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

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May 14, 2025

Abstract

We evaluate the causal effect of district attorney (DA) politics on criminal case dismissal rates and recidivism. Using variation in DA partisanship stemming from close elections, 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. Strikingly, though, defendants in Democratic-led jurisdictions are no more likely to face future criminal charges, consistent with the notion that higher conviction rates have limited deterrence effects. Our findings underscore how the punitiveness of the criminal justice system hinges on the partisanship of local district attorneys.

JEL Classifications: D72, K14, K42

*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 with 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.

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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 seek 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. 2025; Kling 2006; Mueller-Smith 2015).

But a DA's choice of conviction rate also has political implications. Elected prosecutors must answer to their voters, who often see 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).

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 Democratic and Republican DAs exercise their prosecutorial authority, and ultimately assess how

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. Throughout this paper, we use the term "dismiss" to refer to a DA's decision to not seek a conviction. That is, we treat "dismissal" as a disposition synonymous with, for example, "dropped by the DA" or "*nolle prosequi*." We recognize that "dismissal" often refers to a court outcome, not necessarily a DA's decision. Nevertheless, many of the criminal justice datasets in our sample use this term to describe the disposition of cases on which a DA did not seek conviction. 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—as defined above, whereby the accused person is released without penalty—or conviction. Vanishingly few cases result in not-guilty verdicts. We return to these points in Section 2.

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 large urban jurisdictions where Democratic voters hold substantial majorities.[3]

Our study bridges these empirical gaps. We deploy a new administrative dataset that contains over ten million individual criminal case records from seven states, spanning 1999-2021.[4] Crucially, these data include criminal cases that prosecutors ultimately dismissed without penalty. Matching these court data to nearly 1,500 DA election 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 opposite 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 Republican 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

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. Although 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.

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.

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 has the advantage of isolating 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 < 0.01). We also find that electing a Democratic DA leads to 33 percent shorter incarceration sentences on average, or about 3 fewer months spent in jail or prison (p-value < 0.01).[6] Different controls and narrower definitions of "close" elections do not affect these findings. Expanding our attention to cases from all jurisdictions, our matching results indicate that the average Democratic DA dismisses 13 percent more cases—a smaller impact than the marginal Democratic DA, but economically meaningful nonetheless.

We further probe the data to understand whether these effects truly represent a choice of dismissal rate by prosecutor, as we implicitly hypothesize. Heterogeneity analyses show that DA partisanship has a similar effect on dismissal rates across types of cases and accused individuals, including for cases involving misdemeanor and felony offenses. In other words, Democratic DAs do not reduce conviction rates simply by dismissing the least seri-

6. Prosecutors do not unilaterally impose sentences, a power that rests with judges. Still, by choosing whether and which charges to pursue, DAs help 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.

ous offenses. Likewise, we find no evidence that police respond to DA elections by altering arrest rates, while incoming case caseloads remain constant. Our findings, then, are likely not a response to resource constraints. As such, we interpret our findings as evidence that Democratic DAs broadly seek fewer criminal convictions than their Republican counterparts, consistent with the policy preferences that Democratic voters express in surveys.

Democratic DAs' high dismissal and low incarceration rates likely serve the interest of defendants. 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. (2024) 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. Other recent studies—typified by Jordan, Karger, and Neal (2023) 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. 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 is thus theoretically ambiguous.

We find that, relative to the marginal Republican DA, the marginal Democratic DA has no statistically or economically significant effect on re-offense rates. 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). We stress that these estimates do not isolate the causal effects of case dismissal or non-carceral sentencing per se; rather, our results just capture the reduced-form effect of Democratic district attorneys on recidivism rates, which could operate through myriad policy channels. Still, our null findings indicate that DA partisanship, despite driving substantial differences in judicial outcomes, does not affect re-offense rates.[7] 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, we providing rigorous evidence that district attorney political identities substantially influence conviction and incarceration rates. This finding adds 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 do not necessarily 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.

2 Background and Data

Our goal is to examine how DA partisanship causally affects criminal case outcomes. To this end, we compile a unique multi-state dataset that links DA election returns with

7. Likewise, supplemental results indicate that DA partisanship does not affect crime rates, a fact which reinforces our claim that DA partisanship has little effect on criminal behavior.

detailed criminal justice records. While we attempted to collect data from all fifty states, we faced two constraints that ultimately limited the scope of this study. First, district attorney election records can be difficult to come by: many states do not collect these data centrally, while others do not digitize county election returns. Second, few state court systems provide the records of criminal cases that did not result in conviction—a critical omission given our focus on conviction and dismissal rates.

As we detail below, our final dataset includes seven states with suitable elections and criminal court data: Arkansas, Colorado, Kentucky, Maryland, North Carolina, Texas, and Virginia. Together, these states accounted for about one-fifth of the U.S. population as of the 2010 Census and around a quarter of all district attorneys.[8] 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.[9,10]

We begin this section by contextualizing district attorneys' role in the criminal process and clarifying how we define a "dismissed" criminal case. We then summarize the elections data that we use to identify DA partisanship, describe how we clean state criminal court records, and highlight differences between Democrat- and Republican-led court systems. Finally, we discuss how we construct our analytic dataset.

8. Again, we reached out to nearly 50 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. In addition to the states listed here, we collected criminal court data from Florida, New York, North Dakota, Oregon, and Pennsylvania, but could not use them in this study due to inconsistent reporting of dismissed cases (New York and Pennsylvania), lack of information on 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]).

9. Most of our study period 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.

10. These datasets generally cover all jurisdictions in their states, with two exceptions: our data from Colorado only include a subset of cases filed in Denver County, and our data from Virginia do not include any cases filed in Arlington County or Fairfax County.

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- or six-year terms. Once elected, DAs represent the state against individuals accused of felonies and most misdemeanor offenses. Each district attorney serves as the chief law enforcement officer in their judicial district, usually comprised of a single county or a grouping of small counties.[11] Many jurisdictions staff public prosecutors' offices with assistant district attorneys (ADAs), who often function as the day-to-day prosecuting attorneys on cases. Even so, the district attorney retains control of overall prosecutorial policy and supervises ADAs.

District attorneys can directly affect case dispositions by exercising prosecutorial discretion: they must decide whether to seek to convict individuals accused of criminal offenses. Immediately following an arrest by the police, DAs can decline to prosecute the arrested person; later on, even if they initially decided to prosecute, DAs can choose not to pursue a conviction.[12] We refer to both decisions as "dismissals": in both events, arrested individuals face no further criminal penalty.[13] We argue that the choice to dismiss a criminal case represents the most direct channel through which prosecutors can unilaterally affect defendant outcomes, which motivates us to focus on how DA partisanship influences dismissal rates.

Still, beyond the extensive-margin decision over whether to dismiss a case or not, prosecutors also make intensive-margin choices over charges of conviction and sentences to request. Based on the evidence, DAs may decide to seek a conviction on the arresting charge, or they

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

12. 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.

13. Our criminal court data do not distinguish between dismissals at the point of arrest and dismissals at later stages of the criminal process (for example, after the accused person's first court appearance). By the same token, based on data documentation and information from agency staff, we can only make an educated guess about which (if any) of our state datasets even contain offenses that a DA declined to prosecute immediately after arrest (i.e., the stage of the process examined by Agan, Doleac, and Harvey (2023)). To the best of our knowledge, our data contain all filed charges, and some states may also include arresting charges on which the DA immediately declined to pursue a criminal case. Put differently, we likely undercount the true number of "dismissals" (per our definition), although we belive the source of that undercount is (exogenous) state reporting rules rather than (endogenous) DA decisions.

may pursue alternative charges that support a stronger case, which in turn has implications for sentencing depending on the relative severity of those charges. More broadly, as members of the "courtroom workgroup," district attorneys interact with judges and defense attorneys to set the terms of plea bargains, which resolve the vast majority of criminal cases. During this process, DAs stipulate the offenses to which the accused person will plead guilty; by asking for guilty pleas on more serious offenses, DAs can implicitly encourage more serious punishments. Likewise, as part of the bargaining process, DAs can recommend to judges that specific sentences be imposed. One might expect that political considerations influence how DAs engage in this bargaining process, and so we consider as secondary outcomes incarceration rates and sentence lengths.

2.2 Background: Identifying Charges that a DA Dismissed

Our primary outcome is a measure of the rate at which DAs dismiss criminal cases, meaning they did not seek a conviction and the accused person would eventually be released without penalty. As we noted earlier, states refer to this disposition differently and offer DAs different options to achieve the same fundamental outcome for an accused person. Given these idiosyncracies, we consider three categories of dispositions to indicate that a charge was dismissed; in Appendix Table A1, we summarize how many "dismissed" cases have these dispositions by state.

First, we include the charges with the dispositions "dropped by the DA" or "dismissed by the DA," as well as those with a "*nolle prosequi*" disposition. All of these terms—used by Arkansas, Colorado, and Virginia—unambiguously indicate that a DA opted not to seek a conviction.

Second, we include charges that resulted in a diversion, a disposition in which the accused person admits their guilt and receives probation (a non-carceral sentence during which the person promises not to re-offend). Upon completion of their probation, the charge is dropped by the DA. This disposition is common in Maryland, Texas, and Kentucky: Maryland refers to these outcomes as "stets" while Texas refers to them as "deferred adjudications."

Finally, we include charges that are listed as having been "dismissed," provided that there is no indication that the case was dismissed by a judge rather than a DA. That is, when our data (described in the next subsection) make it clear that a judge, not the DA, dismissed a charge, we do not count that charge as "dismissed" for our purposes.

2.3 Data: District Attorney Election Data Cleaning

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 and treat the political party of the most recent election winner as the party of the sitting DA in a given year.[14] We calculate the winning party's margin of victory as a share of the total votes cast in a DA election.[15]

In Table 1, we summarize all 1,447 DA elections that took place in our sampled states between 2000 and 2018. Republican candidates won 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, and most of those 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).[16] Focusing on the 245 "politically competitive" races that included at least one Democrat and one Republican—the sample of greatest interest to us—we find that the average race resulted in a Democratic loss of 6 percentage points. In Section 3, we provide more detail about how

14. 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 party preference. We find that many contested elections include 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 does not affect our findings.

15. 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. 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 research design.

16. 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 further proof that most DA election results are foregone conclusions.

we use these election margins to identify the causal effect of electing a Democratic DA.

2.4 Data: Criminal Justice Data Cleaning

Each of the seven sets 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).

Yet, despite their superficial similarities, the state court records differ in their scope and level of detail. For example, several states include data on non-carceral sentences like probation and fines and fees, though many do not; some court systems include fine-grained information about the underlying offense and charge severity, but the majority do not. These inconsistencies mean that, in order to harmonize the different datasets, we have to accept a lower level of detail than we would like. Our approach to harmonizing our court records follows that of Feigenberg and Miller (2021), who compile a similar multi-state database of court records.

2.4.1 Aggregating Criminal Charges to Criminal Cases

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 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 dismissed by the DA (as defined in Subsection 2.2); an indicator for whether the charge resulted in an incarceration sentence; and the nominal sentence length, in months.[17]

We then aggregate our charge data to the criminal-case level, our unit of observation,

17. 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 on dismissal and incarceration rates.

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."[18] We categorize cases as property, violent, drug, traffic, or other if at least one charge within that case is of that type. Likewise, we define a felony case to include at least one felony charge.[19]

2.4.2 Constructing Primary and Secondary Outcomes

To construct our primary and secondary case-level outcome variables—case dismissal, incarceration, sentence length, and recidivism—we again look across charges within a case. We consider a case to be dismissed if all charges on that case were dismissed, acknowledging again that this catchall term refers to different state-specific charge outcomes, as we discuss above. We likewise 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 incarceration). 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.[20]

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

18. Our aggregation approach is a notable departure from Feigenberg and Miller (2021), whom we otherwise follow closely. Instead of relying on court-provided case identifiers, we group all charges for a particular defendant that either share a case identifier or were filed on the same day. This approach substantially reduces the number of "cases" in our sample relative to Feigenberg and Miller. Moreover, defining cases this way lowers the case dismissal rate and raises the conviction and incarceration rates in our sample: we observe incarceration rates that are 8-10 percentage points higher and dismissal rates that are 6-8 percentage points lower than those in Feigenberg and Miller. Our choice is relatively conservative in this regard as we hope to avoid mistaking charges dropped as part of an overall guilty plea on a case with true case dismissals (anecdotal and empirical evidence suggests that such "charge bargaining" is ubiquitous in the criminal justice system; see, for example, Piehl and Bushway [2007]).

19. Our data from Maryland do not include a reliable measure 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.

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

date. We anchor time around the filing date because time to disposition is endogenous: cases that a DA dismisses are usually resolved more quickly.[21] We say that a person recidivates if we observe them as a defendant on a subsequent criminal charge.[22] 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 analytic sample. When considering defendant recidivism as an outcome, we exclude cases filed within 1 or 2 years (depending on the exact outcome variable) of the end of their state's court data panel.

2.4.3 Harmonizing Court Records

We rely on fields in the raw data to identify defendant demographics at the time of case filing.[23] States consistently report defendant age and sex, but they report 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]

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 also omit cases that only involve traffic offenses because some states do not report low-level traffic infractions and misdemeanors. Lastly, we exclude cases involving defendants younger than 18.

21. As we discuss in Section 5, as a robustness check, we reassess our findings after redefining recidivism as any new criminal charge within 1 or 2 years of initial case disposition. We reach the same conclusion no matter which definition we use.

22. Some researchers justifiably oppose this definition of recidivism because arrest and charging probability depends on police behavior. In our setting, though, looking at outcomes outside the purview of police—such as future incarceration—does not make much sense, since those outcomes are directly influenced by DAs.

23. Court records from Virginia and Maryland provide no age or year of birth field.

24. 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.

2.4.4 Summary of Criminal Case Data

Our final sample contains 14,254,490 criminal cases, which we describe in Table 2. In column 1, we report that DAs dismiss 40 percent of cases, while another 40 percent of cases result in incarceration. Among all cases, 40 percent of defendants face a subsequent criminal case within two years. Most accused individuals are 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 are property offenses (e.g., theft) and "other" offenses against society (e.g., driving while intoxicated), which appear in 31 and 40 percent of cases, respectively.

The remaining columns of Table 2 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 disparities are likely due to a combination of factors. The standards for arrest, the types of charges police and prosecutors have at their disposal, and the types of charges jurisdictions must report to the state government (e.g., low-level traffic offenses, or arrests for offenses that a DA immediately declines to prosecute) vary across states. For example, our data from North Carolina include a large volume of "other" misdemeanor offenses, 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 priorities of district attorneys.

2.5 Data: Constructing Analytic 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.

To help describe the relationship between DA partisanship and features of their jurisdictions, we summarize 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 3 reports 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.

The second panel of Table 3 focuses on average defendant outcomes across court systems represented by Democratic and Republican DAs. 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

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.

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 again shows how DAs of opposing parties serve noticeably different court systems.

As part of our main 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 elections that feature candidates from both parties, we hope to minimize differences in jurisdiction characteristics that might explain variation in court outcomes. Around each of the 245 "politically competitive" elections, 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 is unbalanced, 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. Simply regressing defendant outcomes on an indicator for whether a Democratic DA held office at the time of case filing would yield a biased estimate if underlying determinants of judicial outcomes are correlated with the political identity of the local prosecutor. That concern is real in

26. We summarize other case and defendant characteristics by DA partisanship Appendix Table A3. 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.

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 Appendix Table A3.

our setting: for instance, Table 3 shows that, on average, jurisdictions with Democratic district attorneys elected in competitive races handle larger volumes of cases per capita than jurisdictions with Republican prosecutors elected in uncontested races. 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 prosecutor resources and other similar confounding factors.

3.1 Identification from Close Elections

To address concerns about endogenous DA election outcomes, we focus on jurisdictions in which Democratic and Republican DAs won closely-contested races. As Table 3 shows, jurisdictions served by Democratic and Republican DAs elected in contested races appear to be fairly similar, at least along observable dimensions. Our close-elections research design extends this insight to its logical conclusion: by focusing on those DAs elected in narrowly-decided races, we hope to minimize the confounding differences between jurisdictions served by Democratic and Republican DAs. This intuition echoes 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 apply 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, an election for which $Margin$ lies in the range [-0.08,0.08], equivalent to a 54-46 vote split in a two-person race. 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. As we discuss in the next section, our findings are robust to narrower definitions 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 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 in ways that might independently affect post-election case outcomes. To be clear, we do not claim that caseloads *after* the election will be uncorrelated with election outcomes—it very well could be that the election outcome affects the number and kinds of cases that enter the court system, a point we return to in Section 6. Rather, our identification argument only relies on the assumption our close election outcomes cannot be predicted by observable pre-election features of their jurisdictions.

Using Equation 1, we estimate differences in election-year jurisdiction demographics, as well as average case and defendant characteristics. In Table 4, we present results using data aggregated to the jurisdiction level (the unit of variation); for transparency, in Appendix Table A4, we provide analogous results using data that are disaggregated to the case level (our preferred unit of analysis).

Encouragingly, as we report in Table 4, we find no statistically significant differences in jurisdiction-level demographics, caseload, or average case characteristics that undermine our identification argument. While most point estimates are reassuringly small, we do see non-trivial differences in the incidence of property offenses, which is statistically significant in the case-level data (see Appendix Table A4). However, we do not see any differential pre-election trends in the frequencies of any types of cases leading up to the election that would signal our post-election results might capture an ongoing divergence that predated the election (see Appendix Figure A3). Therefore, we argue that these handful of differences in pre-election case characteristics are not a threat to identification, since it is unclear how they might bias our findings absent a trend towards these types of cases. Moreover, as we show in Section 4, our estimates are robust to controlling for offense types.

In keeping with standard RD assumptions, we also verify that there is no discontinuity in the running variable density that would indicate one party systematically wins these close races. In Appendix Figure A1, 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 1 shows, 79 percent of DA elections are not even competitive, much less close: just 67 elections are decided by 8 percentage points or less. Second, our underlying criminal justice data exhibit substantial variation across states and, indeed, jurisdictions (see Table 2). 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 scarce competitive DA races.

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 ($post_{i\tau}$),

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 7 we introduce a supplementary matching design that, while dependent on 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.

as well as jurisdiction and year fixed effects (λ_j and θ_t, respectively):

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

Here, β_1—the post-election effect of a Democrat winning in a close race—is the coefficient of interest.[30] The vector $\mathbf{X}_i$ 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. Jurisdiction and year fixed effects control for variation in outcomes across court systems and time (see Table 2), which further improves our statistical power. For these reasons, we refer to our case-level panel estimates as our main findings. Still, although the cross-sectional approach delivers noisier point estimates (as expected), we find that these results align quantitatively and qualitatively with our favored panel estimates.[32]

Finally, to 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

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.

32. Qualitatively, we find similar results using alternative versions of Equation 2 that omit the panel framework. We provide these results in Appendix Table A6 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.

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 that would narrow that gap, both as Democrats win in previously Republican-led jurisdictions and as Democrats lose reelection bids.

To illustrate this point, and to provide a sense of the timeframe during which we expect to observe election impacts, we estimate an event study specification in the style of Equation 3, where the outcome is an indicator for whether a Democratic DA won the most recent election in a jurisdiction. As expected, in Figure 1, we see that most "treated" jurisdictions continue to be served by a Democratic DA for four years (or, more exactly, over those four years, they do 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.[33] In other words, our close-elections design succeeds in gen-

33. Note that these null estimates do not necessarily mean that Democratic DAs lost their reelection bids — they just mean that jurisdictions that initially elected DAs of different parties were equally likely to be

erating 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 our primary outcome, case dismissal rates. We then broaden our attention to other salient case outcomes, namely incarceration rates and averages incarceration length. Anecdotal evidence surrounding "progressive prosecutors," plus our descriptive findings in Table 3, 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. Event-study estimates from Equation 3 appear in the first panel of Figure 2.

Importantly, we do not find any evidence of pre-election differences in case dismissal rates that would suggest dismissal rates in treated and untreated jurisdictions were already diverging prior to the election. Note that some cases from the election year (period 0) will be "treated," in that they will still be pending when the new DA term begins, which may explain why we find an insignificant uptick in dismissal rates in the election year.[34]

Panel 1 of Figure 2 shows a rise in dismissal rates almost immediately after a Democratic DA assumes office, although the effects are only statistically significant several years after

served by a Democrat after four years.

34. 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 might be inherited by a new DA, should the incumbent lose their re-election bid.

the election. Four years after the election—at the conclusion of the modal DA's term—cases prosecuted by a marginally elected Democratic DA are about 11 percentage points more likely to be dismissed than those prosecuted by a marginally elected Republican DA. The second panel of Figure 2 tells a similar story: in the fourth year after the election, we see a 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). 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 1).

4.2 Estimating the Effect of DA Partisanship on Case Dismissal Rates

Point estimates from our preferred close-elections panel specification appear in columns 2-4 of Table 5. These coefficients capture the difference-in-differences impact of a close Democratic DA's election over the 4 or 6 years post-election. 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 5, we present results from alternative cross-sectional specifications in order to demonstrate the robustness of our panel design. A straightforward sharp RD-style model following Equation 1 (column 5), examining the impact of a close Democratic DA victory in the fourth year post-election as in Figure 2 (N=67 jurisdiction-elections), yields an understandably noisy estimate, since we do not control for any of the cross-state variation in our raw data.[35] But, taken at face value, the cross-sectional estimate suggests that Democratic DAs raise dismissal rates by 11.3 percentage points—almost identical to

35. Recall that the smallest statistically significant impact on case dismissal that we could detect using this straightforward sharp RD specification is 26 percentage points.

the period 4 estimate from our event study model (see Figure 2).

We run two other cross-sectional specifications that augment our statistical power to provide clearer evidence of Democratic DAs' effect on dismissal rates. The first alternative 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 5, but employs aggregate data and no additional controls (column 6 of Table 5; N=67 jurisdiction-elections). 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 similar to the panel results from columns 2-4, provides some reassurance that using case-level data and a difference-in-differences framework does not drive our findings.

Second, we disaggregate our outcome variable so that each observation is an election-period (column 7 of of Table 5; N=266 jurisdiction-election-periods). 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 to control for secular differences across time and place. The point estimate from that model indicates that Democratic DAs raise dismissal rates by 9.8 percentage points (30 percent of the mean), a slightly larger effect than we recover with our preferred specification. This comparison demonstrates that our findings do not depend on including pre-election data.

4.3 Does DA Partisanship Matter for Other Case Outcomes?

Looking beyond case dismissal rates, Figure 3 offers visual evidence that electing a Democratic DA leads to lower rates of incarceration, as well as shorter incarceration sentences. Event study-style estimates in the first two panels point to declines in incarceration rates soon after a Democratic DA's election victory, including 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 discontinuity in average sentence lengths does not appear to be very robust, a point we return to below.

In the second panel of Table 5, we present panel and cross-sectional estimates of the impact of DA partisanship on the probability of incarceration and sentence lengths. Using our close-election panel specification, we find that Democratic DAs reduce incarceration rates 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 lead to 33-43 percent shorter sentences on average (roughly 3 fewer months in jail or prison). Our cross-sectional approach, focusing on the fourth post-election year, shows an imprecise but economically meaningful effect on the probability of incarceration (around 8 percentage points), but near-zero effects on incarceration length. The imprecise null on sentence length likely stems from the noise around the cutoff visible in Figure 3. When we consider the change in jurisdiction-wide dismissal rates, or include multiple post-election periods alongside jurisdiction and year fixed effects, we find 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 affect our principal findings. That said, our close-elections, RD-style framework requires assumptions over the exact sample and functional form of our control for the election margin, each of which could influence our findings.

We first probe the robustness of our findings by tightening our definition of what constitutes "close election" (that is, one decided by 8 percentage points or less). That is, 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 similar findings (see Appendix Table A5), 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 standard for "close" elections to one decided by 10 percentage points or less. Broadening our definition of "close" elections leads to small and statistically insignificant estimates on dismissal and incarceration rates, but we do not see this as a challenge to the validity of our preferred estimates: expanding our sample this way goes against the logic of our identification approach and might reintroduce sources of bias that we worked to mitigate.

We further consider the robustness of our findings to three other modifications common in the regression discontinuity and difference-in-differences literatures. Our results do not change when we alter our panel specification (Equation 2) to include a quadratic, in addition to a linear, control for the Democratic margin of victory. Further, 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 in this approach, which confirms that our findings do not rely on the closest races. Finally, when we include linear time trends in our panel specification (which help address any lingering concerns about differential pre-trends), we also find slightly larger effects.

Lastly, we consider the extent to which individual states drive our findings (see Appendix Table A7). Removing Texas from our sample noticeably shrinks our topline estimates and reduces their precision (which is to be expected, as most of our close DA elections come from Texas). But, qualitatively, we reach similar conclusions even without data from Texas, and removing other states has virtually no bearing on our findings.

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. For example, one might expect that Democratic DAs would take a more lenient approach to only the least serious offenses. To investigate potential heterogeneity in treatment effects, we estimate our preferred model (Equation 2) on subsamples of our data. These subgroup-specific estimates appear in Appendix Table A8.

While we do find larger impacts among certain groups (including non-White defendants and felony cases), compared to their respective means, these subgroup effects do not vary much, and there is no clear pattern to indicate that DA partisanship differentially impacts more or less severe cases, nor more or less vulnerable defendants. Indeed, in Appendix Table A11, we confirm that we cannot rule out equality in the Democratic treatment effect for all but a few subgroups.[36]

5 Does DA Partisanship Affect Public Safety?

To explore the policy implications of our main findings, we consider the relationship between public safety and the political identify of the local DA. 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?

We first apply our close-elections design to examine the impact of Democratic election victories on recidivism rates. As we describe in Section 2, we define recidivism as facing a new criminal charge within 1 year or 2 years of their original case's filing date. 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. While we prefer to anchor our recidivism measures around the (plausibly exogenous) case filing date, in Appendix Table A10, we show that measuring recidivism over the two years after case disposition leads to more precise estimates that support a similar conclusion to the one we draw below.

36. To test whether 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).

Recall that we can only observe defendant re-offense behavior in three states, Kentucky, North Carolina, and Texas. And we emphasize that we cannot estimate the causal effect of dismissal or incarceration on the probability of re-offense; 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—on overall re-offense rates. 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 recidivism rates.

Figure 4 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 6. 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 indicate larger negative (that is, beneficial) effects, although none of these coefficients is statistically significant. Altogether, these findings support the conclusion that Democratic DAs do not increase re-offense rates relative to their Republican peers.

In Appendix Table A9, we examine heterogeneity in the effects of DA partisanship by whether the defendant had a *prior* criminal charge in the 1-2 years leading up to their focal case. Perhaps surprisingly, 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 (2023) 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 benefit individuals with no observable prior offenses.

5.2 Does DA Partisanship Affect Crime Rates or Policing?

Besides the behavior of individual defendants, it is also possible that DAs affect reported crime and the number of arrests police make. Prosecutors and police officers are closely linked, as Garro and Stashko (2023) demonstrate. One might expect that if Democratic DAs dismiss cases involving certain types of offenses, law enforcement agencies might react by pulling back the policing efforts that lead to arrests for those offenses.

To explore this possibility, 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 transform those counts into per-capita rates using Census population data (see Table 3).

In Appendix Figure A6, we use Equation 3 to illustrate the effects of DA partisanship on crime-related outcomes derived from the UCR. We find that jurisdictions served by Democratic DAs and Republican DAs have statistically similar reported crime and arrest rates. Point estimates are generally negative (indicating that citizens report fewer crimes, and po-

37. We clean the raw UCR data to address known issues in the Return A files. For instance, extreme outliers—which may be assumed to be clerical errors—are set to missing. Additionally, we impute 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.

lice make fewer arrests, per capita, in Democrat-led jurisdictions), but vary considerably across years and do not reveal particularly large differences. These patterns are consistent with the idea that police do not respond to Democratic DAs' prosecution policies by systematically arresting fewer individuals. This finding echoes Arora (2019), who also examines DAs elected in close races and finds no difference in arrest counts between jurisdictions led by Republican and Democratic DAs. 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.

6 Estimating the Average Effect of DA Partisanship

The estimates we have reported thus far have only identified the causal effect of Democratic DAs elected in the most competitive elections—which of course are fairly rare. Given the prevalence of uncontested DA races, identifying the impact of the average DA's partisanship is of particular policy importance. In lieu of instrument for DA partisanship outside of close elections, we employ a matching design that draws on our case-level data to create "matched" samples of cases 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 DAs of opposing parties in order to isolate the average effect of DA partisanship.

6.1 Matching Design

Our approach to estimating the average effect of DA partisanship 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 2 (severity and offense type), which creates groups of observably similar cases. We then run 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 2. Finally, we use the resulting propensity scores to match and assign weights to similar cases within exact-match groups.

We refine our matching approach with two enhancements from the literature 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 to control for the same covariates we use to estimate our propensity score model. This step adjusts for any remaining imbalance in the weighted sample.

6.2 Benchmarking 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 Appendix Table A14, we show that we also achieve balance across the other defendant, case, and county characteristics that we include in our propensity score model. 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 appreciable 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, of course, we cannot disprove such a claim. Helpfully, though, 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.

We present our benchmark matching results, estimated on the same sample as we use for our close-elections model, in the second column of Table 7. We compare these matching results to our multi-period cross-sectional estimates from column 7 of Tables 5 and 6, reported here in column 1. Both specifications make within-year and within-state comparisons without drawing on pre-election jurisdiction data, making them logical counterpoints. Overall, within our close-elections sample, our matching estimates are similar or even identical to our cross-sectional estimates, at least when we consider dismissal, incarceration, and recidivism rates. This similarity bolsters our faith in the matching estimates outside of the close-elections sample. However, our matching approach recovers an effect on sentence length that is less than half as large as our benchmark estimate. As such, we treat our matching estimates on sentencing length with caution.

6.3 Matching Results

In column 4 of Table 7, we show matching estimates of the effect of DA partisanship using our full sample of cases, which we argue approximates the average effect of a Democratic DA on case outcomes. We find that the average Democratic DA is 4.8 percentage points (13 percent of the mean) more likely to dismiss a given case, and cases prosecuted by a Democratic DA are 5.5 percentage points (12 percent) less likely result in incarceration. 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 concerns above about the benchmark sentencing results. 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 Potential Mechanisms

We consider three potential mechanisms that might explain what changes Democratic DAs make that lead to differences in case outcomes post-election. Our overarching hypothesis is that, reflecting the preferences of the modal Democratic voter, Democratic DAs choose to pursue conviction rates than Republican DAs.

Although we cannot observe the target conviction rate of DAs, our hypothesis implies Democratic DAs actually dismiss more cases following their elections—a pattern that would be consistent with our findings, but one which we have yet to show. Appendix Figure A2 provides visual evidence that the Democrats in our close-elections sample do, in fact, dismiss more cases after their election relative to the year prior to the election—statistically significantly so in all post-election periods except period 6. For Republicans, point estimates are noisy but generally point to a lower or flat dismissal rate after the election.[38] These trends suggest that the differences in dismissal rates across jurisdictions that we report in Section 4 arise because Democratic DAs dismiss more cases, not because Republicans dismiss fewer cases, consistent with our hypothesis that Democratic prosecutors choose to pursue fewer convictions. The fact that we find no heterogeneity in Democratic DAs' effects similarly points to a broad-based effort to lower conviction rates.

A competing narrative suggests that new Democratic DAs face constraints that limit their ability to seek convictions on incoming cases. For instance, new Democratic DAs may generate turnover among ADAs that forces them to dismiss more cases while they restaff. We do find some evidence that ADA turnover increases around election years. In Appendix Figure A8, we plot the average number of Colorado and North Carolina ADAs that we observe assigned to criminal cases for the first time by year relative to an election.[39] We see

38. The specification is a modified version of Equation 3 without the *Democrat* indicator, effectively a traditional event-study model. We re-estimate period-specific coefficients separately for cases in jurisdictions where Democrats and Republicans won the election in period 0. Appendix Figure A2 plots the period coefficients.

39. Colorado and North Carolina are the only states that provided ADA identifiers. Because these states have few competitive elections, we do not try to estimate the partisan impact on new ADA appearances.

spikes in the number of new ADAs at period 0 – the year of the election – and at period 4, the end of a DA's elected term. These spikes are consistent with the idea that DAs hire new ADAs at the beginning of their terms. But this pattern does not, on its own, explain persistent partisan differences throughout these terms (see Figure 2).

Finally, Democratic DAs might shape the criminal justice environment in a way that leads to less serious—and more easily dismissed—cases entering the court system. As we showed in Section 5, police do not appear to change their overall arrest numbers in response to the district attorney's political identity. But police might shift the composition of arrests towards less severe offenses when Democratic DAs are in power. In Appendix Figure A3, we show that the number of charges and the share of cases that contain relatively less serious property offenses *decreases* post-election for the marginal Democrat, while the share of cases that contain relatively serious felony offenses actually increases. This pattern suggests that DA partisanship may reduce charging rates for less serious offenses, but, if anything, that effect might be expected to lead to lower, not higher, dismissal rates.

8 Concluding Discussion

Elected district attorneys have considerable influence 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 about the effi-

We say that an ADA appears for the first time if we do not observe them on any prior criminal case.

cacy and equity of criminal convictions and incarcerations. At the same time, we contribute to a robust academic 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. 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. 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.[40]

Our findings substantiate anecdotal evidence that DA partisanship matters and, in fact, causes prosecutorial policies to diverge sharply across jurisdictions. Our work thus highlights the tight relationship between public opinion and the notionally apolitical decision over a person's guilt or innocence, complementing Okafor (2021). The link between DA partisanship and in-individual case outcomes has 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 DAs—and, by extension, voters—determine the punitiveness of the criminal justice system.

40. In this sense, our results also echo those of Ouss and Stevenson (2023), who find that a prosecutor-driven bail reform led more arrested individuals secured pretrial release with no impact on re-offense rates.

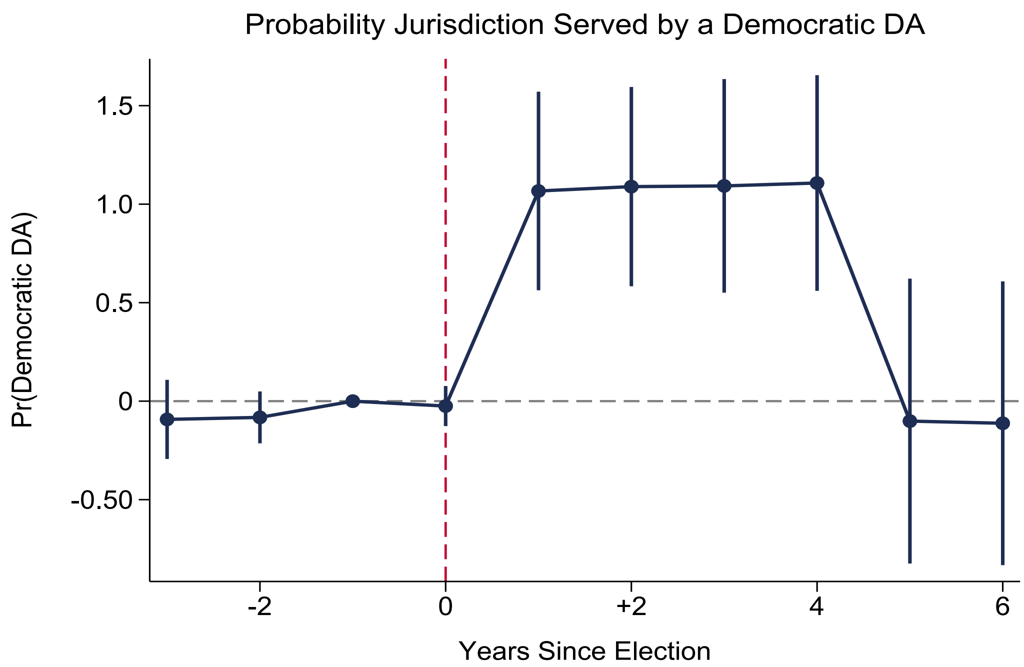

Figure 1: The figure plots panel regression estimates showing the impact of a narrow Democratic DA win on the probability that a Democratic DA serves in office in the years around the election. The specification is Equation 3. All coefficients are relative to the year prior to the election (period -1). The sample includes 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.

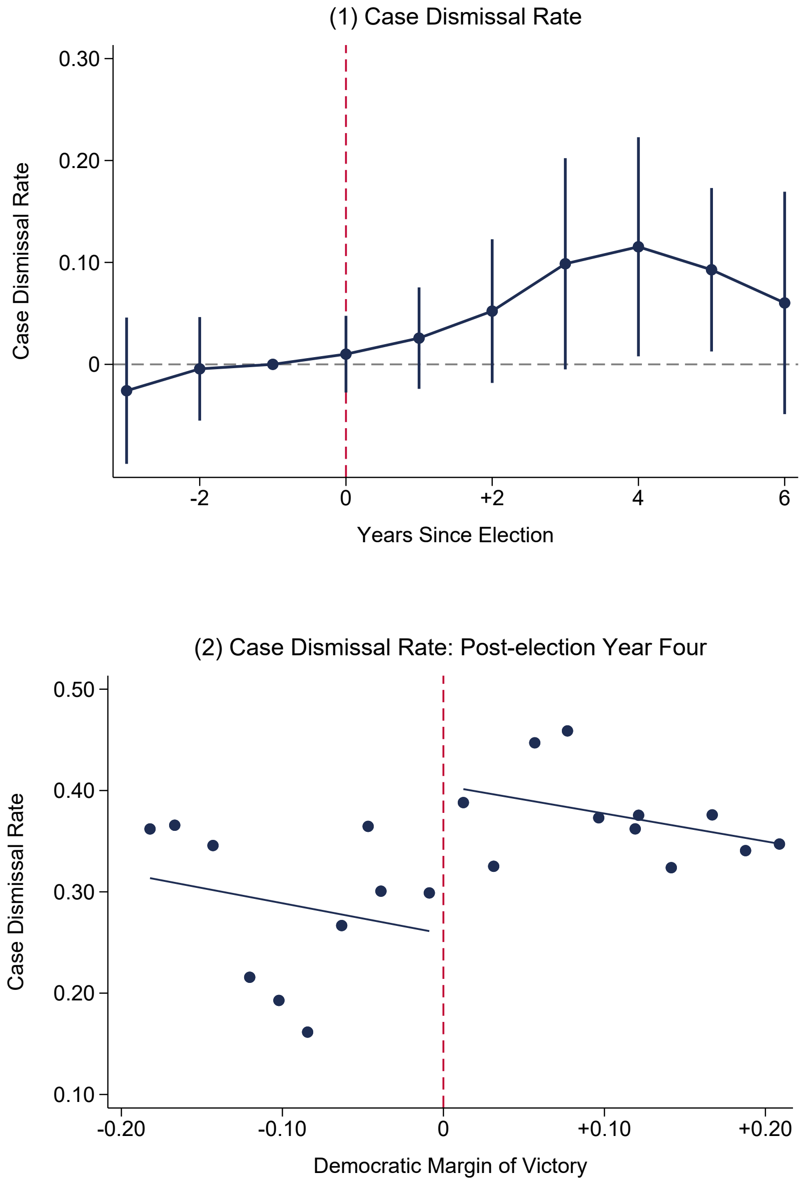

Figure 2: Panel 1 plots panel regression estimates showing the impact of a narrow Democratic DA election win on the probability of case dismissal by year relative to the election. The specification is Equation 3. All coefficients are relative to the year prior to the election (period -1). The sample 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.

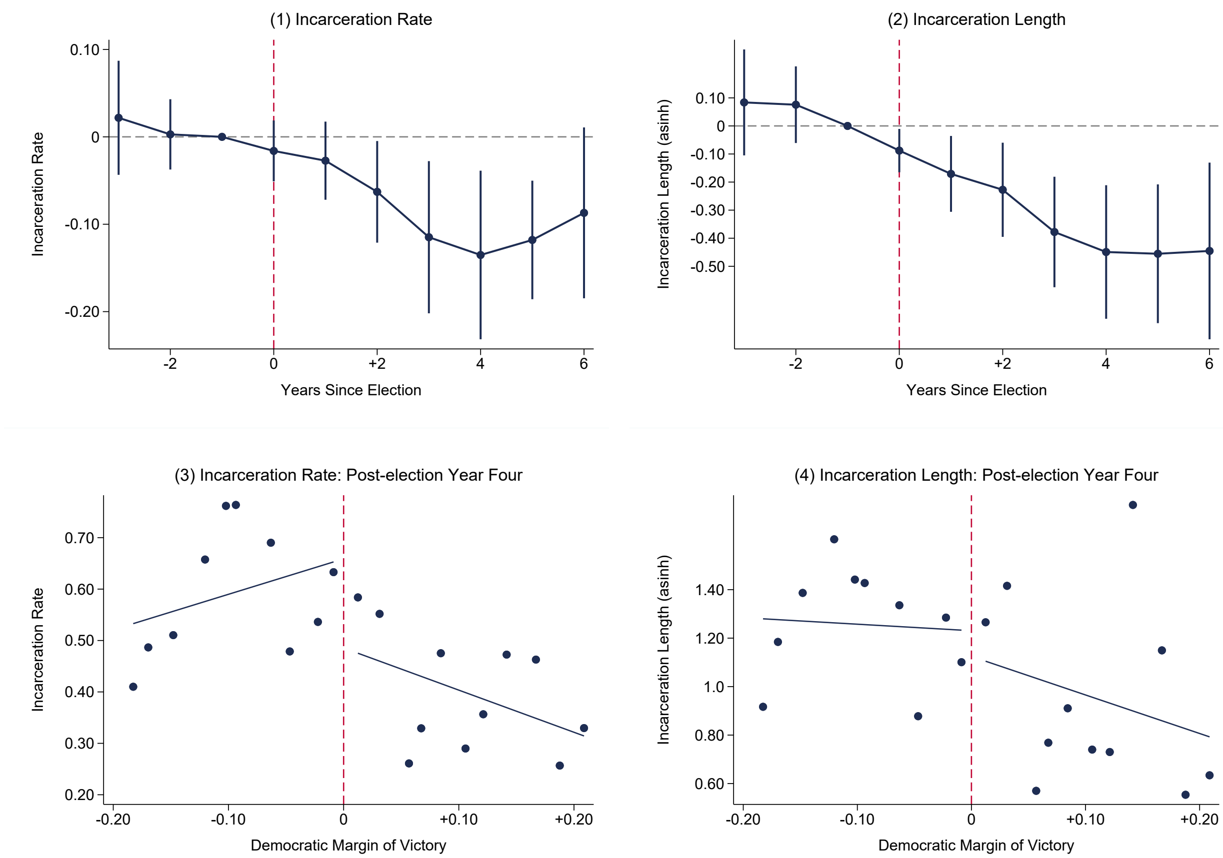

Figure 3: Panels 1 and 2 plot panel regression estimates showing the impact of a narrow Democratic election win on the probability of incarceration (panel 1) and incarceration length (panel 2) by year relative to the year prior to the election. The specification is Equation 3. All coefficients are relative 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.

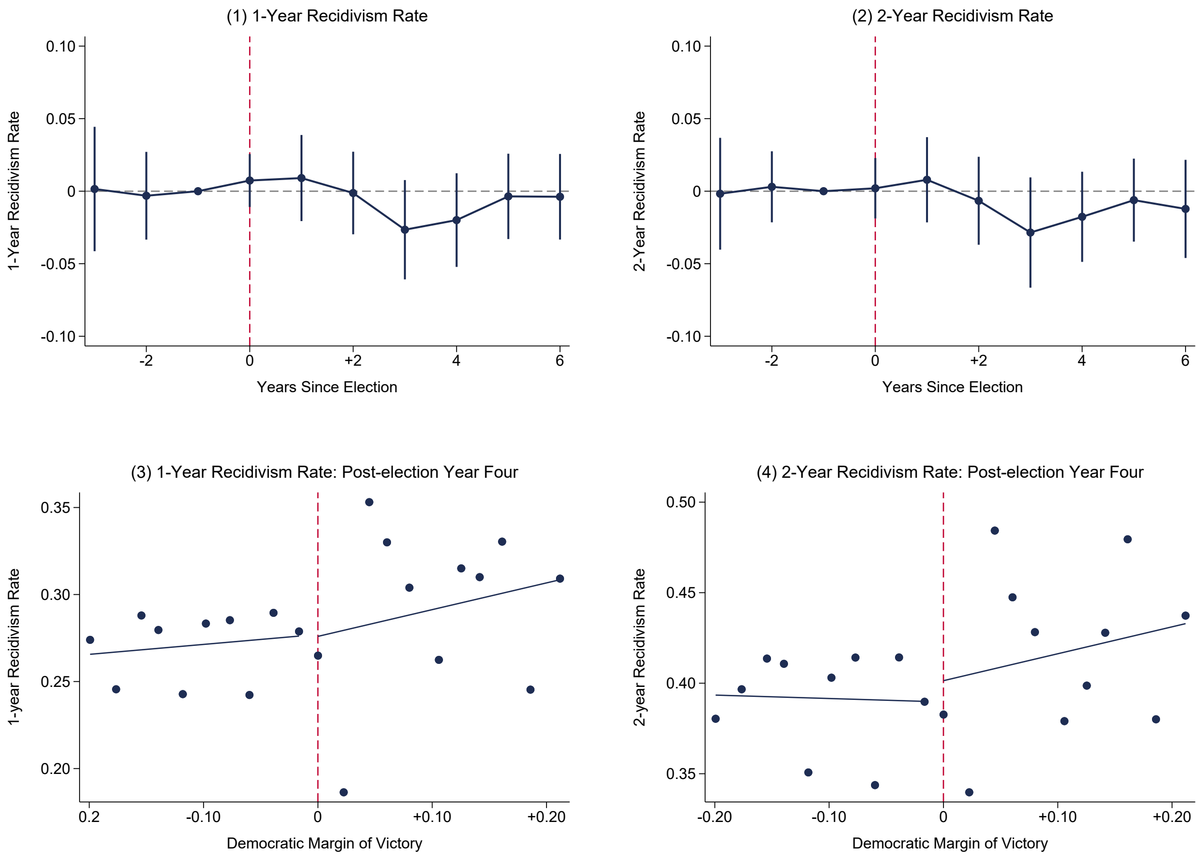

Figure 4: Panels 1 and 2 plot panel regression estimates showing the impact of a narrow Democratic election win on the probability of defendant recidivism within 1 year of initial arrest (panel 1) and the probability of defendant recidivism within 2 years of initial arrest (panel 2), by year relative to the year prior 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 1: 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 2: Criminal Case-level Descriptive Statistics by State

		State of Case Filing						
	All Cases	Arkansas	Colorado	Kentucky	Maryland	N. 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 3: 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 4: 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 5: 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 6: 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	-0.018*
	(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*	*206*
2-year Recidivism	0.408	-0.011	-0.009	-0.010	-0.039	-0.013	-0.028**
	(0.491)	(0.008)	(0.008)	(0.008)	(0.023)	(0.032)	(0.013)
N:	*4,020,494*	*3,375,311*	*3,375,311*	*4,020,494*	*34*	*32*	*195*
Year FEs		Y	Y	Y	N	N	Y
Jurisdiction FEs		Y	Y	Y	N	N	Y
Defendant/Case Covariates		N	Y	Y	—	—	—
Period(s) Included		[-3,4]	[-3,4]	[-3,6]	4	[-3,4]	[1,6]
Unit of Observation		Case	Case	Case	Election	Election	Election-period

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

The sample includes criminal cases filed between pre-election period -3 and post-election period +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 5. Robust standard errors clustered at the jurisdiction-election level appear in parentheses.

Table 7: What is the Average Effect of DA Partisanship? Matching Estimates

	Close-elections Sample		Full Matched Sample	
	Cross-sectional Estimates	Matching Estimate	Comparison Mean	Matching Estimate
	(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 and 2 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 1 reproduces cross-sectional RD estimates from column 7 of Table 5 and column 6 of Table 6. The specification in Equation 1, estimated on jurisdiction-election-period data with jurisdiction and year fixed effects. Column 2 presents matching results using the same sample of cases as in column 1, following the approach described in the text. Columns 3 and 4 include all cases in our sample. Column 4 provides the mean among cases prosecuted by Republican DAs, while column 5 presents matching estimates following the same design as in column 2. Robust standard errors clustered at the jurisdiction-election level appear in parentheses in columns 2 and 4.

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

Brett Fischer and Tyler Ludwig

Online Appendix

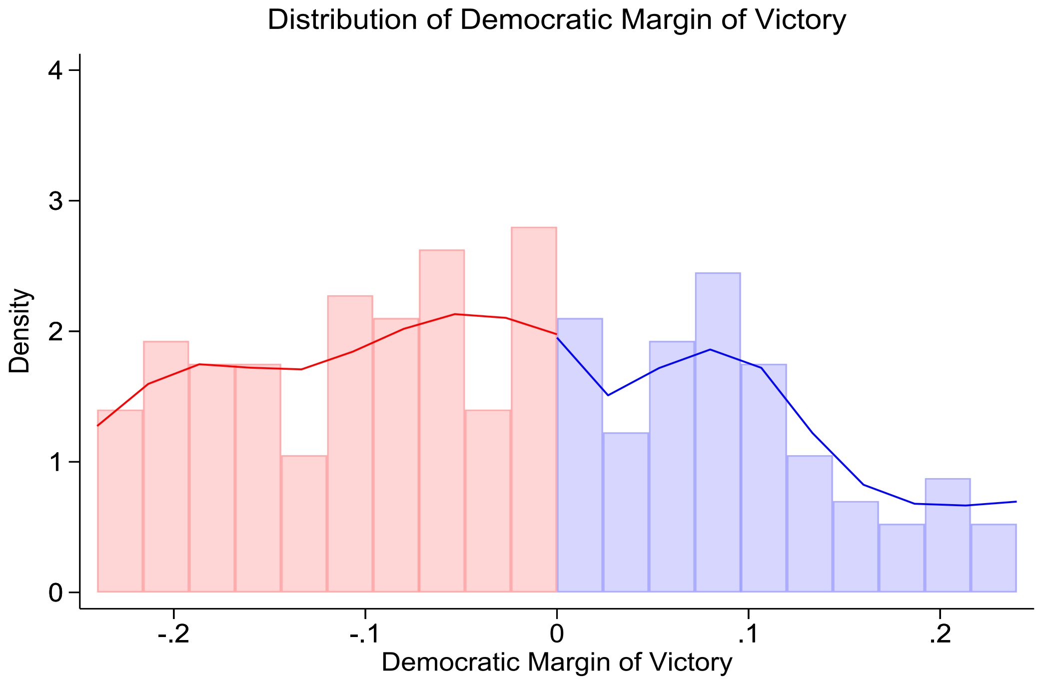

Figure A1: The sample includes 162 competitive district attorney races with at least one Democrat and one Republican running that were decided by 20 percentage points or less. "Democratic margin of victory" refers to the difference between the top-performing Democrat's vote share and the top-performing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

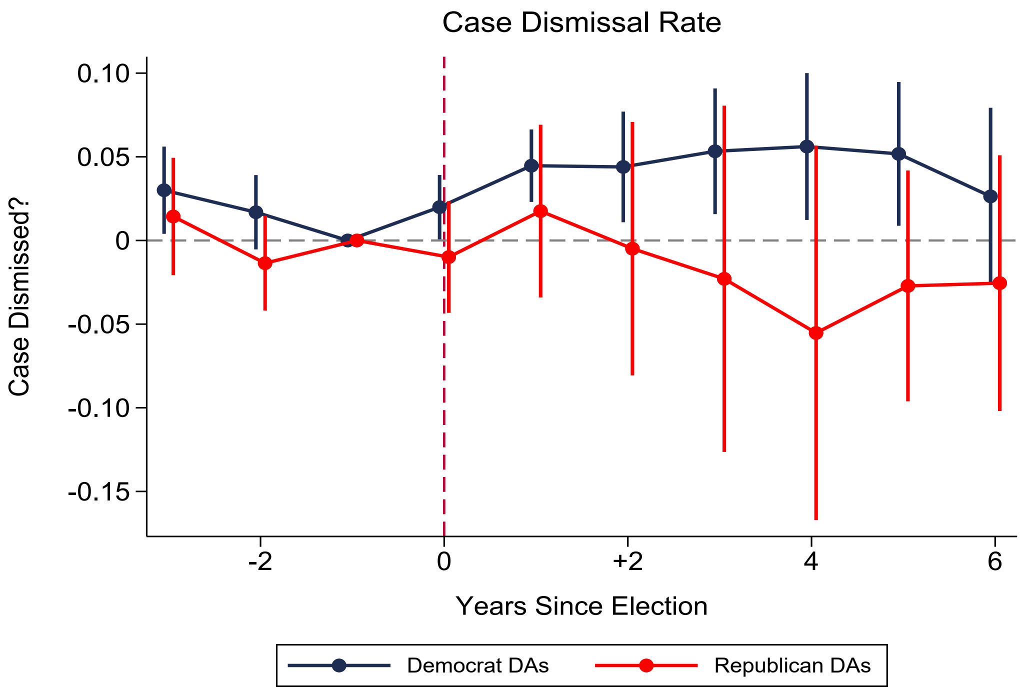

Figure A2: For this figure, the sample of DAs elected in close elections is split into Democrats and Republicans. All coefficients are relative to the year prior to the election (period -1). Vertical bars denote 95 percent confidence intervals using robust standard errors clustered at the jurisdiction-election level.

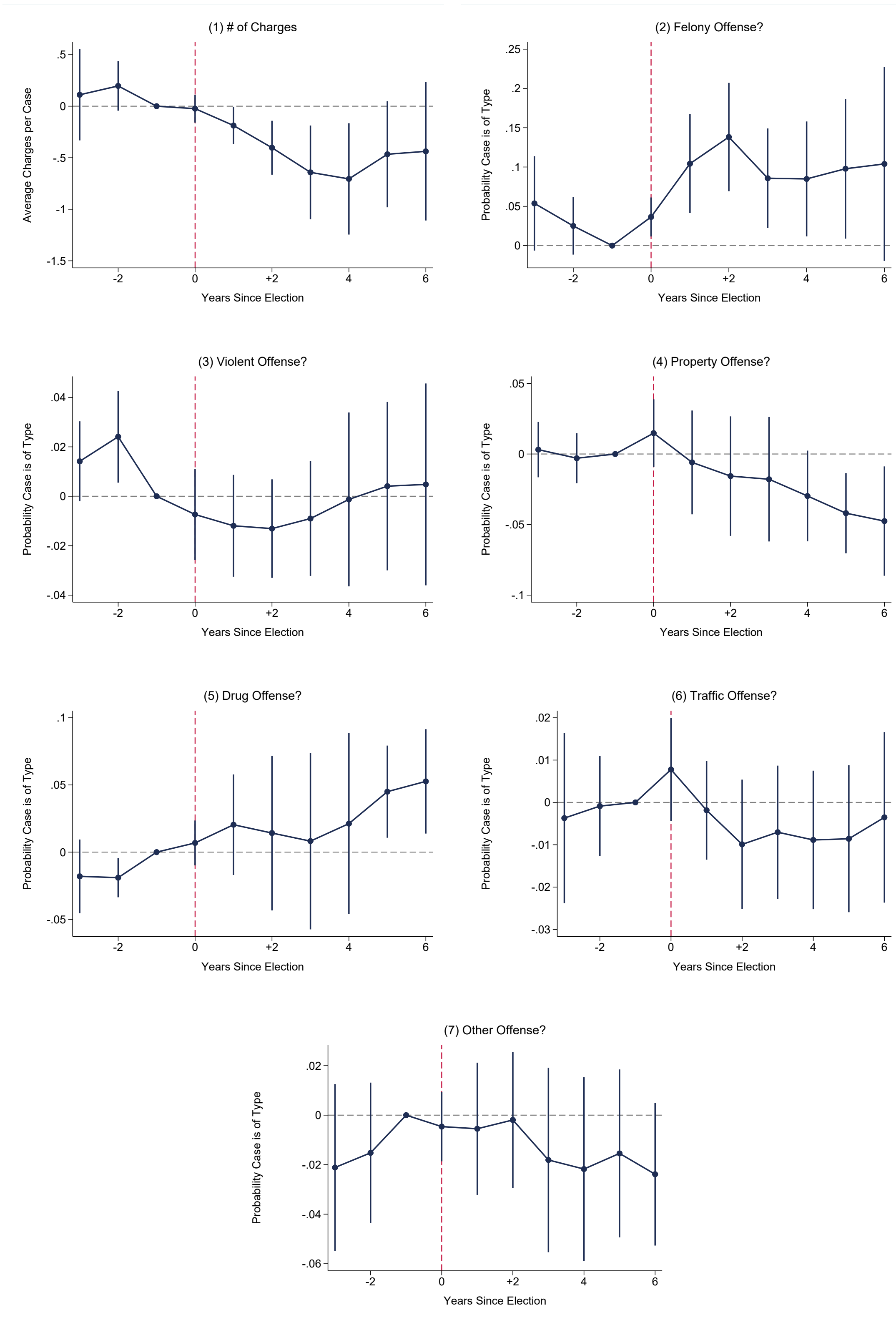

Figure A3: Panels 1 through 7 plot panel close-elections estimates showing the impact of a narrow Democratic election win (8 percentage points or less) on case characteristics. The specification is Equation 3. All coefficients are relative to the year prior to the election (period -1). Vertical bars denote 95 percent confidence intervals using robust standard errors clustered at the jurisdiction-election level. The sample contains 162 jurisdiction-elections.

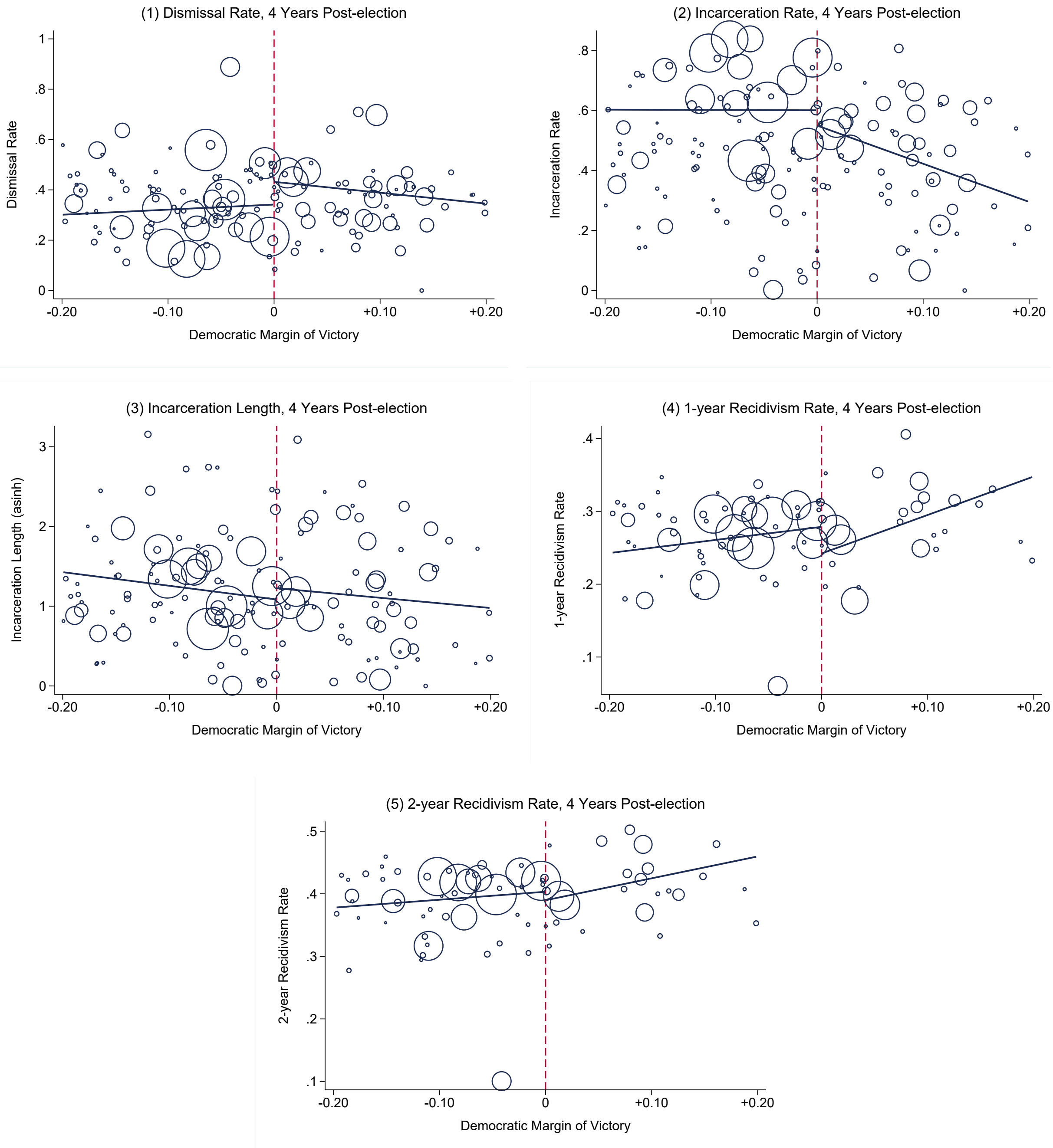

Figure A4: Scatter plots show the jurisdiction-level mean outcome in the fourth year after a contested DA election, along with lines of best fit. Each point corresponds to a single jurisdiction-election. Larger points correspond to more populous jurisdictions. For panels 1-3, N=162 jurisdiction-elections. For panels 4 and 5, N=87 jurisdiction-elections.

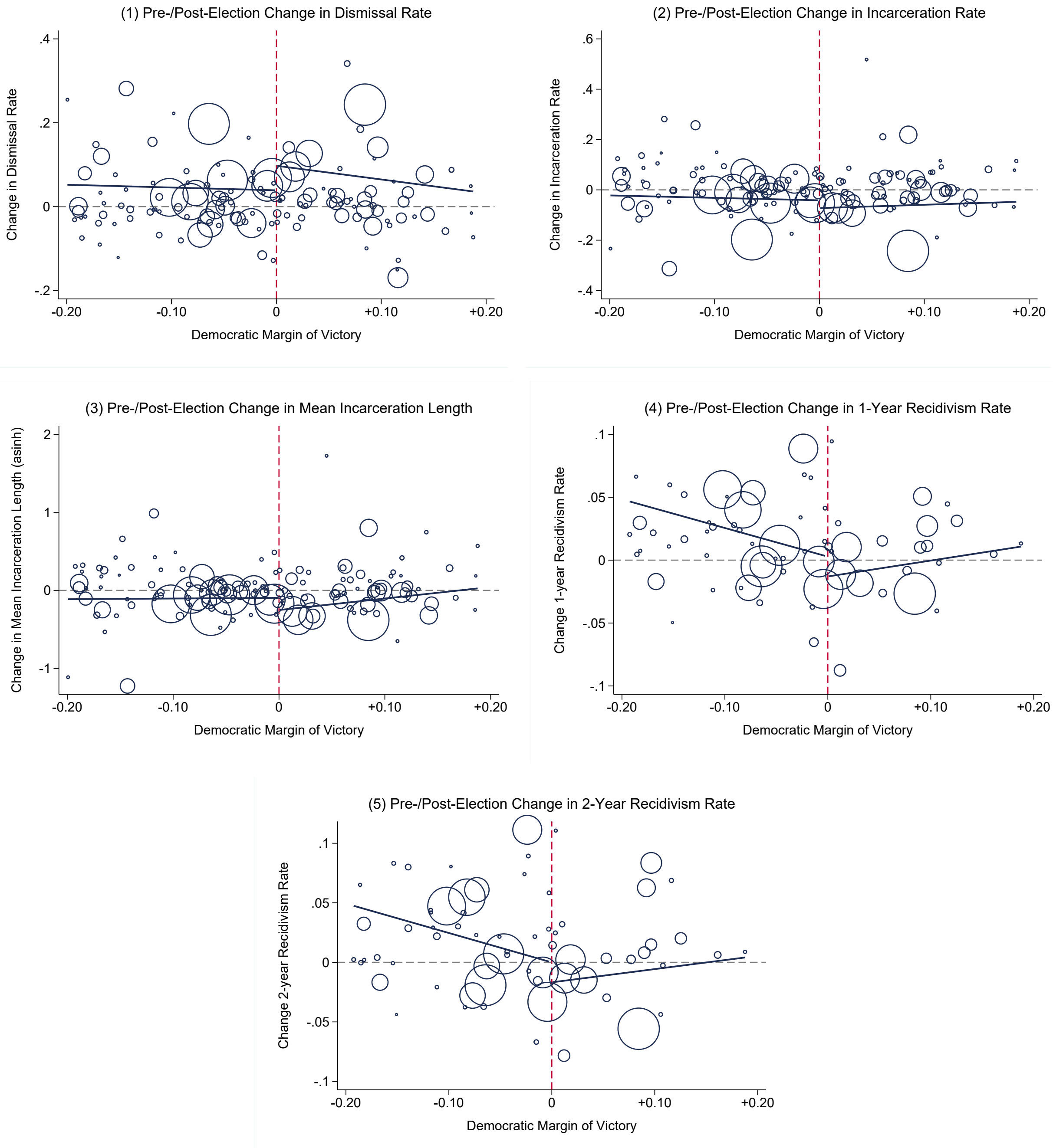

Figure A5: Scatter plots show the jurisdiction-level change in mean outcome between the three years leading up to the election and the four years following the election, along with lines of best fit. Each point corresponds to a single jurisdiction-election. Larger points correspond to more populous jurisdictions. For panels 1-3, N=162 jurisdiction-elections. For panels 4 and 5, N=87 jurisdiction-elections.

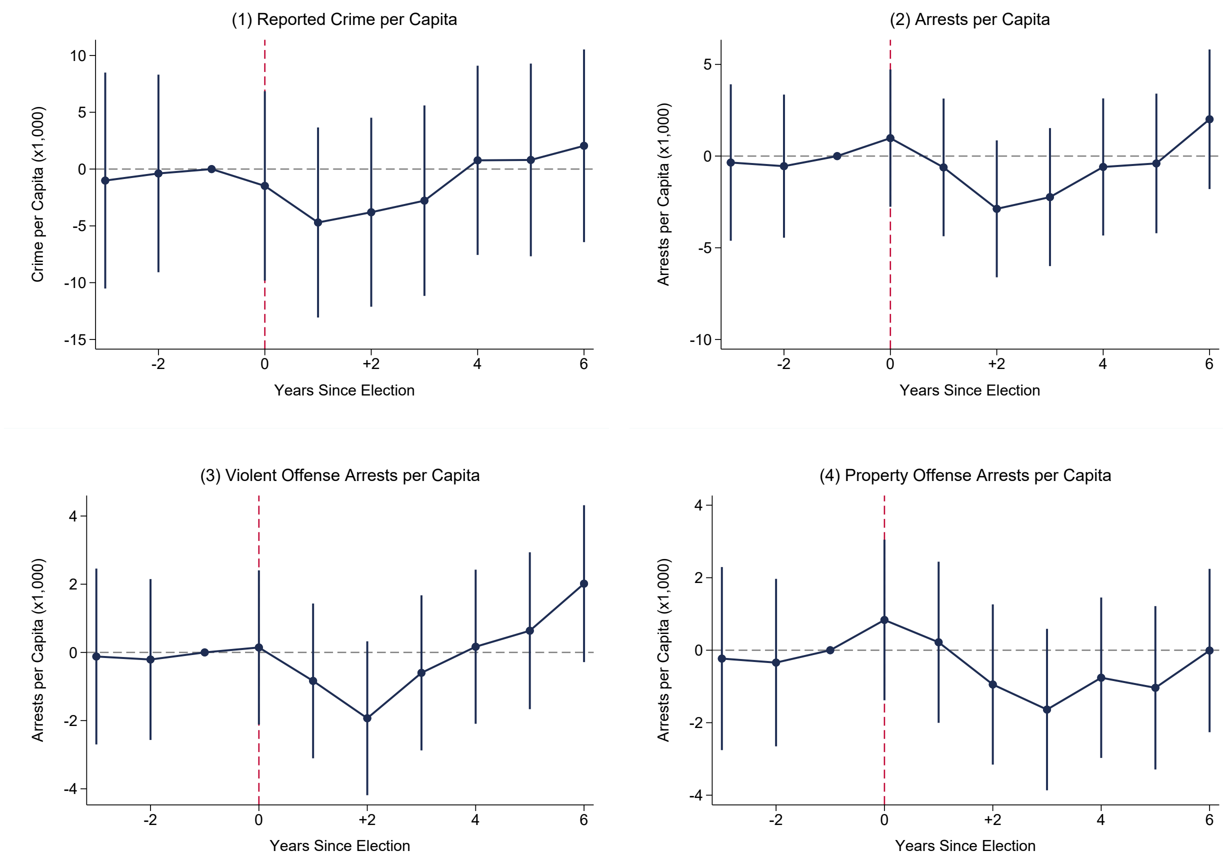

Figure A6: Panels 1 through 4 plot panel RD estimates showing the impact of a narrow Democratic election win on the reported crime (panel 1), arrests (panel 2), violent offense arrests (panel 3), and property offense arrests (panel 4) per 1,000 residents by year relative to the year prior to the election. 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.

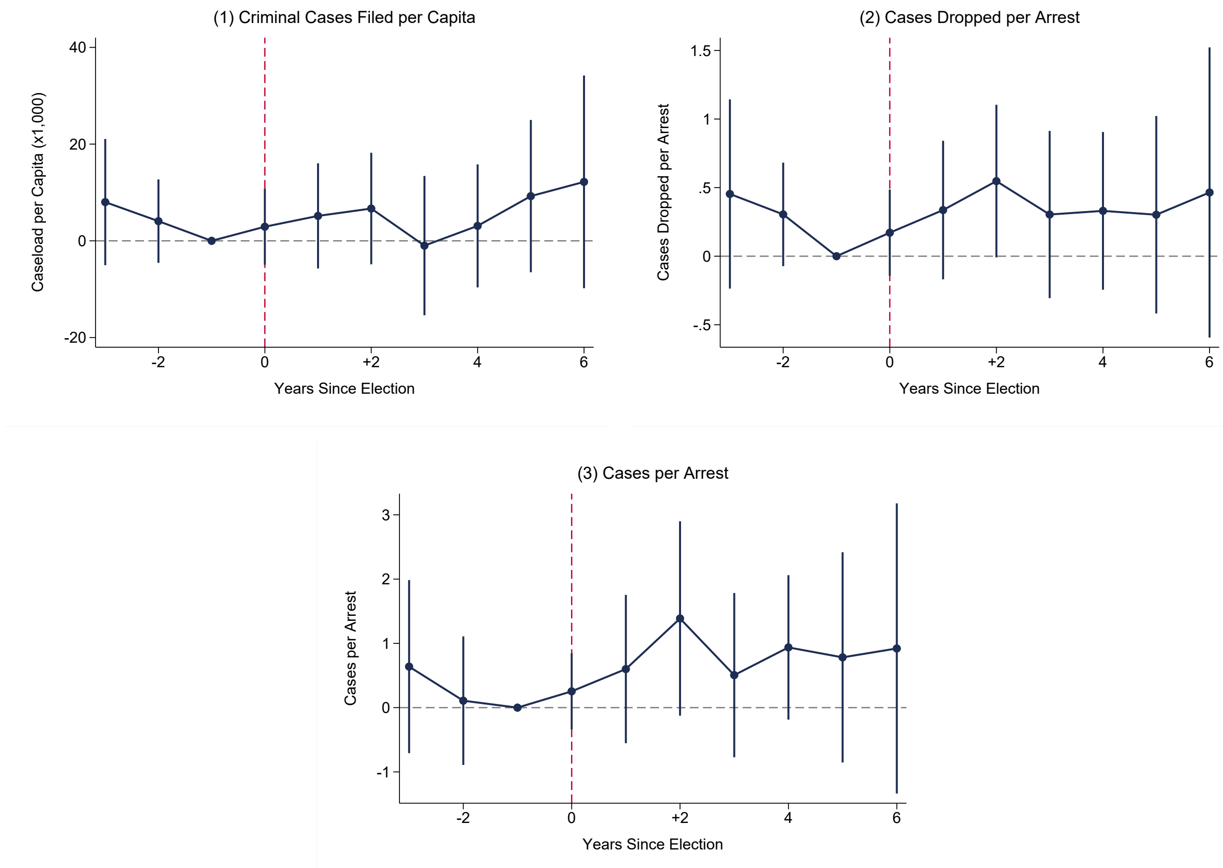

Figure A7: Panels 1 through 3 plot panel RD estimates showing the impact of a narrow Democratic election win on cases per 1,000 residents (panel 1), cases dropped per arrest (panel 2), and cases per arrest (panel 3) by year relative to the year prior to the election. The specification is Equation 3. 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).

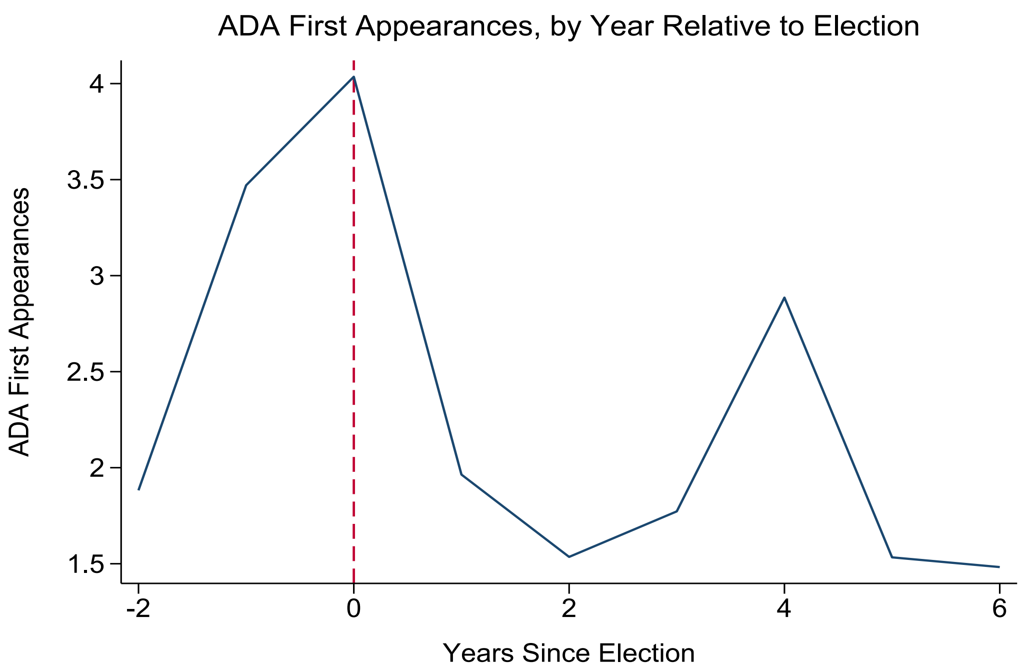

Figure A8: The figure depicts the average number of assistant district attorneys (ADAs) that first appear in a jurisdiction by year. A "first appearance" refers to the first time that we observe an ADAs name assigned to a case in a given jurisdiction. The data are limited to the states of Colorado and North Carolina as only these two states report information about which ADA was assigned to a case.

Table A1: "Dismissal" Dispositions by State

		"Dismissal" Dispositions by State						
	All Cases	Arkansas	Colorado	Kentucky	Maryland	N. Carolina	Texas	Virginia
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dismissed (by the DA)	0.27	0.15	0.69	0.48	0.02	0.08	0.30	0.21
Nolle Prosequi	0.38	0.25	<0.01	< 0.01	0.57	0.67	—	0.26
Dropped by the DA	0.01	—	<0.01	<0.01	0.06	<0.01	—	<0.01
Deferred, Diverted, "Stet", or "Deferred Adjudication"	0.08	0.13	0.14	0.01	—	—	0.15	—
N:	*14,223,788*	*76,303*	*1,588,974*	*1,021,555*	*1,849,565*	*2,681,962*	*5,643,755*	*1,361,674*

Each cell reports the share of cases by state that result in the disposition given in the left-hand column. Note that these dispositions are not mutually exclusive: a case can include charges that result in different dispositions. Column 1 describes all cases in our dataset. The remaining columns describe cases by the state in which they were filed. Empty cells indicate that states never report a given disposition. Cells marked "<0.01" indicate that the disposition appears to be used in a state, but fewer than 1 percent of cases in the state resulted in that disposition. See the text for our rationale about why we count these dispositions as dismissals.

Table A2: 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 A3: 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 A4: Baseline Balance: Election-year Differences in Case Characteristics (Case-Level)

	Control Mean	Balance Estimate
	(1)	(2)
Defendant Age	32.1	0.1
	(10.9)	(0.5)
N:	*354,356*	*495,778*
Female Defendant	0.237	-0.017
	(0.425)	(0.015)
Nonwhite Defendant	0.668	0.009
	(0.471)	(0.080)
# of Charges	3.7	1.1**
	(2.3)	(0.5)
Felony Offense	0.365	-0.016
	(0.482)	(0.050)
Property Offense	0.356	0.068**
	(0.479)	(0.030)
Violent Offense	0.204	0.041
	(0.403)	(0.027)
Drug Offense	0.295	-0.046
	(0.456)	(0.045)
Traffic Offense	0.043	0.007
	(0.204)	(0.020)
Other Offense	0.248	-0.055
	(0.432)	(0.045)
N:	*363,728*	*534,690*

The sample includes all cases in 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 cases in jurisdictions in which Republican candidates win the election. Standard deviations appear in parentheses. Column 2 presents cross-sectional estimates of the Democratic DA effect, following Equation 1. Robust standard errors clustered at the jurisdiction-election level appear in parentheses. Sample sizes vary within columns due to missing jurisdiction demographic and defendant age data, as discussed in the text.

Table A5: 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)	Linear Time Trend (8)
Case Dismissal	0.038	0.069***	0.080**	0.071**	0.029	0.074**	0.096**	0.092***
	(0.023)	(0.022)	(0.031)	(0.027)	(0.029)	(0.030)	(0.036)	(0.017)
Incarceration	-0.057*	-0.073***	-0.094***	-0.080***	-0.032	-0.085***	-0.102***	-0.094***
	(0.030)	(0.018)	(0.028)	(0.024)	(0.030)	(0.027)	(0.029)	(0.019)
Incarceration Length (asinh)	-0.261***	-0.401***	-0.439***	-0.402***	-0.247***	-0.427***	-0.450***	-0.290***
	(0.261)	(0.076)	(0.068)	(0.075)	(0.077)	(0.083)	(0.100)	(0.096)
N:	*2,672,691*	*3,171,482*	*3,890,544*	*4,280,419*	*5,284,386*	*4,280,419*	*3,443,856*	*4,278,427*
1-year Recidivism	0.003	0.003	0.002	-0.006	-0.010	-0.005	-0.030**	-0.002
	(0.009)	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)	(0.011)	(0.006)
N:	*2,347,075*	*2,532,388*	*3,106,312*	*3,490,276*	*4,322,243*	*3,490,276*	*2,688,410*	*3,488,337*
2-year Recidivism	-0.004	0.000	-0.004	-0.009	-0.011	-0.009	-0.055***	-0.011*
	(0.007)	(0.004)	(0.005)	(0.007)	(0.008)	(0.008)	(0.008)	(0.007)
N:	*2,294,396*	*2,463,865*	*3,375,311*	*2,991,347*	*4,153,200*	*3,375,311*	*2,601,498*	*3,373,488*
Year FEs	Y	Y	Y	Y	Y	Y	Y	Y
Jurisdiction FEs	Y	Y	Y	Y	Y	Y	Y	Y
Defendant/Case Covariates	Y	Y	Y	Y	Y	Y	Y	Y
Periods Included	[-3,4]	[-3,4]	[-3,4]	[-3,4]	[-3,4]	[-3,4]	[-3,4]	[-3,4]
"Close" Election Margin	± 5pp	± 6pp	± 7pp	± 8pp	± 10pp	± 8pp	± 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 5 present estimates from Equation 2 estimated using alternative definitions of "close" elections, as described in the header. Column 6 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 7 shows estimates from a sample that excludes elections decided by 1 percentage point or less. Column 8 reports estimates from a specification that includes a linear time trend. Robust standard errors clustered at the jurisdiction-election level appear in parentheses.

Table A6: 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 5. Standard errors in parentheses are clustered at jurisdiction-election level.

Table A7: Panel Estimates on Main Outcomes After Dropping Individual States

	All States	State Dropped from Estimation Sample						
	Included	Arkansas	Colorado	Kentucky	Maryland	N. Carolina	Texas	Virginia
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Case Dismissed	0.080***	0.081***	0.075***	0.081***	0.079**	0.080**	0.032	0.077**
	(0.030)	(0.030)	(0.027)	(0.030)	(0.030)	(0.030)	(0.049)	(0.031)
Incarceration	-0.088***	-0.089***	-0.098***	-0.088***	-0.083***	-0.090***	-0.051	-0.087***
	(0.027)	(0.027)	(0.025)	(0.027)	(0.027)	(0.027)	(0.042)	(0.028)
Incarceration Length (asinh)	-0.333***	-0.336***	-0.354***	-0.333***	-0.298***	-0.339***	-0.095	-0.338***
	(0.063)	(0.063)	(0.058)	(0.063)	(0.056)	(0.063)	(0.135)	(0.067)
N:	*4,280,419*	*4,270,422*	*3,805,179*	*4,278,427*	*4,140,139*	*3,951,392*	*1,071,777*	*4,153,226*
1-year Recidivism	-0.009	—	—	-0.009	—	-0.012**	0.023***	—
	(0.006)			(0.006)		(0.005)	(0.001)	
N:	*3,490,276*			*3,488,337*		*3,171,180*	*317,157*	
2-year Recidivism	-0.011	—	—	-0.011	—	-0.013*	-0.026***	—
	(0.008)			(0.008)		(0.007)	(0.001)	
N:	*3,375,311*			*3,373,488*		*3,073,585*	*299,903*	
Year FEs	Y	Y	Y	Y	Y	Y	Y	Y
Jurisdiction FEs	Y	Y	Y	Y	Y	Y	Y	Y
Defendant/Case Covariates	N	N	N	N	N	N	N	N
Period(s) Included	[-3,4]	[-3,4]	[-3,4]	[-3,4]	[-3-4]	[-3-4]	[-3,4]	[-3,4]
Unit of Observation	Case	Case	Case	Case	Case	Case	Case	Case

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

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 a panel regression estimate, using Equation 2. Column 1 reports estimates using our full sample of states, as in column 2 of Table 5. The remaining columns report the estimates obtained after dropping the state listed in the column header. Missing cells denote states for which we could not observe individual recidivism. Robust stnadard errors clustered at the jurisdiction-election level appear in parentheses.

Table A8: Heterogeneity in Democratic DA Treatment Effects: Panel Estimates

		Defendant Characteristics					
	All	Race/Ethnicity		Gender		Age	
	Cases	Non-white	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 A9: 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 A10: Do Democratic DAs Affect Recidivism Rates? Robustness to Using Case Resolution Date

	Sample Mean	Panel Estimates			Cross-sectional Estimates		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1-year Recidivism	0.275	-0.015**	-0.014**	-0.016**	-0.038**	-0.022	-0.004
	(0.446)	(0.006)	(0.007)	(0.007)	(0.016)	(0.026)	(0.015)
N:	*4,060,787*	*3,404,729*	*3,404,729*	*4,060,787*	*38*	*32*	*137*
2-year Recidivism	0.373	-0.015*	-0.016*	-0.015	-0.045**	-0.031	-0.034**
	(0.484)	(0.008)	(0.008)	(0.008)	(0.021)	(0.032)	(0.016)
N:	*3,825,247*	*3,255,074*	*3,255,074*	*3,825,247*	*35*	*32*	*128*
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 four years post-election (column 7). The cross-sectional specification is Equation 1. All cross-sectional specifications employ jurisdiction population weights. All panel and cross-sectional specifications include a linear control for the Democratic candidate's margin of victory (or loss) and its interaction with an indicator for whether a Democratic candidate won the election. Additional defendant and case covariates are described below Table 5. Robust standard errors clustered at the jurisdiction-election level appear in parentheses.

Table A11: 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 A12: 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 A13: Panel Results by Defendant Prior Criminal Case

	Full Sample	Prior Court Appearance?		
		None	$\leq$ 1 Yr Ago	$\leq$ 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 A14: 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.