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

Brett Fischer and Tyler Ludwig†

August 26, 2024

Abstract

Elected district attorneys (DAs) have wide discretion in choosing whether or not to pursue criminal charges against arrested individuals, yet how they exercise this power remains poorly understood. In this paper, we evaluate the causal effect of DA political partisanship on criminal case conviction rates, sentencing outcomes, and recidivism. Combining court records from seven states with local DA election returns, we analyze how DAs affect case outcomes using a close-elections difference-in-differences design, which homes in on changes within otherwise comparable jurisdictions that elect DAs from opposing parties. We find that the marginal Democratic DA is 24 percent more likely to dismiss criminal cases than their Republican counterparts and 15 percent less likely to incarcerate defendants. Complementary matching estimates indicate that the average Democratic DA also increases dismissal rates and lowers incarceration rates, albeit to a lesser extent than DAs elected in closely contested jurisdictions. Strikingly, we find that defendants in Democratic-led jurisdictions are no more likely to recidivate and face future criminal charges, consistent with the notion that higher conviction and incarceration rates have limited deterrence effects. Our findings shed new light on the complex interplay between partisan politics, the implementation of the law at the local level, and the overall efficacy of the criminal justice system.

JEL Classifications: D72, K14, K42

---

*The authors extend their sincere thanks to Amanda Agan, Leora Friedberg, and Jesse Rothstein for all their incredible feedback, as well as to Kerri Raissian and participants at the APPAM annual conference. We also thank Brandon Garrett and the Wilson Center for Science and Justice for their invaluable help in obtaining the data used in this study. Tyler Ludwig is grateful for financial support from the Bankard Fund for Political Economy. The authors declare that they have no relevant or material financial interests that relate to the research described in this paper. Any mistakes are ours.

†Brett Fischer (corresponding author): Mathematica, 1100 First Street NE, Washington, DC, 20002, brettfischer99@gmail.com; Tyler Ludwig: University of Texas at Austin, 158 W 21st St STOP A1800, Austin, TX 78712, tjludwig@utexas.edu

1 Introduction

Each year, over 17 million criminal cases enter court systems across the United States—at least one for every 15 American adults (Court Statistics Project 2020). The sheer volume of cases empowers local district attorneys (DAs) with near-total discretion to choose how many cases to pursue to a conviction, at the expense of scarce judicial resources, and how many to dismiss, at a potential social cost if non-conviction spurs future criminal behavior (Alschuler 1968; Bibas 2004; Bowers 2010; Stith 2008).[1,2] 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 2017; Agan et al. 2024; Dobbie, Goldin, and Yang 2018; Garin et al. 2023; Kling 2006; Mueller-Smith 2015).

But a DA's choice of conviction rate also has political ramifications. Elected prosecutors must answer to their voters, who consistently view high crime rates as a major policy problem (Gramlich 2016; McCarthy 2020). A median voter framework suggests that all DAs, regardless of their political identities, might pursue high conviction rates as a visible signal of their toughness to the median, crime-averse voter, consistent with Enns (2014). 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' attitudes diverge (Gramlich 2021; McCarthy 2020; Yokley 2021). Despite this theoretical ambiguity, and the central role played by DAs in the criminal justice system, there is limited 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. By "district attorney," we mean, broadly, the chief prosecuting authority in a county or judicial district; 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.

2. 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 result in either dismissal—whereby the accused person is released without penalty—or conviction. Vanishingly few cases result in a not-guilty verdict. As such, throughout this paper, we assume cases that are not dismissed result in conviction.

cratic and Republican DAs exercise their prosecutorial authority, and ultimately assess how those differences shape the efficacy of local criminal 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. While informative, 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 criminal convictions (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 1999-2021.[4] Crucially, these data include criminal cases that prosecutors ultimately dropped, allowing us to measure rates of case dismissal. Matching these court data to elections returns, we can connect case outcomes to the partisanship of the serving DA at the time of case filing.

However, jurisdictions that elect Democratic DAs might differ systematically from those that elect Republican DAs—particularly when they win office in uncontested elections, as is the case for 79 percent of the elected DAs in our sample. To address this endogeneity concern, we use narrowly-decided DA elections to home in on otherwise comparable jurisdictions that elect DAs from opposing parties. Logically, by focusing on the most politically competitive jurisdictions where both parties have a shot at winning elections, we minimize the baseline differences between court systems in our sample served by Democratic and Re-

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

4. Specifically, we use data from Arkansas, Colorado, Kentucky, Maryland, 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 account for roughly one-quarter of all DAs nationwide, as well as a range of urban and rural jurisdictions. We detail our sample of court systems and elections in Section 2.

publican DAs.[5] Indeed, we find that winning DA partisanship in narrowly-decided elections is not significantly correlated with average election-year case, defendant, and jurisdiction characteristics, which supports our claim 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 variation with a difference-in-differences framework. This approach 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 model with a matching design that leverages our detailed case-level data to examine the external validity our close-election identification strategy and to expand our sample beyond a small group of politically competitive jurisdictions. 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 estimate the average impact of DA partisanship.

Close-elections difference-in-differences estimates show that, relative to Republican DAs, the marginal Democratic DA is 24 percent (8 percentage points) *more* likely to dismiss incoming criminal cases (p-value < 0.01). Similarly, cases filed in jurisdictions led by Democratic DAs are 14 percent (8.8 percentage points) *less* likely to result in incarceration (p-value $<$ 0.001). We also find that electing a Democratic DA leads to 33 percent shorter incarceration sentences on average (about 3 fewer months spent in jail or prison), in line with prior findings (p-value < 0.001).[6] These effects remain consistent under different choices of controls and narrower definitions of "close" elections; heterogeneity analyses do not reveal significant differences in these effects across types of cases and accused individuals. Expanding our attention to cases from all jurisdictions, our matching results indicate that the aver-

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 a convicted person might face. DAs can also recommend particular punishments as part of plea deals. 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.

age Democratic DA dismisses 13 percent more cases—a smaller impact than the marginal Democratic DA, but economically meaningful nonetheless.

Democratic DAs' high dismissal and low incarceration rates likely serve the interests of defendants, saving them the social and economic penalties that come with a criminal conviction. Yet, it is unclear whether this agenda serves the interest of justice. One body of research suggests that, at the margin of dismissal and prosecution, a prosecutor's decision to dismiss a case could actually promote public safety. In particular, Agan, Doleac, and Harvey (2023) and Humphries et al. (2023) show how non-prosecution (dismissal) of misdemeanor (felony) charges reduces the probability of re-arrest, while Augustine et al. (2022) and Mueller-Smith and Schnepel (2020) find that diverting felony cases diminishes the likelihood that defendants face future criminal charges. Conversely, other recent studies—typified by Jordan, Karger, and Neal (2022) and Rose and Shem-Tov (2021)—argue that, at the margin of incarceration, jail and prison sentences reduce the probability that an individual re-offends. Motivated by this tension in the literature, we examine how Democratic DAs, whom we have shown to simultaneously lower conviction and incarceration rates, affect the rates at which defendants recidivate and face future criminal charges.

We find that, relative to the marginal Republican DA, 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. Point estimates have negative signs and fairly tight confidence intervals, such that we can rule out increases in recidivism of more than 1 percent of the sample mean, both within 1 year and 2 years of initial case filing (or 0.3 and 0.5 percentage-point increases, respectively).[7] We stress that these estimates do not isolate the causal effects of case dismissal or non-carceral sentencing per se; rather, our results just deliver the reduced-form effect of Democratic district attorneys on recidivism

7. Certain specifications that we include to probe the robustness of our findings actually yield precisely-estimated declines in recidivism following Democratic DA election victories. 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.

rates, which could operate through myriad policy channels.[8] Still, our null findings indicate that DA partisanship, despite driving substantial differences in judicial outcomes, does not affect re-offense rates.[9] That disconnect supports the notion that criminal conviction and incarceration might have limited deterrence effects.

Taken together, our findings suggest that DA partisanship matters and represents a key determinant of the punitiveness of criminal justice systems. In that sense, we provide further evidence that local politicians play pivotal yet underappreciated roles in driving institutional outcomes, building on studies of mayors (Dippel 2022), city councilors (Beach and Jones 2017; Beach et al. 2024), and school board members (Fischer 2023; Macartney and Singleton 2018; Shi and Singleton 2023).

Our results also speak to two strands of research on the criminal justice system. First, by providing rigorous evidence that district attorney political identities substantially influence conviction and incarceration rates, we add to a growing body of work that highlights the degree to which criminal case outcomes depend not on the facts but on idiosyncratic courtroom actors, including judges (Cohen and Yang 2019; Ash and Macleod 2021), defense attorneys (Agan, Freedman, and Owens 2021; Shem-Tov 2022), and assistant prosecutors (Sloan 2020; Tuttle 2023). Second, we provide a new, systemic 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 convictions and incarcerations without affecting re-offense rates. This is consistent with the argument that high conviction and incarceration rates may not be necessary to discourage criminal behavior. Our analysis thus contributes to an active debate not just in the literature, but in public discourse, on the relationship between criminal convictions, incarceration, and recidivism.

8. Interestingly, our results echo those of Ouss and Stevenson (2023), who find that a prosecutor-driven bail reform—wherein more arrested individuals secured pretrial release—had no impact on re-offense rates. Data limitations prevent us from commenting on how DA partisanship affects pretrial outcomes, but we believe our findings are complementary.

9. Likewise, supplemental results indicate that DA partisanship does not affect crime rates, as measured using Uniform Crime Reports data, a fact which reinforces our claim that DA partisanship has little effect on criminal behavior.

2 Background and Data

Our study examines the causal link between DA partisan affiliation and criminal case outcomes. To capture results that can be plausibly attributed to DA partisanship, and to deliver widely-applicable findings, we aimed to compile a multi-state dataset that linked DA election returns with detailed criminal justice records. However, few states provide the records of cases that did not result in conviction—a critical constraint that limits the scope of this study. As we detail below, our final dataset includes seven states with suitable policy environments and data: Arkansas, Colorado, Kentucky, Maryland, North Carolina, Texas, and Virginia. Together, these states contained about one-fifth of the U.S. population as of the 2010 Census and around a quarter of all district attorneys.[10] Though we attempted to collect criminal justice data from 2000 onwards, data availability varies across states: Arkansas records span 2000-2018, Colorado 2002-2018, Kentucky 2002-2018, Maryland 1999-2021, North Carolina 2013-2018, Texas 2000-2019, and Virginia 2008-2018.[11,12]

We begin by contextualizing district attorneys' role in the criminal process. We then summarize the elections data that we use to identify DA partisanship and describe how we derived our sample of criminal court records. After noting how we combined these datasets, we highlight differences between Democrat- and Republican-led court systems. Finally, we discuss how we construct our primary analytic dataset, a jurisdiction-election panel.

10. We reached out to numerous state agencies in an effort to obtain the widest sample possible. The seven states we include here met our needs and could provide data at reasonable cost. We also collected criminal justice data from Florida, New York, North Dakota, Oregon, and Pennsylvania, but could not use them in this study due to poor coverage of dismissed cases (New York and Pennsylvania), a lack of data with which to identify DA partisanship (North Dakota and Oregon), and a limited number of data-years (Florida). Our resulting dataset overlaps with those in recent studies that, like ours, attempt to gather court records from as many states as possible (for example, Dippel and Poyker [2019] and Feigenberg and Miller [2021]).

11. The vast majority of our sample predates the COVID-19 pandemic, except for a handful of jurisdiction-years in Maryland. Excluding those jurisdiction-years during and after the pandemic has no effect on our results.

12. These datasets generally cover all jurisdictions in their states, with two exceptions: our data from Colorado do not include the majority of cases filed in Denver County, and our data from Virginia do not include any cases filed in Arlington and Fairfax counties.

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

Voters in our seven sampled states elect district attorneys to lead public prosecutors' offices for four- to six-year terms. In this role, DAs represent the state against individuals accused of of felonies and most misdemeanor offenses. Each district attorney serves as the chief law enforcement officer in their judicial district, usually a single county or a grouping of small counties.[13] Many jurisdictions staff public prosecutors' offices with assistant district attorneys (ADAs), who often function as the day-to-day prosecuting attorneys on cases. Even in these settings, though, the district attorney retains control of overall prosecutorial policy and directs the ADAs.

A district attorney's principal responsibility is to decide whether to seek to convict individuals accused of criminal offenses. 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.[14] DAs face few constraints on their prosecutorial discretion. In fact, reformist prosecutors have declined to prosecute whole categories of offenses (e.g., drug possession). Should they decide to pursue a conviction, DAs can also influence the eventual sentence imposed, both via the choice of specific charges to pursue and through the plea bargaining process.

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 treat the political party of the most recent election winner as the party of the sitting DA. For six of the seven states in our sample, we directly observe candidates' party affiliations; for one state (Arkansas), we determine candidates' party affiliations using voter registration data.[15] From this information,

13. 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.

14. A notable exception is Virginia, where the police decide whether to charge an individual. Still, in all our sampled states, DAs have the authority to dismiss a case with no further penalty.

15. 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

we calculate the winning party's margin of victory as a share of the total votes cast in the election.[16] Note that one state in our sample (Kentucky) only reports results from contested elections; as we discuss in Section 4, the omission of uncontested elections does not affect our primary research design.

In Table I, we summarize all 1,447 DA elections that took place in our sampled states between 2000 and 2018. Republican candidates won most (55 percent) of these contests, while Democrats won 44 percent. In the remaining 1 percent of elections, either a third-party candidate won or we could not determine the party of the winning candidate. Notably, only 306 races (21 percent of the total) were contested by multiple candidates.[17] And even among these nominally competitive elections, most races had wide margins: on average, the winning candidate defeated her opponent by 19 percentage points (equivalent to a 59.5/40.5 vote split in a two-person contest). Focusing on the 245 "politically competitive" races that included at least one Democrat and one Republican—the sample of greatest interest for our purposes—we find that the average race resulted in a Democratic loss of 6 percentage points. In Section 3, we provide more detail about how we use these election margins to identify the causal effect of electing a Democratic DA.

2.3 Data: Criminal Justice Records

Our primary research question asks whether Democratic DAs dismiss more criminal cases than Republican DAs. Many court records only include criminal charges that resulted in conviction and sentencing, making it impossible to observe dismissal rates. Even among court systems that track dropped or dismissed charges, reporting standards vary. These differences stem in part from inconsistent data collection practices across court systems. But

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 no impact on our findings.

16. 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.

17. Krumholz (2020), who collects data on DA elections held in 40 states between 1990 to 2015, finds that 25 percent of races were contested, slightly more than in our sample, but an affirmation that a large majority of DA elections have trivial outcomes.

they also underscore how actual arrest and charging behavior varies across states: identical crimes committed in different states may not be reported at similar frequencies; the same underlying offense can often support different charges in different states. After describing how we harmonize our state court records, we quantify and discuss the ramifications of the idiosyncrasies we observe across state court systems.

2.3.1 Criminal Justice Data Cleaning

Each set of court records that we obtained includes detailed information about individual charges. Common fields include a description of the offense, its severity (felony or misdemeanor), 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, and race/ethnicity). Our approach to data cleaning echoes that of Feigenberg and Miller (2021), who compile a similar multi-state dataset of criminal court records.[18]

From our raw court records, we first define charge-level characteristics and outcomes. Using the Text-based Offense Classification tool (Choi et al. 2023), we label charges as involving either property, violent, drug, or traffic offenses. We refer to charges that do not fall into these categories—generally, crimes against society, such as prostitution—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

18. While we largely follow Feigenberg and Miller (2021), we note one important point of departure. To define criminal cases, we do not rely on court-provided case identifiers. Rather, we group together all charges for a given defendant that either share a case identifier, or were filed on the same date. This definition reduces the number of "cases" in our sample, relative to that of Feigenberg and Miller. Functionally, defining cases this way lowers the estimated case dismissal rate and raises the estimated conviction and incarceration rates: we observe incarceration rates 8-10 percentage points higher than Feigenberg and Miller, and dismissal rates that are 6-8 percentage points lower. Our choice is relatively conservative in this regard as we hope to 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.[19,20]

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 "case." 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.[21]

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 that case were dismissed; we say a case resulted in incarceration if at least one charge resulted in an incarceration sentence; and we define the sentence 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 a confinement sentence). 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.[22]

We then construct defendant covariates, relying on fields in the raw data to identify

19. Court systems use different terms to refer to cases in which the prosecutor declined to seek conviction, all of 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 "diverted," Specifically in Texas and Maryland, respectively, we count "deferred adjudication" and "stet" dispositions as dismissals. "Deferred adjudications" and "stets," are felony dispositions that amount to probation followed by dismissal, provided the defendant does not re-offend. Note that we exclude from our definition of "dismissed" the rare situations in which charges were dropped because a judge, rather than a DA, dismissed a case (for example, due to a lack of evidence).

20. 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 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.

21. Our data from Maryland do not include a reliable indication of charge severity. Instead, to provide an indication of case severity—albeit an imperfect one—we assume that all cases handled by Maryland Circuit Courts involve a felony. Circuit Courts handle virtually all felony cases in the state, but also handle serious misdemeanors. As such, we almost certainly overstate the incidence of felony cases in Maryland, although, as we discuss at length in the next section, our preferred research design focuses on making intra-state comparisons.

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

defendant gender and age at the time of case filing.[23] Each state reports defendant race and ethnicity differently: some treat "Hispanic" as mutually exclusive with "White," while others treat ethnicity as distinct from race. To minimize measurement error, we group together "non-White" defendants, which includes defendants identified as Hispanic or by any race other than "White."[24]

Finally, we use 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 of their original case's filing date. We say that a person recidivates if we observe a subsequent criminal case. Unfortunately, only three states (Kentucky, North Carolina, and Texas) provide sufficient defendant identifiers to measure recidivism. Still, these three states account for over 60 percent of the criminal cases and DA 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 no disposition or an indeterminate disposition—that is, a disposition that cannot be described as "guilty," "not guilty," or "dismissed" (for example, when cases are bound to a higher court). We omit cases that only involve traffic offenses, since some states do not report low-level traffic infractions and misdemeanors. We also exclude cases involving defendants younger than 18. When measuring defendant recidivism, we exclude cases filed within 1 or 2 years (depending on the exact outcome variable) of the end of the coverage period of their state's court data.

2.3.2 Summary of Criminal Case Data

Our final sample contains 14,254,490 criminal cases, which we describe in Table II. Across our full dataset (shown in column 1), DAs dismiss 40 percent of cases, while another 40 percent of cases result in incarceration. Though we do not show it explicitly here, nearly

23. Court records from Virginia and Maryland provide no age or year of birth field, and so we omit this variable for defendants from these states.

24. Functionally, this approach mirrors that of Feigenberg and Miller (2021), who face a similar challenge and focus on a White/non-White dichotomy. We depart slightly from those authors and include less-represented racial and ethnic groups in our sample, such as people identified as Asian or American Indian.

all cases that do not result in dismissal culminate in conviction; as such, we typically refer to the outcome of cases that were not dismissed as "convicted." In terms of recidivism, 40 percent of defendants face a subsequent criminal case within two years. The individuals in our data are primarily male (just 26 percent are identified as female), while 54 percent are identified as non-White. Most cases involve misdemeanors (only 33 percent include a felony charge), and the most common charge types included in cases are property offenses (e.g., theft) and "other" offenses against society (e.g., driving while intoxicated, or DWI), which appear in 31 and 40 percent of cases, respectively.

The remaining columns of Table II underscore the extent to which defendant outcomes differ across court systems. Under our definitions, we find that case dismissal rates range from 17 percent (Arkansas) to 59 percent (Maryland); incarceration rates range from 6 percent (North Carolina) to 63 percent (Texas). These stark disparities across state lines likely reflect a combination of factors. As we mention above, standards for arrest, what sorts of charges police and prosecutors have at their disposal, and what sorts of charges jurisdictions must report to the state government (e.g., low-level traffic offenses) vary across jurisdictions. For example, our data from North Carolina include a large volume of "other" offenses, mostly misdemeanors, crimes that might not be recorded in other states. At the same time, 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 district attorneys.

Regardless of its underlying causes, the 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 captures 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 justice policy 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 partisanship of the serving DA at the time a case entered the court system.

We summarize our jurisdiction-year panel in Table III, highlighting both demographic features of the locations represented in our sample, as well as the average characteristics of the local court systems. To help (qualitatively) illustrate the relationship between DA partisanship and features of their jurisdictions, we break down our sample of jurisdiction-years by the party of the serving DA, as well as the competitiveness of the election in which they won office. The first panel of Table III summarizes jurisdiction-year demographics, drawing from Census (population, non-White population share) and Bureau of Economic Analysis (income per capita) data.[25] Column 1 describes all jurisdiction-years in our sample (N=4,901); 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 shy of 162,000—22 percent of whom are non-White—and a mean per-capita income of almost $39,000. These averages mask disparities across jurisdictions served by Democratic and Republican DAs: for instance, Democratic DAs elected without competition 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.

25. 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.

The second panel of Table III focuses on average defendant outcomes across court systems represented by Democratic and Republican DAs. These data show some evidence of divisions between politically competitive and noncompetitive localities. Uncontested jurisdictions have relatively low caseloads (with fewer than 3,000 cases filed per year, compared to around 5,000 in contested jurisdictions) and higher incarceration rates than jurisdictions represented by DAs of the same party elected in contested races. Interestingly, Republican- and Democratic-led jurisdictions appear to diverge in their punitiveness: contested jurisdictions with Democratic DAs have higher dismissal rates (37 percent versus 34 percent) and lower incarceration rates (42 percent versus 45 percent) than their Republican counterparts elected in competitive races.[26] At the same time, the average Democratic DA in a contested jurisdiction handles 8 percent more cases per year than the average competitively-elected Republican (5,130 cases per year, compared to 4,736 in Republican-led DAs offices), which encapsulates the fact that DAs of opposing parties serve noticeably different court systems. The question thus remains whether this divergence in outcomes represents the causal impact of DA partisanship, or stems from 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 245 "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

26. 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.

an unbalanced structure, in that periods may fall outside of our sample window 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 5,129,922 case-by-jurisdiction-election observations.[27]

3 Research Design

Our goal is to estimate the causal effect of district attorney partisanship on case dismissal and incarceration rates, average sentence lengths, and recidivism rates. Using a straightforward model estimated using ordinary least-squares, 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 of the effect of DA partisnanship if underlying determinants of judicial outcomes are correlated with the political identity of the local prosecutor. For example, Table III 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. Our research design aims to separate the causal effect of DA partisanship from the role of prosecutorial resources and other confounding factors.

3.1 Identification from Close Elections

To address concerns about endogeneous DA election outcomes, we focus on jurisdictions in which Democratic and Republican DAs hold office as the result of closely-contested races. As Table III shows, jurisdictions served by Democratic and Republican DAs elected in

27. 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.

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 narrowly-decided races, we hope to minimize the confounding differences between jurisdictions served by Democratic and Republican DAs. This intuition mirrors numerous prior studies that use close elections to infer the causal effect of partisan officials (see, for example, Dippel [2022], Ferreira and Gyourko [2009], Lee et al. [2004], and Macartney and Singleton [2018]).

In its most parsimonious form, this close-elections approach lends itself to sharp regression discontinuity (RD) design that captures the cross-sectional impact of close Democratic victories in DA elections. That is, we can 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. For jurisdiction j and post-election year t, we can 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. This aggrgegate cross-sectional specification is given by

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

where the coefficient of interest, α_2, captures the local average effect of electing a Democratic DA on aggregate outcome $\overline{Y}$. Consistent with Abadie et al. (2023), we cluster our standard errors at the jurisdiction-by-election level (effectively, the DA level) since these "clusters" perfectly determine treatment status, and the outcomes of cases handled by the same DA may be correlated. To facilitate comparisons across jurisdictions that vary widely in size (see Section 2.3 and the Appendix), we employ jurisdiction population weights to estimate this cross-sectional model.

As part of our close-elections framework, we restrict our sample to jurisdiction-elections decided by a sufficiently close margin to be comparable. We define a "close" DA election to be one decided by 8 percentage points or less—that is, those for which $Margin$ lies in the range [-0.08,0.08], which, for reference, includes all two-person races that resulted in up to a 54-46 vote split. We use that threshold because, across our outcome variables, it is the narrowest range given by Calonico et al.'s (2020) bandwidth selection procedure, and thus the most conservative data-driven choice. In the Appendix we show that we obtain quantitatively similar similar results when we use more restrictive definitions of narrow elections, a sign that our findings are not sensitive to our exact definition of election competitiveness. Our identifying assumption is that, among close elections and conditional on the election margin, DA partisanship ($Democrat$) is uncorrelated with unobserved determinants of average case outcomes, ϵ.

3.2 Validity of the Close-elections Design

While not testable in itself, our identification assumption implies that close Democratic DA victories should not be correlated with pre-election features of the jurisdiction or its typical caseload that might independently affect post-election case outcomes. Using Equation 1, we estimate differences in election-year jurisdiction demographics, as well as average case and defendant characteristics. Encouragingly, as we report in Table IV, we find no statistically significant differences in jurisdiction demographics, caseload, or average case characteristics that would undermine our identification approach. While most point estimates are reassuringly small, we do see non-trivial differences in the incidence of property and drug offenses. We do not consider this pattern to be a threat to identification, since it is unclear how these potential differences in pre-election case composition might bias our findings. Moreover, as we show in Section 4, our primary estimates are robust to controlling for offense types.

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. Put differently, within our sample of 245 politically competitive elections, there should not be a discontinuity in the running variable density at the cutoff that would indicate one party systematically wins these relatively tight races. In the Appendix, we show 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 et al. [2020]) fails to detect any such difference, with a p-value of 0.91.

3.3 Close-elections Panel Specification

Beyond identification, we face two empirical challenges in developing our research design, neither of which Equation 1 addresses. First, as Table I shows, 79 percent of DA elections are not even competitive, much less close; under our definition of close races, we have just 67 elections at our disposal with which to estimate Equation 1. Second, our underlying criminal justice data exhibit substantial variation across states and, indeed, jurisdictions (see Table II). Equation 1 does not adjust for those patterns in the data, and consequently may not yield very precise or persuasive results.[28] Put differently, while transparent, Equation 1 leaves critical information on the table that could improve statistical inference and make our findings more compelling.[29]

Our solution to both empirical problems is a specification that combines elements of a typical close-elections model with those of a panel design that captures changes in outcomes over time. Our preferred close-elections panel design narrows in on *within-jurisdiction* differences in criminal justice outcomes before and after closely-contested DA elections. This approach controls for heterogeneity across locations and time, while using pre-election data to extract as much information as possible out of the scarce competitive DA races at our disposal.

28. Concretely, using our baseline cross-sectional model, our realized minimum detectable impact on case dismissal rates, assuming a 5 percent significance threshold and 80 percent power, is an implausible 26 percentage points—or 79 percent of the sample mean. By contrast, our preferred panel model, which we discuss below, supports a more realistic minimum detectable impact of 8.4 percentage points.

29. The fact that we rely on a relatively small group of jurisdictions for identification raises separate concerns about external validity. As with all close-elections designs, our coefficient of interest recovers treatment effects only among marginal jurisdictions where candidates compete in close races, a restriction that has real bite in our setting. To help address this limitation, in Section 5 we introduce a supplementary matching design that, though it requires less transparent assumptions to support a causal interpretation, allows us to comment on the average effect of DA partisanship across a wider range of jurisdictions.

The close-election panel design combines Equation 1's RD components with a difference-in-differences framework. We estimate this disaggregate model using our main case-level panel dataset, including pre- and post-election observations. Building on our cross-sectional specification, 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:

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

where β_1—the post-election effect of a Democrat winning in a close race—is the coefficient of interest.[30] The vector $\mathbf{X}$ includes case-level covariates (for example, defendant age) that we include in some specifications to demonstrate robustness.

Intuitively, Equation 2 measures the difference-in-differences effect of a Democratic DA victory, relative to a Republican victory, where the Democratic "treatment" is quasi-randomly assigned via close elections.[31] Including pre-election observations improves our statistical efficiency while heading off concerns that pre-election differences across jurisdictions explain our results. Furthermore, jurisdiction and year fixed effects control for variation in outcomes across court systems and time (see Table II), which further improves our statistical power. For these reasons, we refer to our case-level panel estimates as our main findings. Still, we present results from our case-level panel model and our aggregate cross-sectional model side-by-side for transparency. And, as we discuss in the next section, although the cross-sectional

30. The estimated β_1 from Equation 2 without the panel elements 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 use our detailed data to achieve the greatest precision, as well as to thoroughly explore heterogeneity and alternative explanations for our findings.

31. Similar hybridized RD-difference-in-differences approaches are increasingly common in research on local 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 (2023) use analogous instrumental variables specifications with difference-in-differences components.

approach tends to deliver noisier point estimates (as expected), we find that these results align quantitatively and qualitatively with our favored panel estimates.[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.

3.4 Variation in DA Partisanship After Close Elections

Before estimating our cross-sectional and panel models, we illustrate the "first stage" variation in district attorney partisanship generated by close-elections. In the cross section, our close-elections approach guarantees that 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, and to provide a sense of the timeframe during which we expect to observe the effects of Democratic DA election victories, we estimate an event study specification in the style of Equation 3. In this model, the outcome is an indicator for whether

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 of jurisdiction and year fixed effects in our empirical approach, particularly once we incorporate multiple post-election periods. As expected, the panel component of our design—our inclusion of pre-election observations—does not have much of an effect on the magnitudes of our estimates, but does noticeably improve precision.

a Democratic DA won the most recent election in the jurisdiction, and each observation is a jurisdiction-year. Those estimates appear in Figure I. As expected, we find that during the four years after an election in which a Democratic candidate narrowly beat their Republican opponent, most jurisdictions continue to be served by a Democratic DA (or, more exactly, they did not have another election in which a Republican or independent won); after four years—at which point the modal jurisdiction will have held 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?

We begin our analysis by using our close-election framework to investigate how Democratic DAs affect the probability of case dismissal. We then broaden 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 III, suggest that Democratic DAs might 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?

Using our close-elections framework and panel data, we first provide visual evidence of how narrow Democratic DA elections influence case dismissal rates in their jurisdictions. Democratic DAs' election victories might take time to materially affect case outcomes—incoming DAs might, 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 illustrating the dynamic impact of close Democratic DA elections on case dismissal rates with an event study-style framework, using Equation 3.

These estimates appear in the first panel of Figure II. 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 may explain why we find a small, insignificant uptick in dismissal rates in the election year.[33]

Following the election, panel 1 of Figure II shows a rise in dismissal rates almost immediately after a Democratic DA assumes office, although the effects are only statistically significant several 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 about 11 percentage points more likely to dismiss a criminal case than a marginally elected Republican DA. The second panel of Figure II tells a similar story: in the fourth year after the election, we see roughly 10 percentage-point cross-sectional difference in average dismissal rates between Democratic-led jurisdictions (to the right of the cutoff) and Republican-led jurisdictions (to the left of the cutoff). Though far from conclusive, event study estimates in the first panel 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 (see Figure I).

4.2 Estimating the Effect of DA Partisanship on Case Dismissal Rates

We next provide regression estimates to substantiate the visual evidence from Figure II. Our estimates come from our two specifications: a close-elections panel model, following Equation 2, and a cross-sectional model, following Equation 1. We present these findings in

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.

the first panel of Table V.

Results from our preferred close-elections panel 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 7.1-8 percentage points during this period (22-24 percent of the sample mean), relative to the marginal Republican DA. The smaller estimates in this range come from specifications with additional defendant and case covariates (column 3), but both approaches yield similar results relative to the sample mean. Including additional post-election periods also has a minimal effect on our findings (compare columns 3 and 4).

In columns 5-7 of of Table V, 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. We first estimate a straightforward sharp RD-style model, examining the impact of a close Democratic DA victory in the fourth year post-election as in Figure II (N=67 jurisdiction-elections). That estimate appears in column 5. Understandably, since we do not control for any of the cross-state variation in our raw data, the resulting point estimate is quite noisy—indeed, the smallest statistically significant impact on case dismissal that we could detect would be 26 percentage points (assuming a 0.05 significance threshold and 80 percent power). But, taken at face value, the cross-sectional estimate suggests that the marginal Democratic DA causes an uptick in jurisdiction dismissal rates of 11.3 percentage points—almost identical to the analogous estimate from our event study model, shown in Figure II.

Working within the confines of this cross-sectional model, we run two alternative specifications that augment our statistical power to provide clearer evidence of Democratic DAs' effect on dismissal rates. The first alternative, shown in column 6 of Table V, 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 "first-difference" approach echoes the panel design from the second column of Table V, but employs aggregate data (N=67 jurisdiction-elections) and no additional controls. We find that jurisdictions that narrowly elect a Democratic DA see a marginally significant 6.6 percentage-point increase in dismissal rates post-election. This estimate, which is quantitatively similar to the baseline panel estimates from columns 2-4, provides some reassurance that using case-level data and a difference-in-differences framework does not drive our results.

Second, 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=266). This step means that 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 justice policy environments across locations in our sample. The point estimate from that model, reported in column 7, indicates that Democratic DAs raise dismissal rates by 9.8 percentage points (30 percent of the mean), a slightly bigger effect than we recover with our preferred specification.

4.3 Does DA Partisanship Matter for Other Case Outcomes?

Beyond its effect on dismissal rates, we want to understand how DA partisanship shapes case dispositions broadly, particularly in terms of incarceration outcomes. Figure III 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 intensity soon after a Democratic DA's election victory, culminating in a 14 percentage-point decline in the probability of incarceration and a 44 percent decline in nominal sentence length in the fourth post-election year. Cross-sectional data in the third and fourth panels support a similar conclusion. However, the visual evidence of a drop in average sentence lengths near the cutoff does not appear to be very robust, a point we return to below.

In the second panel of Table V, we present panel and cross-sectional close-elections estimates of 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 8.0 and 8.8 percentage points in the 4-6 years following their election (14-15 percent of the sample mean). Likewise, we find that Democratic DAs impose 33-43 percent shorter sentences (roughly 3 fewer months in jail or prison, on average). Our cross-sectional 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 III. 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. Altogether, these findings suggest 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 close-elections, RD-style 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 the Appendix, we explore the robustness of our main close-elections panel estimates to different choices of sample and control variables.

We first probe the robustness of our findings by tightening our restriction of what constitutes "close election" (that is, one decided by 8 percentage points or less). To demonstrate that our findings do not hinge on this particular definition of a "close" election, we re-estimate our preferred panel specification after narrowing our sample of elections to those decided by 5, 6, and 7 percentage points or less. In general, these more conservative definitions of "close" elections yield quantitatively and qualitatively similar findings to our preferred definition, although we do find much smaller effects on dismissal rates when using the 5 percentage-point definition. For transparency, we also explore the consequences of loosening our definition of "close" elections to those decided by 10 percentage points or less. While raising our close-election margin noticeably expands our sample size, this approach leads to small and statistically insignificant estimates on dismissal and incarceration rates. While our estimates are not robust to this broader definition of "close" elections, we do not believe this pattern undermines the validity of our preferred estimates: expanding our sample of elections this way goes against the logic of our identification approach and might re-introduce sources of bias that we aimed to control for.

We further consider the robustness of our findings to two other modifications common in the regression discontinuity literature, from which we borrow substantial intuition. We alter our panel 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

We next examine whether Democratic DAs' choices of low prosecution and incarceration rates benefit some defendants or types of cases more than others. Any heterogeneity in the effects of DA partisanship—or a lack thereof—would provide useful context for interpreting our results: do Democratic DAs simply decline to 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 potential heterogeneity in treatment effects, estimating our preferred model (Equation 2) on subsamples of our data. We provide these subgroup-specific estimates in the Appendix. Our heterogeneity analysis reveals larger impacts of close Democratic DA elections on dismissal and incarceration rates among non-White 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 effects 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 an individual belongs to the respective subsample (e.g., whether the defendant is over age 30). Broadly, we find little evidence of statistically significant differences, and we cannot rule out equality in all but a few of these estimates. Based on these tests and the fairly consistent estimates across subgroups, we conclude that the effects of DA partisanship are not concentrated among particular case types or defendant subgroups.

5 Does DA Partisanship Affect Public Safety?

To understand the policy implications of our main findings, we consider the relationship between DA political identity and public safety. The estimates we have presented thus far suggest that Democratic DAs tend to lower conviction and incarceration rates, which may have knock-on implications for criminal behavior. To assess these effects, we look at how marginally elected Democratic DAs affect recidivism rates as well as jurisdiction-level crime and arrest rates.

5.1 Does DA Partisanship Affect Recidivism Rates?

That Republican and Democratic DAs pursue different conviction and incarceration rates suggest that they might have disparate views on the utility of criminal convictions and incarceration. Ample prior research has shown that convicted individuals incur considerable economic and social costs following spells in detention, however brief, including wage penalties and lower civic engagement (Mueller-Smith 2015; 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 incarcerating defendants, DAs can deter future criminal behavior. Recent research, including Rose and Shem-Tov (2021) and Humphries et al. (2023) suggest that incarceration does in fact reduce at least short-term re-offending behavior by defendants, partially by incapacitating individuals in jail or prison. However, another strand of research highlights the benefits of forestalling criminal convictions (see, for example, Agan et al. [2023], Augustine et al. [2022], and Mueller-Smith and Schnepel [2020]). The net effect on recidivism of electing a Democratic DA, whom we have shown to simultaneously raise dismissal rates and lower incarceration rates, is thus theoretically ambiguous.[34]

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.[35] Recall that we can only observe defendant re-offense behavior in three states, Kentucky, North Carolina, and Texas. And we emphasize that we cannot directly test the causal effect of case dismissal or conviction without subsequent incarceration on the probability; for the purposes of this study, we only wish to evaluate the effect of electing a Democratic DA—who, among other things, increases dismissal rates and lowers incarceration rates, as we have shown above—on

34. We define recidivism as facing a new criminal case. Some researchers justifiably oppose this definition because arrest and charging probability depends on the behavior of police. In our setting, though, looking at outcomes outside the purview of police—such as incarceration—does not make much sense, since those will just reflect the impact of DA partisanship, which we have already shown to be substantial.

35. Our sample size attenuates rapidly when we look at re-offense rates over longer time horizons. Agan et al. (2023), for one, focus on 2-year recidivism, and so we lean into this outcome as a benchmark of comparison.

the overall re-offense rate in their jurisdiction. This exercise provides both a sense of whether DA partisanship affects the deterrence quality of local criminal justice systems, as well as a descriptive data point on the relationship between relatively lenient prosecutorial policies and re-offense rates.

Figure IV 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 reappear 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 VI. Panel estimates confirm that Democratic DAs have a null impact on the probability of defendant recidivism. The 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 0.3 percentage points (1 percent of the sample mean) and an increase in 2-year re-offense probability greater than 0.5 percentage points (also 1 percent of the sample mean). Cross-sectional estimates in the remaining columns generally find larger negative (that is, beneficial) effects, although none of these coefficients are statistically significant. Altogether, these findings support the conclusion that Democratic DAs do not increase re-offense rates relative to their Republican peers.[36]

36. In the Appendix, we also examine heterogeneity in the effects of DA partisanship by whether the defendant has a *prior* criminal case in the last 1-2 years. We find little evidence of heterogeneous effects on case dismissal, incarceration, and recidivism rates across these types of individuals. While we cannot definitively identify "first-time" offenders, the lack of heterogeneity by prior offense history stands somewhat apart from the literature: Jordan, Karger, and Neal (2022) find that incarcerating first-time offenders lowers their likelihood of recividism, while Agan, Doleac, and Harvey (2023) find that nonprosecution benefits first-time offenders the most. Our estimates, by contrast, suggest that, at the margin of DA partisanship, lower conviction and incarceration rates do not appear to systematically benefit individuals with no observable

5.2 Does DA Partisanship Affect Crime Rates or Policing?

In addition to the future behavior of defendants, it is also possible that the political identity of DAs affects reported crime and the number of arrests police make, which in turn determines the composition of cases that appear in our court data. Prosecutors and police officers are closely linked in law enforcement, a relationship explored more deeply in Garro and Stashko (2023). In our setting, one might expect that if Democratic DAs dismiss certain types of criminal cases, law enforcement agencies might react by pulling back the policing efforts that precede those cases, reducing arrests and potentially the reported levels of crime.

For this exercise, we turn to the Federal Bureau of Investigation's (FBI) Uniform Crime Reports (UCR). These data include categorical counts of crime and arrests for the vast majority of police agencies that report these figures to the FBI.[37] We transformed those counts into per-capita rates using the jurisdiction population given in the UCR.

In the Appendix, we summarize the dynamic effects of DA partisanship on crime-related outcomes derived from the UCR using Equation 3. We find that jurisdictions served by Democratic DAs do not have statistically significant differences in reported crime or arrest rates compared to those served Republican DAs. Point estimates are generally negative (indicating that citizens report fewer crimes, and police make fewer arrests, per capita), but vary considerably across years and do not reveal particularly large differences. These patterns are consistent with the idea that police, within our close-election context, do not respond to Democratic DAs' prosecution policies by arresting fewer individuals. This result echoes Arora (2019), who also examines DAs elected in close races and finds no difference in arrest counts between jurisdictions led by Republican DAs and their Democratic peers. These results, in line with the null effects on recidivism that we report above, reinforce our conclusion that Democratic DAs' policies have no observable adverse effects on public safety.

prior offenses, as one might expect based on these previous findings.

37. We cleaned the raw data to address known issues in the Return A files. For instance, extreme outliers – which may be assumed to be clerical errors – were set to missing. Additionally, we imputed data at the agency-level where necessary in order to develop panels of DA jurisdictions that have fewer issues stemming from police agencies failing to report. More information on the data cleaning procedures may be requested from the authors.

6 Estimating the Average Effect of DA Partisanship

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—which are relatively rare, as we have shown. Given the prevalence of uncontested DA races, identifying the impact of the average DA's partisanship is of particular policy importance. One might doubt that our close-elections difference-in-differences approach delivers that average treatment effect (ATE) of DA politics, not least because of the disparities between contested and uncontested jurisdictions that we document in Table III. But delivering a plausibly causal estimate of the average effect in our context is challenging: we would need a valid instrument for DA partisanship across all jurisdictions, contested and uncontested, which we do not have.

In lieu of such an instrument, we employ a matching design that draws on our case-level data to create "matched" samples of otherwise similar cases from jurisdictions prosecuted by Democratic and Republican DAs. Intuitively, our matching design leverages the full scope of our case-level dataset to zero-in on the most comparable cases from the most comparable jurisdictions led by Democratic and Republican DAs. In so doing, we aim to create matched sample of observably identical treated and control cases, minimizing the differences in confounding jurisdiction-level and case-level factors in order to isolate the average effect of DA partisanship.

6.1 Matching Design

Our matching approach combines aspects of exact matching and propensity score matching. We first exactly match cases based on their state and year of filing, as well as the binary case and defendant characteristics listed in Table II (severity and offense type) that allow us to define groups of observably similar cases. We then use a logistic regression model to estimate the probability that a given case would appear in a Democratic-led jurisdiction, including as covariates all discrete and continuous defendant, case, and jurisdiction characteristics listed in Table II. Our design uses the resulting propensity scores to match and assign weights to suitably similar cases within exact-match groups.

The literature offers a range of potential refinements to this within-group propensity score matching approach. We focus on two enhancements that we believe increase the transparency of our results while reducing potential bias. First, when matching on propensity scores within exact-match groups, we specify a caliper, a maximum allowable difference between treated and matched control units' propensity scores. Following Austin (2011), we set our caliper equal to 20 percent of the propensity score distribution's standard deviation. We include as matches any candidate observations from the comparison pool that fall within that caliper distance of a given "treated" observation; comparison observations then receive a weight proportional to how often they match to treated cases. Second, we make our final matching estimates "doubly robust" by using a regression adjustment to control for the same covariates we use to estimate our propensity score model. This step helps adjust for differences in case, defendant, and jurisdiction characteristics left in our weighted sample after we have completed the matching process.

6.2 Evaluating the Matching Design

Our matching approach ensures that, mechanically, our treated and comparison units will be identical along case and defendant characteristics that we include in our exact-match groups. In the Appendix we show that we also achieve balance across the defendant, case, and county characteristics that we include in our propensity score model but not our exact-match criteria. While we find a statistically significant difference in the average number of charges on cases, it amounts to only 8 percent of the sample mean. Overall, the negligible differences between our treated and control samples indicate that, as intended, our matching design produces a sample of similar criminal cases, for which the only observable difference is the political party of the DA at the time of case filing.

Despite our well-balanced sample, one might argue that any matching approach cannot remove all potential sources of selection bias that distinguish treated and untreated units—and, by definition, we cannot disprove such a claim. Helpfully, though, in our setting, we have a benchmark that we can use to support our argument that our matching design helps address selection bias: our well-identified close-elections estimates. Intuitively, assuming that our main close-elections difference-in-differences estimates capture the "true" causal impact of DA partisanship on case outcomes—locally, within the subsample of narrowly decided DA races—then we would expect a valid matching estimator to yield comparable results when applied to the same close-election sample.

To gauge the performance of our matching estimator, we compare matching estimates to our multi-period cross-sectional results from column 7 of Tables V and VI. As with our matching design, this cross-sectional specification makes within-year and within-state comparisons without drawing on pre-election jurisdiction data, and thus represents a logical point of comparison.

We present our benchmark matching results, estimated on the same sample as in our close-elections model, in the second column of Table VII. We compare these results to the close-election cross-sectional estimates, shown in column 1, that we previously presented in Tables V and VI. Overall, within our close-elections sample, we recover matching estimates that are similar or even identical to our cross-sectional results, at least when we consider dismissal, incarceration, and recidivism rates. Estimates on sentence length, however, differ across the two approaches: our matching approach recovers an effect less than half as large as our primary 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 close-elections sample.

6.3 Matching Results

In column 4 of Table VII, 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 4.8 percentage points

(13 percent) more likely to dismiss a given case, and 5.5 percentage points (12 percent) less likely to impose an incarceration sentence. Taken at face value, the average Democratic DA appears to seek 13 percent shorter incarceration sentences as well, although we caveat that this result may not be reliable, given our aforementioned concerns about 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 criminal justice. Understanding the degree to which DAs drive conviction 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 individual defendant outcomes, including conviction and recidivism rates. We find that, relative to their Republican counterparts, Democratic district attorneys pursue fewer criminal convictions, dismissing more cases and incarcerating fewer defendants, without systematically affecting public safety.

These findings provide a new, system-level perspective on ongoing debates concerning the efficacy and equity of criminal convictions and incarcerations. Our analysis underscores the extent to which commonly-cited judicial statistics do not necessarily reflect underlying crime but instead capture the idiosyncracies of partisan politics. At the same time, we contribute to a robust debate in the literature concerning the marginal benefits of more punitive prosecutorial and judicial decisions: in the aggregate, we show that higher conviction and incarceration rates do not translate into appreciable public safety benefits. Our results suggest that, at a minimum, Democratic DAs do not encourage re-offending any more than their Republican counterparts. While we reiterate once more that our estimates do not capture the causal effect of case dismissal or incarceration on crime or re-offense behavior, we view our null results as consistent with a body of work, as summarized by Leoffler and Nagin (2022), that finds that diverting cases to non-carceral punishments or dismissing them outright does not affect re-offense behavior. Specifically, we highlight how Democratic DAs substantially reduce conviction and incarceration rates without producing any concomitant (aggregate) effects on recidivism.

That said, our findings are less consistent with recent research arguing that, relative to convictions, incarceration sentences reduce short-term recidivism (Humphries et al. 2023; Jordan, Karger, and Neal 2022; Rose and Shem-Tov 2021). While we cannot resolve this debate, our setting gives us an instructive perspective on this tension in the literature. Fundamentally, we show that, at the level of the policymaker (district attorney), decisions over how many cases to dismiss and how many individuals to incarcerate appear to have no meaningful effects on local criminal behavior—whatever their effects at the margins of dismissal/conviction or conviction/imprisonment, which the literature typically focuses on. From that aggregate perspective, we broadly interpret our findings as evidence that lower incarceration and conviction rates need not lead to higher recidivism rates.

At their core, our findings substantiate anecdotal evidence that DA partisanship matters, and in fact causes prosecutorial policies to diverge sharply across jurisdictions. This stark result reveals a tight relationship between public opinion and the notionally apolitical decision over a person's guilt or innocence, with nuanced implications for our understanding of the interplay between society and the courts. On the one hand, the fact that DAs appear to respond to the will of voters opens the door to democratic accountability of local prosecutors; on the other hand, that voters can shape the implementation of the law raises questions about the fairness and impartiality of the judicial system. As researchers and stakeholders have come to appreciate the powers exercised by local officials, we provide new evidence that it is DAs—and, by extension, voters—who determine the punitiveness of the criminal justice system.

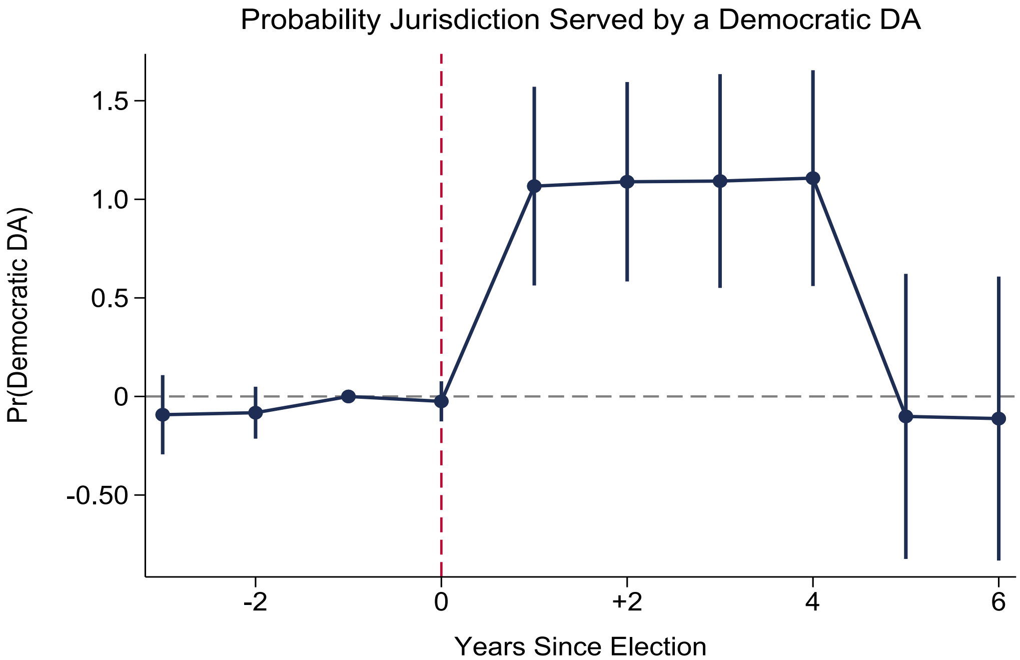

Figure I: The figure plots panel regression estimates showing the impact of a narrow Democratic DA win on the probability that a Democratic DA serves in office in the years around the election. The specification is Equation 3. All coefficients are relative to the year prior to the election (period -1). The sample includes 67 competitive district attorney races with at least one Democrat and one Republican running that were decided by 8 percentage points or less. Standard errors are clustered at the election level.

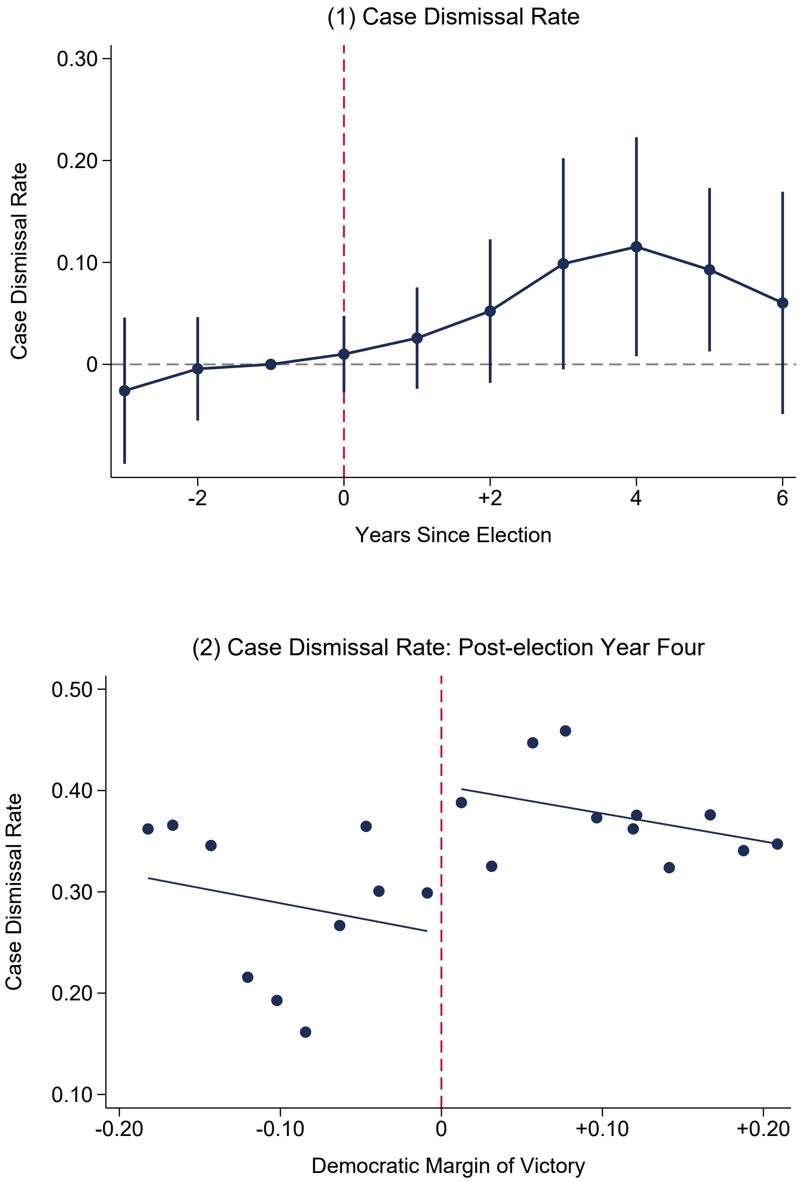

Figure II: Panel 1 plots panel regression estimates showing the impact of a narrow Democratic DA election win on the probability of case dismissal by year relative to the election. The specification is Equation 3. All coefficients are relative to the year prior to the election (period -1). The sample includes cases filed in jurisdiction-elections decided by 8.0 percentage points or less (N= 5,129,922 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 162 jurisdiction-elections.

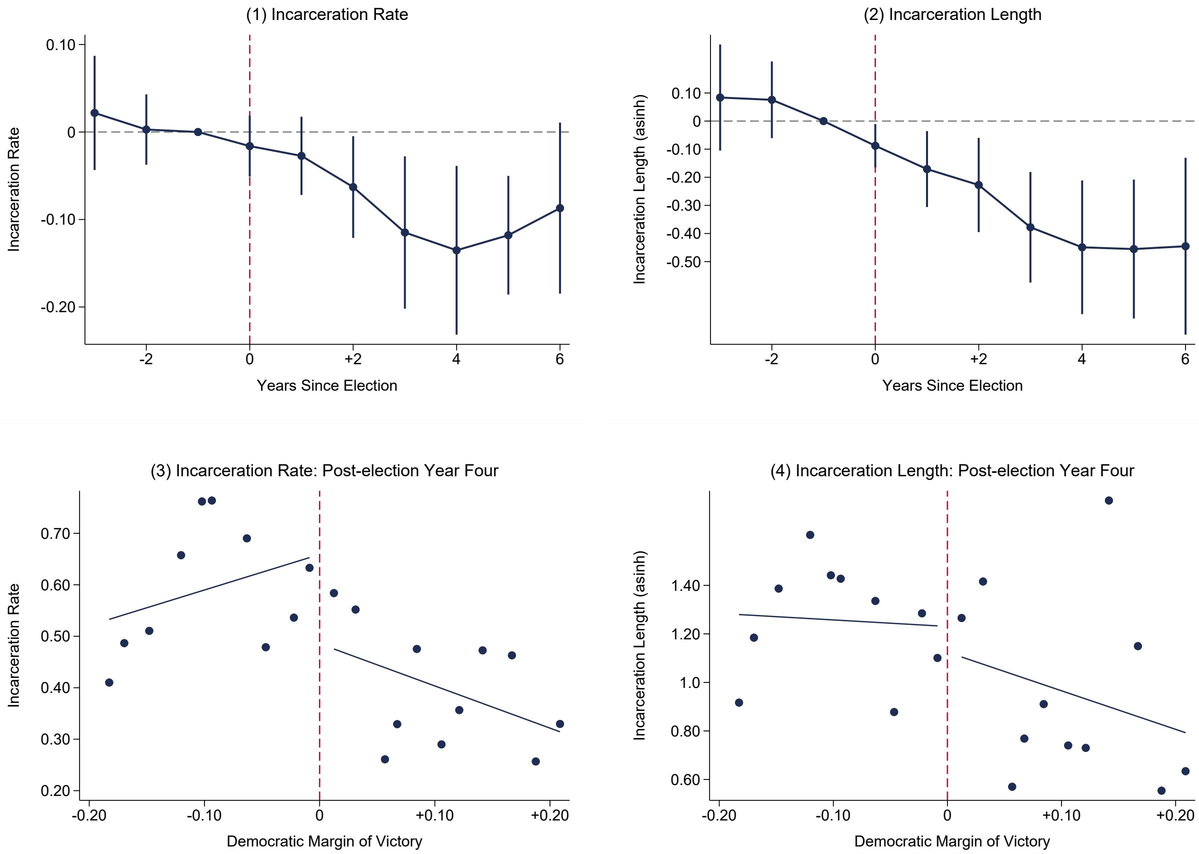

Figure III: Panels 1 and 2 plot panel regression estimates showing the impact of a narrow Democratic election win on the probability of incarceration (panel 1) and incarceration length (panel 2) by year relative to the election. The specification is Equation 3. All coefficients are relative to the year prior to the election (period -1). The sample includes cases filed in jurisdiction-elections decided by 8.0 percentage points or less (N= 5,129,922). 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 162 jurisdiction-elections.

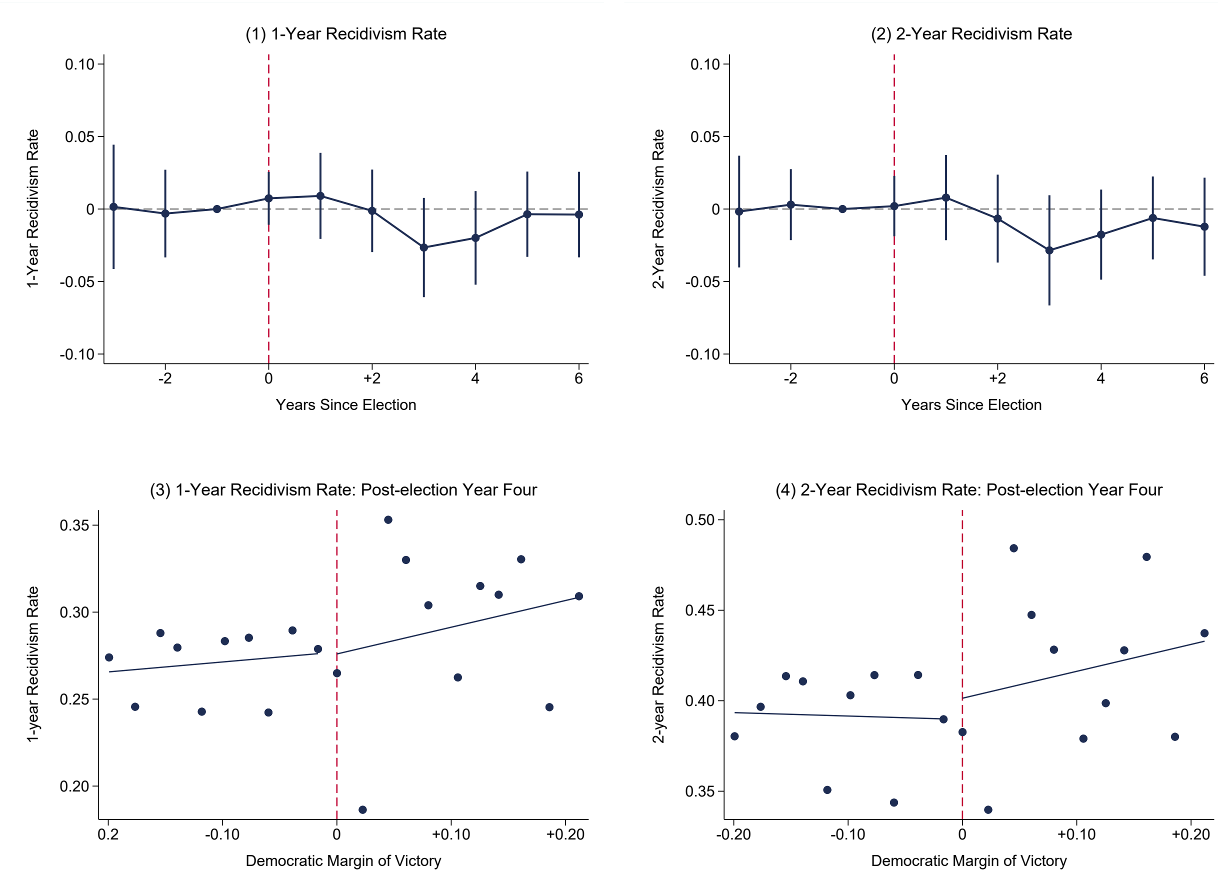

Figure IV: Panels 1 and 2 plot panel regression estimates showing the impact of a narrow Democratic election win on the probability of defendant recidivism within 1 year of initial arrest (panel 1) and the probability of defendant recidivism within 2 years of initial arrest (panel 2), by year relative 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 8.0 percentage points or less (N=4,182,347 for panel 1, and 4,020,494 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.

Table I: Summary of District Attorney Elections

	N	Mean	Median	Std Dev	Min	Max
I. Election Characteristics						
Election year	1,447	2008	2008	6	1996	2018
# of Candidates	1,447	1.22	1.00	0.44	1.00	4.00
# of Democrats	1,447	0.55	1.00	0.50	0.00	2.00
Contested Election?	1,447	0.21	0.00	0.41	0.00	1.00
II. Election Outcomes						
Democrat Won?	1,447	0.44	0.00	0.50	0.00	1.00
Republican Won?	1,447	0.55	1.00	0.50	0.00	1.00
Election Margin	306	0.19	0.15	0.18	0.00	1.00
Dem-Rep Margin	245	-0.06	-0.05	0.20	-0.55	1.00

The table summarizes outcomes from 1,447 district attorney elections held in Arkansas, Colorado, Kentucky, Maryland, North Carolina, Texas, and Virginia between 1996 and 2018. The election margin is the difference between the vote shares of the first- and second-place candidates, irrespective of their political parties, whereas the “Dem-Rep Margin” is 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 II: Criminal Case-level Descriptive Statistics by State

		State of Case Filing						
	All Cases	Arkansas	Colorado	Kentucky	Maryland	North Carolina	Texas	Virginia
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
I. Case Outcomes								
Case Dismissed?	0.40	0.17	0.32	0.31	0.59	0.58	0.31	0.37
	(0.49)	(0.38)	(0.47)	(0.46)	(0.49)	(0.49)	(0.46)	(0.48)
Incarceration?	0.40	0.60	0.34	0.56	0.30	0.06	0.63	0.24
	(0.49)	(0.49)	(0.47)	(0.50)	(0.46)	(0.24)	(0.48)	(0.43)
Incarceration Length (asinh)	0.90	2.27	0.82	1.11	0.98	0.08	1.34	0.47
	(1.49)	(2.28)	(1.46)	(1.43)	(1.74)	(0.46)	(1.64)	(0.98)
N:	*14,254,490*	*76,303*	*1,588,987*	*1,021,555*	*1,849,565*	*2,712,492*	*5,643,755*	*1,361,833*
1-year Recidivism	0.30	—	—	0.30	—	0.34	0.28	—
	(0.46)	—	—	(0.46)	—	(0.47)	(0.45)	—
N:	*9,033,287*	—	—	*972,242*	—	*2,516,277*	*5,544,768*	—
2-year Recidivism	0.40	—	—	0.39	—	0.42	0.40	—
	(0.49)	—	—	(0.49)	—	(0.49)	(0.49)	—
N:	*8,340,464*	—	—	*918,149*	—	*2,156,189*	*5,266,126*	—
II. Defendant Characteristics								
Age	32.81	35.31	32.27	33.83	—	34.59	31.88	—
	(11.30)	(14.42)	(11.30)	(11.07)	—	(12.17)	(10.73)	—
N:	*11,043,092*	*76,303*	*1,588,987*	*1,021,555*	—	*2,712,492*	*5,643,755*	—
Female	0.26	0.26	0.25	0.31	0.19	0.35	0.23	0.29
	(0.44)	(0.44)	(0.43)	(0.46)	(0.39)	(0.48)	(0.42)	(0.45)
N:	*14,254,465*	*76,303*	*1,588,987*	*1,021,555*	*1,849,540*	*2,712,492*	*5,643,755*	*1,361,833*
Nonwhite	0.54	0.24	0.20	0.08	0.86	0.50	0.66	0.41
	(0.50)	(0.43)	(0.40)	(0.28)	(0.35)	(0.50)	(0.47)	(0.49)
N:	*14,132,096*	*69,241*	*1,588,987*	*986,318*	*1,778,159*	*2,712,492*	*5,643,699*	*1,353,200*
III. Case Characteristics								
# of Charges	1.85	3.16	2.26	2.38	2.55	1.91	1.46	1.47
	(2.44)	(4.14)	(1.54)	(5.57)	(3.40)	(2.33)	(1.01)	(1.60)
Felony Offense?	0.33	0.29	0.40	0.28	0.66	0.07	0.36	0.28
	(0.47)	(0.45)	(0.49)	(0.45)	(0.47)	(0.25)	(0.48)	(0.45)
Property Offense?	0.31	0.32	0.34	0.39	0.32	0.13	0.35	0.36
	(0.46)	(0.47)	(0.47)	(0.49)	(0.47)	(0.34)	(0.48)	(0.48)
Violent Offense?	0.19	0.16	0.30	0.21	0.26	0.09	0.21	0.12
	(0.40)	(0.37)	(0.46)	(0.41)	(0.44)	(0.28)	(0.41)	(0.32)
Drug Offense?	0.24	0.25	0.20	0.22	0.32	0.08	0.31	0.21
	(0.42)	(0.43)	(0.40)	(0.41)	(0.47)	(0.27)	(0.46)	(0.40)
Traffic Offense?	0.04	0.15	0.13	0.10	0.03	0.00	0.04	0.03
	(0.21)	(0.35)	(0.34)	(0.30)	(0.16)	(0.05)	(0.19)	(0.17)
Other Offense?	0.40	0.54	0.44	0.44	0.35	0.78	0.21	0.43
	(0.49)	(0.50)	(0.50)	(0.50)	(0.48)	(0.41)	(0.41)	(0.49)
N:	*14,254,490*	*76,303*	*1,588,987*	*1,021,555*	*1,849,565*	*2,712,492*	*5,643,755*	*1,361,833*

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=14,254,490). The remaining columns describe cases by the state in which they were filed. Empty cells denote missing recidivism data and information on defendant age. Sample sizes vary within columns due to missing recidivism and defendant demographic data. See the text for more detail on data missingness and sample construction.

Table III: Comparing Jurisdictions Led by Democratic and Republican DAs

		Democratic DAs		Republican DAs	
	Full Sample	Uncontested Election	Contested Election	Contested Election	Uncontested Election
	(1)	(2)	(3)	(4)	(5)
I. Jurisdiction Characteristics					
Population	161,947	117,029	339,742	326,946	126,728
	(359,978)	(190,257)	(567,480)	(738,573)	(210,417)
N:	*4,901*	*1,808*	*355*	*604*	*2,069*
Share Nonwhite	0.22	0.22	0.25	0.22	0.21
	(0.19)	(0.22)	(0.20)	(0.17)	(0.17)
N:	*4,889*	*1,806*	*350*	*601*	*2,067*
Income per Capita ($2016)	38,934	35,559	43,777	41,008	40,540
	(10,969)	(10,603)	(13,250)	(10,827)	(10,160)
N:	*4,547*	*1,688*	*326*	*556*	*1,924*
II. Court Outcomes					
Annual Caseload	2,908	2,779	5,130	4,736	2,178
	(6,935)	(7,336)	(9,246)	(10,679)	(4,063)
Caseload per 1,000 Pop	20.3	21.8	18.9	19.1	19.4
	(17.1)	(18.3)	(15.8)	(16.7)	(16.2)
Case Dismissal Rate	0.35	0.36	0.37	0.34	0.34
	(0.13)	(0.14)	(0.14)	(0.13)	(0.11)
Incarceration Rate	0.45	0.43	0.42	0.45	0.49
	(0.21)	(0.20)	(0.20)	(0.21)	(0.20)
Avg. Sentence Length (asinh)	1.15	1.08	1.16	1.22	1.22
	(0.68)	(0.60)	(0.81)	(0.78)	(0.66)
N:	*4,901*	*1,808*	*355*	*604*	*2,069*

The data describe the characteristics and judicial outcomes among DA jurisdiction-years between 2000 and 2020. Column 1 reports the mean of the given variable across all jurisdiction-years (N=4,901). 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 and Census population data by race are missing for some jurisdiction-years. Sample sizes do not add up across columns because some DA election winners represent third parties, while for others we cannot identify a partisan affiliation.

Table IV: Baseline Balance: Election-year Differences in Jurisdiction and Case Characteristics

	Control Mean	Balance Estimate
	(1)	(2)
I. Jurisdiction Characteristics		
Population	738,249	20,069
	(1,204,878)	(484,041)
Caseload	22,735	1,747
	(47,818)	(15,944)
Caseload per 1,000 Pop	21.7	3.3
	(12.8)	(6.0)
N:	*37*	*67*
Share Nonwhite	0.251	0.004
	(0.197)	(0.100)
N:	*36*	*62*
Income per Capita ($2016)	43,993	4,771
	(10,825)	(5,468)
N:	*34*	*58*
II. Average Case Characteristics		
Defendant Age	31.9	0.4
	(1.1)	(0.6)
N:	*29*	*47*
Female Defendant	0.229	-0.011
	(0.056)	(0.017)
Nonwhite Defendant	0.499	0.012
	(0.238)	(0.078)
# of Charges	2.1	0.1
	(1.1)	(0.4)
Felony Offense	0.295	-0.016
	(0.137)	(0.068)
Property Offense	0.347	0.048
	(0.070)	(0.030)
Violent Offense	0.239	0.030
	(0.073)	(0.031)
Drug Offense	0.261	-0.039
	(0.084)	(0.048)
Traffic Offense	0.070	0.007
	(0.063)	(0.019)
Other Offense	0.305	-0.031
	(0.143)	(0.036)
N:	*37*	*67*

The sample includes election-year jurisdictions (N=67) for which the upcoming election is decided by 8 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. Specifications used in panel II employ jurisdiction population weights. Sample sizes vary within columns due to missing jurisdiction demographic and defendant age data, as discussed in the text.

Table V: 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	0.327	0.080***	0.071**	0.071**	0.113	0.066*	0.098***
	(0.469)	(0.030)	(0.027)	(0.029)	(0.093)	(0.033)	(0.033)
II. Incarceration Outcomes							
Incarceration	0.572	-0.088***	-0.080***	-0.085***	-0.049	-0.065**	-0.100***
	(0.495)	(0.027)	(0.024)	(0.026)	(0.109)	(0.029)	(0.035)
Incarceration Length (asinh)	1.140	-0.333***	-0.402***	-0.429***	0.048	-0.250***	-0.267**
	(1.537)	(0.063)	(0.068)	(0.079)	(0.240)	(0.089)	(0.124)
N:	*4,280,419*	*4,280,419*	*4,280,419*	*5,129,922*	*67*	*67*	*266*
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]	[1,4]
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 8 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 four 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, as well as defendant age and the total number of charges on the case. Missing covariates are replaced with zeros and all specifications with covariates include indicators for missingness. Robust standard errors clustered at the jurisdiction-election level appear in parentheses.

Table VI: 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	0.289	-0.009	-0.006	-0.007	-0.041	-0.012	-0.004
	(0.453)	(0.006)	(0.006)	(0.007)	(0.029)	(0.027)	(0.010)
N:	*4,182,347*	*3,490,276*	*3,490,276*	*4,182,347*	*37*	*32*	*136*
2-year Recidivism	0.408	-0.011	-0.009	-0.010	-0.039	-0.013	-0.019*
	(0.491)	(0.008)	(0.008)	(0.008)	(0.023)	(0.032)	(0.011)
N:	*4,020,494*	*3,375,311*	*3,375,311*	*4,020,494*	*34*	*32*	*127*
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]	[1,4]
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 +4 in jurisdiction-elections decided by 8 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 V. Robust standard errors clustered at the jurisdiction-election level appear in parentheses.

Table VII: What is the Average Effect of DA Partisanship? Matching Estimates

	Close-elections Sample		All Cases	
	Cross-sectional Estimates	Matching Estimates	Sample Mean	Matching Estimates
	(1)	(2)	(3)	(4)
Case Dismissal	0.098***	0.098***	0.384	0.048***
	(0.033)	(0.021)	(0.486)	(0.009)
Incarceration	-0.100***	-0.094***	0.417	-0.055***
	(0.035)	(0.020)	(0.493)	(0.006)
Incarceration Length (asinh)	-0.267**	-0.117**	0.929	-0.134***
	(0.124)	(0.057)	(1.509)	(0.015)
N:	*266*	*940,606*	*13,395,991*	*12,479,135*
1-year Recidivism	-0.007	0.007	0.298	0.008
	(0.007)	(0.006)	(0.457)	(0.005)
N:	*173*	*864,536*	*9,032,425*	*8,649,890*
2-year Recidivism	-0.014	0.006	0.403	0.004
	(0.009)	(0.007)	(0.491)	(0.007)
N:	*163*	*836,154*	*8,339,683*	*7,983,933*

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

Columns 1-3 include observations filed between post-election periods +1 and +4 in jurisdiction-elections decided by 8 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 V and column 6 of Table VI. 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.

References

Abadie, Alberto, Susan Athey, Guido W Imbens, and Jeffrey M Wooldridge. 2022. "When Should You Adjust Standard Errors for Clustering?" *The Quarterly Journal of Economics* 138 (1): 1–35.

Agan, Amanda, Jennifer L Doleac, and Anna Harvey. 2021. "Prosecutorial Reform and Local Crime Rates." SSRN Working Paper Series.

———. 2023. "Misdemeanor Prosecution." *The Quarterly Journal of Economics* 138 (3): 1453–1505.

Agan, Amanda, Matthew Freedman, and Emily Owens. 2021. "Is Your Lawyer a Lemon? Incentives and Selection in the Public Provision of Criminal Defense." *The Review of Economics and Statistics* 103 (2): 294–309.

Agan, Amanda, Andrew Garin, Dmitri K Koustas, Alexandre Mas, and Crystal Yang. 2024. "Can you Erase the Mark of a Criminal Record? Labor Market Impacts of Criminal Record Remediation." NBER Working Paper Series No.32394.

Agan, Amanda, and Sonja Starr. 2017. "Ban the Box, Criminal Records, and Racial Discrimination: A Field Experiment." *The Quarterly Journal of Economics* 133 (1): 191–235.

Alschuler, Albert W. 1968. "The Prosecutor's Role in Plea Bargaining." *University of Chicago Law Review* 36 (1).

Arora, Ashna. 2019. "Too Tough on Crime? The Impact of Prosecutor Politics on Incarceration." Working Paper.

Ash, Elliott, and Bentley Macleod. 2021. "Reducing partisanship in judicial elections can improve judge quality: Evidence from U.S. state supreme courts." *Journal of Public Economics* 201.

Augustine, Elsa, Johanna Lacoe, Steven Raphael, and Alissa Skog. 2022. "The Impact of Felony Diversion in San Francisco." *Journal of Policy Analysis and Management* 41 (3): 683–709.

Austin, Peter C. 2011. "Optimal caliper widths for propensity-score matching when estimating differences in means and differences in proportions in observational studies." *Pharmaceutical Statistics* 10 (2): 150–161.

Beach, Brian, and Daniel B. Jones. 2017. "Gridlock: Ethnic Diversity in Government and the Provision of Public Goods." *American Economic Journal: Economic Policy* 9 (1): 112–36.

Beach, Brian, Daniel B. Jones, Tate Twinam, and Randall Walsh. 2024. "Racial and Ethnic Representation in Local Government." *American Economic Journal: Economic Policy* 16 (2): 1–36.

Besley, Timothy J., and Stephen Coate. 1997. "An Economic Model of Representative Democracy." *Quarterly Journal of Economics* 112 (1): 85–114.

Bibas, Stephanos. 2004. "Plea Bargaining outside the Shadow of Trial." *Harvard Law Review* 117 (8): 2463–2547.

Bowers, Josh. 2010. "Legal Guilt, Normative Innocence, and The Equitable Decision Not to Prosecute." *Columbia Law Review* 110 (7).

Calonico, Sebastian, Matias Cattaneo, and Max Farrell. 2020. "Optimal Bandwidth Choice for Robust Bias-corrected Inference in Regression Discontinuity Designs." *The Econometrics Journal* 23 (2): 192–210.

Choi, Jay, David Kilmer, Michael Mueller-Smith, and Sema A. Taheri. 2023. "Hierarchical approaches to Text-based Offense Classification." *Science Advances* 9 (9): 1–15.

Cohen, Alma, and Crystal S. Yang. 2019. "Judicial Politics and Sentencing Decisions." *American Economic Journal: Economic Policy* 11 (1): 160–91.

Court Statistics Project. 2020. "State Court Caseload Digest, 2018 Data."

Dippel, Christian. 2022. "Political Parties Do Matter in U.S. Cities...For Their Unfunded Pensions." *American Economic Journal: Economic Policy* 14 (3): 33–54.

Dippel, Christian, and Michael Poyker. 2019. "How Common are Electoral Cycles in Criminal Sentencing?" NBER Working Paper No. 25716.

Dobbie, Will, Jacob Goldin, and Crystal S. Yang. 2018. "The Effects of Pretrial Detention on Conviction, Future Crime, and Employment: Evidence from Randomly Assigned Judges." *American Economic Review* 108 (2): 201–40.

Enns, Peter. 2014. "The Public's Increasing Punitiveness and Its Influence on Mass Incarceration in the United States." *American Journal of Political Science* 58 (4): 857–72.

Feigenberg, Benjamin, and Conrad Miller. 2021. "Racial Divisions and Criminal Justice: Evidence from Southern State Courts." *American Economic Journal: Economic Policy* 13 (2): 207–40.

Ferreira, Fernando, and Joseph Gyourko. 2009. "Do Political Parties Matter? Evidence from U.S. Cities." *The Quarterly Journal of Economics* 124 (1): 399–422.

Fischer, Brett. 2023. "No Spending without Representation: School Boards and the Racial Gap in Education Finance." *American Economic Journal: Economic Policy* 15 (2): 198–235.

Garin, Andrew, Dmitri Koustas, Carl McPherson, Samuel Norris, Matthew Pecenco, Evan Rose, Yotam Shem-Tov, and Jeffrey Weaver. 2023. "The Impact of Incarceration on Employment, Earnings, and Tax Filing." *SSRN Electronic Journal,* https://ssrn.com/abstract=4536659.

Garro, Haritz, and Allison Stashko. 2023. "Prosecutor Elections and Police Killings." *Working Paper.*

Gramlich, John. 2016. "Voters' perceptions of crime continue to conflict with reality." https://www.pewresearch.org/fact-tank/2016/11/16/voters-perceptions-of-crime-continue-to-conflict-with-reality/.

———. 2021. "U.S. public divided over whether people convicted of crimes spend too much or too little time in prison." https://www.pewresearch.org/fact-tank/2021/12/06/u-s-public-divided-over-whether-people-convicted-of-crimes-spend-too-much-or-too-little-time-in-prison/.

Grembi, Veronica, Tommaso Nannicini, and Ugo Troiano. 2016. "Do Fiscal Rules Matter?" *American Economic Journal: Applied Economics* 8 (3): 1–30.

Humphries, John, Aurélie Ouss, Kamelia Stavreva, Megan Stevenson, and Winnie. van Dijk. 2023. "Conviction, Incarceration, and Recidivism: Understanding the Revolving Door." *Working Paper.*

Jordan, Andrew, Ezra Karger, and Derek Neal. 2022. "Heterogeneous Impacts of Sentencing Decisions." *SSRN Electronic Journal,* https://dx.doi.org/10.2139/ssrn.3927995.

Kling, Jeffrey R. 2006. "Incarceration Length, Employment, and Earnings." *American Economic Review* 96 (3): 863–876.

Krumholz, Sam. 2020. "The Effect of District Attorneys on Local Criminal Justice Outcomes." *SSRN Electronic Journal,* https://ssrn.com/abstract=3243162.

Lee, David S., Enrico Moretti, and Matthew J. Butler. 2004. "Do Voters Affect or Elect Policies? Evidence from the U. S. House." *The Quarterly Journal of Economics* 119 (3): 807–859.

Loeffler, Charles, and Daniel Nagin. 2022. "The Impact of Incarceration on Recidivism." *Annual Review of Criminology* 5:133–152.

Macartney, Hugh, and John Singleton. 2018. "School Boards and Student Segregation." *Journal of Public Economics* 164:165–182.

McCarthy, Justin. 2020. "Perceptions of Increased U.S. Crime at Highest Since 1993." https://news.gallup.com/poll/323996/perceptions-increased-crime-highest-1993.aspx.

Mueller-Smith, Michael. 2015. "The Criminal and Labor Market Impacts of Incarceration." Working Paper.

Mueller-Smith, Michael, and Kevin T. Schnepel. 2020. "Diversion in the Criminal Justice System." *The Review of Economic Studies* 88 (2): 883–936.

Ouss, Aurélie, and Megan Stevenson. 2023. "Does Cash Bail Deter Misconduct?" *American Economic Journal: Applied Economics* 15 (3): 150–182.

Piehl, Anne, and Shawn Bushway. 2007. "Measuring and Explaining Charge Bargaining." *Journal of Quantitative Criminology* 23:105–125.

Rose, Evan, and Yotam Shem-Tov. 2021. "How Does Incarceration Affect Reoffending? Estimating the Dose-Response Function." *Journal of Political Economy* 129 (12): 3302–3356.

Shem-Tov, Yotam. 2022. "Make or Buy? The Provision of Indigent Defense Services in the United States." *The Review of Economics and Statistics* 1 (9).

Shi, Ying, and John D. Singleton. 2023. "School Boards and Education Production: Evidence from Randomized Ballot Order." *American Economic Journal: Economic Policy* 15 (1): 438–72.

Sloan, CarlyWill. 2020. "How Much Does Your Prosecutor Matter? An Estimate of Prosecutorial Discretion." Working Paper.

Stith, Kate. 2008. "The Arc of the Pendulum: Judges, Prosecutors, and the Exercise of Discretion." *Yale Law Journal* 117:1420–1497.

Tuttle, Cody. 2023. "Racial Discrimination in Federal Sentencing: Evidence from Drug Mandatory Minimums." Working Paper.

White, Ariel. 2019. "Misdemeanor Disenfranchisement? The Demobilizing Effects of Brief Jail Spells on Potential Voters." *American Political Science Review* 113 (2): 311–324.

Yokley, Eli. 2021. "Most Voters See Violent Crime as a Major and Increasing Problem. But They're Split on Its Causes and How to Fix It." https://morningconsult.com/2021/07/14/violent-crime-public-safety-polling/.

Appendix

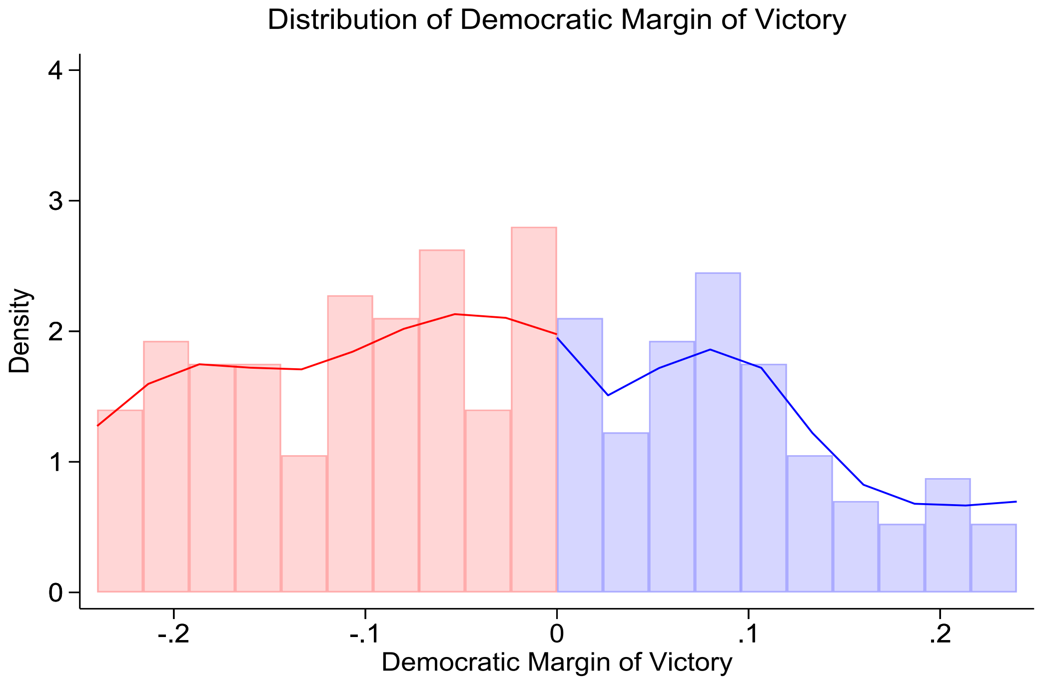

Figure A1: The sample includes 162 competitive district attorney races with at least one Democrat and one Republican running that were decided by 20 percentage points or less. "Democratic margin of victory" refers to the difference between the top-performing Democrat's vote share and the top-performing Republican's vote share in the election.

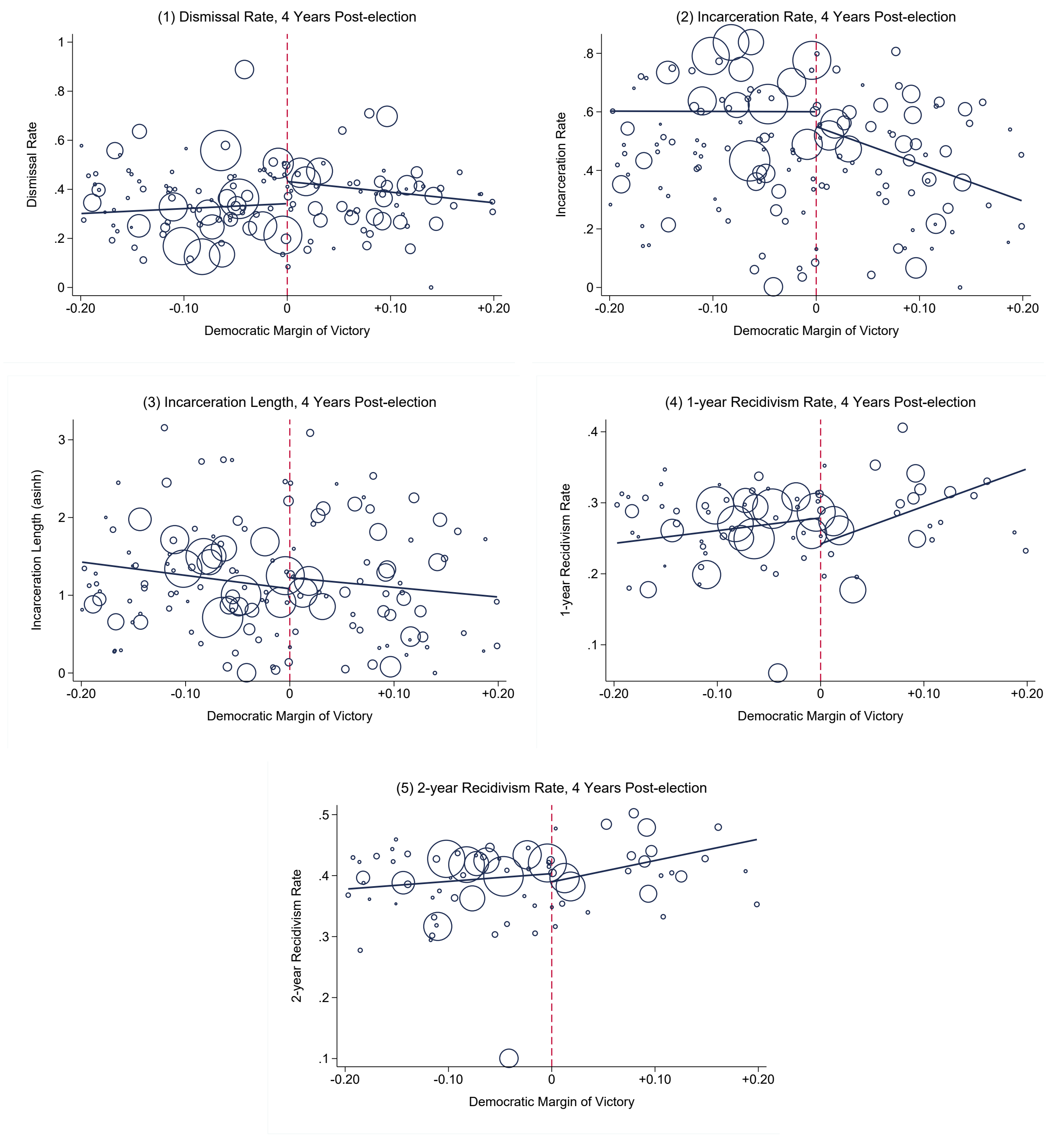

Figure A2: Scatter plots show the jurisdiction-level mean outcome in the fourth year after a contested DA election, along with lines of best fit. Each point corresponds to a single jurisdiction-election. Larger points correspond to more populous jurisdictions. For panels 1-3, N=162 jurisdiction-elections. For panels 4 and 5, N=87 jurisdiction-elections.

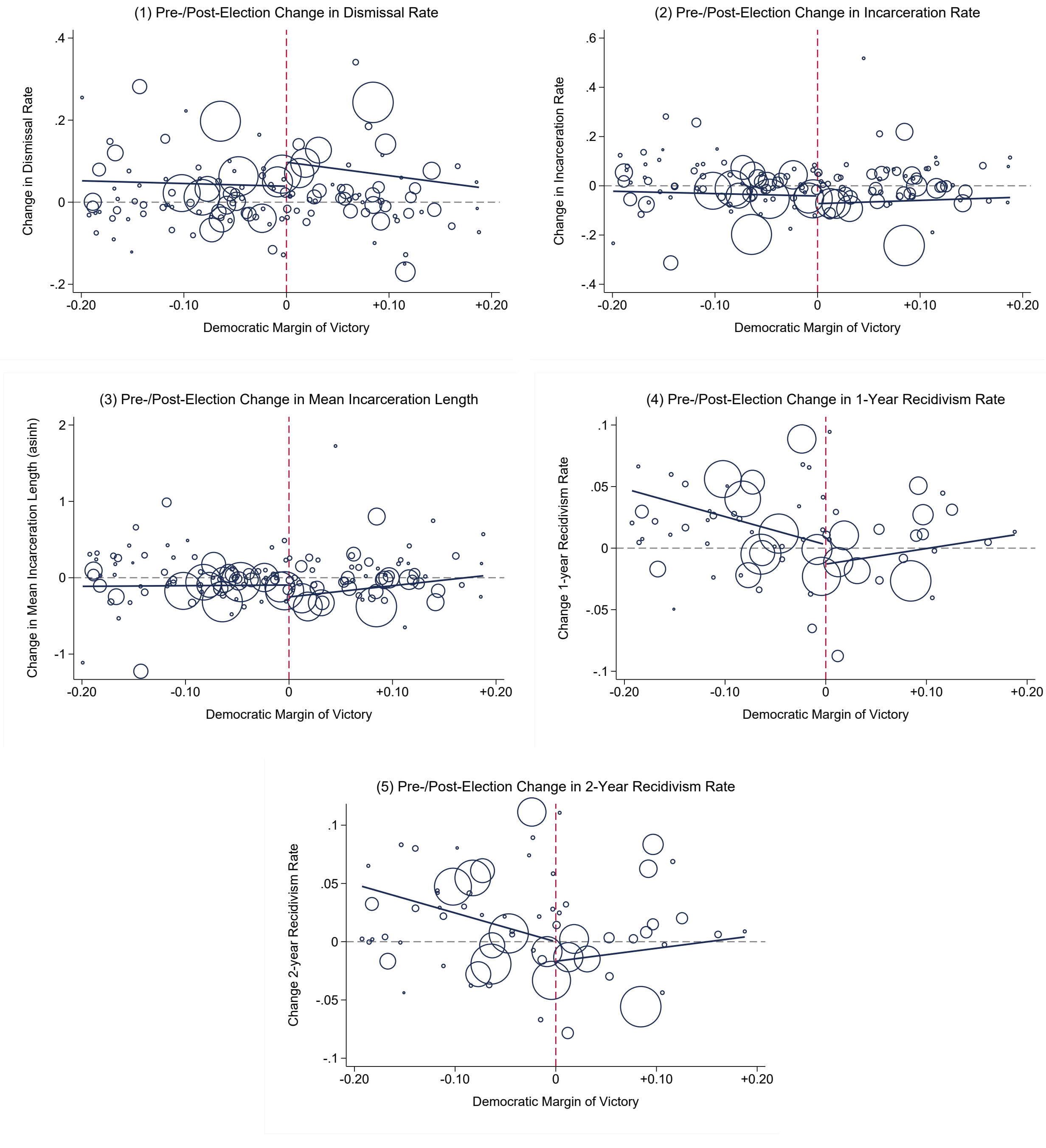

Figure A3: Scatter plots show the jurisdiction-level change in mean outcome between the three years leading up to the election and the four years following the election, along with lines of best fit. Each point corresponds to a single jurisdiction-election. Larger points correspond to more populous jurisdictions. For panels 1-3, N=162 jurisdiction-elections. For panels 4 and 5, N=87 jurisdiction-elections.

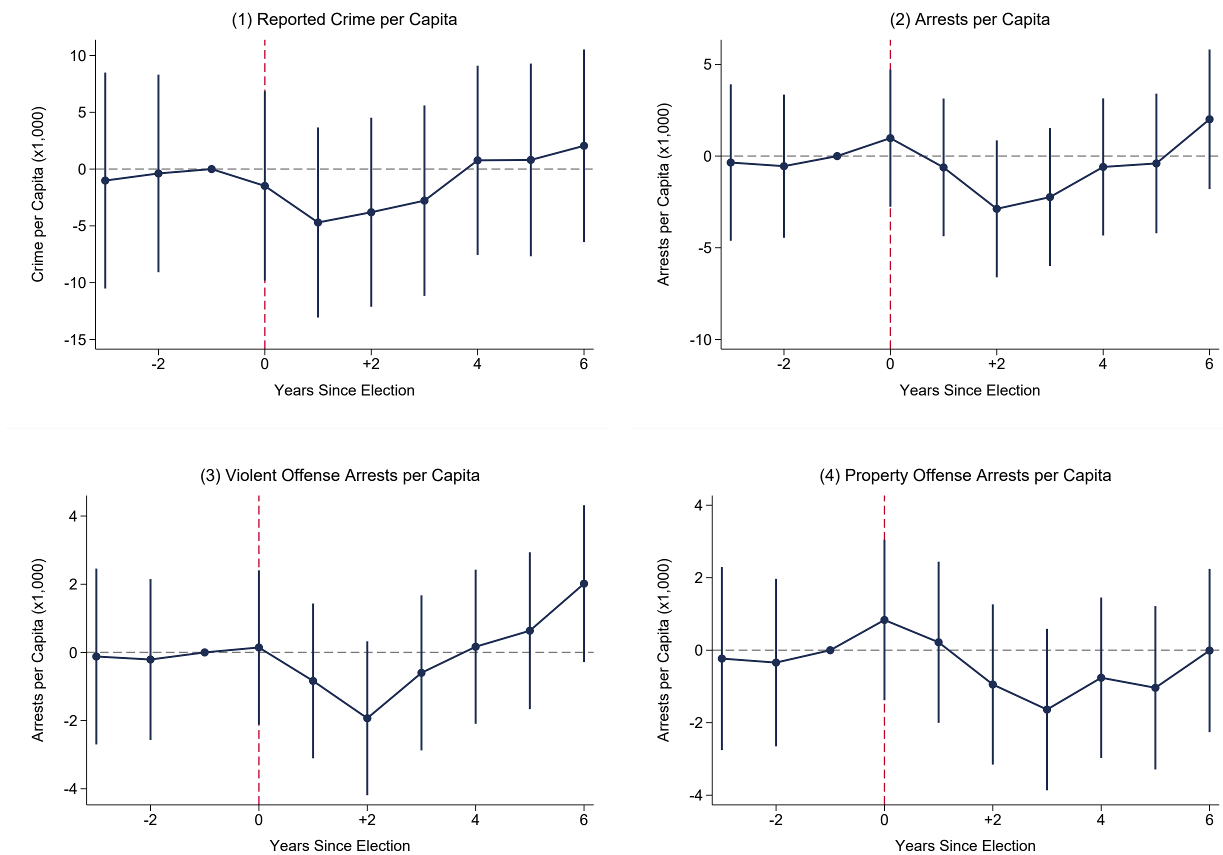

Figure A4: Panels 1 through 4 plot panel RD estimates showing the impact of a narrow Democratic election win on the reported crime (panel 1), arrests (panel 2), violent offense arrests (panel 3), and property offense arrests (panel 4) per 1,000 residents by year relative to the election. The specification is Equation 3. All coefficients are relative to the year prior to the election (period -1). Vertical bars denote 95 percent confidence intervals using robust standard errors clustered at the jurisdiction-election level. The sample contains 162 jurisdiction-elections. Data on reported crime and arrests come from the FBI Uniform Crime Reports (UCR). Mean election-year outcomes in jurisdiction-elections decided by 8 percentage points or less are 46.2, 15.3, 9.1, and 6.2 for each of the panels, respectively.

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

		DA Partisanship	
	Full Sample	Democrat	Republican
	(1)	(2)	(3)
I. Case Outcomes			
Case Dismissal	0.40	0.47	0.34
	(0.49)	(0.50)	(0.48)
Incarceration	0.40	0.33	0.47
	(0.49)	(0.47)	(0.50)
Incarceration Length (asinh)	0.90	0.78	1.02
	(1.49)	(1.44)	(1.54)
N:	*14,254,490*	*6,846,317*	*7,366,227*
1-year Recidivism	0.30	0.30	0.30
	(0.46)	(0.46)	(0.46)
N:	*9,033,287*	*3,811,093*	*5,222,194*
2-year Recidivism	0.40	0.40	0.41
	(0.49)	(0.49)	(0.49)
N:	*8,340,464*	*3,490,446*	*4,850,018*
II. Defendant Characteristics			
Age	32.81	32.99	32.68
	(11.30)	(11.43)	(11.21)
N:	*11,043,092*	*4,560,044*	*6,483,048*
Female?	0.26	0.25	0.27
	(0.44)	(0.44)	(0.44)
Nonwhite?	0.54	0.61	0.47
	(0.50)	(0.49)	(0.50)
III. Case Characteristics			
# of Charges	1.85	1.92	1.79
	(2.44)	(2.57)	(2.31)
Felony?	0.33	0.37	0.30
	(0.47)	(0.48)	(0.46)
Property Offense?	0.31	0.30	0.31
	(0.46)	(0.46)	(0.46)
Violent Offense?	0.19	0.20	0.19
	(0.40)	(0.40)	(0.39)
Drug Offense?	0.24	0.23	0.24
	(0.42)	(0.42)	(0.43)
Traffic Offense?	0.04	0.04	0.05
	(0.21)	(0.19)	(0.22)
Other Offense?	0.40	0.42	0.38
	(0.49)	(0.49)	(0.49)
N:	*14,254,490*	*6,846,317*	*7,366,227*

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=14,254,490). Column 2 focuses on cases filed in jurisdiction-years in which a Democratic DA held office (N=6,846,317). Column 3 focuses on cases filed in jurisdiction-years in which a Republican DA held office (N=7,366,227). 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	Close-elections Sample
	(1)	(2)	(3)
I. Case Outcomes			
Case Dismissed?	0.40	0.35	0.37
	(0.49)	(0.48)	(0.48)
Incarceration?	0.40	0.52	0.52
	(0.49)	(0.50)	(0.50)
Incarceration Length (asinh)	0.90	1.09	1.04
	(1.49)	(1.55)	(1.51)
N:	*14,254,490*	*6,112,048*	*3,092,181*
1-year Recidivism	0.30	0.29	0.29
	(0.46)	(0.45)	(0.45)
N:	*9,033,287*	*4,571,013*	*2,538,451*
2-year Recidivism	0.40	0.41	0.41
	(0.49)	(0.49)	(0.49)
N:	*8,340,464*	*4,307,525*	*2,376,598*
II. Defendant Characteristics			
Age	32.81	32.55	32.85
	(11.30)	(11.13)	(11.25)
N:	*11,043,092*	*5,283,625*	*2,935,312*
Female	0.26	0.25	0.25
	(0.44)	(0.43)	(0.43)
Nonwhite	0.54	0.61	0.63
	(0.50)	(0.49)	(0.48)
III. Case Characteristics			
# of Charges	1.85	1.69	1.61
	(2.44)	(1.91)	(1.55)
Felony Offense?	0.33	0.35	0.33
	(0.47)	(0.48)	(0.47)
Property Offense?	0.31	0.33	0.33
	(0.46)	(0.47)	(0.47)
Violent Offense?	0.19	0.20	0.20
	(0.40)	(0.40)	(0.40)
Drug Offense?	0.24	0.26	0.25
	(0.42)	(0.44)	(0.43)
Traffic Offense?	0.04	0.04	0.04
	(0.21)	(0.20)	(0.19)
Other Offense?	0.40	0.32	0.32
	(0.49)	(0.47)	(0.47)
N:	*14,254,490*	*6,112,048*	*3,092,181*

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 8 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: Robustness of Panel Estimates to Alternative Specifications and Samples

	Margin of a "Close" Election						
	5 Percentage Points (1)	6 Percentage Points (2)	7 Percentage Points (3)	8 Percentage Points (4)	10 Percentage Points (5)	Quadratic Polynomial (6)	"Donut" of ± 1pp (7)
Case Dismissal	0.038	0.069***	0.080**	0.071**	0.029	0.074**	0.096**
	(0.023)	(0.022)	(0.031)	(0.027)	(0.029)	(0.030)	(0.036)
Incarceration	-0.057*	-0.073***	-0.094***	-0.080***	-0.032	-0.085***	-0.102***
	(0.030)	(0.018)	(0.028)	(0.024)	(0.030)	(0.027)	(0.029)
Incarceration Length (asinh)	-0.261***	-0.401***	-0.439***	-0.402***	-0.247***	-0.427***	-0.450***
	(0.261)	(0.076)	(0.068)	(0.075)	(0.077)	(0.083)	(0.100)
N:	*2,672,691*	*3,171,482*	*3,890,544*	*4,280,419*	*5,284,386*	*4,280,419*	*3,443,856*
1-year Recidivism	0.003	0.003	0.002	-0.006	-0.010	-0.005	-0.030**
	(0.009)	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)	(0.011)
N:	*2,347,075*	*2,532,388*	*3,106,312*	*3,490,276*	*4,322,243*	*3,490,276*	*2,688,410*
2-year Recidivism	-0.004	0.000	-0.004	-0.009	-0.011	-0.009	-0.055***
	(0.007)	(0.004)	(0.005)	(0.007)	(0.008)	(0.008)	(0.008)
N:	*2,294,396*	*2,463,865*	*3,375,311*	*2,991,347*	*4,153,200*	*3,375,311*	*2,601,498*
Year FEs	Y	Y	Y	Y	Y	Y	Y
Jurisdiction FEs	Y	Y	Y	Y	Y	Y	Y
Defendant/Case Covariates	Y	Y	Y	Y	Y	Y	Y
Periods Included	[-3,4]	[-3,4]	[-3,4]	[-3,4]	[-3,4]	[-3,4]	[-3,4]
"Close" Election Margin	± 5pp	± 6pp	± 7pp	± 8pp	± 10pp	± 8pp	± 8pp

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

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 Democratic DA victory. Our baseline specification is Equation 2, estimated among cases filed in jurisdiction-elections decided by less than 8 percentage points. Columns 1 through 4 present estimates from Equation 2 estimated using alternative definitions of "close" elections, as described in the header. Column 5 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. Column 6 shows estimates from a sample that excludes elections decided by 1 percentage point or less. Robust standard errors clustered at the jurisdiction-election level appear in parentheses.

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

	Case-level Cross-sectional Model			Case-level Panel Model			
	Period 4 only (1)	+ Periods 1 thru 3 (2)	+ Jurisdiction + Year FEs (3)	+ Period 0 (4)	Preferred Specification (5)	+ Covariates (6)	+ Periods 5 and 6 (7)
Case Dismissed	0.08	0.03	0.10***	0.08*	0.08***	0.07**	0.07**
	(0.08)	(0.09)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)
Incarceration	0.03	0.07	-0.10***	-0.08**	-0.09***	-0.08***	-0.09***
	(0.13)	(0.13)	(0.04)	(0.04)	(0.03)	(0.02)	(0.03)
Incarceration Length (asinh)	0.26	0.37	-0.24**	-0.22**	-0.33***	-0.40***	-0.43***
	(0.25)	(0.23)	(0.11)	(0.09)	(0.06)	(0.07)	(0.08)
N:	529,620	2,242,678	2,242,678	2,777,577	4,280,419	4,272,376	5,120,777
1-year Recidivism	-0.06***	-0.03**	0.01	-0.02	-0.01	-0.01	-0.01
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
N:	415,520	1,846,380	1,846,380	2,277,059	3,490,276	3,490,258	4,182,326
2-year Recidivism	-0.06***	-0.03*	-0.00	-0.01	-0.01	-0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
N:	368,643	1,731,415	1,731,415	2,162,094	3,375,311	3,375,293	4,020,473
Year FEs	N	N	Y	Y	Y	Y	Y
Jurisdiction FEs	N	N	Y	Y	Y	Y	Y
Defendant/Case Covariates	N	N	N	N	N	Y	Y
Period(s) Included	4	[1-4]	[1-4]	[0-4]	[-3-4]	[-3-4]	[-3,6]
Unit of Observation	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 8 percentage points or less. The specification in columns 1-3 is Equation 1, applied to case-level data. The specification in columns 4-7 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 V. Standard errors in parentheses are clustered at jurisdiction-election level.

Table A5: 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	0.080***	0.088***	0.076**	0.089**	0.074***	0.083**	0.075***
	(0.030)	(0.028)	(0.034)	(0.037)	(0.028)	(0.034)	(0.028)
	0.327	*0.309*	*0.361*	*0.417*	*0.298*	*0.331*	*0.323*
Incarceration	-0.088***	-0.097***	-0.076***	-0.093***	-0.084***	-0.092***	-0.082***
	(0.027)	(0.025)	(0.030)	(0.034)	(0.024)	(0.030)	(0.025)
	0.572	*0.631*	*0.465*	*0.471*	*0.604*	*0.580*	*0.564*
Incarceration Length (asinh)	-0.333***	-0.347***	-0.315***	-0.244***	-0.349***	-0.321***	-0.327***
	(0.063)	(0.054)	(0.084)	(0.065)	(0.061)	(0.061)	(0.072)
	1.140	*1.230*	*0.979*	*0.780*	*1.255*	*1.090*	*1.184*
N:	*4,280,419*	*2,757,871*	*1,514,505*	*1,040,502*	*3,239,902*	*2,026,199*	*2,254,220*

	Case Characteristics						
	Case Severity		Violence		Types of Offense		
	Felony	Misdemeanor	Violent	Nonviolent	Property	Drug	Other
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Case Dismissal	0.089*	0.069***	0.052	0.090***	0.092***	0.056	0.128***
	(0.050)	(0.026)	(0.033)	(0.031)	(0.032)	(0.035)	(0.037)
	0.283	*0.350*	*0.397*	*0.309*	*0.281*	*0.262*	*0.338*
Incarceration	-0.095*	-0.075**	-0.078**	-0.092***	-0.102***	-0.055*	-0.125**
	(0.053)	(0.030)	(0.031)	(0.027)	(0.029)	(0.033)	(0.051)
	0.646	*0.533*	*0.522*	*0.584*	*0.649*	*0.667*	*0.466*
Incarceration Length (asinh)	-0.612***	-0.255***	-0.358***	-0.318***	-0.404***	-0.275***	-0.381***
	(0.147)	(0.041)	(0.082)	(0.060)	(0.074)	(0.085)	(0.098)
	2.134	*0.626*	*1.445*	*1.061*	*1.205*	*1.373*	*0.892*
N:	*1,459,129*	*2,821,288*	*877,314*	*3,403,105*	*1,467,578*	*1,132,986*	*1,256,303*
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 8 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 A6: Heterogeneity in Democratic DA Treatment Effect on Recidivism: 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)
1-year Recidivism	-0.009	-0.010*	-0.002	-0.002	-0.011*	-0.008	-0.012**
	(0.006)	(0.006)	(0.009)	(0.009)	(0.006)	(0.007)	(0.006)
	0.287	*0.279*	*0.309*	*0.249*	*0.299*	*0.308*	*0.265*
N:	*3,490,276*	*2,543,163*	*947,095*	*844,101*	*2,646,175*	*1,764,110*	*1,726,166*
2-year Recidivism	-0.011	-0.016**	0.008	-0.010	-0.010	-0.009	-0.014*
	(0.008)	(0.007)	(0.013)	(0.012)	(0.007)	(0.009)	(0.007)
	0.406	*0.398*	*0.428*	*0.354*	*0.423*	*0.438*	*0.374*
N:	*3,375,311*	*2,460,216*	*915,077*	*815,634*	*2,559,677*	*1,712,256*	*1,663,055*

	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	0.001	-0.012*	-0.011*	-0.010	-0.012	-0.000	-0.030***
	(0.009)	(0.006)	(0.006)	(0.006)	(0.008)	(0.009)	(0.012)
	0.253	*0.304*	*0.253*	*0.304*	*0.340*	*0.291*	*0.284*
N:	*1,185,083*	*2,305,193*	*677,399*	*2,812,877*	*1,187,381*	*965,846*	*919,056*
2-year Recidivism	0.003	-0.018**	-0.012	-0.013	-0.017	0.004	-0.039**
	(0.011)	(0.008)	(0.010)	(0.008)	(0.011)	(0.011)	(0.016)
	0.389	*0.415*	*0.389*	*0.415*	*0.464*	*0.429*	*0.394*
N:	*1,145,683*	*2,229,628*	*651,244*	*2,724,067*	*1,155,904*	*937,307*	*881,626*
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 8 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 A7: Heterogeneity in Democratic DA Treatment Effect

			Defendant Characteristics			Case Characteristics			
	(1) Sample Mean	(2) Baseline	(3) Nonwhite	(4) Female	(5) Young	(6) Felony	(7) Violent	(8) Drug	(9) Property
I. Probability of Dismissal									
Case Dismissed	0.327	0.080***	0.000	0.014	0.008	0.069	-0.035	-0.022	0.033
		(.030)	(0.021)	(0.018)	(0.015)	(0.082)	(0.026)	(0.027)	(0.035)
			0.080	*0.075*	*0.073*	*0.059*	*0.089*	*0.089*	*0.069*
II. Incarceration Outcomes									
Incarceration	0.572	-0.088***	-0.011	-0.012	-0.009	-0.062	0.009	0.042	-0.032
		(0.027)	(0.020)	(0.019)	(0.015)	(0.091)	(0.022)	(0.031)	(0.033)
			-0.079	*-0.084*	*-0.081*	*-0.072*	*-0.091*	*-0.104*	*-0.076*
Incarceration Length (asinh)	1.140	-0.333***	-0.035	0.001	-0.046	-0.403	0.044	0.094	-0.079**
		(0.063)	(0.054)	(0.068)	(0.048)	(0.263)	(0.064)	(0.136)	(0.033)
			-0.306	*-0.327*	*-0.300*	*-0.304*	*-0.338*	*-0.372*	*-0.306*
N:	*4,280,419*	*4,280,419*	*4,272,376*	*4,280,404*	*4,280,419*	*4,280,419*	*4,280,419*	*4,280,419*	*4,280,419*
III. Recidivism Rate									
1-year Recidivism	0.287	-0.009	-0.011	0.019	0.012	0.004	-0.016	0.005	-0.009
		(0.006)	(0.023)	(0.012)	(0.012)	(0.016)	(0.014)	(0.018)	(0.020)
			0.003	*-0.013*	*-0.016*	*-0.009*	*-0.007*	*-0.011*	*-0.005*
N:	*3,490,276*	*3,490,276*	*3,490,258*	*3,490,276*	*3,490,276*	*3,490,276*	*3,490,276*	*3,490,276*	*3,490,276*
2-year Recidivism	0.406	-0.011	-0.019	0.007	0.015	0.019	-0.016	0.017	-0.004
		(0.008)	(0.030)	(0.011)	(0.012)	(0.016)	(0.020)	(0.022)	(0.027)
			0.012	*-0.004*	*-0.014*	*-0.013*	*-0.003*	*-0.009*	*-0.006*
N:	*3,375,311*	*3,375,311*	*3,375,293*	*3,375,311*	*3,375,311*	*3,375,311*	*3,375,311*	*3,375,311*	*3,375,311*

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

Each cell comes from a separate regression. The sample consists of cases filed in jurisdiction-elections decided by 8 percentage points or less. Column 1 provides the sample mean of each outcome in the left-hand column. Each cell reports panel RD estimates of the difference in the effect of a Democratic DA victory for a specific subgroup. Estimates come from a modified Equation 2, where the triple interaction becomes a quadruple interaction with the subgroup variable The specific subgroup in the interaction, as defined by a defendant or case characteristic, is described in the column header. Robust standard errors in parentheses are clustered at jurisdiction-election level. The baseline estimate of the effect of a Democratic DA victory from a given regression appears in italics below the standard error.

Table A8: Comparing Democratic- and Republican-led DA Jurisdictions: UCR Statistics

		Democratic DAs		Republican DAs	
	Full Sample	Uncontested Election	Contested Election	Contested Election	Uncontested Election
	(1)	(2)	(3)	(4)	(5)
I. UCR Outcomes					
Arrests per Capita ($\times$1,000)	12.36	12.04	14.40	13.53	12.11
	(8.09)	(8.13)	(9.38)	(8.45)	(7.67)
Reported Crime per Capita ($\times$1,000)	35.97	35.98	45.18	39.23	33.94
	(19.92)	(21.13)	(22.76)	(20.33)	(17.62)
Violent Offense Arrests per Capita ($\times$1,000)	7.27	7.06	8.69	8.07	7.06
	(5.06)	(4.99)	(5.97)	(5.37)	(4.82)
Property Offense Arrests per Capita ($\times$1,000)	5.09	4.99	5.71	5.46	5.05
	(3.52)	(3.59)	(3.82)	(3.58)	(3.40)
II. Court Outcomes					
Cases Dropped per Arrest	1.83	2.48	3.59	1.12	1.18
	(9.84)	(14.17)	(15.96)	(2.71)	(2.68)
Criminal Cases Filed per 1,000 Pop	24.99	25.90	49.00	22.26	20.97
	(47.26)	(51.06)	(114.94)	(23.58)	(22.87)
N:	*4,831*	*1,788*	*340*	*596*	*2,042*

The data describe the crime, arrest, 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=4,831). 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. Crime and arrest information come from the FBI's Uniform Crime Reports (UCR). Sample sizes vary across columns because some DA election winners represent third parties, while others do not have an identified partisan affiliation.

Table A9: Panel Results by Defendant Prior Criminal Case

	Full Sample	Prior Court Appearance?		
		None	≤ 1 Yr Ago	≤ 2 Yrs Ago
	(1)	(2)	(3)	(4)
Case Dismissed?	0.071**	0.056	0.053**	0.059***
	(0.027)	(0.042)	(0.023)	(0.021)
Incarceration?	-0.081***	-0.070**	-0.067***	-0.072***
	(0.024)	(0.032)	(0.025)	(0.022)
Incarceration Length (asinh)	-0.402***	-0.240***	-0.411***	-0.426***
	(0.068)	(0.064)	(0.107)	(0.092)
N:	*4,272,376*	*1,901,896*	*1,900,408*	*2,370,480*
1-year Recidivism	-0.005	-0.009*	-0.000	-0.011
	(0.006)	(0.005)	(0.014)	(0.016)
N:	*3,490,258*	*1,875,914*	*1,149,908*	*1,614,344*
2-year Recidivism	-0.008	-0.013	-0.002	-0.009
	(0.008)	(0.008)	(0.014)	(0.016)
N:	*3,375,293*	*1,808,584*	*1,116,232*	*1,566,709*

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

Each cell reports a panel regression estimate following Equation 2. All specifications include jurisdiction and year fixed effects but no other covariates. Column 1 reports full-sample estimates which correspond to output shown in Tables 3 and 7. The remaining columns present subgroup-specific effects. Column 2 focuses on defendants who did not appear on any court case within two years. Column 3 focuses on defendants who appeared on a criminal case within the last twelve months. Column 4 focuses on defendants who appeared on a criminal case within the last 24 months. Note that column 4 includes all defendants who also appear in the sample for column 3. Robust standard errors clustered at the jurisdiction-election level appear in parentheses.

Table A10: Balance in Matched Sample

	Control Mean	Balance Estimate
	(1)	(2)
I. Jurisdiction Characteristics		
Income per Capita ($2016)	44,114	770
	(9296)	(647)
N:	*7,110,617*	*12,774,662*
log(Population)	12.8	-0.03
	(1.47)	(0.12)
N:	*7,362,996*	*12,479,135*
Share Nonwhite	0.272	0.005
	(0.272)	(0.013)
N:	*7,360,583*	*13,387,748*
II. Defendant and Case Characteristics		
Defendant age	28.6	-0.1
	(15.1)	(0.6)
N:	*7,362,996*	*12,479,135*
# of Charges	1.73	0.14***
	(1.2)	(0.03)
N:	*7,362,996*	*12,479,135*

*** $p < 0.01$

The sample includes cases in the matching sample. Column 1 reports the mean of the outcome variable in the left-hand column among cases created under a Republican district attorney within the matching sample. Standard deviations appear in parentheses. Column 2 presents matching estimates of the Democratic DA effect. 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.