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

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Abstract

We evaluate the causal effect of district attorney (DA) politics on criminal conviction rates, sentencing, and recidivism. Using variation in DA partisanship stemming from close elections, we find that the marginal Democratic DA is 25 percent more likely to dismiss criminal cases than Republican counterparts, and 16 percent less likely to incarcerate defendants. Strikingly, though, defendants in Democratic-led jurisdictions are no more likely to re-appear in future criminal cases, consistent with the notion that higher conviction rates have limited deterrence effects. Our findings underscore how the punitiveness of the criminal justice system hinges on the partisanship of local district attorneys.

JEL Classifications: D72, K14, K42

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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). The sheer volume of cases empowers local district attorneys (DAs) with near-total discretion to choose how many cases on which they want to pursue conviction, at the expense of scarce judicial resources, and how many to dismiss, at a potential social cost if non-conviction spurs future crime (Alschuler 1968; Bibas 2004; Bowers 2010; Stith 2008).[1] The decision to pursue a conviction carries significant consequences for accused individuals, who face economic and social fallout from incarceration and accruing a criminal record (Agan and Starr 2018; Dobbie, Goldin, and Yang 2018; Kling 2006; Mueller-Smith 2015).

But a DA's choice of conviction rate also has political ramifications. Elected prosecutors must answer to voters who, polls indicate, consistently see high crime rates as a major problem (Gramlich 2016; McCarthy 2020).[2] A median voter framework suggests that all DAs, regardless of their own political identities, might pursue high conviction rates as a visible signal of their toughness 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 their decision over whether to seek conviction or dismiss a case.

This paper examines the causal impact of DA partisan affiliation on case dismissal rates, sentencing outcomes, and re-offense rates. Our goal is to identify differences in how Demo-

1. As we note in Section 2, in our sampled jurisdictions, as in most criminal justice systems in the United States, the vast majority of filed criminal cases either result in dismissal—whereby the accused person is released without penalty—or conviction. Vanishingly few cases result in a not-guilty verdict.

2. 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.

cratic and Republican DAs exercise their prosecutorial authority, and ultimately assess how those differences shape the efficacy of their local justice systems. Prior work suggests that DA partisanship influences local incarceration patterns: Arora (2019) and Krumholz (2020) show that electing Democratic DAs leads to fewer prison admissions. However, these studies lack the criminal case-level data necessary to observe dismissal rates or re-offense behavior. And while recent anecdotal evidence suggests that "progressive prosecutors" pursue fewer cases (following campaign promises to do so), these examples capture only a subset of Democratic DAs drawn mostly from large urban jurisdictions where Democratic voters hold substantial majorities.[3] Both of these confounding factors could independently influence conviction 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 administrative dataset that contains over ten million individual criminal case records spanning 2000-2019.[4] Crucially, these data include, to the best of our knowledge, data on all criminal cases filed in our six sampled states, including defendants whose cases were ultimately dropped, allowing us to accurately measure rates of case dismissal. Matching these court data to elections returns, we can also observe the partisanship of the serving DA at the time of case filing.

However, jurisdictions that elect Democratic DAs differ systematically from those that elect Republican DAs—particularly when they win office in uncontested elections, as is the case for 81 percent of the elected DAs in our sample. Specifically, we find that jurisdictions served by Democratic prosecutors have noticeably higher caseloads (both in absolute terms and relative to their population) than those served by Republican DAs. The net bias of that added caseload is unclear: jurisdictions that handle more cases likely have more resources to do so, while higher caseloads could put pressure on DAs to quickly dismiss more low-level

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

4. Specifically, we use data from Arkansas, Colorado, Kentucky, North Carolina, Texas, and Virginia, states for which we could obtain comprehensive criminal justice data and DA election outcomes. While these states are mostly in the South, they include roughly one-quarter of all DAs nationwide, as well as a range of urban and rural jurisdictions that in many ways resemble the country more broadly. We detail our sample of court systems and elections in Section 2.

offenses in order to focus on relatively serious cases.

To address this endogeneity concern, we use narrowly-decided DA elections to home in on otherwise comparable jurisdictions that barely elect DAs from each party. Logically, by focusing on the most politically competitive jurisdictions where both parties have a shot of winning elections, we minimize the baseline differences between court systems in our sample served by Democratic and Republican DAs.[5] Indeed, we find that winning DA partisanship in narrowly-decided elections is uncorrelated with election-year jurisdiction and case characteristics, which suggests that any post-election differences in case outcomes represent the causal impact of DA partisanship. Our preferred research design combines this close-election, regression discontinuity-style (RD) variation with a difference-in-differences specification, which isolates the effect of DA partisanship both within jurisdictions across time and across jurisdictions with different election outcomes. Additionally, we supplement our main close-elections approach with a matching design, leveraging our detailed case-level data to examine the external validity our close-election identification strategy. That is, we compare outcomes for similar cases from similar jurisdictions that are prosecuted by DAs from opposing parties, irrespective of their election margins, to consider the average impact of DA partisanship.

Our close-elections difference-in-differences estimates show that, relative to Republican DAs, the marginal Democratic DA is 25 percent (8 percentage points) *more* likely to dismiss incoming criminal cases. Similarly, cases filed in jurisdictions led by Democratic DAs are 17 percent (9.7 percentage points) less likely to result in incarceration. We also find that electing a Democratic DA leads to 32 percent shorter incarceration sentences on average (about 1 less month spent in jail or prison), in line with prior research.[6] These effects remain con-

5. See Dippel (2022) and Macartney and Singleton (2017) for recent examples of studies that follow this close-elections approach to estimate the policy impact of electing partisan officials.

6. Prosecutors do not unilaterally impose sentences, a power that rests with judges. Still, by choosing whether and which charges to pursue, DAs effectively determine the minimum and maximum jail or prison sentences that a judge could choose from. DAs can also recommend particular punishments as part of a plea deal. Our point estimate on sentencing lies almost exactly between those of Arora (2019)—who finds that Democratic DAs generate roughly 55 percent shorter incarceration sentences—and Krumholz (2020), who finds that Democratic DAs reduce total sentenced months in a jurisdiction by 6 percent.

sistent under different choices of controls and definitions of "close" elections; heterogeneity analyses point to larger effects among nonwhite and female defendants, as well as among those accused of felony and nonviolent offenses, although these differences are not statistically significant. Moreover, our matching estimators back up our main findings. Within our primary close-elections research sample, our matching approach produces comparable results to our preferred specification, which we argue attests to its validity. Expanding our attention to cases from all jurisdictions, matching estimates indicate that the average Democratic DA dismisses 18 percent more cases—a smaller impact than the marginal Democratic DA, but economically meaningful nonetheless. We interpret these results as evidence that Democratic DAs pursue higher dismissal rates and impose less punitive sentences across a range of jurisdictions and defendants.

Democratic DAs' high dismissal rates clearly serve accused individuals' interests, avoiding the social and economic penalties that come with a criminal record. Yet, it remains unclear whether this agenda serves the interest of justice. Recent studies suggest that, at the margin of dismissal, a prosecutor's decision to drop a case could actually promote public safety, in that non-prosecution reduces the probability that arrestees commit future crimes. In particular, 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.[7] Motivated by these findings, we examine how Democratic DAs affect the rate at which affected individuals recidivate and reappear in future criminal cases, as a way of gauging DAs' impact on the efficacy of the local court system.

We find that the marginal Democratic DA has no statistically or economically significant effect on the probability of re-offense, measured within 1 or 2 years of initial case filing, relative to the marginal Republican DA. Point estimates have negative signs and fairly tight confidence intervals, such that we can rule out increases in recidivism of more than 2 percent,

7. As we discuss in Section 2, we consider diversion—typically probation, followed by charge dismissal—to be a form of case dismissal.

both within 1 year and 2 years of initial arrest (or 0.6 and 0.8 percentage-point increases, respectively).[8] We stress that these estimates do not capture the causal effect of case dismissal per se; rather, our results just capture the reduced-form effect of Democratic district attorneys on recidivism rates—which could operate through numerous policy channels, including higher case dismissal rates. Still, our null findings echo an emerging empirical consensus that criminal convictions might have limited deterrence effects at the margin.

Overall, our work sheds new light on how partisan politics impacts the criminal justice system. This analysis speaks to two strands of the literature. First, by providing rigorous evidence that a district attorney political identities substantially influence case dismissal rates, we contribute to a growing body of research that highlights the degree to which case outcomes in the legal system depend not on the facts but 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 whether a less punitive criminal justice system comes at the cost of reduced deterrence. Across a range of jurisdictions, we show how Democratic DAs pursue fewer criminal convictions and lower incarceration rates without systematically adverse effects on recidivism. While we cannot directly estimate the impact of case dismissal on re-offense behavior, our results support the argument—advanced by academics and policymakers alike—that high conviction rates are not necessarily efficient. Altogether, our findings underscore the extent to which politics shapes the implementation of the law at the local level, and how the punitiveness of the court system hinges on the partisanship of district attorneys.

8. Certain specifications yield precisely-estimated declines in recidivism of up to 16 percent (6 percentage points) among cases filed four years after a Democratic DA wins a close election. We treat these results with caution, as we discuss in the text, although they broadly support our argument that Democratic DAs do not adversely affect re-offense rates.

2 Background and Data

Our study examines the causal link between DA partisan affiliation and the rate at which they dismiss criminal cases. To capture results that can be plausibly attributed to DA partisanship, and to deliver widely-applicable findings, we compile a multi-state dataset that links DA election returns with detailed criminal justice records. However, few states provide the comprehensive criminal justice records necessary to conduct an analysis focused on pre-sentencing outcomes, a critical constraint which limits the scale of this study. As we detail below, our final dataset includes six states with suitable policy environments and data—Arkansas, Colorado, Kentucky, North Carolina, Texas, and Virginia.[9] Together, these states comprise 17.8 percent of the U.S. population as of the 2010 Census and around a quarter of its district attorneys.[10] Though we attempted to collect criminal justice data for the period 2000-2018, 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.

We begin by contextualizing district attorneys' role in the criminal process. We then summarize the DA elections data that we use to identify the partisanship of DAs serving in our sampled jurisdiction-years before describing how we construct our sample of criminal court records. Combining these datasets, we summarize key features of our sampled criminal cases and jurisdictions, highlighting differences between Democrat- and Republican-led court systems. Finally, we discuss how we construct our primary analytic dataset, a jurisdiction-election panel.

9. As of July 17, 2023, the findings presented in this paper do not include Kentucky, in order to comply with requirements from the Administrative Office of the Courts. Including Kentucky—which is a small part of our sample anyway—has virtually no effect on our results. We will include these findings in the final version of this paper.

10. 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 lack of data to identify DA partisanship (North Dakota and Oregon). Our resulting dataset overlaps extensively with those in recent research that, like us, attempt to gather court records from as many states as possible (for example, Dippel and Poyker [2019] and Feigenberg and Miller [2021]).

2.1 Background: The Role of District Attorneys in the Criminal Justice Process

Voters in the six states we study elect district attorneys to lead public prosecutors' offices and represent the state 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.[11] 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 retains control of overall prosecutorial policy and directs the ADAs. In our sampled states, voters elect DAs to office every four to six years.

District attorneys' principal responsibility involves deciding whether to seek to convict individuals accused of a criminal offense. Following an arrest by the police, DAs can opt to pursue the arresting charge, impose alternative or further charges on an individual based on the evidence, or dismiss the case against the person altogether.[12] DAs 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 a defendant's conviction, 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: District Attorney Elections

For each jurisdiction and year represented in our sampled states, we determine the political party of the serving DA using election records. We compile jurisdiction-level DA election returns, which include candidate-level vote totals, and use the partisan identity of the most recent election winner as the party of the sitting DA. For five of the six states in our sample,

11. For example, Texas Judicial District 97 in the northern part of the state includes the counties of Archer, Clay, and Montague, which have a collective population of around 38,000.

12. In five of our six sampled states, DAs formally charge arrestees. The exception is Virginia, where the police decide whether to charge. Still, in all states, DAs have the authority to dismiss a case entirely, with no further penalty.

we directly observe candidates' party affiliations; for one state (Arkansas), we determine candidates' political parties using voter registration data.[13] From this information, we calculate the winning party's margin of victory as a share of the total votes cast in the election.[14] Note that one state in our sample (Kentucky) reports only competitive election results, though, as we discuss in Section 4, the omission of non-competitive elections does not affect our primary research design.

In Table 1, we summarize all 1,311 DA elections that took place in our sampled states and time periods. Republican candidates won most (55 percent) of these contests, while Democrats won 43 percent of the time. In the remaining 2 percent of elections, either a third-party candidate won, or else we could not determine the partisanship of the winning candidate. Strikingly, only 255 races (18 percent of the total) were contested by multiple candidates.[15] And even among these nominally competitive elections, most races had wide victory margins: on average, the winning candidate defeated her opponent by 19 percentage points. Focusing on the 197 "politically competitive" races that include at least one Democrat and one Republican—the sample of greatest interest to this study—we find that the average race resulted in a Democratic loss of 6 percentage points. In Section 3, we discuss in greater detail how we leverage these election margins to identify the causal effect of electing a Democratic DA.

2.3 Data: Criminal Justice Records

Our primary research question considers the rate at which DAs dismiss criminal cases. Many criminal justice datasets only include criminal offenses that result in conviction and

13. Arkansas is the only state in our sample that holds nonpartisan DA contests but has partisan voter registration. As such, we match candidates to the Arkansas voter roll to determine their registered political party preference. We find that many contested elections feature candidates from opposing parties—however, our ability to observe candidate partisanship hinges on the match quality to the Arkansas voter roll, which may introduce bias. Omitting Arkansas from our sample has a negligible impact on our findings.

14. For elections with more than two candidates, we define the margin of victory as the difference between the first- and second-placed candidates' vote shares. We separately define the winning *party's* margin of victory as the difference between the top Democratic candidate's vote share and the top Republican's vote share.

15. Krumholz (2019) collects data on DA elections held in 40 states between 1990 to 2015, finding that 25 percent of races were contested, slightly higher than the rate in our sample, but confirmation that DA races rarely feature multiple candidates.

sentencing. We therefore compile charge-level administrative records that describe all felony and misdemeanor charges filed in our sampled states. These data contain, to the best of our knowledge, all criminal charges filed in each state during their coverage periods, including those that district attorneys ultimately chose to dismiss. That said, arrest and charging behavior varies substantially across states. Unequal arrest and charging rates on the part of the police mean that identical crimes committed in different states may not be reported at similar frequencies. Likewise, the same underlying offense can often support different charges in different states. We quantify and discuss the ramifications of these cross-state differences below.

2.3.1 Criminal Justice Data Cleaning

Each state data source records fairly detailed 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 charge disposition (e.g., dismissed, found or pleaded guilty), sentencing outcomes associated with the charge, and characteristics of the defendant (age, gender, race/ethnicity). Our approach to data cleaning echoes Feigenberg and Miller (2021), who compile a similar multi-state dataset.[16]

We first define charge-level characteristics and outcomes. Following the Universal Crime Reporting (UCR) system's offense classification scheme, we label charges as either property, violent, drug, or traffic offenses. We refer to charges that do not fall into these categories—generally, crimes against society, such as driving while intoxicated (DWI)—as "other." For each charge, we define three disposition outcomes: an indicator for whether the charge was dropped or dismissed by the DA; an indicator for whether the charge resulted in any

16. While our overall approach to data cleaning resembles that of Feigenberg and Miller (2021), two differences stand out. First, to define criminal cases, we group together charges based not only on identifying fields in the data but also on date of filing, a choice which we believe reflects how the courts will process these charges. This choice reduces the number of "cases" in our sample, relative to that of Feigenberg and Miller. Second, likely as a result of how we define cases, we observe incarceration rates 8-10 percentage points higher than Feigenberg and Miller, and dismissal rates that are 6-8 percentage points lower. We believe both approaches to the data are reasonable ex ante; we prefer our approach because, by defining cases relatively broadly, we avoid mistaking a case dismissal for charges dropped as part of an overall guilty plea on a case (anecdotal and empirical evidence suggests that such "charge bargaining" is ubiquitous in the criminal justice system; see, for example, Piehl and Bushway [2007]).

incarceration sentence; and the nominal length of that sentence, in months.[17,18]

We then aggregate our charge data to the criminal-case level, our primary unit of observation, using defendant identifiers and charge filing dates. We assume that the courts will process charges filed on the same day for the same defendant together as a single unit. We categorize cases as property, violent, drug, traffic, or other if any charge within that case is of the given type. Likewise, we define a felony case to include at least one felony charge.

To construct our primary case-level outcomes—case dismissal, incarceration, and sentence length—we again look across charges within a case. We consider a case to be dismissed if all charges on the case are dismissed; we say a case results in incarceration if at least one charge results in incarceration; and we take the incarceration length imposed on a case to be the maximum confinement sentence imposed across all charges (which we set to zero if no charge on the case resulted in an incarceration term). Since our sentence length variable has a large variance and a mode of zero, we prefer to use its inverse hyperbolic sine (asinh), which has a similar interpretation and properties as the natural log function, but is well-defined at zero.[19]

We then construct defendant covariates, relying on fields in the raw data to identify defendant gender and age at the time of case filing.[20] Each state reports defendant race/ethnicity identification differently: some treat "Hispanic" as a race, mutually exclusive with "white," while others treat ethnicity as distinct from race. To minimize measurement error, we group

17. Different court systems refer to non-prosecution dispositions by different names, which we group under the term "dismissed." We consider a charge to be dismissed if its disposition is given as "dropped," "dismissed," "*nolle prosequi*," or, specifically in Texas, "deferred adjudication." "Deferred adjudication" is a diversion outcome unique to felony offenses filed in that state, which amounts to probation followed by dismissal, provided the defendant does not re-offend. Note that we exclude from this definition the rare situations in which charges were dropped because a judge dismissed a case (for example, due to a lack of evidence), rather than a DA.

18. We do not observe the actual sentences served by defendants, but rather the incarceration term set at the time of sentencing. External factors unobservable to us, such as parole board leniency, could cause nominal and actual incarceration sentences to diverge in ways that local DAs (as well as judges and defendants) may anticipate and factor into their decision-making. For this reason, we tend to stress our extensive-margin results, which consider dismissal and the probability of any incarceration sentence.

19. Feigenberg and Miller (2021) and Shem-Tov (2021) use this transformation as well to measure length of incarceration. We obtain virtually identical results when we instead use a typical log transformation.

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

together "nonwhite" defendants, which includes defendants identified as Hispanic or by any race other than "white."[21]

Finally, we use our extensive data and defendant identifiers to track re-offense behavior. For each defendant-case (i.e., every appearance a defendant makes in the data), we create indicators for whether the defendant re-appears on a new criminal case within 1 or 2 years their original case's filing date, which we refer to as defendant recidivism. Unfortunately, only three states (Kentucky, North Carolina, and Texas) provide sufficient data to track defendants across time and observe future offenses. Still, these three states account for over 60 percent of the criminal cases and elections in our main research sample.

To ensure that our dataset contains comparable types of cases across jurisdictions, we impose several sample restrictions. We drop cases that have indeterminate dispositions—such as cases binding to a higher court—or no disposition. We omit cases that exclusively involve traffic offenses, since some states do not report low-level traffic infractions and misdemeanors. We also exclude juveniles, those younger than 18 at the time of case filing. When considering defendant recidivism, we exclude cases filed within 1 or 2 years (depending on the exact outcome variable) of the end of their state's criminal justice records' coverage period.

2.3.2 Summary of Criminal Case Data

Our final sample contains 9,787,182 criminal cases, which we summarize in Table 2. Across our full dataset (described in column 1), DAs dismiss 37 percent of cases, while 47 percent of cases result in incarceration. Though not shown here, virtually all cases that do not result in dismissal culminate in conviction; as such, we typically refer to defendants whose cases were not dismissed as "convicted." In terms of recidivism, 41 percent of defendants re-appear on a subsequent criminal case within two years. Defendants are overwhelmingly male (just 25 percent are identified as female), while 54 percent are identified as nonwhite. Most cases involve misdemeanors (only 34 percent include a felony charge), and the most

21. Functionally, this approach mirrors that of Feigenberg and Miller (2021), who face a similar challenge and focus on a white/nonwhite dichotomy. Unlike those authors, though, we include less-represented races in our sample, such as Asian and American Indian.

common charge types included in cases are property offenses (e.g., theft) and "other" offenses against society (e.g., DWI), which appear in 36 and 39 percent of cases, respectively.

The remaining columns of Table 2 underscore the extent to which defendant outcomes diverge across court systems. Breaking down our sample by state, we find that case dismissal rates range from 19 percent (Arkansas) to 58 percent (North Carolina); incarceration rates range from 15 percent (North Carolina) to 62 percent (Texas). These stark disparities across state lines likely reflect a combination of factors. As we mention above, despite the fact that our data comprise arrest-to-sentencing records, standards for arrest, and what sorts of charges police and prosecutors have at their disposal, vary across jurisdictions. Equally, actual judicial punitiveness almost certainly varies across court systems, driven by racial attitudes (as Feigenberg and Miller [2021] demonstrate), mandatory sentencing laws, and—of particular relevance for us—the political considerations of judges and district attorneys.

Regardless of its underlying causes, the immense cross-state variation in criminal case composition and outcomes poses an empirical challenge as we try to disentangle the impact of DA partisanship from other confounding factors. In general, our preferred approach is to focus on within-jurisdiction variation, usually in the form of jurisdiction fixed effects, that account for unrelated state-by-state (and, indeed, jurisdiction-by-jurisdiction) differences in the legal environment. We elaborate on how our research design isolates the effect of DA partisanship in Section 3.

2.4 Constructing Panel Datasets

We combine our DA election and criminal justice records to create two analytic datasets. The first uses the elections returns to create a jurisdiction-level panel, which tracks the partisanship of each jurisdiction's serving DA over time, as well as basic information from the most recent DA election (e.g., the winning candidate's margin of victory). Using county and year of filing, we match individual cases to this panel of jurisdictions, which allows us to observe the DA's partisanship at the time a case entered the court system.

We summarize our jurisdiction-year panel in Table 3, highlighting both demographic features of the locations represented in our data, as well as the average characteristics of the local court systems. To help illustrate the (qualitative) relationship between DA partisanship and features of their jurisdictions, we break down our sample of jurisdiction-years by the party affiliation of the serving DA, as well as the competitiveness of the election in which they secured their office. The first panel of Table 3 summarizes jurisdiction-year demographics, drawing from Census (population, nonwhite population share) and Bureau of Economic Analysis (income per capita) data.[22] Column 1 describes all jurisdiction-years in our sample (N=3,874); the remaining columns separate jurisdiction-years by the partisanship of the sitting DA, as well as by the competitiveness of the most recent DA election.

The average jurisdiction-year in our sample has a population of just over 171,000—24 percent of whom are nonwhite—and a mean per-capita income of almost \$39,000. These averages mask substantial disparities across jurisdictions served by Democratic and Republican DAs: Democratic DAs hold office in less populous and less affluent places than their Republican peers. Interestingly, differences between contested and uncontested jurisdictions, regardless of DA partisanship, stand out, whereas contested jurisdictions represented by DAs of opposing parties appear relatively similar. This pattern presages our research design, which focuses on these competitive jurisdiction-elections.

The second panel of Table 3 focuses on the local court outcomes of systems represented by Democratic and Republican DAs. These data show further evidence of divisions between politically competitive and noncompetitive localities. Uncontested jurisdictions have relatively low caseloads (with fewer than 2,000 cases filed per year, compared around 5,000 in contested jurisdictions) and relatively high incarceration rates (40-48 percent, versus 39-45 percent in contested jurisdictions). Interestingly, Republican- and Democratic-led jurisdictions also appear to diverge in their punitiveness: contested jurisdictions with Democratic DAs have higher dismissal rates (39 percent versus 35 percent) and lower incarceration rates

22. Note that BEA data is missing for incorporated cities in Virginia, which operate independently from their surrounding counties and elect their own DAs. For jurisdictions that span multiple counties, we aggregate Census and BEA data from these different counties. We inflate BEA income data to 2016 dollars.

(39 percent versus 45 percent) than their Republican counterparts elected in competitive races.[23] At the same time, the average Democratic DA in a contested jurisdiction handles 6 percent more cases per year than the average competitively-elected Republican (5,267 cases per year, compared to 4,987 in Republican-led DAs offices), which encapsulates the fact that DAs of opposing parties serve very different court systems. The question thus remains whether this divergence in outcomes represent the causal impact of DA partisanship, or result from the underlying differences in caseloads and demographics across jurisdictions. Our identification strategy, which we discuss in the next section, aims to disentangle these factors from the causal impact of DA partisanship.

As part of that research design, we construct a second analytic dataset: a jurisdiction-election panel, focusing exclusively on contested DA races. Intuitively, as we discuss in depth in Section 3, by restricting our attention to these politically competitive locations, we hope to minimize differences in jurisdiction characteristics that might explain variation in court outcomes. Around each of the 197 "politically competitive" elections in our sample that feature both Democratic and Republican candidates, we create a panel that includes all criminal cases filed between the third year prior to the election and the sixth year following the election. Each observation in the panel is a case-by-jurisdiction-election. The panel has an unbalanced structure, where periods may be missing for some elections but not others. Within a given jurisdiction-election's panel, each criminal-case observation shares time-invariant, election-level data, such as the election margin. The dataset has a "stacked" structure, in which cases can appear more than once in the panels of different elections. This jurisdiction-election panel contains 8,196,244 case-by-jurisdiction-election observations.[24]

23. We summarize other case and defendant characteristics by DA partisanship in the Appendix. The case-level summary data show similar patterns in judicial outcomes, with cases filed in Republican-led jurisdictions more likely to result in prosecution and incarceration. By contrast, we find few differences in average case characteristics.

24. We describe post-election cases from this sample of contested jurisdiction-elections, alongside our full sample (discussed above) and our final research sample, in the Appendix.

3 Research Design

Our goal is to estimate the causal effect of district attorney partisanship on the probability of criminal case dismissal, sentence length, and the likelihood of defendant re-arrest. Using a straightforward ordinary least-squares (OLS) framework, we could 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 judicial system punitiveness and the political identity of the local prosecutor. For example, Table 3 shows that, on average, jurisdictions with Democratic district attorneys elected in competitive races handle larger volumes of cases per capita than jurisdictions with Republican prosecutors elected in uncontested races. The direction of the resulting bias is unclear: those additional cases could reflect greater judicial capacity to pursue cases to conviction, or the high caseload could oblige DAs to dismiss more cases in order to conserve judicial resources for the most serious offenses. Disentangling the causal effect of DA partisanship from the role of prosecutorial resources and other confounding factors represents the primary goal of our research design.

3.1 Identification from Close Elections

To address this endogeneity concern, we focus on jurisdictions in which Democratic and Republican DAs hold office as the result of closely-contested elections. As Table 3 shows, jurisdictions served by Democratic and Republican DAs elected in contested races appear to be fairly similar, at least along observable dimensions. Our close-elections research design extends this insight to its logical conclusion: by focusing on those DAs elected in the most narrowly-decided races, we hope to minimize the confounding differences between jurisdictions served by Democratic and Republican chief prosecutors. This approach echoes numerous prior studies that use close elections to infer the causal effect of partisan officials.[25]

25. See, for example, Dippel (2022), Ferreira and Gyourko (2009), Lee et al. (2004), and Macartney and Singleton (2019).

3.1.1 Cross-sectional Specification

Our most parsimonious empirical approach captures the effect of close Democratic victories in DA elections using a sharp regression discontinuity (RD) framework. For this specification, we aggregate our data to the jurisdiction-year level, and use the result of the most recent DA election as an instrument for the political identity of the serving DA at the time of case filing.[26] For jurisdiction j and year t, we regress average case outcome $\overline{Y}$ (for instance, the share of cases dismissed by the DA) on an indicator for whether a Democratic candidate won the last DA election in j ($Democrat$), along with a linear control for the margin by which she won (or lost) that election, and their interaction.[27] Our main cross-sectional specification is

$$\overline{Y}_{jt} = \alpha_0 + \alpha_1 Margin_{jt} + Democrat_{jt}(\alpha_2 + \alpha_3 Margin_{jt}) + \epsilon_{jt}, \tag{1}$$

where the coefficient of interest, α_2, captures the local average effect of barely electing a Democratic DA on aggregate outcome $\overline{Y}$.

Throughout our analysis, we restrict our sample to those jurisdiction-elections decided by a sufficiently narrow margin to be comparable. Specifically, we focus on elections in which the Democratic candidate's margin of victory (or loss) was at most 7.1 percentage points (that is, those for which $Margin$ lies in the range [-0.071,0.071]). We use that threshold since it is the smallest—and therfore, we argue, most conservative—bandwidth given by Calonico, Cattaneo, and Farrell's (2020) selection procedure across our different samples

26. Our estimator is "intent-to-treat," insofar as we do not confirm the identity of the district attorney who actually prosecuted a given case. For instance, if a Democratic DA is voted into office, but is recalled after one year and replaced by an appointed successor, cases prosecuted in her jurisdiction will still be in our "treated" group. More generally, some cases filed around the time of an election will remain in the system when a new DA assumes office. Since we use the date of case filing to assign cases to DAs—rather than the date of case disposition, which is potentially endogenous (i.e., more case dismissals will shorten average disposition times)—these election-year cases could be misclassified as "treated" or "untreated." Since we observe that DA partisanship takes time to express itself on case outcomes, we do not believe the resulting bias substantially affects our results.

27. To demonstrate the robustness of our results, we estimate an alternative specification that includes a quadratic polynomial in *Margin*. As we discuss in the next section, our results are qualitatively similar under both the linear and polynomial approaches.

and outcome variables.[28] Still, to address concerns that this sample restriction explains our findings, in Section 4 we show that we obtain similar results using narrower and wider definitions of "close" races. Across all specifications, we cluster our standard errors at the jurisdiction-by-election level to correct for the correlation of outcomes among cases handled by the same DA.[29] Our identifying assumption is that, among narrowly-decided contests, and conditional on the election margin, DA partisanship (*Democrat*) is uncorrelated with unobserved determinants of average case outcomes, ϵ.

3.1.2 Internal Validity of Close-elections Approach

While not testable in itself, our identification assumption implies that, pre-election, we should not find any observable differences distinguishing the jurisdictions where Democratic DA candidates barely won and lost. That is, close Democratic DA victories should not be correlated with features of the jurisdiction or its typical caseload that might independently drive post-election case outcomes. Using Equation 1, we estimate differences in election-year jurisdiction demographics, as well as case and defendant characteristics. We disaggregate case and defendant data here, since our primary research design (which we discuss below) leverages case-level data, although we obtain quantitatively similar results when we aggregate to the jurisdiction-election level. We report our estimates in Table 4. Encouragingly, we find no significant differences in these jurisdiction- or case-level covariates that would undermine our identification approach.

In keeping with standard RD assumptions, we also ensure 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. In other words, within our sample of 197 politically competitive elections, there should not be a discontinuity in the running variable density at the cutoff that would indicate one party

28. In the Appendix, we compare cases filed in all contested jurisdictions to those filed in our close-elections sample. We find few differences in average case characteristics, suggesting our sample of cases filed in narrowly-contested jurisdictions is observably similar to the larger sample of cases filed in competitive jurisdictions.

29. Functionally, clustering at the jurisdiction-election level amounts to clustering on the running variable ($Margin$).

systematically wins these tight races. Figure 2 shows that the distribution of our running variable (the Democratic margin of victory) does not vary discontinuously at zero. A formal test for any jump in the density at the cutoff (following Calonico, Cattaneo, and Farrell [2020]) fails to detect any such difference, with a p-value of 0.987.

3.1.3 External Validity of Close-elections Approach

Our close-elections comparison approach, combined with our policy setting, raises a potential empirical concern: we rely on a small group of jurisdictions for identification, with just 49 DA elections in our sample decided by a margin that falls within our bandwidth. As with all election-RD designs, our coefficient of interest recovers treatment effects among cases in marginal jurisdictions where candidates compete in close races. And, more specific to our context, the fact that most DA elections are not even contested could limit the external validity of our findings, as the effects we observe in competitive jurisdictions may not provide much insight into the numerous uncontested jurisdictions in our data. To help address these points, in Section 5 we introduce a supplementary matching design that, while perhaps offering a less airtight causal claim, allows us to comment on the average effect of DA partisanship across a wider range of jurisdictions.

3.2 Panel Specification

While intuitive, the cross-sectional approach given by Equation 1 might not yield very persuasive results, given the small sample of competitive elections at our disposal. Equally, cross-sectional estimates across jurisdictions must contend with the substantial variation in outcomes we see across court systems (see Table 2), which could lead to noisy results.

In fact, the traditional sharp RD approach leaves information on the table that could make our results both more precise and more compelling. Our panel structure allows us to compare defendant outcomes both across jurisdictions with different election outcomes as well as within jurisdictions across time, which can assist with statistical inference. Likewise, our case-level data allow us to control for a wide array of defendant and case characteristics to help discount alternative explanations for our results.

To strengthen our research design and leverage these features of our data, we combine our close-elections, RD-style intuition with a difference-in-differences panel framework. We estimate this panel model using our main disaggregated, case-level panel dataset, including pre- and post-election observations, and focusing on outcome Y for individual case i (for example, an indicator for whether the case was dropped). Building on our cross-sectional specification from Equation 1, we add controls for whether case i was filed in a post-election period τ ($post_{i\tau}$), as well as jurisdiction and year fixed effects (λ_j and θ_t, respectively). Our baseline panel specification is:

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

where β_1—the post-election effect of a Democrat winning in a close race—is the coefficient of interest.[30]

Intuitively, Equation 2 measures the difference-in-differences effect of a Democratic DA victory, relative to a Republican win, where the Democratic victory "treatment" is quasi-randomly assigned via close elections.[31] Including pre-election observations improves statistical efficiency, and addresses the possibility that pre-election circumstances drive our results. Furthermore, the addition of jurisdiction and year fixed effects help mitigate the substantial variation in outcomes across court systems and time (see Table **??**), which further improves

30. The estimated β_1 from Equation 2 without the panel components is effectively the same as α_1 from Equation 1 with jurisdiction-caseload weights. We prefer the case-level approach because it allows us to leverage our detailed data to achieve the greatest precision, as well as to thoroughly explore heterogeneity and alternative explanations for our findings. In any case, we obtain comparable estimates using both our cross-sectional and panel RD approaches, implying that our case-level design—and its associated weighting choice—does not drive our results.

31. Similar hybridized RD-difference-in-differences approaches are increasingly common in research that 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 (2023) and Shi and Singleton (2022) use analogous instrumental variables specifications with difference-in-differences components.

the precision of our estimates. For these reasons, we favor these case-level panel estimates. Still, we appeal to our aggregate cross-sectional results for transparency and to demonstrate the robustness our findings to alternative approaches.[32]

Finally, to more explicitly rule out pre-election trends in our outcome variables that might bias our results, and to comment on the dynamics of Democratic DAs' impact on criminal justice outcomes, we modify Equation 2 to take an event-study approach. This specification highlights period-specific impacts of DA partisanship, using 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 $\delta_{1\tau}$ captures the difference in outcome Y between the last pre-election year (period -1) and period τ attributable to a marginal Democratic election victory.

3.3 Variation in DA Partisanship After Close Elections

Before estimating our cross-sectional and panel models, we first illustrate the "first stage" variation in district attorney partisanship that our close-elections design generates. In the cross section, our close-elections approach guarantees that, in the year after a DA election, jurisdictions in which a Democratic candidate won the race will be served by a Democratic DA. Over time, though, subsequent elections could result in incumbent losses, in both "treated" and "control" jurisdictions, which could narrow that gap.

To illustrate this pattern, we estimate an event study specification in the style of Equa-

32. Qualitatively, we find similar results using alternative versions of Equation 2 that omit the panel framework. We provide these results in the Appendix to highlight the different roles played by our panel approach and choices of fixed effects. This comparison underscores the important role played by jurisdiction and year fixed effects in our empirical approach, particularly once we incorporate multiple post-election periods. As expected, our panel design—which includes pre-election observations—does not have a large effect on the magnitudes of our estimates, but does noticeably improve precision.

tion 3, where the outcome is whether a Democratic DA holds office, and each observation is a jurisdiction-year. Those estimates appear in Figure 1. As expected, we find that during the four years after an election in which a Democratic candidate edged out their opponent, most jurisdictions continue to be served by a Democratic DA; after four years—at which point the modal jurisdiction holds another DA election—we find no significant differences in DA partisanship across locations that originally elected Democratic and Republican prosecutors. We interpret this pattern as evidence that our close-elections design succeeds in generating quasi-random differences in DA partisanship for a meaningful stretch of time. We note, too, that point estimates for the first four post-election years are virtually equal to 1, and there are no discernible pre-election trends, which affirms that the partisan identity of the pre-election DA is balanced across treated and untreated locations.

4 How Do Democratic DAs Affect Case Outcomes?

Our study investigates how DA partisanship causally influences criminal case outcomes. We begin our analysis by using our close-election framework to investigate how Democratic DAs affect the probability of case dismissal before broadening our attention to other salient case outcomes, namely the probability of incarceration and incarceration length. Anecdotal evidence surrounding "progressive prosecutors," plus our descriptive findings in Table 3, suggest Democratic DAs increase case dismissal rates while lowering incarceration rates and sentence lengths. Yet, as we have noted, specific features of Democrat-led jurisdictions—their lower income levels and higher caseloads, for instance—might explain these trends.

4.1 Does DA Partisanship Matter for Dismissal Rates? Visual Evidence

Using our close-elections framework and panel data, we first provide visual evidence of how narrow Democratic DA elections shape case dismissal rates in their jurisdictions. Democratic DAs' election victories might take time to materially affect case outcomes—incoming DAs could, for example, need to replace assistant prosecutors, or lay out specific prosecution policies, two challenges faced by recent reform-minded DAs. We therefore begin by assessing the dynamic impact of close Democratic DA elections with an RD event study-style framework, using Equation 3 to estimate the treatment effect on case dismissal rates over time.

These estimates appear in the first panel of Figure 3. Importantly, we do not find any evidence of pre-election differences in case dismissal rates that would bias our results. The lack of differential pre-trends helps alleviate concerns that dismissal rates in treated and untreated jurisdictions might have already been diverging prior to electing a Democratic DA. Note that some case observations from the election year (period 0) will be "treated," in that they will still be pending when the new DA term begins in the subsequent year, which explains why we find a small (albeit insignificant) uptick in dismissal rates in the election year itself.[33]

Following the election, panel 1 of Figure 3 shows a rise in dismissal rates almost immediately after a Democratic DA assumes office, although the effects are only statistically significant beginning three years after the election. Four years after the election—at the conclusion of the modal DA's term in office—a marginally elected Democratic DA is 13 percentage points more likely to dismiss a criminal case than a marginally elected Republican DA.[34] Though noisy, point estimates indicate that this effect may attenuate 5-6 years after the election, which corresponds to the start of a new DA term in most jurisdictions.

In the second panel of Figure 3, we focus on that fourth post-election year, when the effects of DA partisanship appear most pronounced. We visualize the cross-sectional discontinuity in aggregate case dismissal rates that underpins our panel estimates, using a bin scatterplot to show the average dismissal rate across jurisdiction-elections by the Democratic DA candidate's margin of victory (or loss).[35] This nonparametric approach reveals

33. For context, the average case in our sample takes 110 days to reach a disposition, meaning that the average case filed after mid-September of an election year will be disposed by a new DA, should the incumbent lose their re-election bid.

34. Point estimates from this and other event study-style plots in the main text appear in the Appendix.

35. We provide versions of all our bin scatterplots without the bins—that is, scatterplots where each point corresponds to exactly one jurisdiction-election—in the Appendix. These include figures in which the dependent variable is the within-jurisdiction change in mean outcomes, which we use in Table 5 below. These Appendix figures more directly mimic our cross-sectional RD approach by weighting observations by

the stark variation behind our findings: at the cutoff, cases prosecuted by Democrats who just won their elections appear to have about a 12 percentage-point higher probability of being dismissed than those prosecuted by Republicans who just won their elections.

4.2 Estimating the Effect of DA Partisanship on Case Dismissal Rates

We next provide formal regression estimates to substantiate the visual evidence from Figure 3. Our estimates come from our two RD specifications: a typical cross-sectional RD estimator, following Equation 1, and an RD panel estimator, following Equation 2. We present these findings in the first panel of Table 5.

Results from our preferred panel RD specification appear in columns 2-4. These coefficients capture the difference-in-differences impact of a close Democratic DA's election over the 4 or 6 years post-election. Point estimates indicate that the marginal Democratic DA increases the probability of case dismissal by 7.4-8.3 percentage points during this period (26-28 percent of the sample mean), relative to the marginal Republican DA. Our estimates are robust to the inclusion of additional defendant and case covariates (column 3), as well as to different choices of post-election sample periods (compare columns 3 and 4).

In columns 5-7 of of Table 5, we present results from alternative cross-sectional specifications in order to demonstrate the robustness of our panel design, using Equation 1 and aggregate, jurisdiction-election-level data. To facilitate comparisons across jurisdictions that vary widely in size, we employ jurisdiction population weights (see Section 2.3 and the Appendix).[36] We first estimate a straightforward sharp RD model, examining the impact of a marginal Democratic DA victory in the fourth year post-election as in Figure 3 (N=49 jurisdiction-elections). That estimate appears in column 5. We find that the marginal Democratic DA causes an uptick in jurisdiction dismissal rates of 14.7 percentage points. Still, this effect is not precisely estimated— unsurprisingly, given the lack of controls for jurisdiction population.

36. We use population as our weighting variable since it is unlikely to be influenced by the election outcome. Our panel regression design, which effectively weights jurisdiction-elections by caseload, arrives at comparable estimates, suggesting our choice of weight does not drive our findings.

unrelated cross-state and over-time variation that our panel design includes.[37]

To mitigate these unrelated sources of variation, we present results from two alternative cross-sectional specifications. The first, shown in column 6 of Table 5, examines how a Democratic DA affects the *change* in dismissal rates between the pre-election and post-election periods within their jurisdiction (that is, the difference in average dismissal rates between the three years prior to the election and the four years after the election). This approach echoes the panel design from the second column of Table 5, but employs aggregate data (N=49 jurisdiction-elections) and no additional controls. We find that jurisdictions that barely elect a Democratic DA see a marginally significant 8 percentage-point increase in dismissal rates post-election. The estimate, which is quite similar to the baseline panel RD result from column 2, provides some reassurance that using case-level data and a formal difference-in-differences framework does not drive our results.

Lastly, to demonstrate that our findings do not hinge on the inclusion of pre-election data, we disaggregate our outcome variable slightly so that each observation is an election-period (N=282). This step means our observations are no longer unique at the jurisdiction-election level, and so we can augment our model with jurisdiction and year fixed effects, which control for secular variation in outcomes driven by differences in the legal environments across jurisdictions. The point estimate from that model, reported in column 7, indicates that Democratic DAs raise dismissal rates by 10 percentage points (31 percent)—slightly larger than the analogous panel estimate from column 4. Overall, we argue our results provide robust evidence that DA partisanship meaningfully affects case dismissal rates, with Democratic DAs pursuing fewer convictions than their Republican counterparts.

37. Using bias-corrected standard errors, as prescribed in Calonico et al. (2020), does not substantially alter our conclusions from our cross-sectional RD results. Estimates from our baseline model (column 5) remain imprecise, while those in column 6 are actually more precise after using corrected standard errors. Note that we cannot use the same bias-correcting procedure with our panel RD specification, nor with the fixed effects specification shown in column 7.

4.3 Does DA Partisanship Matter for Other Case Outcomes?

Beyond case dismissal, we want to understand how DA partisanship shapes case dispositions broadly, particularly in terms of salient incarceration outcomes. Figure 4 presents visual evidence that electing a Democratic DA leads to lower rates of incarceration as well as lower incarceration sentences. Drawing on Equation 3, event study-style estimates in the first two panels point to marked declines in incarceration severity soon after a Democratic DA's election victory, culminating in a 14 percentage-point decline in incarceration probability and a 30 percent decline in sentence length in the fourth post-election year. Cross-sectional data in the third and fourth panels tell a similar story. However, the discontinuity in sentence length does not appear to be very robust near the cutoff, a point we return to below.

In the second panel of Table 5, we present panel and cross-sectional RD estimates describing Democratic DAs' impact on the probability of incarceration and incarceration sentence length. Using our main panel specification, we find that Democratic DAs reduce the probability of incarceration by between 9.2 and 9.8 percentage points in the 4-6 years following their election (16-17 percent of the sample mean). Likewise, we find that Democratic DAs impose 32-38 percent shorter sentences (roughly 3 fewer months in jail or prison, on average). Our cross-sectional RD approach, focusing on the fourth post-election year, shows an imprecise though economically meaningful decline in the probability of incarceration (around 8 percentage points), but near-zero effects on incarceration length. The imprecise null on sentence length likely reflects the noise near in the cutoff visible in Figure 4. When we consider the change in jurisdiction-wide dismissal rates, or include multiple post-election periods alongside jurisdiction and year fixed effects, we recover statistically significant Democratic DA effects on both incarceration probability and length. We conclude that DA partisanship plays a meaningful role in determining incarceration outcomes.

4.4 Robustness

Our twin panel and cross-sectional specifications provide some assurance that our choices of specification and observation level do not qualitatively affect our principal findings. That said, the RD framework underlying our panel approach requires assumptions over the exact sample and functional form of our control for the election margin, each of which could influence our findings. In Table 6, we further explore the robustness of our main close-elections panel estimates to different choices of sample and RD controls.

The second and third columns of Table 6 probe the robustness of our findings to alternative samples of elections. That is, we modify our bandwidth—our definition of what constitutes a "close election"—to include more or fewer jurisdiction-elections. In column 2, we narrow our sample to jurisdiction-elections decided by 5.7 percentage points or less (20 percent less than our primary bandwidth of 7.1 percentage points; this leaves us with 40 elections); in column 3, we widen our bandwidth to include jurisdiction-elections decided by 14.2 percentage points or less (twice our primary bandwidth; this gives us a total of 92 elections).[38] In general, our point estimates remain fairly stable and precise across these different samples. In column 4, we alter our panel RD specification (Equation 2) to include a quadratic, in addition to a linear, control for the Democratic margin of victory. This change has almost no effect on our results. Finally, to ensure our estimates do not depend on observations right at the cutoff—that is, DA elections decided by the slimmest margins, whose outcomes might be most susceptible to nonrandom factors—we exclude DA elections decided by 1 percentage point or less (that is, we take out a 1 percentage-point "donut"). We find uniformly larger treatment effects once we exclude the closest DA contests in our sample, which confirms our findings do not rely on those races.

4.5 Heterogeneity

To round out our main findings, we examine whether Democratic DAs' choices of low prosecution and incarceration rates benefit some defendants or types of cases more than others. Documenting any heterogeneous effects of DA partisanship—or the lack thereof—provides useful context for interpreting our results: do Democratic DAs simply decline to

38. As hinted at earlier, in practice the "optimal" bandwidths for our different outcomes lie between 5.4 percentage points and 9.8 percentage points. Our choices of "narrow" and "wide" bandwidths here are meant to be salient but also extremes on that scale, to demonstrate that the choice of bandwidth does not materially affect our conclusions.

prosecute specific types of cases (e.g., drug offenses), which single-handedly drives down prosecution rates, or does DA partisanship matter for a wide range of defendants?

We consider multiple dimensions of potentially heterogeneous treatment effects in Table 8. Each column presents panel RD results, applying our preferred model (Equation 2), estimated on a particular subsample of our data, described in the column headers. For context, below each point estimate and standard error we report the subsample mean of the outcome variable, which we find varies considerably.

Qualitatively, our heterogeneity analysis reveals larger effects of close Democratic DA elections on dismissal and incarceration rates among nonwhite and female defendants, as well as among cases involving felony and nonviolent offenses. We find larger impacts on sentence length among young and male defendants, as well as among cases involving felony and property offenses. In relative terms compared to their respective means, however, these effect sizes do not vary much, and there is no clear pattern to indicate that DA partisanship primarily impacts more or less severe cases, or more or less vulnerable defendants.

To test whether these differences in the treatment effect estimates across subsamples are statistically meaningful, we run a series of pooled specifications. That is, we modify Equation 2 to interact our treatment indicator (whether a Democrat wins the close DA election) with an indicator for the respective subsample (e.g., whether the defendant is over age 30). Broadly, we find little evidence of statistically significant differences. So while Table 8 points to larger effects among some types of cases and defendants, we cannot rule out equality in all but a few of these estimates. Based on these tests and the fairly consistent estimates in Table 8, our results suggest that Democratic DAs have a substantial effect across their caseload.

5 Do Democratic DAs Increase Recidivism Rates?

The differences in case outcomes generated by Republican and Democratic DAs that we have documented thus far point to substantially different official views on the value of criminal prosecution and incarceration. Ample prior research has shown that defendants incur considerable economic and social penalties following spells in detention, however brief, including wage penalties and lower civic engagement (Mueller-Smith 2016; White 2019). Although we do not have adequate data to examine those downstream effects of DA partisanship, we can still weigh in on the relative efficacy of Democratic DAs' policy choices by exploring how prosecutor partisanship influences re-offense rates.

A popular narrative suggests that, by prosecuting and potentially imprisoning defendants, DAs can deter future criminal behavior. A growing body of research has undercut this argument (for example, Agan et al. [2021a], Augustine et al. [2022], and Mueller-Smith and Schnepel [2020]). Nonetheless, these studies focus on the marginal non-prosecuted defendant arrested in specific, oftentimes coastal, jurisdictions. One might argue that a different, negative relationship between non-prosecution and recidivism holds in a the majority of smaller court systems, like those in our sample. We stress that we cannot directly test the impact of case dismissal on the probability that a defendant re-appears on a future criminal case. However, we can evaluate the effect of electing a Democratic DA—who, among other things, increases dismissal rates and lowers incarceration rates—on the overall re-arrest rate in their jurisdiction. This exercise helps provide both a sense of whether DA partisanship affects the deterrence of local criminal justice systems, as well as a descriptive data point on the relationship between relatively lenient prosecutors policies and re-offense rates.[39]

We apply our close-elections empirical framework to examine the impact of Democratic election victories on recidivism rates. We have two outcomes of interest, the probabilities a defendant re-offends within 1 year or 2 years of their original case's filing date.[40] Recall that we can only observe defendant re-arrests in three states, Kentucky, North Carolina, and Texas.

39. We define recidivism as synonymous with re-arrest or, more precisely, re-appearance on a new criminal case. Many researchers oppose this definition, since arrest probability depends on the behavior of police. In our setting, though, looking at outcomes downstream of arrest—such as incarceration—does not make sense, since they will just reflect the impact of DA partisanship, which we have already shown to be substantial.

40. Our sample size attenuates rapidly when we look at re-offense rates over longer time horizons. Agan et al. (2022), for one, focus on 2-year recidivism, and so we lean into this outcome as a benchmark against the rest of the literature.

Figure 5 depicts the relationship between Democratic DA election victories and defendant recidivism, again using our (parametric) panel and (nonparametric) cross-sectional approaches. Event-study plots in the first two panels show no evidence of any effect of DA partisanship on the probability that defendants re-appear on cases within 1 or 2 years of initial arrest. Moreover, relatively tight confidence intervals and point estimates very near to zero indicate a true null effect. Similarly, cross-sectional aggregate data from four years post-election show no discontinuity in recidivism rates among closely-decided jurisdiction-elections, supporting the same conclusion as our parametric panel evidence.

We formally estimate the causal impact of DA partisanship on recidivism using Equations 1 and 2; our results appear in Table 7. Our panel RD point estimates confirm that Democratic DAs have a null impact on the probability of defendant recidivism. These results in columns 2 through 4 amount to zero effects: the 95 percent confidence intervals on our preferred estimates in column 2 imply that we can rule out an increase in 1-year re-offense probability greater than 0.6 percentage points (2 percent of the sample mean) and an increase in 2-year re-offense probability greater than 0.8 percentage points (also 2 percent of the sample mean). Cross-sectional RD estimates in the remaining columns point to sizeable declines in recidivism following a Democratic DA's election. Imprecise coefficients point to a decline in 1-year recidivism of between 1.1 and 1.5 percentage points (about 4 percent of the sample mean). Interestingly, we find a very precise and economically large reduction of 5.7 percentage points (14 percent) in 2-year recidivism in the fourth year following a Democratic DA's election. However, we caution that the visual evidence of this effect (in Figure 5) is wanting, and so we hesitate to accept this estimate at face value.

Still, these results suggest that, at a minimum, Democratic DAs do not encourage greater re-offense rates than their Republican counterparts, and potentially that they reduce defendant recidivism several years after their election. While we reiterate that these estimates do not capture the causal effect of case dismissal, we view these null results as part of a growing body of work that finds diverting or dismissing criminal cases discourages defendants from committing future crimes. Our findings provide suggestive evidence consistent with the conclusion that higher dismissal and lower incarceration rates do not encourage criminal behavior, even if we cannot identify these effects directly.

6 External Validity

Heretofore, we have described only the local average treatment effect (LATE) of Democratic DAs on case and defendant outcomes. The estimates we have reported identify the causal effect of DA partisanship among those officials elected in the most competitive elections. Given the prevalence of uncontested DA races, identifying the impact of the average DA's partisanship is of particular policy importance, and there is ample reason to doubt whether our RD approach delivers that average treatment effect (ATE), not least because of the disparities between contested and uncontested jurisdictions we document in Table 3. But delivering a plausibly causal estimate of the average effect of a Democratic DA is challenging: we would need a valid instrument for DA partisanship across all jurisdictions, contested and uncontested, which we do not have.

In the absence of such an instrument, we employ a matching design, which draws on our case-level data to create "matched" samples of otherwise similar cases from similar jurisdictions prosecuted by Democratic and Republican DAs. Our approach combines a typical propensity score design to match jurisdictions with exacting matching to compare similar cases within those jurisdictions. Functionally, we use a logit model to determine the likelihood that a given jurisdiction has a Democratic prosecutor based on its demographic characteristics (those given in Table 3: population, income per capita, nonwhite population share, and caseload per capita). Within matched jurisdictions with similar propensity scores, we then require exact matches of treated and control cases based on key characteristics—the defendant and case covariates listed in Table 2, in addition to the year of case filing, and the lead (most serious) charge on a case.[41] In so doing, we aim to create matched

41. In most state datasets, the lead charge is denoted as the first charge on the case; when the data do not indicate the lead charge, we assume it to be the charge with the longest expected incarceration sentence.

sample of comparable treated and control cases, minimizing the differences in confounding jurisdiction-level and case-level factors in order to isolate the effect of DA partisanship.

6.1 Evaluating the Matching Design

We conduct three preliminary analyses to assess the viability of our matching design. The key concern with any matching approach is that the chosen mix of covariates used to construct the matched sample are not sufficient to remove selection bias that distinguishes treated and untreated units. Helpfully, in our setting, we have a benchmark that we can use to assess the performance of our matching approach: our well-identified RD estimates. Intuitively, assuming that our RD estimates capture the "true" causal impact of DA partisanship on case outcomes—locally within the RD subsample—then we would expect a valid matching estimator to yield comparable results when applied to the RD sample. Specifically, we attempt to mimic the multi-period cross-sectional RD approach from column 7 of Table 5 and column 6 of Table 7, which shares key features with our matching design (namely, both make within-year and within-state comparisons).

These matching results appear in the third column of Table 9, alongside the benchmark cross-sectional RD estimates in column 2. Overall, within our RD sample, we recover matching estimates that are virtually identical to our cross-sectional results, at least when we consider dismissal, incarceration, and recidivism rates. Estimates on sentence length, however, differ markedly: our matching approach recovers an effect less than half as large as our RD specification. As such, we treat our remaining matching estimates of sentencing effects with caution. On the other hand, the similarity across estimates for the remaining outcomes bolsters our faith in the matching estimates outside of the RD sample.

As a second assessment of our matching design, we mimic our panel RD evidence in Figures 3 and 4 by plotting event-study estimates using our matched sample. Ideally, our matching framework should produce similar event-study coefficients as our panel RD specification, even without the RD controls. Those plots appear in the Appendix, and show that, within the RD sample, our matching and RD estimates track one another reasonably well.

Finally, to confirm that our matching design actually yields similar treatment and control groups, we compare our matched treated and control observations from our full case sample (shown in columns 6 and 7 of Table 9). As expected, we find that jurisdiction and case characteristics are balanced across matched cases prosecuted by DAs of opposing parties. Taken together, our preliminary matching exercises provide some reassurance that our matching design can replicate most of our main RD findings, and that our mix of covariates represents a reasonable basis for creating a matched sample.

6.2 Matching Results

In column 5 of Table 9, we show matching estimates of the effect of DA partisanship using our full sample of cases, which we argue approximates the average effect a Democratic DA has on case outcomes. We find that the average Democratic DA is 6.7 percentage points (18 percent) more likely to dismiss a given case, and 8.5 percentage points (18 percent) less likely to impose an incarceration sentence. Taken at face value, the average Democratic DA appears to seek 18 percent shorter incarceration sentences as well, although we caveat that this result may be suspect, given the concerns above about the reliability of our matching approach when it comes to examining sentence length. Altogether, our matching results indicate that the average effect of DA partisanship on case outcomes is smaller than the marginal effect, though still economically meaningful. Furthermore, the matching estimator recovers null effects on re-offense rates, suggesting that neither the average nor the marginal Democratic DA increases the likelihood of defendant recidivism.

7 Concluding Discussion

Elected district attorneys hold considerable sway over local court systems, opening the door for political partisanship to shape the implementation of justice. Understanding the degree to which DAs drive prosecution and incarceration rates provides valuable context to ongoing discussions surrounding the punitiveness and efficacy of the American criminal justice system. To our knowledge, this study provides the first causal analysis of how DA partisanship affects prosecution and incarceration rates, as well as defendant recidivism. Unlike prior work, which considers the consequences of non-prosecution, we evaluate a critical determinant of non-prosecution, shedding new light on the political economy of criminal justice in the process.

We compile a dataset of over 10 million criminal cases, which we then use to evaluate the impact of district attorneys on case outcomes. Applying a close-elections difference-in-differences design, we find that the marginal Democratic DA is 8 percentage points (26 percent) more likely to dismiss (decline to prosecute) criminal cases than their Republican counterpart. Likewise, cases handled by Democratic DAs are 9.7 percentage points (17 percent) less likely to result in incarceration, and receive 32 percent shorter incarceration sentences on average. At the same time, we do not find any evidence that electing Democratic DAs raises re-offense or local crime rates. These findings substantiate anecdotal evidence that DA partisanship matters, and in fact causes prosecutorial policies to diverge sharply across jurisdictions. Equally, our study provides unique evidence of how the institutional structure of the justice system—and its reliance on elected prosecutors—can generate more prosecutions and higher incarceration rates without raising measures of criminal behavior.

Our work builds on prior research by highlighting the critical role DA politics plays in determining dismissal rates and, by extension, which defendants experience the harsh economic consequences of accruing a criminal record. As an empirical consensus develops that high prosecution and incarceration have social costs and sometimes elusive benefits, evaluating the roots of more punitive criminal justice policies could highlight avenues for change. This paper contributes to that effort, documenting the critical role DAs can play in supporting harsher, though not necessarily more effectual, court systems. Our results suggest that any future efforts to make the criminal justice system less punitive must overcome the partisan divide among public prosecutors, and run straight through the ballot box.

Figures

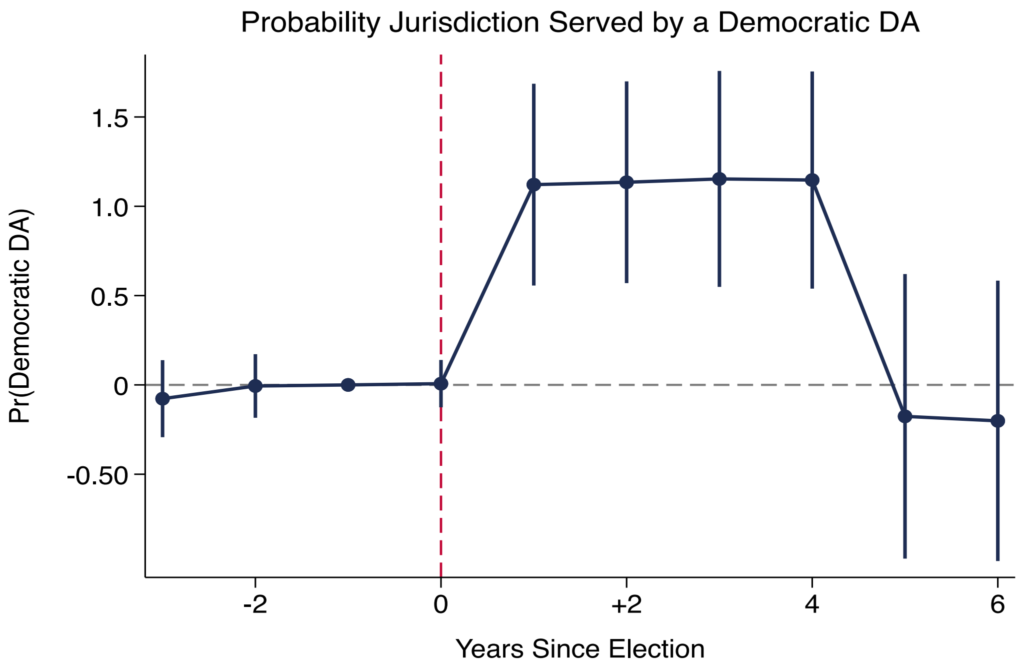

Figure 1: The figure plots panel regression estimates showing the impact of a narrow Democratic DA win on the probability that a Democratic DA serves in office in the years around the election. The specification is Equation 3. All coefficients are relative to the year prior to the election (period -1). The sample includes 49 competitive district attorney races with at least one Democrat and one Republican running that were decided by 7.1 percentage points or less. Standard errors are clustered at the election level.

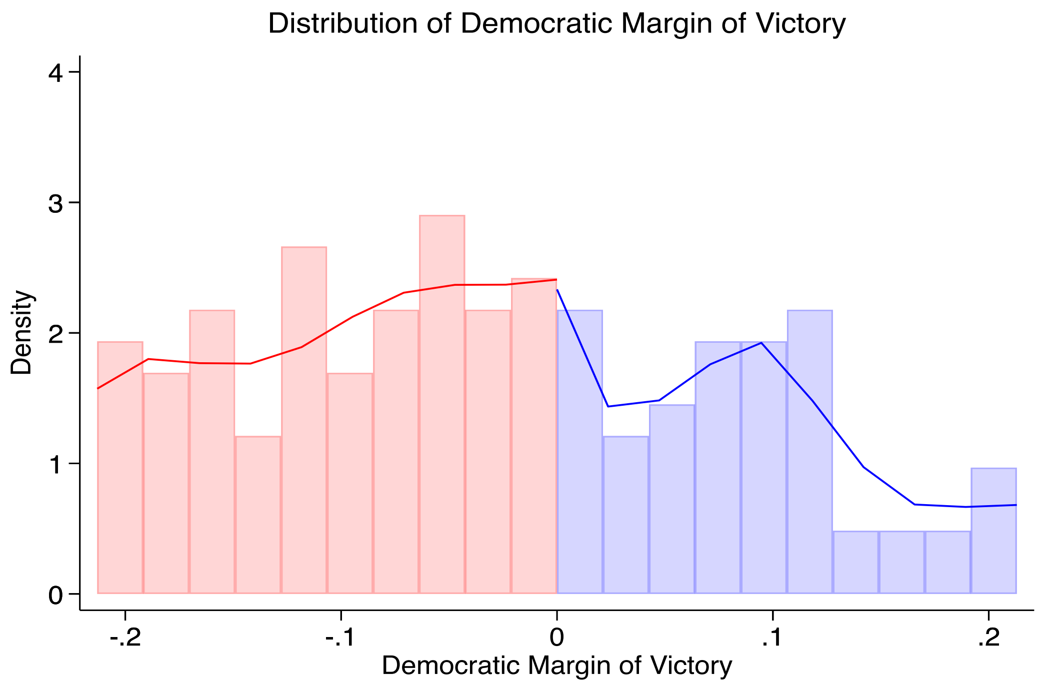

Figure 2: The sample includes 134 competitive district attorney races with at least one Democrat and one Republican running that were decided by 20 percentage points or less. "Democratic margin of victory" refers to the difference between the top-performing Democrat's vote share and the top-performing Republican's vote share in the election.

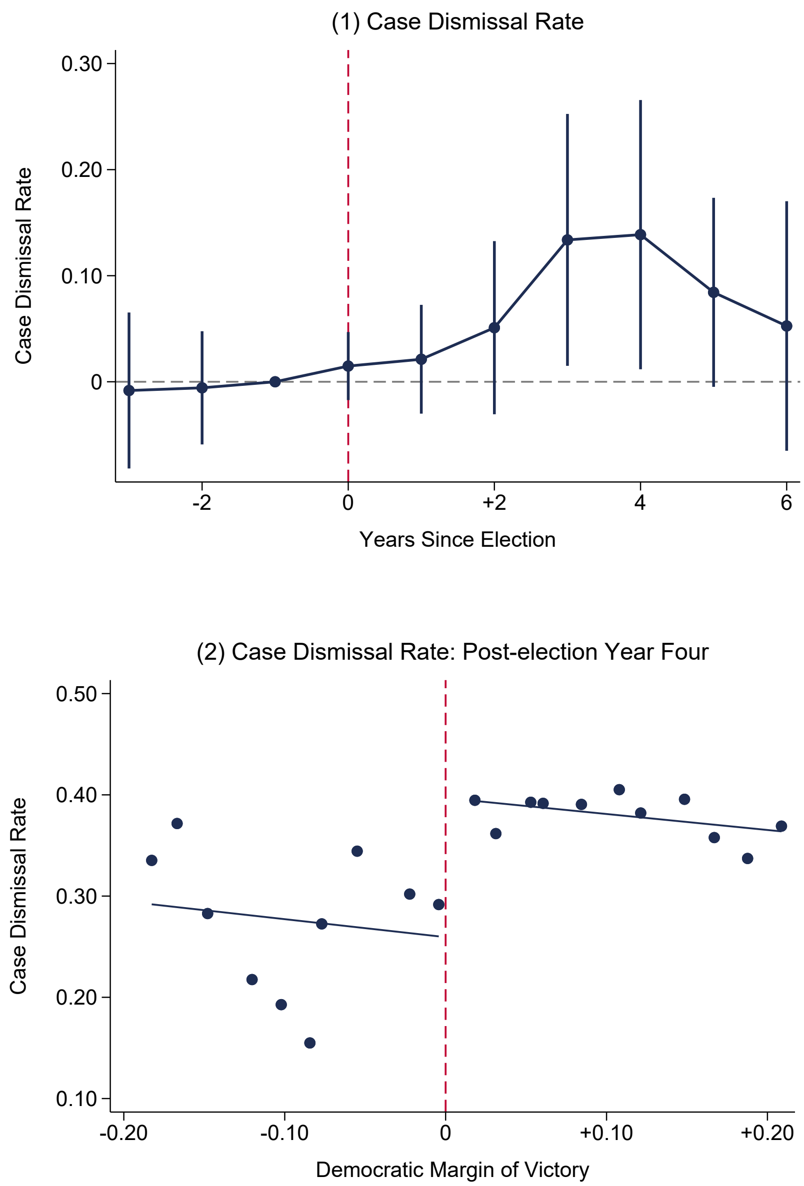

Figure 3: Panel 1 plots panel RD estimates showing the impact of a narrow Democratic DA election win on the probability of case dismissal by year relative to the election. The specification is Equation 3. All coefficients are relative to the year prior to the election (period -1). The sample includes cases filed in jurisdiction-elections decided by 7.1 percentage points or less (N= 4,160,428 cases). Vertical bars denote 95 percent confidence intervals using robust standard errors clustered at the jurisdiction-election level. Panel 2 presents a bin scatter plot of case jurisdiction-level dismissal rates in the fourth year post-election by the Democratic candidate's margin of victory (or loss). The sample contains 134 jurisdiction-elections.

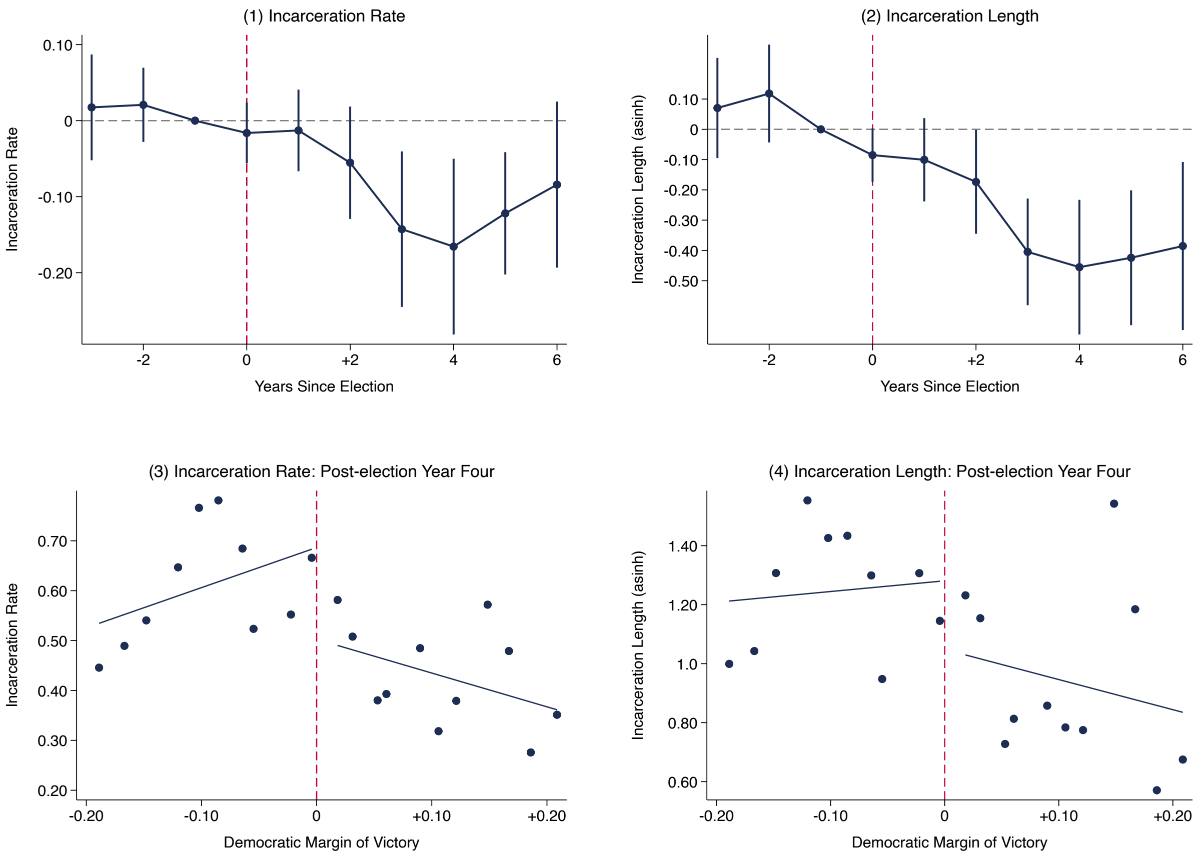

Figure 4: Panels 1 and 2 plot panel RD estimates showing the impact of a narrow Democratic election win on the probability of incarceration (panel 1) and incarceration length (panel 2) by year relative to the election. The specification is Equation 3. All coefficients are relative to the year prior to the election (period -1). The sample includes cases filed in jurisdiction-elections decided by 7.1 percentage points or less (N= 4,160,428). Vertical bars denote 95 percent confidence intervals using robust standard errors clustered at the jurisdiction-election level. Panels 3 and 4 shows bin scatter plots of jurisdiction-wide incarceration rates and mean sentence lengths in the fourth year post-election by the Democratic candidate's margin of victory (or loss). The sample contains 134 jurisdiction-elections.

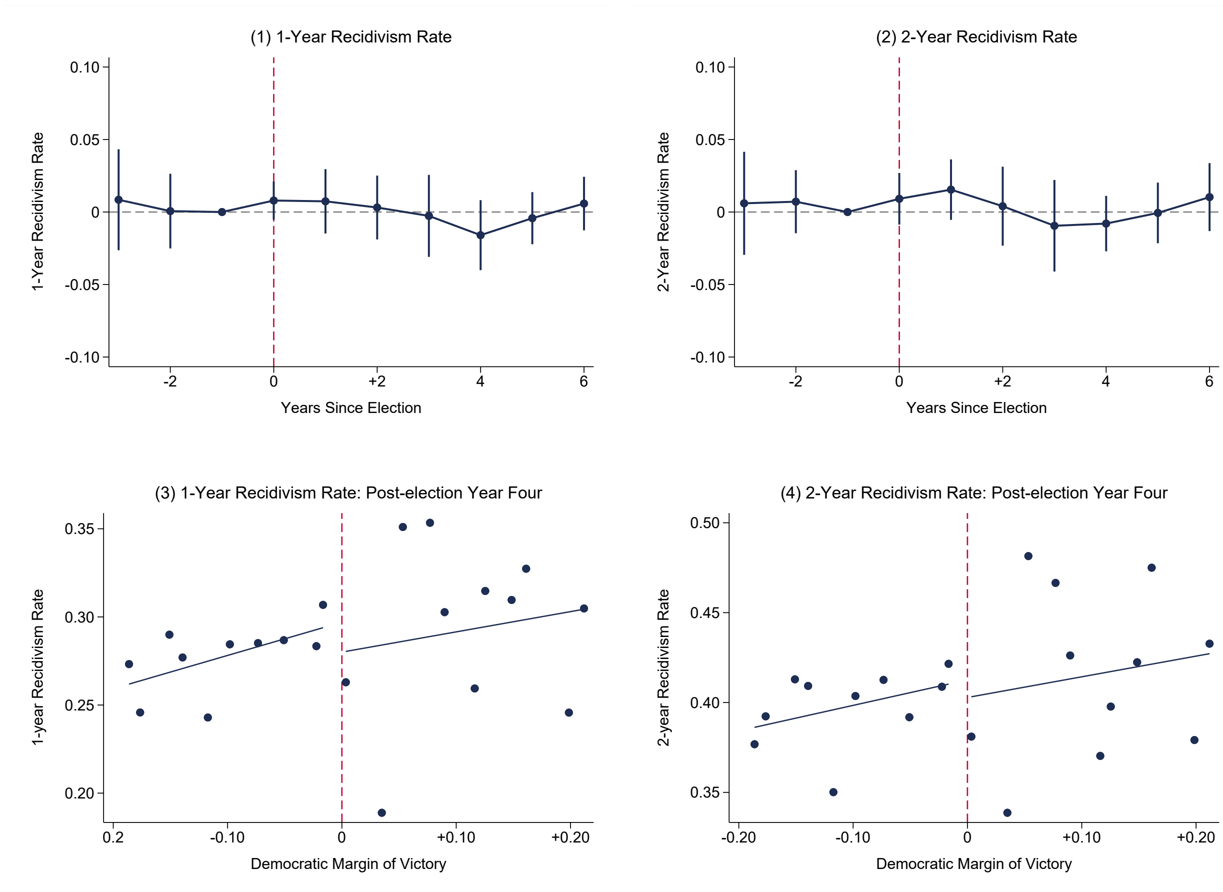

Figure 5: Panels 1 and 2 plot panel RD estimates showing the impact of a narrow Democratic election win on the probability of defendant recidivism within 1 year of initial arrest (panel 1) and the probability of defendant recidivism within 2 years of initial arrest (panel 2), by year relative to the election. The specification is Equation 3. All coefficients are relative to the year prior to the election (period -1). The sample includes cases filed in jurisdiction-elections decided by 7.1 percentage points or less (N=3,474,473 for panel 1, and 3,322,341 for panel 2). Vertical bars denote 95 percent confidence intervals using robust standard errors clustered at the jurisdiction-election level. Panels 3 and 4 shows bin scatterplots of jurisdiction-wide incarceration rates and mean sentence lengths in the fourth year post-election by the Democratic candidate's margin of victory (or loss). The sample contains 87 jurisdiction-elections.

Tables

Table 1: Summary of District Attorney Elections

	N	Mean	Median	Std Dev	Min	Max
I. Election Characteristics						
Election year	1,311	2008	2008	6	1996	2017
# of Candidates	1,311	1.21	1	0.43	1	4
# of Democrats	1,311	0.54	1	0.50	0	2
Contested Election?	1,311	0.19	0	0.40	0	1
II. Election Outcomes						
Democrat Won?	1,311	0.43	0	0.50	0	1
Republican Won?	1,311	0.55	1	0.50	0	1
Election Margin	255	0.18	0.14	0.17	0	1
Dem-Rep Margin	197	-0.06	-0.05	0.19	-0.55	0.36

The table summarizes outcomes from 1,311 district attorney elections held in Arkansas, Colorado, Kentucky, North Carolina, Texas, and Virginia between 1996 and 2017. The election margin describes the difference between the first- and second-place candidates' vote shares, irrespective of their political parties, 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 contested, and not all contested elections have both a Democratic and a Republican candidate.

Table 2: Criminal Case-level Descriptive Statistics by State

		State of Case Filing				
	All Cases	Arkansas	Colorado	North Carolina	Texas	Virginia
	(1)	(2)	(3)	(4)	(5)	(6)
I. Case Outcomes						
Case Dismissed?	0.37	0.19	0.29	0.58	0.31	0.43
	(0.48)	(0.39)	(0.45)	(0.49)	(0.46)	(0.49)
Incarceration?	0.47	0.56	0.37	0.15	0.62	0.28
	(0.50)	(0.50)	(0.48)	(0.36)	(0.48)	(0.45)
Incarceration Length (asinh)	0.99	2.08	0.91	0.21	1.31	0.55
	(1.50)	(2.26)	(1.51)	(0.71)	(1.63)	(1.03)
N:	*9,787,182*	*79,729*	*1,376,024*	*1,616,202*	*5,519,997*	*1,195,230*
1-year Recidivism	0.30	—	—	0.38	0.28	—
	(0.46)	—	—	(0.49)	(0.45)	—
N:	*6,896,017*	—	—	*1,474,430*	*5,421,587*	—-
2-year Recidivism	0.41	.	—	0.48	0.40	—
	(0.49)	—	—	(0.50)	(0.49)	—
N:	*6,352,006*	—	—	*1,205,969*	*5,146,037*	—-
II. Defendant Characteristics						
Age	32.33	35.60	32.47	33.29	31.97	—
	(11.09)	(14.51)	(11.21)	(11.81)	(10.76)	—
N:	*8,591,952*	*79,729*	*1,376,024*	*1,616,202*	*5,519,997*	—
Female	0.25	0.26	0.23	0.29	0.23	0.30
	(0.43)	(0.44)	(0.42)	(0.45)	(0.42)	(0.46)
Nonwhite	0.54	0.30	0.20	0.50	0.66	0.41
	(0.50)	(0.46)	(0.40)	(0.50)	(0.47)	(0.49)
III. Case Characteristics						
# of Charges	1.70	2.92	2.32	2.19	1.43	1.48
	(1.91)	(3.64)	(1.58)	(3.61)	(0.99)	(1.69)
Felony Offense?	0.34	0.27	0.42	0.22	0.36	0.32
	(0.47)	(0.44)	(0.49)	(0.41)	(0.48)	(0.47)
Property Offense?	0.35	0.37	0.31	0.45	0.34	0.34
	(0.48)	(0.48)	(0.46)	(0.50)	(0.47)	(0.47)
Violent Offense?	0.26	0.16	0.30	0.35	0.24	0.16
	(0.44)	(0.36)	(0.46)	(0.48)	(0.43)	(0.36)
Drug Offense?	0.27	0.24	0.21	0.24	0.31	0.22
	(0.44)	(0.42)	(0.41)	(0.43)	(0.46)	(0.41)
Traffic Offense?	0.07	0.19	0.25	0.03	0.04	0.03
	(0.25)	(0.40)	(0.43)	(0.17)	(0.20)	(0.17)
Other Offense?	0.38	0.52	0.64	0.30	0.34	0.43
	(0.49)	(0.50)	(0.48)	(0.46)	(0.47)	(0.49)
N:	*9,787,182*	*79,729*	*1,376,024*	*1,616,202*	*5,519,997*	*1,195,230*

Each cell reports the mean of the variable in the left-hand column, with standard deviations in parentheses. Column 1 describes all cases in our dataset (N=9,787,182). The remaining columns describe cases by the state in which they were filed. Empty cells reflect missing recidivism data and information on defendant age. Sample sizes vary within columns due to missing recidivism data. See the text for more detail on data missingness and sample construction.

Table 3: Comparing Democratic- and Republican-led DA Jurisdictions

		Democratic DAs		Republican DAs	
	Full Sample	Uncontested Election	Contested Election	Contested Election	Uncontested Election
	(1)	(2)	(3)	(4)	(5)
I. Jurisdiction Characteristics					
Population	171,319	117,029	342,808	376,969	133,534
	(391,646)	(186,787)	(637,433)	(804,165)	(225,494)
Share Nonwhite	0.24	0.25	0.24	0.24	0.24
	(0.19)	(0.22)	(0.22)	(0.18)	(0.17)
N:	*3,874*	*1,335*	*252*	*498*	*1,735*
Income per Capita ($2016)	38,888	34,877	40,597	40,104	41,369
	(10,516)	(10,031)	(11,159)	(10,314)	(10,010)
N:	*3,574*	*1,229*	*234*	*459*	*1,610*
II. Court Outcomes					
Annual Caseload	2,526	1,922	5,267	4,987	1,947
	(5,311)	(3,026)	(8,902)	(10,648)	(2,902)
Caseload per 1,000 Pop	17.2	17.2	18.6	17.9	16.7
	(10.1)	(10.5)	(9.7)	(11.2)	(9.3)
Case Dismissal Rate	0.37	0.41	0.39	0.35	0.35
	(0.12)	(0.13)	(0.11)	(0.13)	(0.12)
Incarceration Rate	0.44	0.40	0.39	0.45	0.48
	(0.19)	(0.18)	(0.17)	(0.20)	(0.20)
Avg. Sentence Length (asinh)	1.08	1.00	0.94	1.13	1.18
	(0.62)	(0.57)	(0.60)	(0.72)	(0.61)
N:	*3,874*	*1,335*	*252*	*498*	*1,735*

The data describe the characteristics and judicial outcomes among DA jurisdiction-years between 2000 and 2019. Column 1 reports the mean of the given variable across all jurisdiction-years (N=3,874). Columns 2 and 3 describe jurisdiction-years with serving Democratic DAs elected in uncontested (column 2) and contested (column 3) elections, while columns 4 and 5 describe jurisdiction-years with serving Republican DAs elected in uncontested (column 4) and contested (column 5) elections. Standard deviations appear in parentheses. Population data come from the Census intercensal estimates, while income per capita data come from the Bureau of Economic Analysis (BEA). Sample sizes vary within column because the BEA income data do not include many localities in Virginia. Sample sizes vary across columns because some DA election winners represent third parties, while others do not have an identified partisan affiliation.

Table 4: RD Validity: Election-year Differences in Jurisdiction and Case Characteristics

	Control Mean	Balance Estimate
	(1)	(2)
I. Jurisdiction Characteristics		
Population	807,711	57,216
	(1,310,659)	(653,494)
Caseload per 1,000 Pop	15.5	-1.1
	(6.3)	(3.2)
N:	*29*	*49*
Share Nonwhite	0.255	-0.006
	(0.212)	(0.124)
N:	*29*	*47*
Income per Capita ($2016)	44,299	5,278
	(11,351)	(6,253)
N:	*27*	*43*
II. Case Characteristics		
Defendant Age	32.2	0.2
	(10.9)	(0.4)
N:	*307,769*	*428,855*
Defendant Female?	0.232	-0.016
	(0.422)	(0.014)
Defendant Nonwhite?	0.680	0.019
	(0.466)	(0.073)
# of Charges	1.49	-0.01
	(1.11)	(0.15)
Felony Offense?	0.373	0.021
	(0.483)	(0.027)
Property Offense?	0.338	0.027
	(0.473)	(0.031)
Violent Offense?	0.226	0.040
	(0.418)	(0.030)
Drug Offense?	0.308	-0.051
	(0.462)	(0.046)
Traffic Offense?	0.062	-0.005
	(0.241)	(0.023)
Other Offense?	0.352	0.022
	(0.478)	(0.038)
N:	*313,870*	*444,159*

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

The sample includes election-year jurisdictions (N=49) and cases (N=444,160) for which the upcoming election is decided by 7.1 percentage points or less. Column 1 reports the mean of the outcome variable in the left-hand column among jurisdictions in which Republican candidates win the election. Standard deviations appear in parentheses. Column 2 presents cross-sectional estimates of the Democratic DA effect, following Equation 1. Robust standard errors clustered at the jurisdiction-election level appear in parentheses. Sample sizes vary within columns due to missing jurisdiction demographic and defendant age data, as discussed in the text.

Table 5: How Do Democratic DAs Affect Case Dispositions? Panel and Cross-sectional Estimates

	Sample Mean	Panel Estimates			Cross-sectional Estimates		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
I. Probability of Dismissal							
Case Dismissal	0.319	0.083**	0.075**	0.074**	0.147	0.080*	0.101**
	(0.466)	(0.035)	(0.033)	(0.034)	(0.095)	(0.042)	(0.039)
II. Incarceration Outcomes							
Incarceration	0.579	-0.097***	-0.092***	-0.098***	-0.084	-0.079**	-0.113***
	(0.493)	(0.032)	(0.030)	(0.03)	(0.104)	(0.037)	(0.041)
Incarceration Length (asinh)	1.134	-0.318***	-0.364***	-0.385***	0.010	-0.224**	-0.231***
	(1.521)	(0.062)	(0.057)	(0.063)	(0.189)	(0.093)	(0.105)
N:	*8,303,796*	*3,490,129*	*3,490,129*	*4,160,428*	*49*	*49*	*282*
Year FEs	Y	Y	Y	N	N	Y	
Jurisdiction FEs	Y	Y	Y	N	N	Y	
Defendant/Case Covariates	N	Y	Y	—	—	—	
Period(s) Included	[-3,4]	[-3,4]	[-3,6]	4	[-3,4]	[1,6]	
Unit of Observation	Case	Case	Case	Election	Election	Election-period	

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

The sample includes criminal cases filed between pre-election period -3 and post-election period +6 in jurisdiction-elections decided by 7.1 percentage points or less. Column 1 provides the sample mean of each outcome in the left-hand column. Standard deviations appear in parentheses. Columns 2-4 report panel estimates of the post-election effect of a Democratic DA victory over the four years following an election (columns 2 and 3) and over the six years following an election (column 4). The panel specification is Equation 2. Columns 5-7 report cross-sectional estimates of the impact of a Democratic DA victory on jurisdiction-wide mean outcomes the fourth year following an election (column 5), in the difference between pre- and post-election outcomes (column 6), and over the six years post-election (column 7). The cross-sectional RD specification is Equation 1. All cross-sectional specifications employ jurisdiction population weights. All panel and cross-sectional specifications include linear controls for the Democratic candidate's margin of victory (or loss) and its interaction with an indicator for whether a Democratic candidate won the election. Additional defendant and case covariates in the panel specification include indicators for whether a defendant is nonwhite or female, whether the case includes felony, property, violent, traffic, drug, or other charges, and the total number of charges on the case. Robust standard errors clustered at the jurisdiction-election level appear in parentheses.

Table 6: Robustness of Panel Estimates to Alternative Specifications and Samples

	Baseline Specification (1)	20 Percent Narrower BW (2)	100 Percent Wider BW (3)	Quadratic Polynomial (4)	"Donut" of ± 1pp (5)
Case Dismissal	0.083**	0.066***	0.050**	0.089**	0.150***
	(0.035)	(0.022)	(0.020)	(0.039)	(0.048)
Incarceration	-0.097***	-0.077***	-0.042*	-0.105***	-0.141***
	(0.032)	(0.020)	(0.025)	(0.037)	(0.052)
Incarceration Length (asinh)	-0.318***	-0.282***	-0.218***	-0.340***	-0.392***
	(0.062)	(0.058)	(0.056)	(0.075)	(0.101)
N:	*3,490,121*	*2,721,254*	*5,527,517*	*3,490,121*	*2,688,401*
Year FEs	Y	Y	Y	Y	Y
Jurisdiction FEs	Y	Y	Y	Y	Y
Defendant/Case Covariates	N	N	N	N	N
Periods Included	[-3,4]	[-3,4]	[-3,4]	[-3,4]	[-3,4]
"Close" Election Margin	± 7.1pp	± 5.7pp	± 14.2pp	± 7.1pp	± 7.1pp

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

The sample includes criminal cases filed between pre-election period -3 and post-election period +4. Each cell reports a point estimate capturing the post-election effect of a Demcoratic DA victory. Our baseline specification is Equation 2, estimated among cases filed in jurisdiction-elections decided by less than our optimal bandwidth of 7.1 percentage points. Columns 2, 3, and 5 present estimates from Equation 2 estimated on alternative samples described in the header. Column 4 presents results from a modified version of Equation 2 that includes a quadratic control for the election margin, interacted with an indicator for whether a Democrat won the election. Robust standard errors clustered at the jurisdiction-election level appear in parentheses.

Table 7: How Do Democratic DAs Affect Recidivism Rates? Panel and Cross-sectional Estimates

	Sample Mean	Panel Estimates			Cross-sectional Estimates		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1-year Recidivism	0.290	-0.004	0.002	-0.001	-0.011	-0.011	-0.015*
	(0.454)	(0.005)	(0.005)	(0.004)	(0.041)	(0.028)	(0.008)
N:	*3,474,473*	*2,920,924*	*2,920,924*	*3,474,473*	*31*	*28*	*173*
2-year Recidivism	0.411	-0.002	0.002	-0.001	-0.057***	-0.010	-0.022*
	(0.492)	(0.005)	(0.005)	(0.005)	(0.015)	(0.034)	(0.011)
N:	*3,322,341*	*2,815,563*	*2,815,563*	*3,322,341*	*28*	*28*	*163*
Year FEs	Y	Y	Y	N	N	Y	
Jurisdiction FEs	Y	Y	Y	N	N	Y	
Defendant/Case Covariates	N	Y	Y	—	—	—	
Period(s) Included	[-3,4]	[-3,4]	[-3,6]	4	[-3,4]	[1,6]	
Unit of Observation	Case	Case	Case	Election	Election	Election-period	

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

The sample includes criminal cases filed between pre-election period -3 and post-election period +6 in jurisdiction-elections decided by 7.1 percentage points or less. Column 1 provides the sample mean of each outcome in the left-hand column. Standard deviations appear in parentheses. Columns 2 and 3 report panel estimates of the post-election effect of a Democratic DA victory over the four years following an election. The panel specification is Equation 2. Columns 5-7 report cross-sectional estimates of the impact of a Democratic DA victory on jurisdiction-wide mean outcomes in the fourth year following an election (column 5), in the difference between pre- and post-election outcomes (column 6), and over the six years post-election (column 7). The cross-sectional specification is Equation 1. All cross-sectional specifications employ jurisdiction population weights. All panel and cross-sectional specifications include a linear control for the Democratic candidate's margin of victory (or loss) and its interaction with an indicator for whether a Democratic candidate won the election. Additional defendant and case covariates are described below Table 5. Robust standard errors clustered at the jurisdiction-election level appear in parentheses.

Table 8: Heterogeneity in Democratic DA Treatment Effects: Panel Estimates

		Defendant Characteristics					
	All	Race/Ethnicity		Gender		Age	
	Cases	Nonwhite	White	Female	Male	Age≤30	Age>30
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Case Dismissal	0.083**	0.093***	0.078**	0.095**	0.077**	0.082**	0.080**
	(0.031)	(0.030)	(0.038)	(0.045)	(0.032)	(0.038)	(0.032)
	0.315	*0.306*	*0.334*	*0.398*	*0.290*	*0.326*	*0.305*
Incarceration	-0.097***	-0.118***	-0.071**	-0.113**	-0.090***	-0.109***	-0.086***
	(0.032)	(0.027)	(0.034)	(0.043)	(0.029)	(0.036)	(0.029)
	0.602	*0.650*	*0.503*	*0.511*	*0.629*	*0.597*	*0.607*
Incarceration Length (asinh)	-0.318***	-0.348***	-0.280***	-0.247***	-0.330***	-0.340***	-0.295***
	(0.062)	(0.054)	(0.077)	(0.071)	(0.059)	(0.063)	(0.067)
	1.149	*1.211*	*1.023*	*0.808*	*1.253*	*1.085*	*1.211*
N:	*3,490,121*	*2,347,973*	*1,142,148*	*814,374*	*2,675,747*	*1,711,906*	*1,778,215*

	Case Characteristics						
	Case Severity		Violence		Types of Offense		
	Felony	Misdemeanor	Violent	Nonviolent	Property	Drug	Other
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Case Dismissal	0.105*	0.072**	0.058*	0.092**	0.096***	0.067*	0.101**
	(0.054)	(0.032)	(0.034)	(0.037)	(0.034)	(0.040)	(0.041)
	0.283	*0.334*	*0.390*	*0.292*	*0.285*	*0.273*	*0.316*
Incarceration	-0.108*	-0.091**	-0.067**	-0.107***	-0.110***	-0.061*	-0.134***
	(0.056)	(0.038)	(0.031)	(0.033)	(0.031)	(0.035)	(0.044)
	0.650	*0.574*	*0.536*	*0.623*	*0.646*	*0.658*	*0.554*
Incarceration Length (asinh)	-0.648***	-0.254***	-0.329***	-0.315***	-0.391***	-0.246***	-0.0376***
	(0.150)	(0.047)	(0.074)	(0.063)	(0.071)	(0.077)	(0.068)
	2.116	*0.594*	*1.419*	*1.064*	*1.178*	*1.301*	*0.990*
N:	*1,273,577*	*2,216,544*	*839,378*	*2,650,743*	*1,189,213*	*1,004,548*	*1,264,743*
Year FEs	Y	Y	Y	Y	Y	Y	Y
Jurisdiction FEs	Y	Y	Y	Y	Y	Y	Y
Defendant/Case Covariates	N	N	N	N	N	N	N
Periods Included	[-3,4]	[-3,4]	[-3,4]	[-3,4]	[-3,4]	[-3,4]	[-3,4]

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

The sample includes criminal cases filed between pre-election period -3 and post-election period +4 in jurisdiction-elections decided by 7.1 percentage points or less. Each cell reports panel estimates of the effect of a Democratic DA victory, following Equation 2. Each coefficient comes from a separate regression, estimated among the subsample of cases described in the column header. Robust standard errors clustered at the jurisdiction-election level appear in parentheses. The subsample mean of the outcome variable in the left-hand column appears in italics below the standard error.

Table 9: Assessing the Average Effect of DA Partisanship: Matching Estimates

	Close-elections Sample			All Cases	
	Sample Mean	Regression Estimates	Matching Estimates	Sample Mean	Matching Estimates
	(1)	(2)	(3)	(4)	(5)
Case Dismissal	0.363	0.101**	0.100**	0.367	0.067***
	(0.481)	(0.039)	(0.042)	(0.482)	(0.022)
Incarceration	0.556	-0.113***	-0.112**	0.466	-0.085**
	(0.497)	(0.041)	(0.054)	(0.499)	(0.034)
Incarceration Length (asinh)	1.057	-0.231***	-0.099	0.988	-0.179***
	(1.487)	(0.081)	(0.098)	(1.504)	(0.060)
N:	*2,453,178*	*282*	*1,858,927*	*9,787,182*	*7,979,231*
1-year Recidivism	0.291	-0.007	-0.006	0.302	0.006
	(0.454)	(0.007)	(0.009)	(0.459)	(0.007)
N:	*2,044,758*	*173*	*1,706,426*	*6,896,017*	*6,152,973*
2-year Recidivism	0.410	-0.014	-0.000	0.414	0.006
	(0.492)	(0.009)	(0.008)	(0.492)	(0.008)
N:	*1,892,626*	*163*	*1,563,230*	*6,352,006*	*5,686,948*

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

Columns 1-3 include observations filed between post-election periods +1 and +6 in jurisdiction-elections decided by 7.1 percentage points or less. Column 1 provides the sample mean of the each outcome in the left-hand column. Standard deviations appear in parentheses. Column 2 reproduces cross-sectional RD estimates from column 7 of Table 5 and column 6 of Table 7. The specification in Equation 1, estimated on jurisdiction-election-period data with jurisdiction and year fixed effects. Column 3 presents matching results using the same sample of cases as in column 1, following the approach described in the text. Columns 4 and 5 include all cases in our sample. Column 4 provides the sample mean, while column 5 presents matching estimates following the same design as in column 3. The differences in sample sizes between columns 1 and 3, as well as between columns 4 and 5, represent observations that were not matched in our matching process. Robust standard errors clustered at the jurisdiction-election level appear in parentheses in columns 2, 3, and 5.

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Appendix

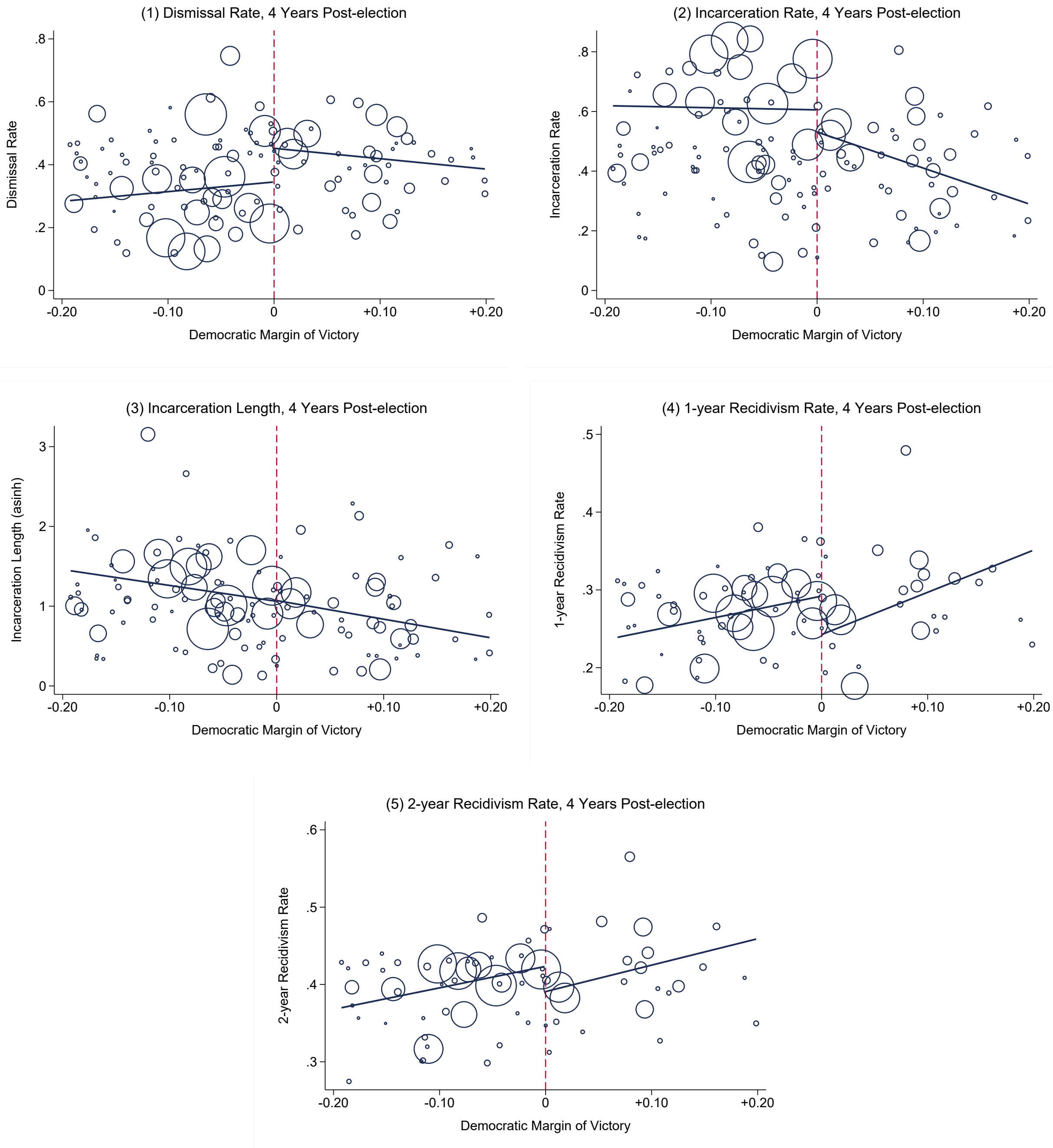

Figure A1: Scatter plots show the jurisdiction-level mean outcome in the fourth year after a contested DA election, along with lines of best fit. Each point corresponds to a single jurisdiction-election. Larger points correspond to more populous jurisdictions. For panels 1-3, N=134 jurisdiction-elections. For panels 4 and 5, N=87 jurisdiction-elections.

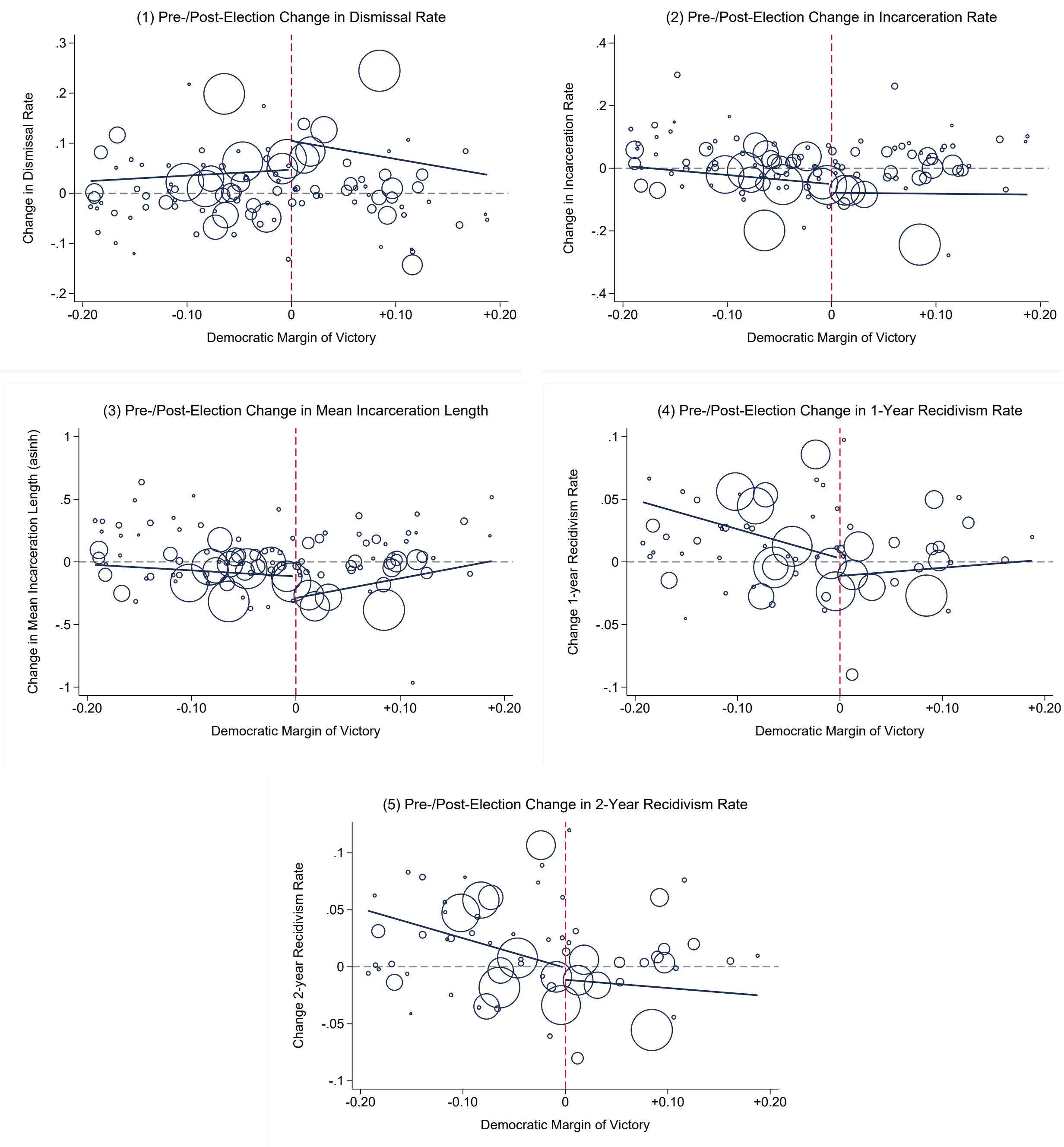

Figure A2: Scatter plots show the jurisdiction-level change in mean outcome between the three years leading up to the election and the four years following the election, along with lines of best fit. Each point corresponds to a single jurisdiction-election. Larger points correspond to more populous jurisdictions. For panels 1-3, N=134 jurisdiction-elections. For panels 4 and 5, N=87 jurisdiction-elections.

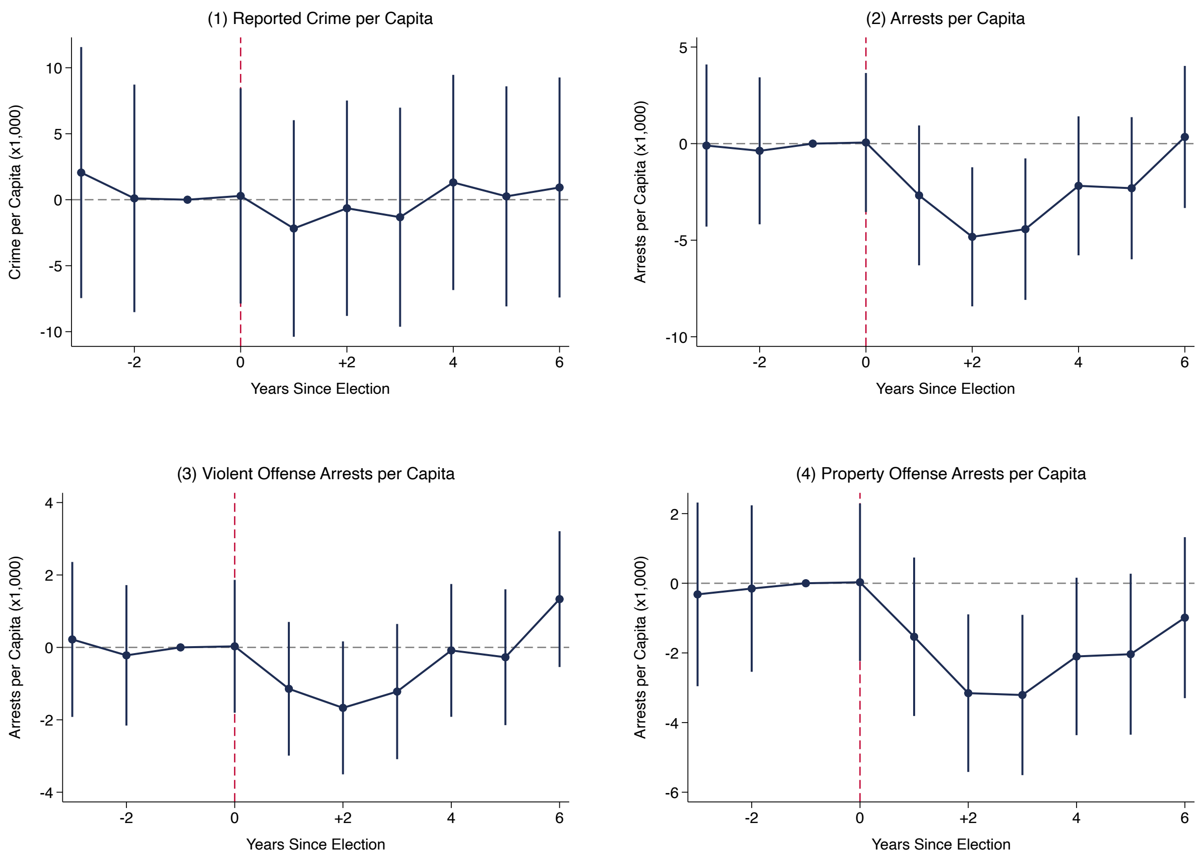
(1) Reported Crime per Capita
Crime per Capita (x1,000)
10
5
0
-5
-10
-2
0
+2
4
6
Years Since Election
(2) Arrests per Capita
Arrests per Capita (x1,000)
5
0
-5
-10
-2
0
+2
4
6
Years Since Election
(3) Violent Offense Arrests per Capita
Arrests per Capita (x1,000)
4
2
0
-2
-4
-2
0
+2
4
6
Years Since Election
(4) Property Offense Arrests per Capita
Arrests per Capita (x1,000)
2
0
-2
-4
-6
-2
0
+2
4
6
Years Since Election

Figure A3: .

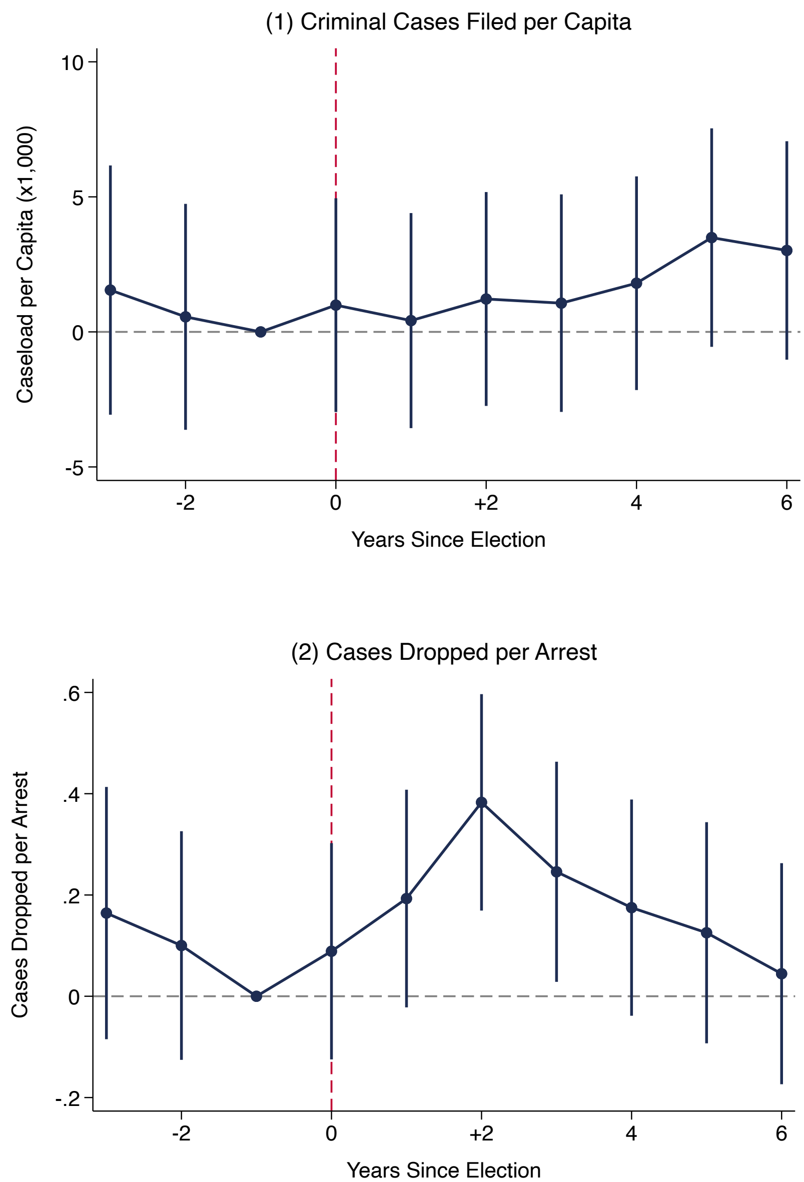
(1) Criminal Cases Filed per Capita
Caseload per Capita (x1,000)
10
5
0
-5
-2
0
+2
4
6
Years Since Election
(2) Cases Dropped per Arrest
Cases Dropped per Arrest
.6
.4
.2
0
-.2
-2
0
+2
4
6
Years Since Election

Figure A4: .

Table A1: Criminal Case-level Descriptive Statistics by DA Partisanship

		DA Partisanship	
	Full Sample	Democrat	Republican
	(1)	(2)	(3)
I. Case Outcomes			
Case Dismissal	0.37	0.43	0.33
	(0.48)	(0.49)	(0.47)
Incarceration	0.47	0.39	0.52
	(0.50)	(0.49)	(0.50)
Incarceration Length (asinh)	0.99	0.82	1.11
	(1.50)	(1.40)	(1.56)
N:	9,787,182	3,892,610	5,856,861
1-year Recidivism	0.30	0.31	0.30
	(0.46)	(0.46)	(0.46)
N:	6,896,017	2,661,861	4,234,156
2-year Recidivism	0.41	0.42	0.41
	(0.49)	(0.49)	(0.49)
N:	6,352,006	2,425,282	3,926,724
II. Defendant Characteristics			
Age	32.33	32.41	32.28
	(11.09)	(11.21)	(11.02)
N:	8,591,952	3,286,171	5,305,781
Female?	0.25	0.25	0.25
	(0.43)	(0.43)	(0.43)
Nonwhite?	0.54	0.58	0.51
	(0.50)	(0.49)	(0.50)
III. Case Characteristics			
# of Charges	1.70	1.73	1.68
	(1.91)	(2.22)	(1.68)
Felony?	0.34	0.34	0.34
	(0.47)	(0.47)	(0.47)
Property Offense?	0.35	0.36	0.35
	(0.48)	(0.48)	(0.48)
Violent Offense?	0.26	0.27	0.25
	(0.44)	(0.44)	(0.43)
Drug Offense?	0.27	0.26	0.28
	(0.44)	(0.44)	(0.45)
Traffic Offense?	0.07	0.07	0.07
	(0.25)	(0.25)	(0.26)
Other Offense?	0.38	0.39	0.38
	(0.49)	(0.49)	(0.49)
N:	9,787,182	3,892,610	5,856,861

Each cell reports the mean of the variable in the left-hand column, with standard deviations in parentheses. Column 1 describes all cases in our dataset (N=9,787,182). Column 2 focuses on cases filed in jurisdiction-years in which a Democratic DA held office (N=3,892,610). Column 3 focuses on cases filed in jurisdiction-years in which a Republican DA held office (N=5,856,861). Sample sizes vary within columns because of missing recidivism and defendant age information in particular states. See the text for more detail on data missingness and sample construction.

Table A2: Criminal Case-level Descriptive Statistics Across Samples

	Full Sample	Contested Races	RD Sample
	(1)	(2)	(3)
I. Case Outcomes			
Case Dismissed?	0.37	0.34	0.36
	(0.48)	(0.47)	(0.48)
Incarceration?	0.47	0.56	0.56
	(0.50)	(0.50)	(0.50)
Incarceration Length (asinh)	0.99	1.11	1.06
	(1.50)	(1.52)	(1.49)
N:	*9,787,182*	*5,121,134*	*2,453,178*
1-year Recidivism	0.30	0.29	0.29
	(0.46)	(0.46)	(0.45)
N:	*6,896,017*	*4,238,301*	*2,044,758*
2-year Recidivism	0.41	0.41	0.41
	(0.49)	(0.49)	(0.49)
N:	*6,352,006*	*3,963,392*	*1,892,626*
II. Defendant Characteristics			
Age	32.33	32.40	32.65
	(11.09)	(11.03)	(11.18)
N:	*8,591,952*	*4,887,943*	*2,399,296*
Female	0.25	0.24	0.23
	(0.43)	(0.43)	(0.42)
Nonwhite	0.54	0.64	0.67
	(0.50)	(0.48)	(0.47)
III. Case Characteristics			
# of Charges	1.70	1.54	1.51
	(1.91)	(1.47)	(1.31)
Felony Offense?	0.34	0.35	0.36
	(0.47)	(0.48)	(0.48)
Property Offense?	0.35	0.34	0.35
	(0.48)	(0.48)	(0.48)
Violent Offense?	0.26	0.25	0.26
	(0.44)	(0.43)	(0.44)
Drug Offense?	0.27	0.28	0.28
	(0.44)	(0.45)	(0.45)
Traffic Offense?	0.07	0.05	0.06
	(0.25)	(0.22)	(0.23)
Other Offense?	0.38	0.36	0.37
	(0.49)	(0.48)	(0.48)
N:	*9,787,182*	*5,121,134*	*2,453,178*

Each cell reports the mean of the variable in the left-hand column, with standard deviations in parentheses. Column 1 describes all cases in our dataset. The remaining columns describe cases filed in the six years post-election, by the degree of election competitiveness. The RD sample includes cases filed after elections decided by 7.1 percentage points or less. Sample sizes vary within columns due to missing data on recidivism and defendant age. See the text for more detail on data missingness and sample construction.

Table A3: Heterogeneity in Democratic DA Treatment Effect on Recidivism: Panel RD Estimates

		Defendant Characteristics					
	All	Race/Ethnicity		Gender		Age	
	Cases	Nonwhite	White	Female	Male	Age≤30	Age>30
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1-year Recidivism	-0.004	-0.007	0.005	0.006	-0.006	-0.001	-0.009
	(0.005)	(0.004)	(0.009)	(0.007)	(0.005)	(0.006)	(0.006)
	0.288	*0.279*	*0.318*	*0.249*	*0.300*	*0.307*	*0.269*
N:	*2,920,924*	*2,208,175*	*712,749*	*673,428*	*2,247,496*	*1,485,648*	*1,435,276*
2-year Recidivism	-0.002	-0.011**	0.020*	0.000	-0.002	0.002	-0.010**
	(0.005)	(0.004)	(0.011)	(0.009)	(0.005)	(0.007)	(0.005)
	0.411	*0.400*	*0.444*	*0.357*	*0.426*	*0.439*	*0.381*
N:	*2,815,563*	*2,815,563*	*685,730*	*649,089*	*2,166,474*	*1,438,237*	*1,377,326*

	Case Characteristics						
	Case Severity		Violence		Types of Offense		
	Felony	Misdemeanor	Violent	Nonviolent	Property	Drug	Other
	(9)	(10)	(11)	(12)	(13)	(14)	(15)
1-year Recidivism	0.013	-0.009*	-0.005	-0.003	-0.001	0.007	-0.018***
	(0.012)	(0.005)	(0.007)	(0.006)	(0.010)	(0.010)	(0.005)
N:	*0.252*	*0.308*	*0.212*	*0.312*	*0.346*	*0.290*	*0.261*
	1,058,852	*1,862,072*	*695,777*	*2,225,147*	*1,008,306*	*878,917*	*932,016*
2-year Recidivism	0.017	-0.012*	0.000	-0.004	-0.003	0.013	-0.021**
	(0.012)	(0.006)	(0.006)	(0.011)	(0.006)	(0.010)	(0.009)
	0.389	*0.423*	*0.318*	*0.439*	*0.469*	*0.427*	*0.378*
N:	*1,019,120*	*1,796,443*	*663,877*	*2,151,686*	*974,833*	*849,920*	*895,719*
Year FEs	Y	Y	Y	Y	Y	Y	Y
Jurisdiction FEs	Y	Y	Y	Y	Y	Y	Y
Defendant/Case Covariates	N	N	N	N	N	N	N
Periods Included	[-3,4]	[-3,4]	[-3,4]	[-3,4]	[-3,4]	[-3,4]	[-3,4]

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

The sample includes criminal cases filed between pre-election period -3 and post-election period +4 in jurisdiction-elections decided by 7.1 percentage points or less. Each cell reports panel RD estimates of the effect of a Democratic DA victory, following Equation 2. Each coefficient comes from a separate regression, estimated among the subsample of cases described in the column header. Robust standard errors clustered at the jurisdiction-election level appear in parentheses. The subsample mean of the outcome variable in the left-hand column appears in italics below the standard error of each estimate. Sample sizes vary within columns due to missing recidivism data.

Table A4: Specification Robustness: Case-level Estimates of Democratic DA Effect

	Case-level Cross-sectional RD				Case-level Panel RD			
	Period 4 only (1)	+ Periods 1 thru 3 (2)	+ Jurisdiction FEs (3)	+ Year FEs (4)	+ Period 0 (5)	Preferred Specification (6)	+ Covariates (7)	+ Periods 5 and 6 (8)
Case Dismissed	0.16*	0.10	0.07	0.09**	0.08*	0.08**	0.07**	0.07**
	(0.08)	(0.09)	(0.11)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)
Incarceration	-0.08	-0.00	-0.07	-0.09*	-0.08**	-0.10***	-0.09***	-0.10***
	(0.11)	(0.10)	(0.10)	(0.05)	(0.04)	(0.03)	(0.03)	(0.03)
Incarceration Length (asinh)	-0.00	0.18	-0.25	-0.18	-0.19**	-0.32***	-0.36***	-0.38***
	(0.20)	(0.19)	(0.35)	(0.12)	(0.08)	(0.06)	(0.06)	(0.06)
N:	433,090	1,782,871	1,782,871	1,782,871	2,227,033	3,490,121	3,490,121	4,160,428
1-year Recidivism	-0.04	-0.03**	0.01	-0.01**	-0.01	-0.00	0.00	-0.00
	(0.03)	(0.01)	(0.01)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)
N:	342,142	1,491,209	1,491,209	1,491,209	1,863,078	2,920,924	2,920,924	3,474,473
2-year Recidivism	-0.06***	-0.03**	0.00	-0.02***	-0.00	-0.00	0.00	-0.00
	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)
N:	295,725	1,385,848	1,385,848	1,385,848	1,757,717	2,815,563	2,815,563	3,322,341
Year FEs	N	N	N	Y	Y	Y	Y	Y
Jurisdiction FEs	N	N	Y	Y	Y	Y	Y	Y
Defendant/Case Covariates	N	N	N	N	N	N	Y	Y
Period(s) Included	4	[1-4]	[1-4]	[1-4]	[0-4]	[-3-4]	[-3-4]	[-3,6]
Unit of Observation	Case	Case	Case	Case	Case	Case	Case	Case

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

Each cell comes from a separate regression. The sample consists of cases filed in jurisdiction-elections decided by 7.1 percentage points or less. The specification in columns 1-4 is Equation 1, applied to case-level data. The specification in columns 5-8 is Equation 2, applied to different samples of election-periods. Sample sizes differ within columns due to missing recidivism data, as discussed in the text. Case-level covariates are described in the footnote to Table 5. Standard errors in parentheses are clustered at jurisdiction-election level.