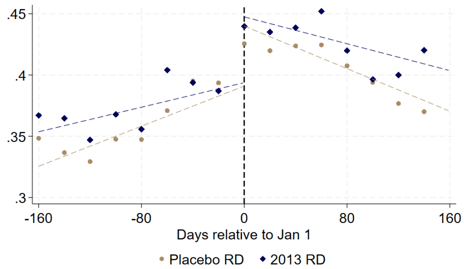
As an additional check, I match CLUE records to MD Case Explorer (MDCE), another dataset of web-scraped Maryland court records that began scraping in 2020. I construct the MDCE comparison sample by linking CLUE cases to MDCE cases using available case numbers, so the MDCE analysis is estimated on the subset of CLUE observations with a one-to-one matched MDCE record. The match rate is 80.5% for all citation-eligible defendants and 77.9% for citation-eligible first-time defendants. Because MDCE was scraped several years after CLUE, unmatched records may reflect expungement, differences in scraping coverage, or other data-cleaning differences. Furthermore, cases filed in Prince George's County account for 67.6% of CLUE cases not matched to MDCE. Appendix Table B8 provides the number of cases in CLUE and MDCE over time. Starting in 2003, coverage of Prince George's County is extremely limited in MDCE, with jumps in coverage in 2010 and 2014. With Prince George's County dropped, the match rate of MDCE cases in CLUE is 93.7% for all eligible defendants and 93.2% for first-time eligible defendants.

Further, I reproduce my main estimates using MDCE to assess whether the results are comparable. I exclude cases filed in Prince George's County in the analysis, due to poor coverage in Prince George's County and how the estimates in MDCE may reflect such level differences. Appendix Table B7 shows that the main estimates for a citation issuance, pretrial detention, failure-to-appear, and recidivism are similar when estimated using MDCE rather than CLUE. There is some attenuation for case dismissal, consistent with dismissed cases being eligible for expungement. Last, I use whether a case filing appears in MDCE as an outcome. There is a decrease with treatment, though neither estimate for all or first-time defendants is statistically significant. Paired with Figure 1, the results suggest that the primary results are not influenced by expungement.

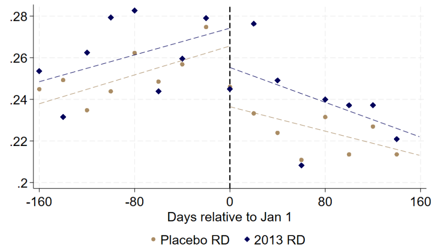
# Appendix B Figures and Tables

Figure B1: Covariates across all citation-eligible defendants

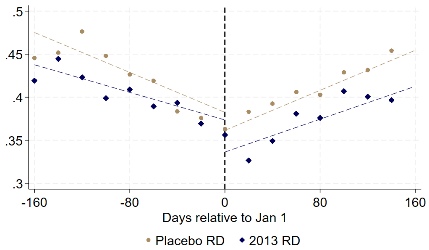
(a) Any drug crime



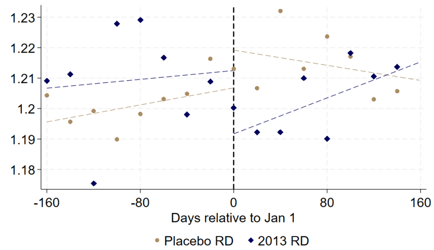

(b) Any property crime



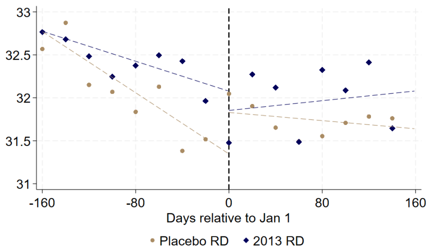

(c) Any other type crime



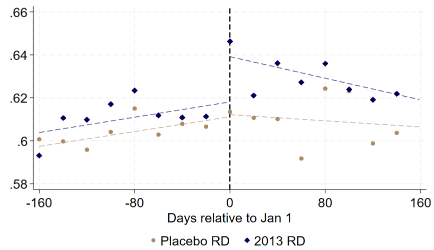

(d) Number of charges



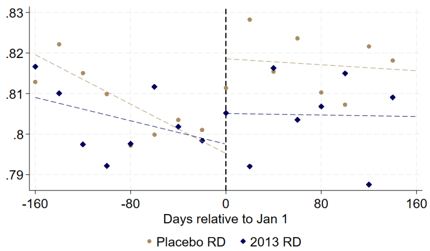

(e) Age




(f) Black



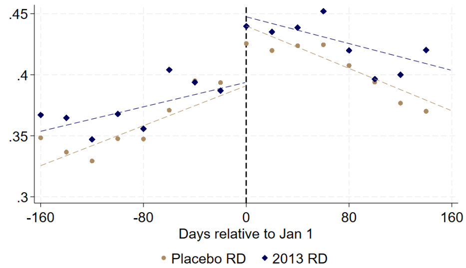

(g) Male



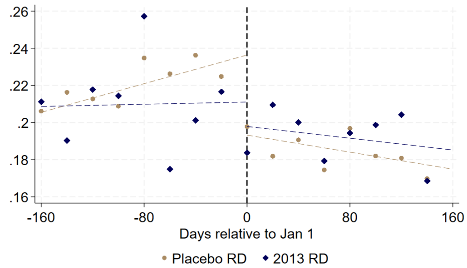

Notes: The unit of observation is a case filing from Maryland Judiciary Case Search records previously provided online via CLUE. Each subfigure plots the mean outcome in 20-day bins, with a linear fit on either side of the cutoff. Outcome means are estimated separately for the 2013 RD and the placebo RD (2011–2012).

Figure B2: Covariates across all citation-eligible first-time defendants

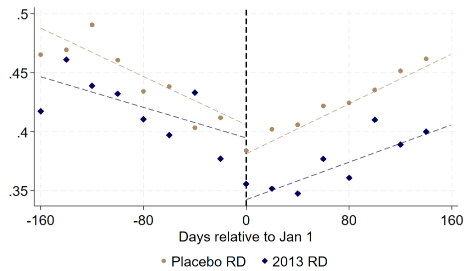
(a) Any drug crime



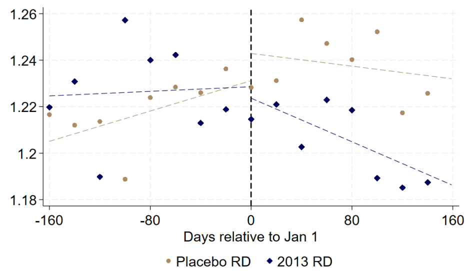

(b) Any property crime



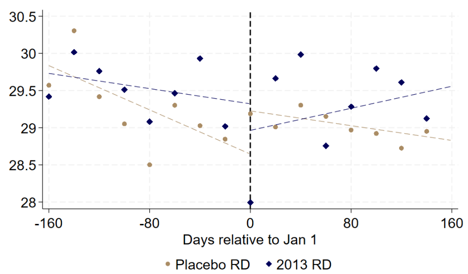

(c) Any other type crime



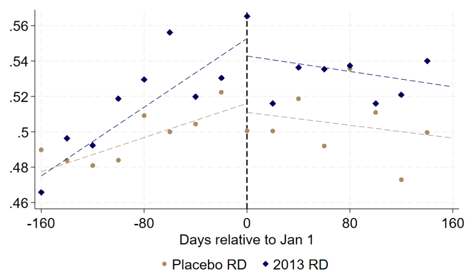

(d) Number of charges



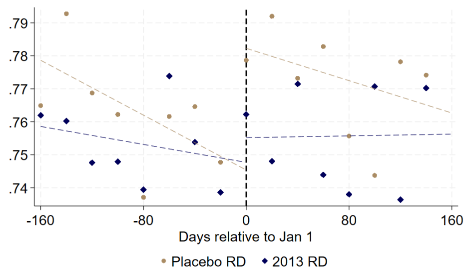

(e) Age




(f) Black

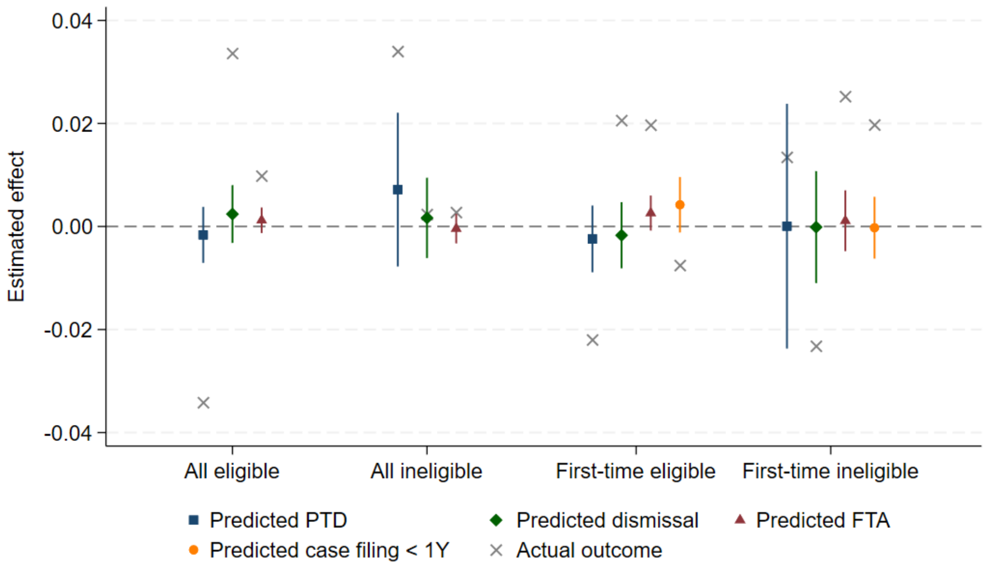



(g) Male




Notes: The unit of observation is a case filing from Maryland Judiciary Case Search records previously provided online via CLUE. Each subfigure plots the mean outcome in 20-day bins, with a linear fit on either side of the cutoff. Outcome means are estimated separately for the 2013 RD and the placebo RD (2011–2012).

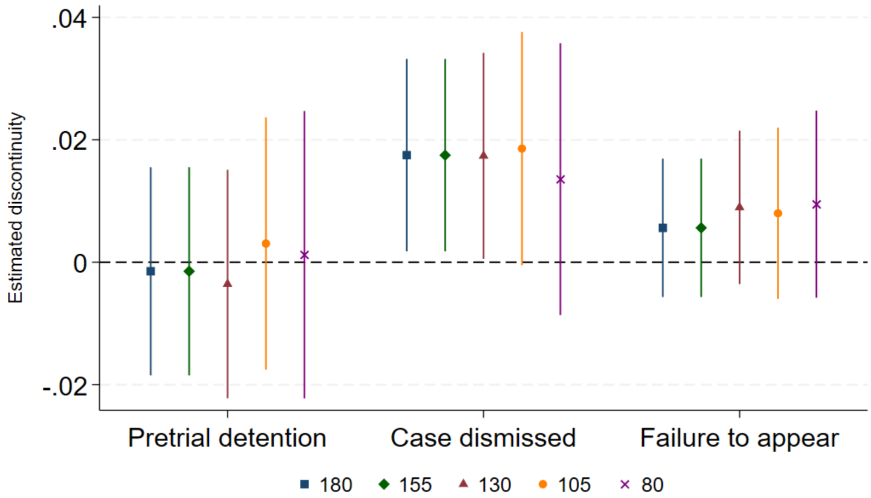
Figure B3: Predicted value sensitivity across samples of defendants



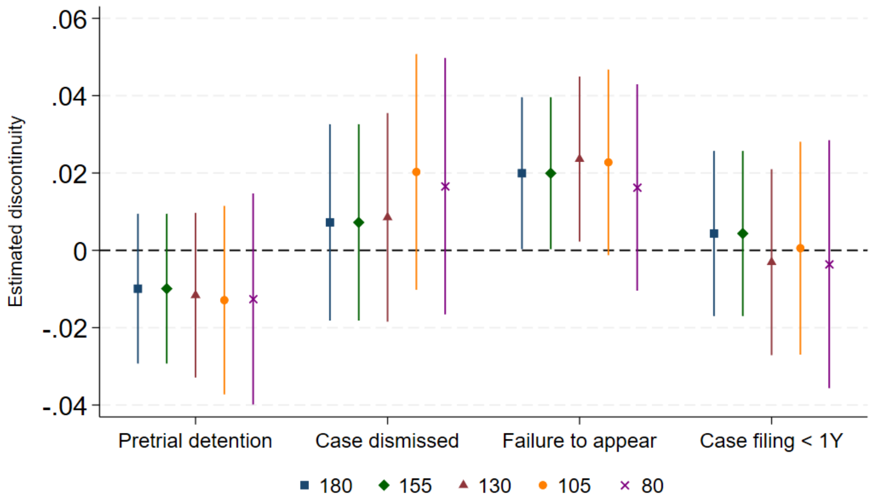

Notes: The unit of observation is a case filing from Maryland Judiciary Case Search records previously provided online via CLUE. Each point reports the difference-in-discontinuities estimate from Equation (2). Each estimate includes court fixed effects. Vertical lines denote 95% confidence intervals, with standard errors clustered by the standardized running variable (days relative to January 1). "All" denotes the sample of all defendants, while "First-time" denotes first-time defendants. "Eligible" denotes whether the defendant is charged with only citation-eligible offenses, and "ineligible" denotes if the defendant is not charged as eligible for citation. "Actual outcome" denotes the estimate when using Equation (2) without covariates.

Figure B4: Bandwidth sensitivity across the full sample of defendants

(a) All defendants



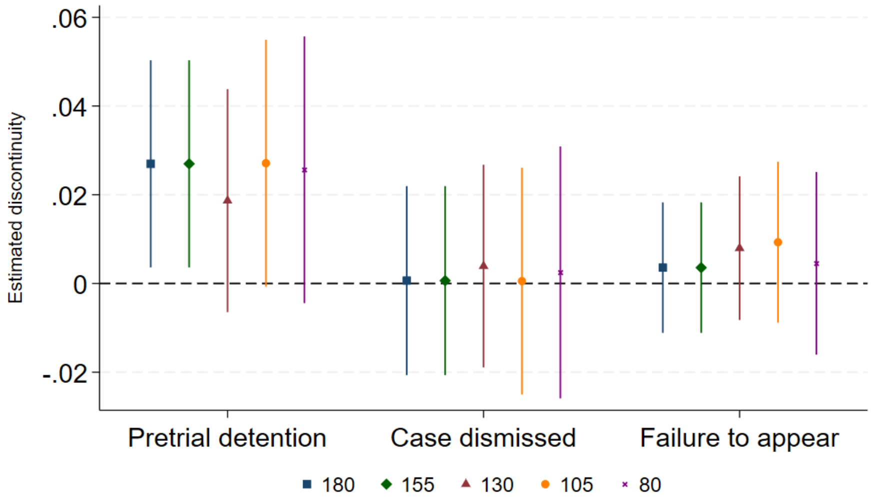

(b) First-time defendants



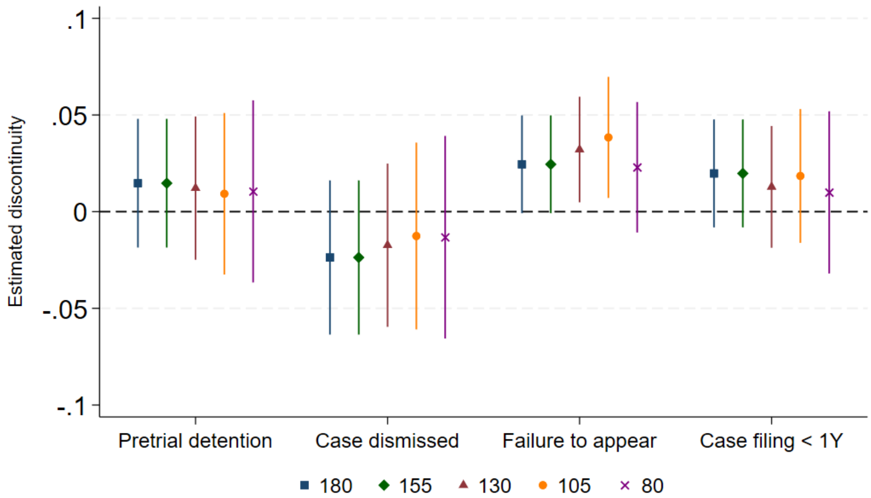

Notes: The unit of observation is a case filing from Maryland Judiciary Case Search records previously provided online via CLUE. Each point reports the difference-in-discontinuities estimate from Equation (2) under the indicated bandwidth (in days) around January 1. Each estimate includes court fixed effects and covariates. Vertical lines denote 95% confidence intervals, with standard errors clustered by the standardized running variable (days relative to January 1). The baseline specification uses a 155-day bandwidth.

Figure B5: Bandwidth sensitivity across samples of ineligible defendants

(a) All defendants



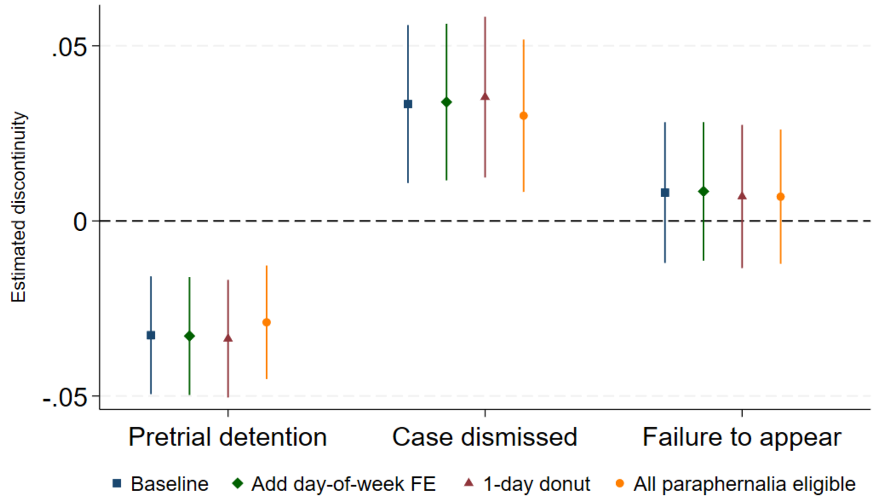

(b) First-time defendants



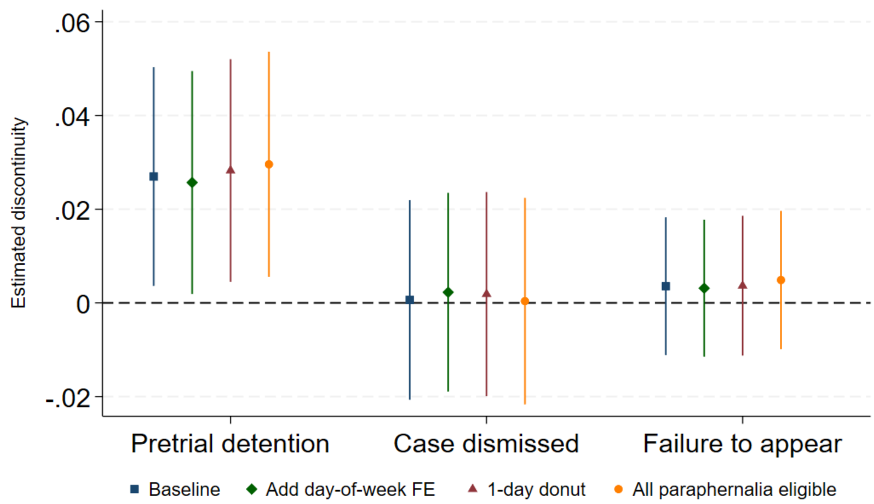

Notes: The unit of observation is a case filing from Maryland Judiciary Case Search records previously provided online via CLUE. Each point reports the difference-in-discontinuities estimate from Equation (2) under the indicated bandwidth (in days) around January 1. Each estimate includes court fixed effects and covariates. Vertical lines denote 95% confidence intervals, with standard errors clustered by the standardized running variable (days relative to January 1). The baseline specification uses a 155-day bandwidth.

Figure B6: Alternative specifications for all defendants

(a) Citation-eligible



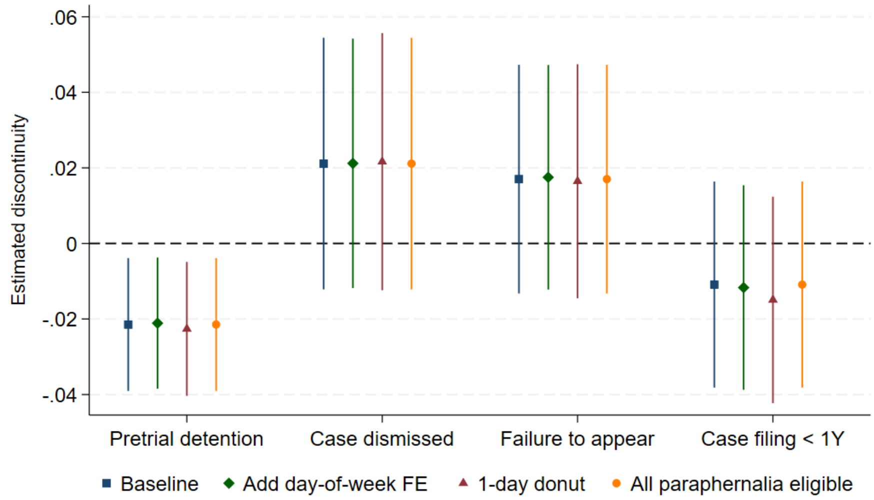

(b) Citation-ineligible



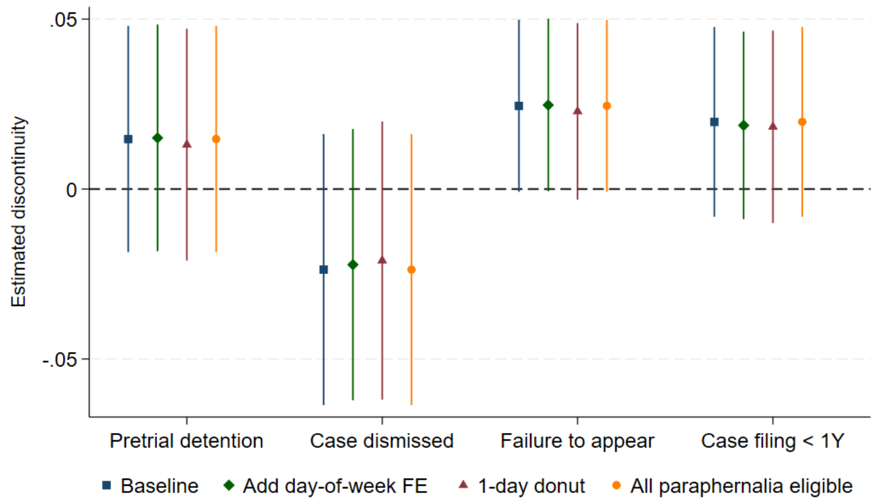

Notes: The unit of observation is a case filing from Maryland Judiciary Case Search records previously provided online via CLUE. Each point reports the difference-in-discontinuities estimate from Equation (2). Each estimate includes court fixed effects and covariates. Vertical lines denote 95% confidence intervals, with standard errors clustered by the standardized running variable (days relative to January 1). "Add day-of-week FE" includes day-of-week fixed effects. The 1-day donut removes observations filed at values of $Days_{ict}$ = -1 or 0. "All paraphernalia eligible" allows all paraphernalia charges to be classified as citation-eligible.

Figure B7: Alternative specifications for first-time defendants

(a) Citation-eligible



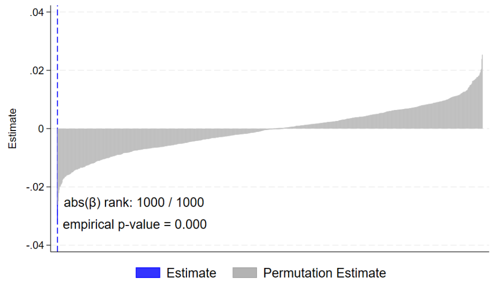

(b) Citation-ineligible



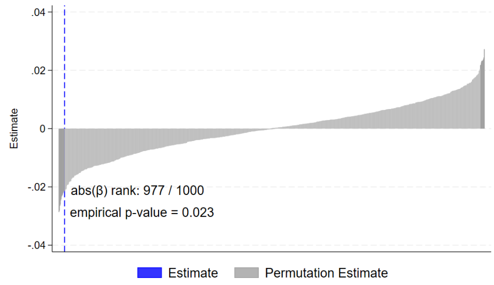

Notes: The unit of observation is a case filing from Maryland Judiciary Case Search records previously provided online via CLUE. Each point reports the difference-in-discontinuities estimate from Equation (2). Each estimate includes court fixed effects and covariates. Vertical lines denote 95% confidence intervals, with standard errors clustered by the standardized running variable (days relative to January 1). "Add day-of-week FE" includes day-of-week fixed effects. The 1-day donut removes observations filed at values of $Days_{ict}$ = -1 or 0. "All paraphernalia eligible" allows all paraphernalia charges to be classified as citation-eligible.

Figure B8: Randomized treatment status for citation-eligible defendants

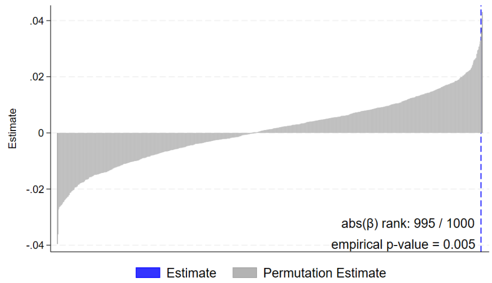
(a) PTD, all defendants



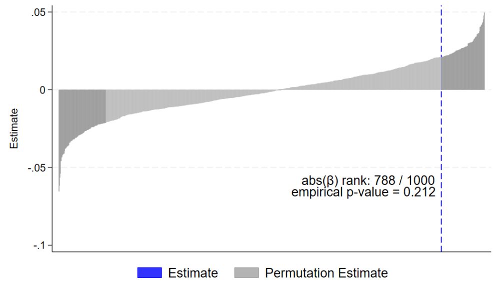

(b) PTD, first-time defendants



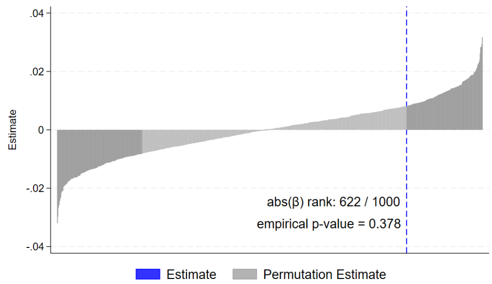

(c) Dismissed, all defendants



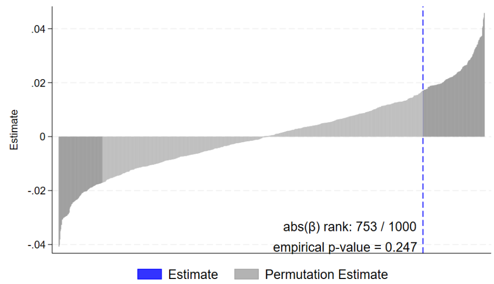

(d) Dismissed, first-time defendants




(e) FTA, all defendants




(f) FTA, first-time defendants




(g) Case filing < 1Y, first-time defendants


Notes: The unit of observation is a case filing from Maryland Judiciary Case Search records previously provided online via CLUE. Permutation estimates are calculated by randomly assigning whether or not an individual belongs to the 2013 RD or the placebo RD. The blue dashed line denotes the estimated treatment effect from January 1, 2013. Darker shaded estimates reflect permutation estimates greater in absolute value than the reported estimate.

Table B1: Alternative recidivism measures for citation-eligible first-time defendants

| | (1) | (2) | Mean |
|---|---|---|---|
| Case filing < 3M | 0.010 | 0.008 | 0.053 |
| | (0.010) | (0.010) | |
| | {0.347} | {0.380} | |
| Case filing < 6M | 0.006 | 0.004 | 0.082 |
| | (0.012) | (0.012) | |
| | {0.722} | {0.806} | |
| Case filing < 1Y | -0.008 | -0.011 | 0.128 |
| | (0.014) | (0.014) | |
| | {0.611} | {0.410} | |
| Case filing < 2Y | -0.001 | -0.006 | 0.176 |
| | (0.017) | (0.016) | |
| | {0.936} | {0.643} | |
| N | 48,466 | 48,466 | |
| Covariates | N | Y | |

Notes: The unit of observation is a case filing from Maryland Judiciary Case Search records previously provided online via CLUE. Each coefficient reports a difference-in-discontinuities estimate. Standard errors clustered by the standardized running variable are in parentheses. Wild bootstrap p-values clustered by District Court are in braces. Statistical significance is denoted as * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B2: Difference-in-discontinuity estimates for citation-eligible defendants, excluding Prince George's County

| | All defendants | | | | First-time defendants | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | Mean | (5) | (6) | (7) | Mean |
| Citation | 0.267*** | 0.287*** | 0.276*** | 0.463 | 0.245*** | 0.262*** | 0.262*** | 0.512 |
| | (0.020) | (0.019) | (0.016) | | (0.027) | (0.025) | (0.023) | |
| | {0.000} | {0.000} | {0.000} | | {0.002} | {0.002} | {0.001} | |
| PTD | -0.031*** | -0.035*** | -0.032*** | 0.099 | -0.022* | -0.026** | -0.024** | 0.060 |
| | (0.010) | (0.010) | (0.010) | | (0.012) | (0.012) | (0.011) | |
| | {0.057} | {0.033} | {0.028} | | {0.104} | {0.046} | {0.079} | |
| Dismissed | 0.046*** | 0.025* | 0.022 | 0.564 | 0.049** | 0.022 | 0.021 | 0.591 |
| | (0.016) | (0.015) | (0.015) | | (0.023) | (0.022) | (0.022) | |
| | {0.005} | {0.114} | {0.058} | | {0.150} | {0.697} | {0.771} | |
| FTA | 0.005 | 0.014 | 0.012 | 0.177 | 0.019 | 0.027 | 0.026 | 0.178 |
| | (0.012) | (0.012) | (0.012) | | (0.018) | (0.018) | (0.018) | |
| | {0.694} | {0.211} | {0.256} | | {0.207} | {0.074} | {0.089} | |
| Case filing $<$ 1Y | — | — | — | — | 0.002 | 0.000 | -0.004 | 0.139 |
| | — | — | — | | (0.017) | (0.017) | (0.016) | |
| | — | — | — | | {0.950} | {0.994} | {0.803} | |
| N | 80,897 | 80,897 | 80,897 | | 33,999 | 33,999 | 33,999 | |
| Court FE | N | Y | Y | | N | Y | Y | |
| Covariates | N | N | Y | | N | N | Y | |

Notes: The unit of observation is a case filing. The underlying data are drawn from the Maryland Judiciary Case Search and were previously provided online via the Client Legal Utility Engine (CLUE). Each coefficient reports a difference-in-discontinuities estimate. Standard errors clustered by the standardized running variable are in parentheses. Wild bootstrap p-values clustered by District Court are in braces. Statistical significance is denoted as * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B3: Difference-in-discontinuity estimates for citation-ineligible defendants, excluding Prince George's County

| | All defendants | | | | First-time defendants | | | |
|---|---|---|---|---|---|---|---|---|
| Outcome | (1) | (2) | (3) | Mean | (5) | (6) | (7) | Mean |
| Citation | -0.012* | -0.005 | -0.003 | 0.061 | -0.048*** | -0.043*** | -0.045*** | 0.078 |
| | (0.007) | (0.006) | (0.006) | | (0.014) | (0.014) | (0.012) | |
| | {0.443} | {0.713} | {0.760} | | {0.211} | {0.220} | {0.138} | |
| PTD | 0.043*** | 0.041*** | 0.032** | 0.517 | 0.028 | 0.032 | 0.027 | 0.454 |
| | (0.015) | (0.015) | (0.013) | | (0.021) | (0.021) | (0.020) | |
| | {0.017} | {0.028} | {0.021} | | {0.394} | {0.375} | {0.291} | |
| Dismissed | 0.002 | -0.009 | -0.011 | 0.397 | -0.046* | -0.052** | -0.051** | 0.438 |
| | (0.013) | (0.013) | (0.012) | | (0.026) | (0.024) | (0.023) | |
| | {0.932} | {0.620} | {0.432} | | {0.117} | {0.063} | {0.071} | |
| FTA | -0.005 | -0.000 | 0.001 | 0.126 | 0.013 | 0.014 | 0.014 | 0.124 |
| | (0.008) | (0.008) | (0.008) | | (0.015) | (0.015) | (0.015) | |
| | {0.568} | {0.960} | {0.860} | | {0.342} | {0.252} | {0.235} | |
| Case filing < 1Y | — | — | — | — | 0.018 | 0.018 | 0.020 | 0.158 |
| | — | — | — | | (0.017) | (0.017) | (0.017) | |
| | — | — | — | | {0.541} | {0.512} | {0.453} | |
| N | 131,680 | 131,680 | 131,680 | | 36,423 | 36,423 | 36,423 | |
| Court FE | N | Y | Y | | N | Y | Y | |
| Covariates | N | N | Y | | N | N | Y | |

Notes: The unit of observation is a case filing. The underlying data are drawn from the Maryland Judiciary Case Search and were previously provided online via the Client Legal Utility Engine (CLUE). Each coefficient reports a difference-in-discontinuities estimate. Standard errors clustered by the standardized running variable are in parentheses. Wild bootstrap p-values clustered by District Court are in braces. Statistical significance is denoted as * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B4: Difference-in-discontinuity estimates for the full sample of defendants

| | All defendants | | | | First-time defendants | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | Mean | (5) | (6) | (7) | Mean |
| Citation | 0.099*** | 0.111*** | 0.109*** | 0.250 | 0.110*** | 0.122*** | 0.117*** | 0.333 |
| | (0.011) | (0.011) | (0.009) | | (0.020) | (0.020) | (0.016) | |
| | {0.038} | {0.018} | {0.016} | | {0.110} | {0.072} | {0.030} | |
| PTD | 0.005 | -0.001 | -0.001 | 0.340 | -0.010 | -0.015 | -0.010 | 0.248 |
| | (0.010) | (0.010) | (0.009) | | (0.014) | (0.014) | (0.010) | |
| | {0.750} | {0.934} | {0.850} | | {0.640} | {0.460} | {0.300} | |
| Dismissed | 0.028*** | 0.020** | 0.017** | 0.519 | 0.018 | 0.009 | 0.007 | 0.583 |
| | (0.009) | (0.009) | (0.008) | | (0.014) | (0.014) | (0.013) | |
| | {0.070} | {0.282} | {0.140} | | {0.388} | {0.730} | {0.682} | |
| FTA | 0.001 | 0.006 | 0.006 | 0.147 | 0.019* | 0.022** | 0.020** | 0.146 |
| | (0.006) | (0.006) | (0.006) | | (0.010) | (0.010) | (0.010) | |
| | {0.866} | {0.288} | {0.316} | | {0.144} | {0.050} | {0.072} | |
| Case filing < 1Y | — | — | — | — | 0.006 | 0.006 | 0.004 | 0.138 |
| | — | — | — | | (0.011) | (0.011) | (0.011) | |
| | — | — | — | | {0.688} | {0.688} | {0.766} | |
| N (All courts) | 265,209 | 265,209 | 265,209 | | 94,662 | 94,662 | 94,662 | |
| Court FE | N | Y | Y | | N | Y | Y | |
| Covariates | N | N | Y | | N | N | Y | |

Notes: The unit of observation is a case filing from Maryland Judiciary Case Search records previously provided online via CLUE. Each coefficient reports a difference-in-discontinuities estimate. Standard errors clustered by the standardized running variable are in parentheses. Wild bootstrap p-values clustered by District Court are in braces. Statistical significance is denoted as * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Outcome means are computed for observations in the 2013 RD with a running variable value less than 0. Covariates include columns 1-8 in Table 1. In the first-time defendant sample, an individual appears only once at their first recorded case.

Table B5: Difference-in-discontinuity results by kernel for the full sample of defendants

| | All defendants | | | First-time defendants | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Citation | 0.109*** | 0.105*** | 0.106*** | 0.117*** | 0.124*** | 0.123*** |
| | (0.009) | (0.010) | (0.010) | (0.016) | (0.019) | (0.018) |
| | {0.016} | {0.016} | {0.018} | {0.030} | {0.016} | {0.020} |
| PTD | -0.001 | -0.000 | 0.000 | -0.010 | -0.012 | -0.011 |
| | (0.009) | (0.009) | (0.009) | (0.010) | (0.010) | (0.010) |
| | {0.850} | {0.972} | {0.998} | {0.300} | {0.362} | {0.304} |
| Dismissed | 0.017** | 0.015* | 0.017** | 0.007 | 0.011 | 0.011 |
| | (0.008) | (0.008) | (0.008) | (0.013) | (0.013) | (0.013) |
| | {0.140} | {0.166} | {0.154} | {0.682} | {0.422} | {0.490} |
| FTA | 0.006 | 0.007 | 0.007 | 0.020** | 0.021* | 0.021** |
| | (0.006) | (0.006) | (0.006) | (0.010) | (0.011) | (0.010) |
| | {0.316} | {0.248} | {0.268} | {0.072} | {0.092} | {0.090} |
| Case filing $<$ 1Y | — | — | — | 0.004 | -0.001 | -0.001 |
| | — | — | — | (0.011) | (0.013) | (0.012) |
| | — | — | — | {0.766} | {0.914} | {0.940} |
| N | 265,209 | 265,209 | 265,209 | 94,662 | 94,662 | 94,662 |
| Weighting method | U | T | E | U | T | E |

Notes: The unit of observation is a case filing from Maryland Judiciary Case Search records previously provided online via CLUE. Each coefficient reports a difference-in-discontinuities estimate, including court fixed effects and covariates. Standard errors clustered by the standardized running variable are in parentheses. Wild bootstrap p-values clustered by District Court are in braces. Statistical significance is denoted as * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Weight symbol "U" denotes uniform, "T" denotes triangular, and "E" denotes Epanechnikov.

Table B6: Difference-in-discontinuity results by kernel for citation-ineligible defendants

| | All defendants | | | First-time defendants | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Citation | 0.005 | 0.011* | 0.010 | -0.019 | -0.004 | -0.007 |
| | (0.006) | (0.007) | (0.006) | (0.012) | (0.013) | (0.013) |
| | {0.682} | {0.382} | {0.436} | {0.542} | {0.910} | {0.834} |
| PTD | 0.027** | 0.024* | 0.025** | 0.015 | 0.011 | 0.012 |
| | (0.012) | (0.012) | (0.012) | (0.017) | (0.018) | (0.017) |
| | {0.012} | {0.042} | {0.036} | {0.490} | {0.628} | {0.556} |
| Dismissed | 0.001 | 0.001 | 0.002 | -0.024 | -0.019 | -0.018 |
| | (0.011) | (0.011) | (0.011) | (0.020) | (0.021) | (0.021) |
| | {0.958} | {0.978} | {0.920} | {0.450} | {0.608} | {0.628} |
| FTA | 0.004 | 0.005 | 0.005 | 0.024* | 0.029** | 0.029** |
| | (0.007) | (0.008) | (0.008) | (0.013) | (0.013) | (0.013) |
| | {0.620} | {0.552} | {0.476} | {0.146} | {0.088} | {0.080} |
| Case filing $<$ 1Y | — | — | — | 0.020 | 0.013 | 0.013 |
| | — | — | — | (0.014) | (0.016) | (0.015) |
| | — | — | — | {0.334} | {0.564} | {0.526} |
| N | 156,078 | 156,078 | 156,078 | 46,196 | 46,196 | 46,196 |
| Weighting method | U | T | E | U | T | E |

Notes: The unit of observation is a case filing from Maryland Judiciary Case Search records previously provided online via CLUE. Each coefficient reports a difference-in-discontinuities estimate, including court fixed effects and covariates. Standard errors clustered by the standardized running variable are in parentheses. Wild bootstrap p-values clustered by District Court are in braces. Statistical significance is denoted as * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Weight symbol "U" denotes uniform, "T" denotes triangular, and "E" denotes Epanechnikov.

Table B7: Results for citation-eligible defendants across various data sources

| | All defendants | | First-time defendants | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Citation | 0.276*** | 0.277*** | 0.262*** | 0.262*** |
| | (0.016) | (0.017) | (0.023) | (0.024) |
| | {0.000} | {0.000} | {0.001} | {0.002} |
| PTD | -0.032*** | -0.035*** | -0.024** | -0.024** |
| | (0.010) | (0.010) | (0.011) | (0.012) |
| | {0.028} | {0.019} | {0.079} | {0.081} |
| Dismissed | 0.022 | 0.016 | 0.021 | 0.012 |
| | (0.015) | (0.015) | (0.022) | (0.023) |
| | {0.058} | {0.165} | {0.771} | {0.802} |
| FTA | 0.012 | 0.015 | 0.026 | 0.023 |
| | (0.012) | (0.012) | (0.018) | (0.019) |
| | {0.256} | {0.178} | {0.089} | {0.161} |
| Case filing < 1Y | — | — | -0.004 | -0.004 |
| | — | — | (0.016) | (0.018) |
| | — | — | {0.803} | {0.848} |
| MDCE | -0.012 | — | -0.024* | — |
| | (0.008) | — | (0.013) | — |
| | {0.105} | — | {0.087} | — |
| CLUE | Y | N | Y | N |
| MDCE | N | Y | N | Y |
| N | 84,987 | 79,629 | 33,699 | 31,401 |

Notes: The unit of observation is a case filing. The underlying data are drawn from the Maryland Judiciary Case Search and were previously provided online via the Client Legal Utility Engine (CLUE) and MD Case Explorer (MDCE). Each coefficient reports a difference-in-discontinuities estimate. Standard errors clustered by the standardized running variable are in parentheses. Wild bootstrap p-values clustered by District Court are in braces. Statistical significance is denoted as * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B8: Differences in coverage of Prince George's County across various data sources

| Year | CLUE | MDCE | Match % |
|---|---|---|---|
| 1995 | 22,491 | 22,202 | 98.72% |
| 1996 | 22,247 | 21,971 | 98.76% |
| 1997 | 23,077 | 22,786 | 98.74% |
| 1998 | 24,297 | 23,961 | 98.62% |
| 1999 | 23,953 | 23,607 | 98.56% |
| 2000 | 20,739 | 20,432 | 98.52% |
| 2001 | 19,187 | 18,880 | 98.40% |
| 2002 | 18,950 | 13,517 | 71.33% |
| 2003 | 17,744 | 6 | 0.03% |
| 2004 | 18,113 | 10 | 0.06% |
| 2005 | 20,189 | 11 | 0.05% |
| 2006 | 21,017 | 2 | 0.01% |
| 2007 | 24,897 | 11 | 0.04% |
| 2008 | 25,515 | 16 | 0.06% |
| 2009 | 24,493 | 16 | 0.07% |
| 2010 | 26,731 | 12,326 | 46.11% |
| 2011 | 28,576 | 12,454 | 43.58% |
| 2012 | 28,687 | 12,122 | 42.26% |
| 2013 | 28,881 | 14,164 | 49.04% |
| 2014 | 25,848 | 22,409 | 86.70% |
| 2015 | 21,002 | 17,998 | 85.70% |
| 2016 | 18,970 | 17,748 | 93.56% |

Notes: The unit of observation is a case filing. The underlying data are drawn from the Maryland Judiciary Case Search and were previously provided online via the Client Legal Utility Engine (CLUE) and MD Case Explorer (MDCE). The match percentage reflects the percentage of MDCE cases that appear in CLUE.