Politically Charged: District Attorney Partisanship, Prosecution Rates, and Recidivism*

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Abstract

Elected district attorneys (DAs) have wide discretion to raise or lower criminal prosecution rates, yet the extent to which DAs' politics shape their decision-making remains unclear. In this paper, we evaluate the causal impact of DA partisan affiliation on prosecution rates, sentencing outcomes, and recidivism. Using quasi-random variation in DA partisanship stemming from close elections, we find that the marginal Democratic DA is 36 percent less likely to prosecute criminal cases than her Republican counterpart and imposes 13 percent shorter incarceration sentences. However, defendants in jurisdictions with Democratic DAs are more 5 percent more likely to re-appear in the court system. We find similar patterns using an alternative matching specification, suggesting our results capture the average effect of prosecutor partisanship. Our findings underscore the extent to which the punitiveness of the court system depends on the partisanship of local district attorneys.

*The authors declare that they have no relevant or material financial interests that relate to the research described in this paper. Any mistakes are ours.

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1 Introduction

Each year, over 17 million criminal cases enter the United States court system—at least one case for every 15 American adults (Court Statistics Project 2018). According to legal scholars, the sheer volume of cases empowers local district attorneys (DAs) with near-total discretion to choose how many cases to actually prosecute, at the expense of scarce judicial resources, and how many to dismiss without prosecution, at a potential social cost if non-prosecution spurs future crime (Alschuler 1968; Bibas 2004; Bowers 2010; Stith 2008). The decision to prosecute carries significant consequences for defendants, who face economic fallout from incarceration and accruing a criminal record (Agan and Starr 2018; Dobbie, Goldin, and Yang 2018; Kling 2006; Mueller-Smith 2016).

But a DA's choice of prosecution rate also has political ramifications. Prosecutors must answer to voters who, polls suggest, consistently see high crime rates as a major problem (Gramlich 2016; McCarthy 2020).[1] A median voter framework suggests that all DAs, regardless of their own political identities, might pursue high prosecution rates that appeal to the median, crime-averse voter. Conversely, as "citizen-candidates" (Besley and Coate 1997), DAs might incorporate their own partisan perceptions of crime rates and deterrence, topics on which Democrats' and Republicans' views diverge (Gramlich 2021; McCarthy 2020; Yokley 2021). Surprisingly, given this theoretical ambiguity and DAs' central role in the criminal justice system, there exists no rigorous evidence showing how elected prosecutors' political identities affect prosecution rates.

This paper examines the causal impact of DA partisan affiliation on prosecution rates, sentencing outcomes, and recidivism. Our goal is to identify differences in how Democratic and Republican DAs exercise prosecutorial discretion, and ultimately assess how those differences affect the efficacy of their local criminal justice systems. Prior work suggests that DA partisanship may influence defendant outcomes: Arora (2019) and Krumholz (2020) show

1. By "district attorney," we mean, broadly, the chief prosecuting authority in a given jurisdiction; some states refer to these officials by other names, such as "state's attorney" or "commonwealth attorney." The majority of district attorneys nationwide are elected in partisan contests, although some states elect public prosecutors through nonpartisan ballots. Our study focuses only on states with partisan DA elections.

that electing Democratic DAs leads to fewer prison admissions. However, these studies lack the comprehensive arrest-to-sentencing data necessary to isolate effects on prosecution rates and recidivism. And while recent anecdotal evidence suggests that "progressive prosecutors" reduce prosecution rates (following campaign promises to do so), these examples capture only a subset of Democratic DAs drawn mostly from urban jurisdictions where Democratic voters hold large majorities.[2] Both of these confounding factors could independently influence prosecution rates. Thus, it remains an open question whether DA partisanship matters, both at the margin and on average.

Our study bridges these empirical gaps. We deploy a multi-state dataset that contains over two and a half million individual criminal case records spanning 2000-2018, alongside an identification approach that relies on close DA elections to highlight the causal effect of prosecutor partisanship on judicial outcomes.[3] Using a regression discontinuity framework (RD), we provide evidence that DA partisanship among these narrowly-decided elections is uncorrelated with election-year case characteristics, which attests to the validity of our research design. Furthermore, our detailed data, which track defendants from arrest to sentencing, allow us to explore the internal and external validity our close-election RD approach using a matching design. That is, we compare outcomes for similar defendants facing identical charges who are prosecuted by DAs from opposing parties, irrespective of their election margins. Reassuringly, our RD and matching strategies yield very similar estimates on prosecution rates.

Specifically, our RD estimates show that, relative to Republican DAs, the marginal Democratic DA is 36 percent (10 percentage points) *less* likely to prosecute incoming criminal cases. Echoing prior research, we also find that Democratic DAs impose 13 percent shorter

2. Agan, Doleac, and Harvey (2021b) inventory many of these reform-minded prosecutors.

3. Specifically, we use data from Arkansas, Colorado, Kentucky, North Carolina, Texas, and Virginia, states for which we could obtain comprehensive criminal justice data and that hold partisan DA elections. While these states are almost all in the South, they include a range of urban and rural counties that in many ways resemble the country more broadly, and so we believe this setting is meaningful for studying the impact of DAs on criminal justice outcomes. We detail our sample of court systems and elections in Section 2.

incarceration sentences (about 4 fewer months in jail or prison).[4] RD estimates point to larger effects among women and relatively young defendants, but no meaningful differences across case types.[5] As noted above, we find quantitatively similar effects using our matching estimator. We interpret these results as evidence that Democratic DAs pursue lower prosecution rates and impose less punitive sentences across a range of defendants.

We then explore what these relatively lenient prosecution policies mean for public safety. Recent evidence drawn from specific jurisdictions suggests that lower prosecution rates might lead to lower re-offense rates: Agan, Doleac, and Harvey (2021a) show that dismissing misdemeanor charges reduces the probability of re-arrest, while Augustine et al. (2022) and Mueller-Smith and Schnepel (2019) find that diverting felony defendants diminishes the likelihood that they face future criminal charges.[6] To be clear, in our context, we cannot isolate the causal effect of non-prosecution per se, since DAs have numerous policy levers at their disposal, including the ability to preclude arrests by publicly declining to prosecute certain charges. Still, we can describe Democratic DAs' reduced-form impact on recidivism, which provides a heuristic to assess the criminogeneic impact of their prosecution policies.

Using our RD model, we find that the marginal Democratic DA increases re-offense rates, measured within 1 or 2 years of initial arrest. Still, these effects are small relative to the estimated increase in non-prosecution rates, amounting to a 1.9 percentage-point (5.9 percent) increase in the probability that a defendant faces new charges within two years. Our matching estimates also indicate that the average Democratic DA's election raises two-year re-offense probability by 1.1 percentage points (3.4 percent). While purely reduced-form, these findings suggest that as prosecution and incarceration rates fall, recidivism rates rise,

4. Our point estimate on sentencing lies between those of Arora (2019)—who finds that Democratic DAs impose roughly 55 percent shorter incarceration sentences—and Krumholz (2020), who finds that Democratic DAs reduce total sentenced months in a jurisdiction by only 6 percent.

5. We also find no significant differences in the partisan DA effect across white and nonwhite defendants. However, as we caution in Section 3, we are skeptical of our data on defendant race/ethnicity because it has a high degree of missingness and variability in reporting across jurisdictions. Consequently, we do not view the lack of heterogeneity across white and nonwhite defendants as particularly instructive.

6. As we discuss in Section 2, we consider diversion—typically probation, followed by charge dismissal—to be a form of non-prosecution.

albeit at a much slower rate. We stress that we do not see this result as being in conflict with the recent literature: in our setting, we cannot exclude all the potential causal channels linking the election of Democratic DAs to defendant re-offense rates.[7] Therefore, we do not argue that any particular policy change we observe—lower prosecution and incarceration rates, nor reduced sentences—causes an uptick in recidivism. Rather, we believe our results provide valuable context to understanding the substantial heterogeneity in criminal justice outcomes across the country, and a further example of the pivotal role played by district attorneys in the court system.

Overall, our work sheds new light on how partisan politics drives the implementation and efficacy of criminal justice at the local level. This analysis speaks to two strands of the literature. First, by providing rigorous evidence that district attorney partisanship influences prosecution rates, we contribute to a growing body of research that emphasizes the degree to which defendant outcomes depend on idiosyncratic courtroom actors, including judges (Cohen and Yang 2019), defense attorneys (Agan, Freedman, and Owens 2021; Shem-Tov 2022), and assistant prosecutors (Sloan 2020; Tuttle 2019). Second, we provide a new perspective on the long-standing question of how "tough-on-crime" policy agendas shape criminal justice outcomes. While inherently holistic, our findings point to a potential trade-off between lenient prosecution policies and defendant recidivism, a relationship that, though not directly attributable to prosecutorial discretion, provides added context to the ongoing debate surrounding criminal justice reform and the role DAs might play in making the court system both more efficacious. Altogether, our findings underscore the extent to which the punitiveness of the court system depends on the partisanship of local district attorneys.

7. To the extent that the uptick in recidivism may be driven by Democratic DAs' lower prosecution rates, we note that our setting differs substantially from the mostly coastal metropolitan areas studied in the dedicated literature. The counties represented in our RD sample in particular include several large urban areas, but also smaller cities, and all are predominately in the South. While we argue our sample provides a meaningful snapshot of local criminal justice systems—and thus the impact of the average DA—it could be that we would find smaller or negative effects on recidivism if we focused on large urban areas. Data availability and power considerations prevent us from exploring this point empirically.

2 Background and Data

Our study examines the causal link between DA partisan affiliation and prosecutorial discretion. To deliver results that can be plausibly attributed to DA partisanship, and to arrive at the most widely-applicable findings, we compile a multi-state dataset that links DA election returns with detailed criminal justice records. We face two substantial constraints on data availability. First, most states do not provide the comprehensive criminal justice records necessary to conduct an analysis focused on pre-sentencing outcomes. And, among the states with appropriate outcome data, some either do not have partisan DA elections, or else have too-few competitive contests to be of use. Thus, our final dataset includes six states with suitable data—Arkansas, Colorado, Kentucky, North Carolina, Texas, and Virginia. Together, these states comprise NN percent of the U.S. population and include approximately NN percent of its district attorneys.[8]

2.1 Background: District Attorneys Role in the Criminal Process

Voters in these six states elect district attorneys to lead public prosecutors' offices and represent the state in cases against defendants in felony and most misdemeanor cases. Each district attorney serves as the chief law enforcement officer in their judicial district, usually a single county, but sometimes a grouping of small counties.[9] Many jurisdictions staff public prosecutors' offices with assistant district attorneys (ADAs), who often function as the actual prosecuting attorneys on cases. Even in these settings, though, the district attorney remains in charge of overall prosecution policy and directs any ADAs. In our sampled states, DAs are partisan officials elected to office every four years.

District attorneys' principle responsibility is to decide whether and how to charge arrested

8. We also collected criminal justice data from North Dakota, Oregon, and Pennsylvania, but could not use them in this study, due to quality issues (Pennsylvania) and statewide nonpartisan DA elections (North Dakota and Oregon). Our resulting dataset overlaps with those in recent papers that, like us, attempt to gather criminal record data from as many states as possible (for example, Dippel and Poyker [2019] and Feigenberg and Miller [2021]).

9. For example, Texas Judicial District 97 in the northern part of the state includes the counties of Archer, Clay, and Montague, which had a collective population of around 38,000 as of the 2020 Census.

individuals with a criminal offense. Following an arrest by the police, DAs can opt to pursue the arresting charge(s), impose an alternative or further charge(s) on the defendant based on the evidence, or decline to prosecute the defendant at all. DA face few constraints on their prosecutorial discretion. In fact, recent reformist prosecutors have declined to prosecute whole categories of offenses (e.g., drug possession). Should they decide to pursue charges, DAs also have leverage over the eventual sentence imposed, both via the choice of specific charge to prosecute and through the plea bargaining process, which resolves the vast majority of cases. Their wide remit positions DAs to substantially influence criminal justice outcomes in their local court systems.

2.2 Data: Criminal Justice Records

For the six states in our sample, we compile charge-level administrative data that describe the felony and misdemeanor cases throughout the state. Crucially, the data include, to the best of our knowledge, virtually all criminal cases filed, including those that district attorneys ultimately decided not to pursue. Though we attempted to collect criminal justice data for the period 2000-2019, many states do not have records from the early 2000s. As such, data availability varies across states: Arkansas records span 2000-2018, Colorado 2002-2018, Kentucky 2002-2018, North Carolina 2013-2018, Texas 2000-2019, and Virginia 2008-2018.

The data include information about individual charges, such as a description of the offense, its severity (felony, misdemeanor, or infraction), the date the charge was filed, the date the charge was disposed, the actual disposition, and sentencing outcomes associated with the charge. We group together charges into cases, using case identifiers when available, and otherwise assuming that charges filed on the same day for the same defendant comprise a single criminal case. Using the Universal Crime Reporting (UCR) system's offense classification scheme, we label charges as either property, violent, drug, or traffic offenses. We create indicators for whether a case contains any of these types of charges, as well as whether the case contains a felony charge. The datasets also include information on defendants, such as gender and age or year of birth.[10] While the data do include indicators for defendant race and sometimes ethnicity, we find that reporting varies substantially across jurisdictions and even within states, with many locations reporting defendant ethnicity inconsistently. Due to concerns over accuracy and quality of the data, we exclude defendant race/ethnicity from our analysis, although, in the Appendix, we present results indicating that our primary findings do not vary by defendant race/ethnicity.

We construct three key outcomes from these criminal justice records. First, we create an indicator for whether a DA declined to prosecute a case, which equals one if all charges on a case were dropped or dismissed.[11] Note that, for cases filed in Texas, we consider a case to be dropped (not prosecuted) if charges resulted in a "deferred adjudication," a diversion outcome specific to that state for felony offenses that amounts to probation followed by case dismissal, provided the defendant does not re-offend. Second, we sum up the total sentence time imposed across all charges to arrive at the incarceration length imposed for a given case. Since most cases do not have any incarceration sentence imposed, we prefer to use the inverse sine of this measure, an increasingly common way of addressing outcomes with large variance that frequently takes a value of zero. Finally, we construct a binary outcome indicating whether a defendant received any incarceration sentence.

Five of these states—Colorado, Kentucky, North Carolina, Texas, and Virginia—provide us sufficient data to track defendants across time, which allows us to comment on whether defendants recidivate and appear on a subsequent criminal case. For each defendant-case in these states, we create indicators for whether a defendant receives a new criminal charge within 1 or 2 years their original arrest date.

Finally, we impose several sample restrictions that reflect data limitations

10. Court records from Virginia provide no age or year or birth field, and so we omit age for Virginia defendants.

11. Different court systems refer to nonprosecution by different names. We consider a case to be dropped if all charge dispositions are listed as dropped, dismissed, *nolle prosequi*, or, specifically in Texas, deferred.

2.3 Data: District Attorney Elections

For the six states in our sample, we compiled information on counties' district attorney elections.[12] We also observe candidate names, political party affiliations, and vote totals. From this information, we calculate the victor party's margin of victory as a share of the total votes cast in the election. Note that one state in our sample (Kentucky) reports only competitive election results, though, as we discuss below, the omission of non-competitive elections does not affect our primary research design.

We summarize these election outcomes in Table 1.

Efforts were made to collect every election for our sample states and time period. For four states—namely, Arkansas, North Carolina, Texas, and Virginia—we collected the information from state-level election archives and acquired a complete set of elections. Kentucky election archives report only competitive election results, which therefore do not influence our regression discontinuity methods, but do for matching.

2.4 Criminal Cases

We collected criminal case data from nine states. Of these, Arkansas, Colorado, Kentucky, North Carolina, Texas, and Virginia have partisan elections for district attorneys, and thus form the geographic basis of our sample. These states vary in the time spanned by the court records: Arkansas (1996-2018), Colorado (2002-2018), Kentucky (2002-2018), North Carolina (2013-2018), Texas (1993-2019), and Virginia (2008-2018). In addition to the data-imposed limitations, we restrict our sample to 2000 to 2018.

Our main outcome of interest is whether a case is dropped or dismissed rather than being otherwise disposed (e.g. plea bargaining, trial). Our other outcomes are whether a case results in incarceration or probation, the logged length of incarceration, and whether the defendant appears in the data again within 1 or 2 years. Defendant characteristics include sex and age. For much of the paper we suppress race and ethnicity since the quality of these

12. Most judicial districts are coterminous with a single county. However, some include multiple counties. For example, Texas Judicial District 97 in northern Texas includes the counties of Archer, Clay, and Montague, which have a collective population of around 38,000 as of the 2020 Census. Indeed, usually these multi-county jurisdictions have small populations.

records differ across and within states. Case characteristics include the number of charges and whether the case involves a felony. Further, for each case we classified the lead charge as property, violent, or drug based on the description of the statute involved.

In Table 2, we show the criminal case summary statistics from the election year of districts. Column 1 includes all cases in our data set. Columns 2 and 3 condition the sample based on whether: 1) the incoming district attorney is a Democrat or a Republican, and 2) the election which took place that year was decided by less than 6 percent.[13]

2.5 Census Data

We also use county-level demographic information from the 2010 Census. From this, we create indicators for whether a county has per capita income exceeding 40,000, a density exceeding 1,000 inhabitants per square mile, a black population exceeding 15%, or a Hispanic population exceeding 30%. These variables are used in supplementary results in which we match like cases to like cases.

3 Research Design

Our goal is to estimate the causal effect of district attorney partisanship on the probability of criminal prosecution, sentence length, and likelihood of recidivism. In a straightforward ordinary least-squares (OLS) framework, we would regress defendant outcomes on an indicator for whether a Democratic DA held office at the time of case filing. However, that approach would yield a biased estimate if there is any correlation between the underlying determinants of criminal prosecution rates and the political identity of the local prosecutor. For example, Table 2 shows that, on average, jurisdictions with Democratic district attorneys handle larger volumes of cases—and specifically more felony offenses—than counties with Republican prosecutors. The net effect of those confounding factors is ambiguous: larger caseloads could oblige prosecutors to conserve judicial resources by dismissing more cases, while DAs of all political stripes might be less inclined to dismiss serious felony charges.

13. The 6 percent is the optimal bandwidth, which is discussed further in the Research Design section.

3.1 Cross-sectional Regression Discontinuity Design

To address this endogeneity concern, we focus on jurisdictions in which Democratic and Republican DAs hold office as the result of closely-contested elections. Logically, jurisdictions in which Democratic DAs just edge out their Republican opponents are potentially comparable to those in which Republicans barely beat Democratic challengers. This approach echoes numerous prior studies that use close elections to infer the causal effect of partisan officials.[14]

We estimate the effect of close Democratic victories in DA elections using a sharp regression discontinuity (RD) design. Our most parsimonious specification treats our data as cross-sectional, and considers the result of the most recent DA election as an instrument for the political identity of the DA who prosecutes a given defendant.[15] For defendant i whose case enters jurisdiction j in year t, we regress his case outcome Y (for instance, an indicator for whether his case is prosecuted) on an indicator for whether a Democratic candidate won the last DA election in j (*Democrat*), along with controls for the margin by which she won (or lost) that election:

$$Y_{ijt} = \alpha_0 + \alpha_1 Margin_{jt} + Democrat_{jt}(\alpha_2 + \alpha_3 Margin_{jt}) + \alpha_4 X_{ijt} + \epsilon_{ijt}. \tag{1}$$

The coefficient of interest from Equation 1, α_2, captures the average effect of electing a Democratic DA on defendant outcome Y. While not strictly necessary in an RD setting, in some specifications we include defendant (case) and election-level covariates X_{ijt} to probe the robustness of our findings, and to improve the precision of our estimates. The identifying assumption is that, within the optimal bandwidth and conditional on the given controls, the DA's political identity (*Democrat*) is uncorrelated with unobserved determinants of case outcomes, ϵ.

14. See, for example, Ferreira and Gyourko (2009), Lee et al. (2004), and Macartney and Singleton (2019).
15. Our estimator is "intent-to-treat," insofar as we do not confirm the identity of the district attorney holding office in a given year. For instance, if a Democratic DA is voted into office, but is recalled after one year, cases prosecuted in her jurisdiction will still be in our "treated" group.

We estimate our model only on the subsample of elections for which $Margin$ falls within a narrow bandwidth given by Calonico, Cattaneo, and Titunik's (2014) optimal selection procedure. In practice, we focus on elections decided by 6 percentage points or less (that is, elections for which $Margin$ lies in the range [-0.06,0.06]), which is generally the optimal bandwidth given for our primary outcome, the probability of prosecution. Still, we show that our main results are robust to narrower and wider bandwidths. In all specifications, we cluster our standard errors at the election level.

3.2 Panel Regression Discontinuity Design

While canonical, Equation 1 leaves information on the table. Our panel data allow us to compare defendant outcomes both across jurisdictions with different election outcomes, as well as within jurisdictions across time. We leverage both sources of variation by including both pre- and post-election observations within a panel framework that adds controls for whether case i was filed post-election ($post_{i\tau}$) as well as jurisdiction and year fixed effects (λ_j and θ_t, respectively). We estimate the following specification:

$$\begin{aligned} Y_{ijt\tau} =& \alpha_0 + \alpha_1 Margin_{jt} + Democrat_{jt}(\alpha_2 + \alpha_3 Margin_{jt}) + \\ & post_{i\tau}\Big(\gamma_1 + \gamma_2 Margin_{jt} + Democrat_{jt}(\beta_1 + \beta_2 Margin_{jt})\Big) + \\ & \lambda_j + \theta_t + \epsilon_{ijt\tau}, \end{aligned} \tag{2}$$

where β_1—the post-election effect of a Democrat winning in a close race—is the coefficient of interest.

Intuitively, Equation 2 measures the difference-in-differences impact of an election in which a Democratic candidate wins, where the treatment is quasi-randomly assigned using close elections.[16] Including pre-election observations improves statistical efficiency, and al-

16. This approach is increasingly common in research that broadly examines the impact of local government politics and policies. Beach and Jones (2017) and Grembi, Nannicini, and Troiano (2016) employ similar "difference-in-discontinuities" designs, while Fischer (2022) and Shi and Singleton (2021) use analogous instrumental variables specifications with difference-in-differences components.

lows us to assess both the time dynamics of DAs' effects on case outcomes as well as the presence of confounding pre-election trends. For these reasons, we favor these panel estimates. Still, we appeal to our cross-sectional results to support three critical points. First, the sharp RD model given in Equation 1 most transparently adheres the standard RD intuition, and helps confirm that our preferred panel specification does not drive our findings. Second, the cross-sectional model helps validate our panel approach, applying the rigorous RD methods prescribed in Calonico et al. (2014) to recover robust estimates. Third, the robustness of our DiD-style estimates to a cross-sectional approach helps mitigate concerns that our panel specification might introduce bias.

Finally, to explicitly examine pre-election trends, and to comment on the dynamics of Democratic DAs' impact on criminal justice outcomes, we modify Equation 2 to take an event study-style approach. This specification highlights period-specific effects, using a period fixed effects, $\kappa_{i\tau}$, to denote whether case i was filed in pre- or post-election period τ:

$$
\begin{aligned}
Y_{ijt\tau} = & \alpha_0 + \alpha_1 Margin_{jt} + Democrat_{jt}(\alpha_2 + \alpha_3 Margin_{jt}) + \\
& \sum_{\tau=-3}^{\tau=6} \Big(\kappa_{i\tau} + \rho_\tau Margin_{jt} + Democrat_{jt}(\delta_{1\tau} + \delta_{2\tau} Margin_{jt}) \Big) + \\
& \lambda_j + \theta_t + \epsilon_{ijt\tau}.
\end{aligned} \tag{3}
$$

Each event-study coefficient $\delta_{1\tau}$ reflects the difference in outcome Y between the election year (period 0) and period τ attributable to a marginal Democratic election victory.

While credibly causal, Equations 1, 2, and 3 only capture the local average treatment effect of Democratic district attorneys on criminal justice outcomes. As with all RD designs, our coefficients of interest recovers treatment effects among cases in marginal jurisdictions where Democratic and Republican candidates compete in close elections. And, more specific to our context, most DA elections are not even contested (see Table 1), so our preferred research design focuses on a narrow subset of districts attorneys, which forces us to rely on a relatively small sample for inference and could limit the external validity of our findings. To address these points, in Section 5 we discuss an alternative matching design that, while perhaps less credible, allows us to comment on the average effect of Democratic DAs.

3.3 Validity of Regression Discontinuity Design

Both our cross-sectional and panel RD approaches yield causal estimates under the assumption that, among narrowly-decided elections, Democratic DA victories are as good as randomly assigned. While not testable in itself, this assumption implies that, pre-election, we should not find any systematic differences in case characteristics or outcomes across jurisdictions where Democratic DA candidates barely won or lost. In Table 3, we estimate discontinuities in defendant and case characteristics at the victory threshold, focusing on cases filed in the election year. We find no evidence of significant differences in these outcomes that would bias our estimates.

We also must assume that our running variable—the difference in vote share between the Democratic and Republican DA candidates in the election—is balanced at the cutoff separating Democratic and Republican victories. That is, within our sample of 189 competitive elections, there should not be a discontinuity in the running variable density at the cutoff. Figure 1 shows that the running variable density under a Republican victory is not statistically different from that under a Democratic victory. Altogether, we find no signs of baseline imbalance that would undermine our identification strategy.

4 Regression Discontinuity Results

Using our RD framework, we analyze whether and how Democratic DAs causally shape criminal justice outcomes, relative to their Republican counterparts. Our primary outcome of interest is whether the DA's office decides to prosecute a given case. We first present visual evidence of discontinuous changes in the probability of prosecution across jurisdictions in which Democratic candidates barely won and lost. Using Equations 1 and 2, we then quantify the magnitude of these discontinuities, and probe their robustness. Finally, we examine a range of other defendant outcomes, including incarceration probability, sentence length, and the probability of re-appearance in the criminal justice system .

4.1 Visual Evidence

Figure 2 provides visual evidence that the political party of district attorneys causally affects how they exercise prosecutorial discretion. We plot the Democratic district attorney's margin of victory against the proportion of cases that the district attorney dropped or dismissed in the four years following election using a scatterplot consisting of 20 bins, drawing lines of best fit on either side of the victory threshold. From Figure 2, we see that the proportion of cases dropped or dismissed increases by a little under 10 percentage points at the threshold for a Democratic victory relative to a sample average of around 35 percentage points.

In Figure 3, we further explore temporal changes in the propensity to drop or dismiss cases. Here, we plot the difference in drop/dismissal rate between closely elected Democratic and Republican district attorneys as a function of time. We include the four years prior and the six years after the district attorney comes to office, using the election year as the base year against which each difference is compared. The difference between the parties grows throughout the term, reaching a peak in the fourth year of approximately 18 percentage points. Even though the overwhelming majority of the sample is composed of districts with four-year terms, we also see that effects persist into years five and six. Prior to assumption of office there is little difference between district attorneys from each party, providing confidence that our results are not simply capturing existing trends within districts. Namely, it rules out the idea that districts were already trending towards greater leniency when the electorate voted for a Democratic district attorney. Instead, once a narrowly-elected Democratic district attorney assumes office, the district increasingly drops or dismisses cases. There are significant effects in all years in office, with a peak of around 18 percentage points in the fourth year. We also find evidence that the impact of a Democratic district attorney persists into years five and six. This persistence in leniency is likely partially due to district attorneys re-assuming office in the following term; we discuss this more later in the paper.

4.2 RD Point Estimates

We estimate the relationships captured in the previous figures in a series of regressions compiled in Table 4. First, we implement the bandwidth selection procedure proposed by Calonico, Cattaneo, and Titiunik (2014), yielding an estimated mean-square-error minimizing bandwidth of 6 percent which we use in all of our regressions unless otherwise specified. In our preferred model, we fully interact the Democratic margin of victory with indicators for Democratic victory and post-election and include district and time fixed-effects, resulting in a regression discontinuity difference-in-difference (RDDiD). Correspondingly, column 2 of 4 indicates that non-traffic criminal cases are dropped or dismissed 10 percentage points more in districts where a Democratic district attorney barely wins over a Republican one. The control mean (column 1), which stems from the cases within the panel during a Republican district attorney's tenure, is 28.1 percent of cases dropped or dismissed. Thus, reinterpreting the result, Democratic district attorneys raise the rate of dropped or dismissed cases by 36% relative to Republicans. In column 3, we include defendant and case characteristics, reducing point estimates some (about 2.4 p.p.) compared to our preferred model. Though not reported, defendant gender and the number of charges are important correlates of the drop/dismissal rate, positively and negatively respectively. Each of the estimates in columns 2, 3, and 4 are larger when we expand the post period to include the fifth and sixth year after the district attorney's election, as shown in the second row.

We also estimate a number of cross-sectional regression discontinuity models, which use only the case observations post-election, to further substantiate our results. Column 4 of 4 shows the estimates using a triangular kernel and a bandwidth of 6 percent around the victory threshold. We present further iterations with other kernels and bandwidths as Appendix Tables A1 and A2. The cross-sectional RD estimates qualitatively agree with our previously presented results, though tend to feature both larger point estimates and standard errors. For this reason we prefer the more conservative and precise estimates yielded by the panel-based

RD approach.

4.3 Robustness

In Table 5, we examine the robustness of the result to other specifications. In columns 1 and 2, we show that the result is qualitatively similar (though half the size in column 1 and significant for column 2) when the optimal bandwidth is halved and doubled, respectively, so that the sample correspondingly includes elections decided by margins of 3% and 12%. In column 3, we remove elections that were decided by a margin of 1%, thus carving a "donut" out of our sample by removing elections close to the threshold. Here, the point estimates are larger but otherwise mostly unaffected. Our results are also robust to replacing the district fixed effect with a district-election fixed effect (column 4) - that is, a fixed effect for each panel we created. Lastly, controlling for quadratic changes in our running variable, Democratic election margin, as shown in column 5, does little to our estimate. As before in Table 4, estimates in the second row additionally including cases from years 5 and 6 after the district attorney's election are larger.

Another set of results is shown in Appendix Table A3. First, we show that having an unbalanced panel, due either to late elections (after 2016) or data restrictions, does not drive results. However, in columns 2 and 3 we find that close elections in Texas play an out-sized role in results. We note that Texan criminal cases do compose about 85% of the close election observations and over half of our elections. Still, this may be suggestive that prosecutorial partisan differences are larger in Texas than other states.

4.4 Other Outcomes

The rate at which a district drops or dismisses cases is just one aspect of prosecutorial discretion. We explore these other aspects in Table A5, where we replace the outcome variable with the logarithm of incarceration length, an incarceration indicator, and 1- and 2-year recidivism measures. Columns 2 and 4 show the 4-year and 6-year estimates respectively, while columns 1 and 3 show the control means — the averages for cases in the panel during a Republican district attorney's tenure. From column 2, we demonstrate that the election of a Democratic district attorney in a close election where they are opposed by a Republican causally reduces the length of prison sentences by 14.7 percentage points (12.8 log points). The estimated impact on the incarceration rate is insignificant. These add to our evidence that party plays an important role in the prosecution of criminal cases.

The leniency which we have thus far documented raises the question: does the rise in dropped or dismissed cases and decline in incarceration lead to more reoffending? To answer this question, we add in indicators for whether an individual reoffends in one year or two years from the creation of their criminal case. We do find that both 1- and 2-year recidivism rises significantly between 1 and 2 percentage points in response to the election of a Democratic district attorney, so leniency may be partially "punished". However, we also note that these are small relatively: the difference in 1-year recidivism is less than 5 percent of the control mean.

5 Matching Results

To supplement our RD approach, we also use matching. That is, we compare outcomes for similar defendants facing identical charges who are prosecuted by DAs from opposing parties, irrespective of their election margins. Thus, matching captures the average treatment effect (ATE) as opposed to the RD approach's local average treatment effect (LATE) from close elections.

We implement exact matching, coarsening some variables to avoid overly small cells due to the curse of dimensionality. Our base model matches cases on whether the district attorney is a Democrat, state, year, and lead charge. Most important of these is the match on lead charge: we compare only cases where the lead charge involves the same criminal statute. The lead charge's statute is specific. In example, there are over 3000 unique statutes for the lead charge within Texas in our data.[17] We present our matching results in Table 7. In the first row of column 1, the estimated ATE of a Democrat DA is a 9 percentage point reduction in the likelihood of prosecuting a case. Thus, matching methods produce an estimate compa-

17. For instance, in Texas assault causing bodily injury against a family member is classified differently than assault causing bodily injury.

rable to those produced by RD methods (about 10% smaller). This result reassures us that the effects of Democrats are not specific to close elections or the districts which have them. The other rows of the same column also show estimated ATEs similar to the LATEs for the outcomes of incarceration length (-13.5 log points), incarceration likelihood (-8.8 percentage points), 1-year recidivism (1.1 percentage points), and 2-year recidivism (1.1 percentage points). The other columns vary the matching model. Column 2 adds defendant sex and age (in quartiles). Column 3 instead adds indicators for whether the case's county of origin exceeds a threshold income ($40,000 per capita), urbanness (1,000 residents per square mile), black resident percentage (15 percent), or Hispanic resident percentage (30 percent) based on the 2010 Decennial Census. Column 4 uses both sets of variables. Lastly, in column 5 defendant and Census county characteristics are included as a propensity score, rather than exact matches. Each of these present qualitatively similar results, though comparisons are not perfect as the sample changes and size shrinks with more restrictive exact matches.

In Appendix Table A4, we alter the matching in further ways. These ways are: (1) including dummies for whether the defendant is white or black, (2) matching on month, and (3) using only cases with one charge. Though we eschew using race and ethnicity in our main estimates due to data quality issues, we note that matching defendants based on racial groups does not impact estimates, as shown in the second column. Similarly, matching cases closer in time (by months) does not affect them. Restricting to only one-charge cases reduces the magnitude of estimates compared to those in column 1 and Table 7.

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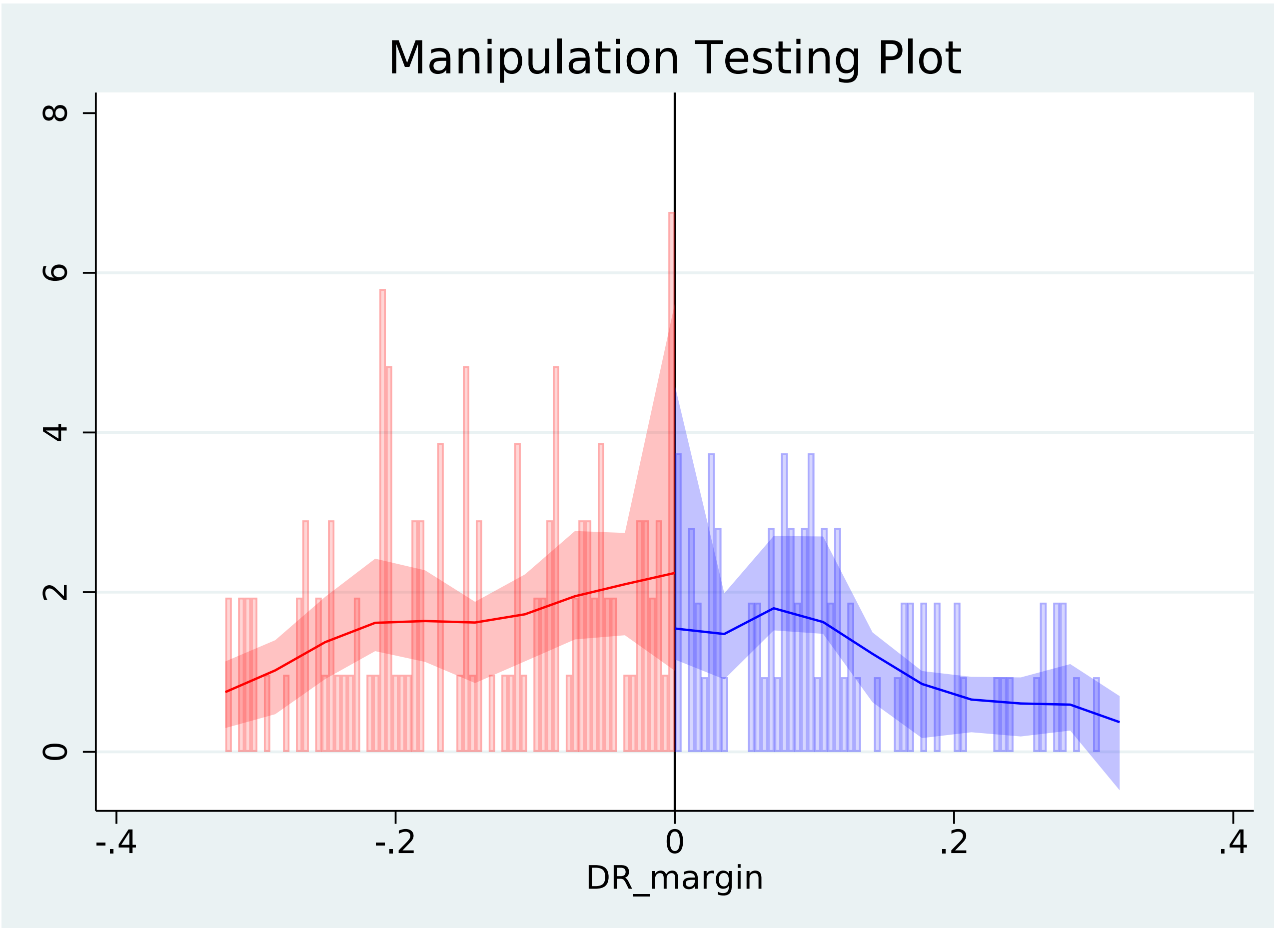
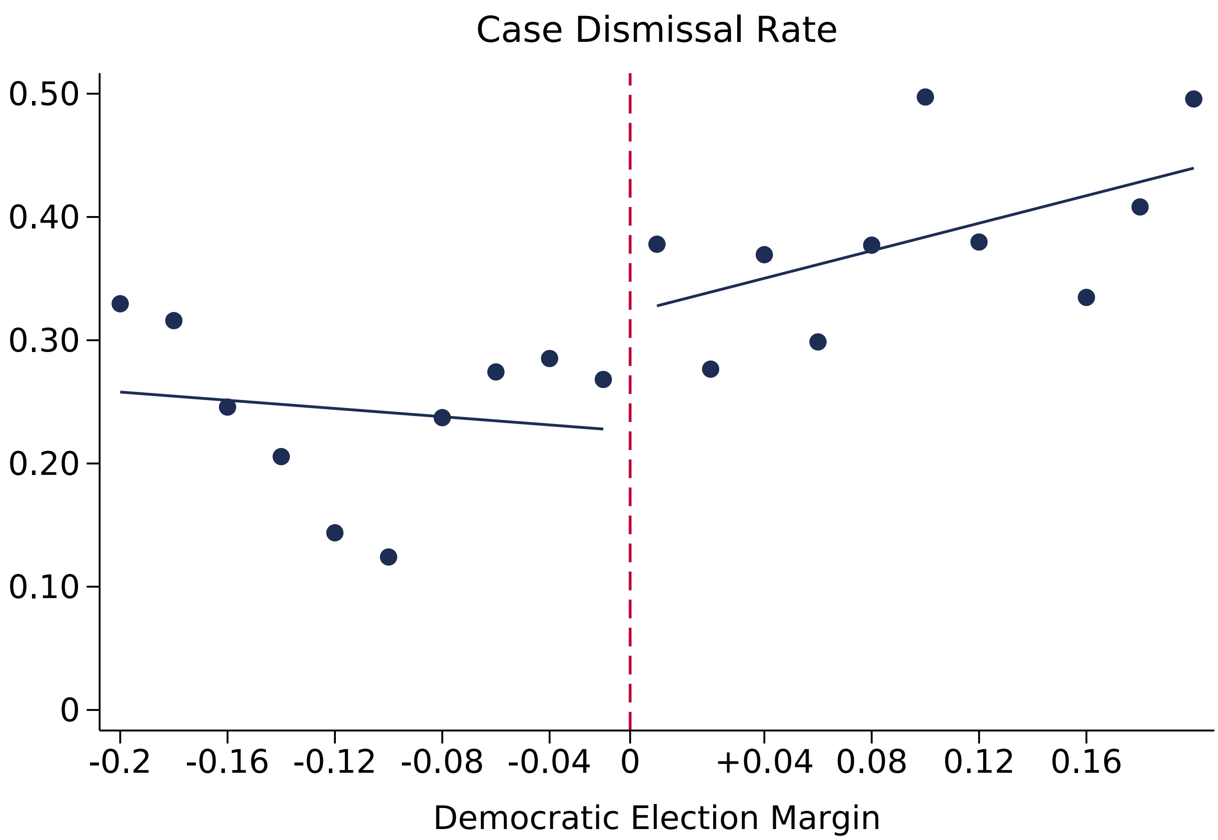
Figures

Figure 1: Manipulation Testing Plot



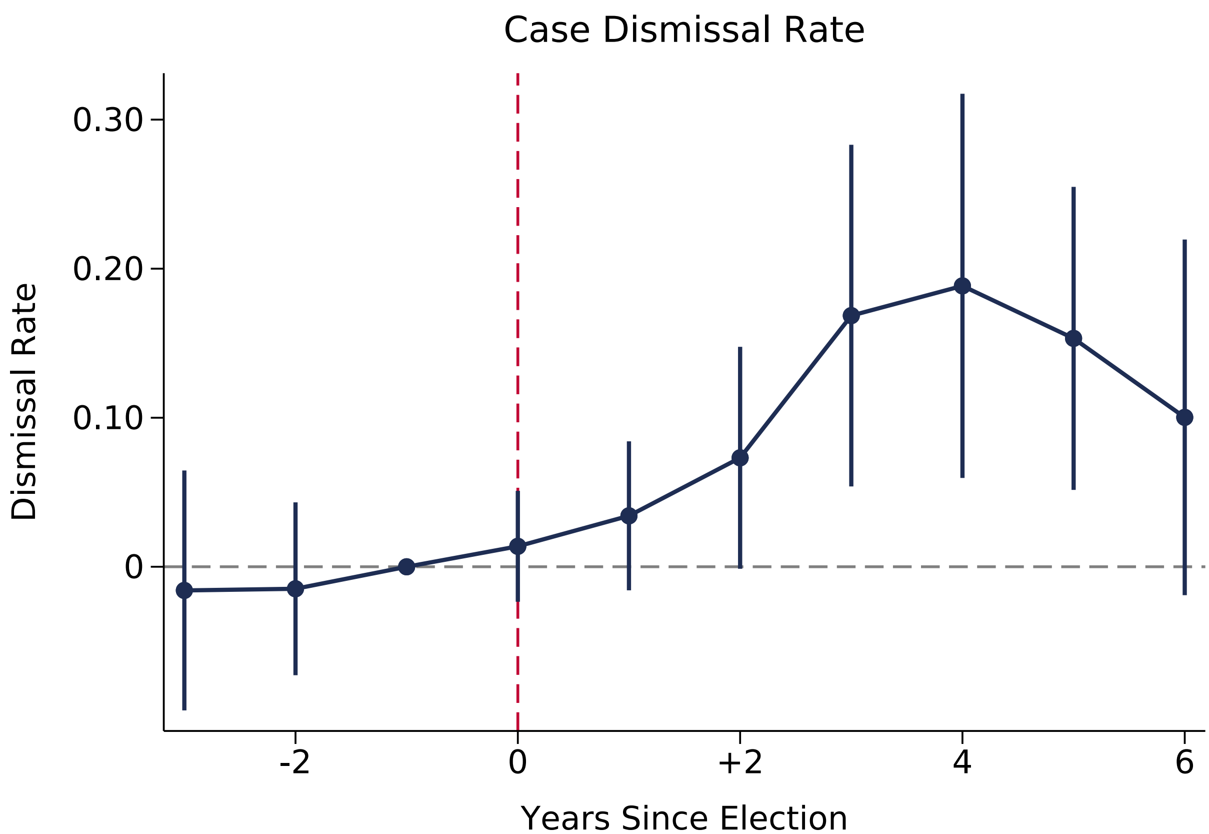
Notes: DR_{margin} refers to the margin of victory of contested district attorney elections in which a Democratic candidate faced a Republican candidate, where margins are the difference of the former's vote share with the latter's. Data source: Researcher-collected election outcomes, 2000 to 2018.

Figure 2: Regression Discontinuity



Notes: Elections are restricted to only those in the sample that have a Democratic winner and Republican loser, or vice-versa. "Democratic Election Margin" refers to the margin of victory of contested district attorney elections in which a Democratic candidate faced a Republican candidate, where margins are the difference of the former's vote share with the latter's. We include all cases within 6 years of the district attorney coming into office. Data source: Several states' court data and researcher-collected election outcomes, 2000 to 2018.

Figure 3: Regression Discontinuity Difference-in-Differences



Notes: Graph shows the difference in drop/dismissal rate between closely elected Democratic and Republican district attorneys as a function of time. We include the four years prior and the six years after the district attorney comes to office, using the election year as the base year against which each difference is compared. 95% confidence intervals are provided around point estimates. ”Dismissal rate” is the probability that a case created in a given year is either dropped or dismissed. Underlying regressions include district and year fixed effects. Data source: Several states court data and researcher-collected election outcomes, 2000 to 2018.

Tables

Table 1: Summary Statistics - DA Elections

	N	Mean	Min	Max	SD
Democrat Won?	1,409	0.374	0	1	0.484
Republican Won?	1,409	0.498	0	1	0.500
Competitive Race?	1,409	0.194	0	1	0.396
Election Margin	274	0.192	0.001	1	0.172
Dem.-Rep. Margin	189	-0.061	-0.546	0.182	0.198

Notes: The table summarizes outcomes from 1,409 district attorney elections held in the states of Arkansas, Colorado, Kentucky, North Carolina, Texas, and Virginia between 2000 and 2016. The election margin describes the difference between the winning and losing candidates' vote shares, irrespective of political party, whereas the "Dem.-Rep. Margin" describes the difference in vote share between the leading Democratic and Republican candidates. Sample sizes vary because not all DA elections are competitive, and not all competitive elections have both a Democratic and a Republican candidate.

Table 2: Summary Statistics - Criminal Cases (Election Year)

	All Cases	Democrat Win, RD sample	Republican Win, RD sample
Case Outcomes			
Dismissal	0.327	0.321	0.258
	(0.469)	(0.467)	(0.438)
log(Incarceration Length)	0.717	0.893	0.842
	(1.206)	(1.303)	(1.237)
Incarceration	0.425	0.489	0.555
	(0.494)	(0.500)	(0.497)
Probation	0.254	0.345	0.232
	(0.435)	(0.475)	(0.422)
Recidivism, <1 yr.	0.213	0.248	0.221
	(0.409)	(0.432)	(0.415)
Recidivism, <2 yr.	0.289	0.352	0.322
	(0.453)	(0.478)	(0.467)
Defendant Characteristics			
Female	0.265	0.246	0.244
	(0.441)	(0.431)	(0.430)
Age	31.501	31.732	31.077
	(11.730)	(11.216)	(11.367)
Case Characteristics			
No. of Charges	1.651	1.526	1.440
	(2.779)	(1.165)	(0.931)
Felony	0.328	0.347	0.364
	(0.470)	(0.476)	(0.481)
Property	0.355	0.341	0.336
	(0.478)	(0.474)	(0.472)
Violent	0.231	0.248	0.217
	(0.422)	(0.432)	(0.412)
Drug	0.269	0.265	0.294
	(0.443)	(0.441)	(0.455)
Observations	2,646,498	147,035	256,070

Notes: Case-level characteristics reported are from all non-traffic criminal cases in the election year. ”RD sample” is restricted to elections between a Republican and a Democratic candidate decided by less than 6 percent. Standard deviations are reported in parentheses. Data source: Several states’ court data and researcher-collected election outcomes, 2000 to 2018.

Table 3: Election Year Differences Between Cases in Treated and Control Jurisdictions

	RD Estimate	Control Mean
Case Outcomes		
Dismissal	0.061	0.258
	(0.140)	
log(Incarceration Length)	0.205	0.842
	(0.162)	
Incarceration	-0.014	0.555
	(0.091)	
Probation	0.161**	0.232
	(0.071)	
Recidivism, <1 yr.	-0.016	0.221
	(0.042)	
Recidivism, <2 yr.	-0.023	0.322
	(0.056)	
Defendant Characteristics		
Female	-0.020	0.244
	(0.012)	
Age	-0.010	31.077
	(0.441)	
Case Characteristics		
No. of Charges	0.039	1.440
	(0.162)	
Felony	-0.003	0.364
	(0.032)	
Property	0.015	0.336
	(0.030)	
Violent	0.027	0.217
	(0.033)	
Drug	-0.055	0.294
	(0.042)	

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Shows balance of main outcome and covariates in the election year. Coefficients are the result of a regression discontinuity model with district and year fixed effects. N=403,105, except for Age where N=389,855. Data source: Several states' court data and researcher-collected election outcomes, 2000 to 2018.

Table 4: Impact of Democratic Victory on Dropping or Dismissing Cases

	(1) Control Mean	(2) Panel RD (Base)	(3) With Covariates	(4) Cross-sectional RD
4 Years Post				
Post × Dem. Win	0.281	0.100***	0.076***	0.150
		(0.021)	(0.019)	(0.109)
Observations	1,001,610	3,081,859	2,991,150	3,701,731
6 Years Post				
Post × Dem. Win	0.292	0.113***	0.088***	0.193**
		(0.027)	(0.024)	(0.095)
Observations	1,399,367	3,662,350	3,565,098	5,057,258

Standard errors in parentheses, clustered by election.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Sample is restricted to elections between a Republican and a Democratic candidate decided by less than 6 percent, unless otherwise stated. The regressions producing the first (second) row of estimates include the 4 years before and 4 (6) years after the district attorney taking office. The control means reported in column 1 are case outcomes under Republicans 4 years (6 years) after election. The coefficients in columns 2 and 3 are the result of a regression discontinuity difference-in-difference model where Democratic victory, margin of Democratic victory, and an indicator for the post period are fully interacted. They further include the 4 years prior to the district attorney taking office, as well as district and year fixed effects. Column 3 adds defendant and case characteristics into the specification. The defendant characteristics are sex and age, and the case categories are the number of charges and indicators for felony, property, violent, and drug cases. Column 4 is a cross-sectional regression discontinuity design. Data source: Several states' court data and researcher-collected election outcomes, 2000 to 2018.

Table 5: Alternative Specifications Testing Impact of Democratic Victory on Dropping or Dismissing Cases

	(1) Half-optimal Bandwidth	(2) Twice-optimal Bandwidth	(3) Donut of 1p.p.	(4) District-Election Fixed-effect	(5) Second-degree Polynomial
4 Years Post					
Post $\times$ Dem. Win	0.044	0.074***	0.139***	0.096***	0.097***
	(0.030)	(0.024)	(0.041)	(0.021)	(0.022)
Observations	1,906,158	6,242,169	2,196,788	3,081,859	3,081,859
6 Years Post					
Post $\times$ Dem. Win	0.042	0.083***	0.138**	0.108***	0.111***
	(0.039)	(0.028)	(0.067)	(0.028)	(0.027)
Observations	2,299,783	7,391,181	2,636,190	3,662,350	3,662,350

Standard errors in parentheses, clustered by election.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Sample is restricted to elections between a Republican and a Democratic candidate decided by less than 6 percent, unless otherwise stated. The coefficients in the first 4 columns are the result of a regression discontinuity difference-in-difference model where Democratic victory, margin of Democratic victory, and an indicator for the post period are fully interacted. Columns 1 and 2 use half and double the optimal bandwidth of 0.06, respectively. Column 3 removes all observations one percentage point left and right of the threshold of victory. Column 4 replaces the district fixed effect with an election fixed effect. Column 5 contains an additional full interaction with margin of Democratic victory. The regressions producing the first (second) row of estimates include the 4 years before and 4 (6) years after the district attorney taking office. Data source: Several states' court data and researcher-collected election outcomes, 2000 to 2018.

Table 6: Impact of Democratic Victory on Other Outcomes

	(1) 4-Year Post (Control Mean)	(2) 4-Year Post Panel RD	(3) 6-Year Post (Control Mean)	(4) 6-Year Post Panel RD
log(Incarceration Length)	0.827	-0.128*** (0.042)	0.816	-0.170*** (0.044)
Incarceration	0.545	-0.021 (0.017)	0.537	-0.039 (0.023)
Recidivism, <1 yr.	0.227	0.011** (0.005)	0.225	0.010** (0.004)
Recidivism, <2 yr.	0.320	0.019*** (0.005)	0.313	0.0166*** (0.004)

Standard errors in parentheses, clustered by election.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Sample is restricted to elections between a Republican and a Democratic candidate decided by less than 6 percent. Coefficients are the result of regression discontinuity difference-in-difference models where Democratic victory, margin of Democratic victory, and post-election are fully interacted. The regressions producing the second (fourth) column of estimates include the 4 years before and 4 (6) years after the district attorney taking office. All regressions include district and year fixed effects. Data source: Several states' court data and researcher-collected election outcomes, 2000 to 2018.

Table 7: Matching: Impact of Democratic Victory

	Coarsened Exact Matching (CEM) (1)	CEM with Defendant Characteristics (2)	CEM with Census (3)	CEM with Def. Chars. and Census (4)	Propensity Score with Covariates (5)
Case Outcomes					
Dismissal	0.090***	0.095***	0.053***	0.063***	0.090***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
log(Incarceration Length)	-0.135***	-0.146***	-0.091***	-0.105***	-0.154***
	(0.005)	(0.006)	(0.007)	(0.008)	(0.006)
Incarceration	-0.088***	-0.095***	-0.057***	-0.066***	-0.092***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Recidivism, <1 yr.	0.011***	0.012***	0.000	0.001	0.017***
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Recidivism, <2 yr.	0.011***	0.012***	0.000	0.000	0.020***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Matched Observations	$1.29{\times}10^7$	$1.16{\times}10^7$	$8.27{\times}10^6$	$6.76{\times}10^6$	$1.11{\times}10^7$

Standard errors in parentheses, clustered by election.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Coefficients are the result of (coarsened) exact matching. In column 1, observations are matched by whether the district attorney is a Democrat, state, year, and lead charge. Column 2 adds defendant sex, and defendant age (in quartiles). Column 3 instead adds indicators for whether the case's county of origin exceeds a threshold income ($40,000 per capita), urbanness (1,000 residents per square mile), black resident percentage (15 percent), or Hispanic resident percentage (30 percent) based on the 2010 Decennial Census. Column 4 uses both sets of covariates. Column 5 uses propensity score matching for the defendant characteristics and 2010 Census information. Standard errors are clustered by state, year, and lead charge.

Appendix A. Additional Material

Table A1: Traditional RD: All Post Observations

	(1) 6 Years Choose BW	(2) 6 Years Choose BW	(3) 6 Years Main BW	(4) 6 Years Main BW
RD Estimate	0.200**	0.213**	0.179**	0.163*
	[0.099]	[0.103]	[0.089]	[0.083]
Robust 95% CI	[.003 ; .447]	[.038 ; .451]	[-.026 ; .447]	[-.019 ; .455]
Kernel Type	Triangular	Uniform	Triangular	Uniform
BW Type	MSE-optimal	MSE-optimal	MSE-optimal	MSE-optimal
Observations	5069729	5069729	5069729	5069729
Conventional p-value	0.043	0.040	0.044	0.050
Robust p-value	0.047	0.020	0.082	0.071
Order Loc. Poly. (p)	1.000	1.000	1.000	1.000
Order Bias (q)	2.000	2.000	2.000	2.000
BW est. (h)	0.055	0.046	0.060	0.060
BW Bias (b)	0.086	0.099	0.060	0.060

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Sample is restricted to elections between a Republican and a Democratic candidate decided within the bandwidth shown. Coefficients are the result of the robust regression discontinuity estimation procedure detailed in Calonico, Cattaneo, and Titiunik (2014). All regressions include the 6 years after the district attorney takes office. Data source: Several states' court data and researcher-collected election outcomes, 2000 to 2018.

Table A2: Traditional RD: Limited Post Observations

	(1) 4 Years Main BW	(2) 4 Years Choose BW	(3) Year 3 Choose BW	(4) Years 3-4 Choose BW	(5) Years 3-5 Choose BW	(6) Years 3-6 Choose BW
RD Estimate	0.138	0.150	0.143	0.167*	0.204**	0.217***
	[0.102]	[0.108]	[0.104]	[0.093]	[0.086]	[0.080]
Robust 95% CI	[-.072 ; .451]	[-.05 ; .424]	[-.079 ; .386]	[-.01 ; .396]	[.04 ; .412]	[.068 ; .418]
Kernel Type	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
BW Type	MSE-optimal	MSE-optimal	MSE-optimal	MSE-optimal	MSE-optimal	MSE-optimal
Observations	3714202	3714202	897498	1720309	2414215	3075836
Conventional p-value	0.176	0.163	0.168	0.074	0.018	0.007
Robust p-value	0.156	0.121	0.196	0.062	0.017	0.007
Order Loc. Poly. (p)	1.000	1.000	1.000	1.000	1.000	1.000
Order Bias (q)	2.000	2.000	2.000	2.000	2.000	2.000
BW Loc. Poly. (h)	0.060	0.060	0.060	0.068	0.065	0.060
BW Bias (b)	0.060	0.089	0.087	0.099	0.097	0.098

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Sample is restricted to elections between a Republican and a Democratic candidate decided within the bandwidth shown. Coefficients are the result of the robust regression discontinuity estimation procedure detailed in Calonico, Cattaneo, and Titiunik (2014). Data source: Several states' court data and researcher-collected election outcomes, 2000 to 2018.

Table A3: Robustness of Drop/Dismiss Result to Sample Restriction

	(1) Balanced Panel	(2) Non-Texas	(3) Non-Texas, Non-N.Carolina
Post × Dem. Win	0.0934***	-0.0192	-0.00659
	(0.0222)	(0.0329)	(0.0562)
Observations	2829132	495402	419965

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Sample is restricted to elections between a Republican and a Democratic candidate decided by less than 6 percent, and further restricted as indicated by the column head. Coefficients are the result of regression discontinuity difference-in-difference models where Democratic victory, margin of Democratic victory, and post-election are fully interacted. All regressions include the 4 years before and 4 years after the district attorney takes office. All regressions include district and year fixed effects. Data source: Several states' court data and researcher-collected election outcomes, 2000 to 2018.

Table A4: Matching: Additional Covariates

	Coarsened Exact Matching (CEM) (1)	CEM with Defendant Race (2)	CEM with Months (3)	CEM, only Cases with One Charge (4)
Case Outcomes				
Dismissal	0.0900***	0.0889***	0.0901***	0.0683***
	(0.00232)	(0.00236)	(0.00237)	(0.00355)
log(Incarceration Length)	-0.135***	-0.134***	-0.136***	-0.0900***
	(0.00532)	(0.00522)	(0.00543)	(0.00906)
Incarceration	-0.0879***	-0.0874***	-0.0885***	-0.0899***
	(0.00225)	(0.00224)	(0.00229)	(0.00364)
Probation	-0.000385	-0.00193	0.000261	0.0837***
	(0.00238)	(0.00238)	(0.00243)	(0.00451)
Recidivism, <1 yr.	0.0112***	0.0122***	0.0114***	-0.00605***
	(0.000656)	(0.00065)	(0.000655)	(0.00115)
Recidivism, <2 yr.	0.0114***	0.0120***	0.0115***	-0.00903***
	(0.000776)	(0.000748)	(0.000772)	(0.00128)
Matched Observations	1.29×10^7	1.29×10^7	1.26×10^7	4.04×10^6

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Coefficients are the result of (coarsened) exact matching. In column 1, observations are matched by whether the district attorney is a Democrat, state, year, and lead charge. Column 2 additionally matches using an indicator for white defendants and an indicator for black defendants. Column 3 instead matches on months, rather than just years. Column 4 includes defendant characteristics (gender, age, and race) and 2010 Census information (county income, urbanness, black population proportion, Hispanic population proportion) and additionally restricts matches to only cases with a single charge. Standard errors are clustered by state, year, and lead charge.

Table A5: Impact of Democratic Victory on Other Outcomes

	(1) Panel RD (Base)	(2) With Covariates	(3) Half-optimal Bandwidth	(4) Twice-optimal Bandwidth	(5) Cross-sectional RD
6 Years Post					
log(Incarceration Length)	-0.170***				
	(0.0444)	()	()	()	()
Incarceration	-0.0387				
	(0.0230)	()	()	()	()
Probation	-0.0564***				
	(0.0139)	()	()	()	()
Recidivism, <1 yr.	0.00948*				
	(0.00545)	()	()	()	()
Recidivism, <2 yr.	0.0132				
	(0.00864)	()	()	()	()

Standard errors in parentheses, clustered by election.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Sample is restricted to elections between a Republican and a Democratic candidate decided by less than 6 percent. Coefficients are the result of regression discontinuity difference-in-difference models where Democratic victory, margin of Democratic victory, and post-election are fully interacted. All regressions include the 4 years before and 6 years after the district attorney takes office. All regressions include district and year fixed effects. Data source: Several states' court data and researcher-collected election outcomes, 2000 to 2018.