

Prosecutor Politics: The Impact of Election Cycles on Criminal Sentencing in the Era of Rising Incarceration*

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Abstract

I investigate how political incentives affect the behavior of district attorneys (DAs). I compile a new comprehensive dataset on the election cycles and offices sought for all district attorneys in office during the steepest rise in incarceration in U.S. history (roughly 1986–2006). Exploiting variation in the timing of elections, I find that being in a DA election year increases per capita admissions and months sentenced to state prisons. I estimate that the election year effects on admissions are akin to moving 0.85 standard deviations along the distribution of DA behavior within state (e.g., going from the 50th to 80th percentile in the intensity of sentencing activity). I also find evidence that sentencing outcomes are associated with public sentiment: (1) election effects are higher in Republican counties; (2) election effects depend on county political ideology more than DA ideology, with effects larger in contested elections; (3) anti-Black/pro-White county-level bias is associated with greater punitiveness on Black prisoners throughout the entire election cycle; (4) election effects declined in the era of rising incarceration, closely coinciding with softening public opinion on punishment; and (5) election effects disappeared at the national level after the era of rising incarceration ended. Taken together, these findings suggest DA behavior and sentencing outcomes may respond to voter preferences—including to racial sentiment and preferences regarding the harshness of the court system. This paper thus highlights how collective approaches to transforming U.S. public opinion, and not simply technocratic approaches to policy, may be instrumental in curbing mass incarceration.

Keywords: criminal justice, inequality, mass incarceration, politics, prosecutors

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1 Introduction

State and federal prisoners in the U.S. increased by over 600 percent between 1970 and 2000—from roughly 200,000 to 1.3 million. As of 2020, the U.S. had the highest total number of incarcerated individuals in the world (with more than approximately 2.1 million people in jail or prison) and the highest rate of incarceration (at approximately 600 people per 100,000).

Leading scholars attest that the unprecedented rise in incarceration rates is in no small part attributable to policy choices (Travis, Western, and Redburn 2014). For example, in the 1960s and 1970s, the use of incarceration was expanded in a multitude of ways, including: prison time being increasingly required for lesser offenses; sentences being lengthened for violent crimes and repeat offenders; and drug crimes becoming more severely punished. In the 1980s, more policy changes were adopted, such as mandatory minimums for various offenses. And in the 1990s, Congress and more than half of states adopted “three strikes and you’re out” laws that mandated minimum sentences of 25 years or longer for relevant offenders. Scholars attest that policy choices represent the “best single proximate explanation” for the unprecedented rise in incarceration (Travis, Western, and Redburn 2014), yet it remains unknown whether these policy choices are the *prime* cause, or instead simply a proximate but intermediate one.

Policy choices are not made in a vacuum; they can reflect political moments encouraged by the public will. To understand how the public’s preferences may have impacted rising incarceration, there is perhaps no better elected official to research than local prosecutors (frequently called district attorneys or DAs). Many legal practitioners hold that it is DAs who guide who gets incarcerated and for how long—more so than any other actor in the criminal justice system. The decision on whether to charge an individual, on what charges to file, on whether to drop a case, on whether to offer a plea bargain, and on the specific terms of the plea offer remains an unreviewable power of DAs. Significantly, how district attorneys arrive at these [sometimes literally] life-and-death decisions remains under-explored. Furthermore,

given the importance of initial charging decisions and the extremely high prevalence of plea bargaining—a process mostly negotiated by the district attorney—many believe sentencing outcomes are more linked to the decisions and discretion of the respective district attorney than they are to the corresponding judge.¹ Lastly, the vast majority of DAs are elected officials, which suggests their decisions might be influenced by politics (e.g., by considerations for re-election or for viability as a candidate for higher office later).

How do political incentives and pressures affect the decisions of district attorneys? During the era of rising incarceration, the political landscape may have incentivized elected officials to enact harsher punishments. The 1980s witnessed the expansion of the War on Drugs, with the coincident crack-cocaine epidemic² and the doubling of handgun-related homicides between 1985 and 1990. The 1990s saw public and political mobilization in favor of harsher punishments culminate in the landmark 1994 Crime Bill, the largest crime bill in U.S. history. And throughout this period, many politicians sought to be viewed as “tough-on-crime.”

Past research has found voters can weigh performance in election years more than in non-election years (see, e.g., Healy and Lenz 2014). As such, this paper studies the impact of DA election cycles on criminal sentencing outcomes. I compile a new comprehensive dataset on the election cycles and offices sought for all district attorneys in office during the steepest rise in incarceration in U.S. history (roughly 1986–2006). Exploiting variation in the timing of elections, I find that being in a DA election year increases total admissions per capita to state prisons and total months sentenced per capita. In assessing the magnitude of the findings, I estimate that the election year effects on admissions are akin to moving 0.85 standard deviations along the distribution of prosecutor sentencing behavior within state (akin to going from the 50th percentile to the 80th percentile in level of sentencing). Similarly, the

1. In recent times, plea bargaining has been the mode of conviction for over 95 percent of convicted felons in counties, meaning individual sentences were strongly influenced by negotiations with the district attorney, and not by the outcomes of judge or jury trials. More information can be found on the ACLU Website (<https://www.aclu.org/blog/smart-justice/across-america-single-most-powerful-person-local-criminal-justice-systems>).

2. The term “War on Drugs” was coined by the media after a press conference by Richard Nixon in 1971. From 1980 to 1984 the federal annual budget of the FBI’s drug enforcement units went from 8 million to 95 million.

election year effects on months sentenced are akin to moving 0.62 standard deviations (akin to going from the 50th percentile to the 73rd percentile in level of sentencing).

What are the implications of these findings on understanding key drivers behind the unprecedented rise in incarceration? First, the findings support the notion that voter preferences influence incarceration above and beyond policy choices, since the empirical strategy controls for differences in policy choices across place and time (among other controls). Second, after dispelling alternative explanations such as differences in arrest or crime rates, this paper provides evidence that political incentives have at least a short-term cyclical impact on DA behavior—and in turn on the punitiveness of the criminal justice system. Relatedly, political incentives could also increase the baseline level of sentencing throughout the *entire* election cycle.³ Lastly, I explore various mechanisms and find further evidence that sentencing outcomes are associated with public sentiment: (1) election effects are higher in Republican counties; (2) election effects depend on county political ideology more than DA ideology, with effects larger in contested elections; (3) anti-Black/pro-White county-level racial bias is associated with greater punitiveness on Black prisoners throughout the entire election cycle; (4) election effects declined in the era of rising incarceration, closely coinciding with softening public opinion on punishment as measured through the General Social Survey (a longitudinal survey administered yearly since the 1970s); and (5) election effects disappeared at the national level after the era of rising incarceration ended. Taken together, these findings suggest DA behavior and sentencing outcomes may respond to voter preferences—including to anti-Black/pro-White racial sentiment and preferences regarding the harshness of the court system.

This paper makes four main contributions. First, this paper has compiled a new comprehensive dataset on the election cycles of—and future offices sought by—district attorneys. I compile information on the political careers of over 4,200 DAs—every district attorney in office between roughly 1986–2006. Second, this paper provides causal evidence that DA

3. For example, the analysis on racial bias shows that criminal sentencing for Black prisoners throughout the entire election cycle increases with greater anti-Black/pro-White racial bias in the county.

election years increase the severity of certain criminal sentencing outcomes. The empirical analysis covers approximately 40 states and spans over 20 years at the height of the rise in U.S. incarceration levels. No other study has been found to date that compares election dynamics of district attorneys between U.S. regions and across time. Third, this paper explores key mechanisms behind these findings along spatial, political, and racial lines—including introducing new insights on the link between county-level psychological racial bias and racial bias in criminal sentencing outcomes. In particular, I find evidence that (1) greater levels of anti-Black/pro-White racial bias are associated with more punitive sentencing outcomes for black prisoners throughout the entire election cycle, and (2) in election years both black *and* white prisoners experience more punitive sentencing outcomes. I have not found prior research that has provided such a link between racial bias in community sentiment and racial bias in criminal sentencing. Fourth, I find that district attorney election effects decline over the period 1986–2006, in tandem with U.S. public opinion softening regarding criminal punishment. No prior studies have been found that evaluate the evolution in DA election year effects over time and compare them to trends in public opinion.

In short, district attorneys appear to respond to political incentives from the local electorate, which is important given the public’s greater desire for punitiveness toward those convicted of crimes during the era of rising incarceration. Other elected officials, including some of the legislators who enacted policy changes to the criminal justice system from the 1960s through 1990s, may similarly have been responding to public sentiment. If so, then the policy choices during that era in fact represent an intermediate—rather than a prime—cause of the unprecedented rise in incarceration. Consequently, collective approaches to transforming U.S. public opinion, and not simply technocratic approaches to policy choices, may be instrumental in curbing mass incarceration.

Related Literature. Research has explored more broadly the role of prosecutorial discretion in sentencing outcomes (Bjerk [2005](#)), and its relevance to perpetuating sentencing disparities in light of reforms in determinate sentencing (Miethe [1987](#); Stemen, Rengifo, and

Wilson 2005). A couple studies have explored the effects of district attorney election cycles in individual states, like Florida (Nadel, Scaggs, and Bales 2017) and North Carolina (Dyke 2007). Yet none of these studies has employed a dynamic difference-in-differences approach, which allows more granular comparisons of election years to the years immediately preceding and following the election. This identification strategy provides more compelling evidence for the existence (or absence) of election year effects. Among other findings, this paper provides new insight on heterogeneity in DA election year effects over time, by geographic region, and based on the political ideology of the corresponding district.⁴

Another contribution of this paper is its scope, which spans the vast majority of U.S. states over a 20-year time period, thereby allowing greater understanding of the role of election cycles on national trends in incarceration. In contrast, the focus of past studies on individual states creates challenges in understanding the role of election cycles on national trends in mass incarceration. Furthermore, the analysis on national incarceration trends cannot be performed by solely relying on the extant literature on judges, mainly because the vast majority of criminal sentences result from plea bargaining, a process in which the role of the judge is more limited than that of the district attorney.⁵ Lastly, this paper aims to further our understanding of mechanisms behind the election year effects, in particular exploring the significance of public sentiment and political ideology.

Past research suggests that appointed district attorneys tend to have lower conviction rates than their elected counterparts, who might be more concerned with appearing competent to their voters (Rasmusen, Raghav, and Ramseyer 2009). The literature also suggests that those DAs who are in office for longer terms tend to prosecute fewer cases, which might be a result of the fact that they do not have to worry about re-election quite as much as DAs who only hold office for four-year terms. Bandyopadhyay and McCannon (2014) compares

4. Dippel and Poyker (2019) have found significant heterogeneity across states in the context of judicial election cycles.

5. Various studies have explored the complex dynamics involved with plea bargaining (Landes 1971; Baker and Mezzetti 2001), as well its relationship to the imposition of Federal Sentencing Guidelines (Schulhofer and Nagel 1996).

data from North Carolina DA elections and appointed DA states to find that re-election incentives led to a 10% increase in the proportion of cases taken to trial; the presence of a challenger led to an additional 15% increase. The paper found that when re-election pressures increase, DAs tend to take more cases to trial and they tend to offer less plea bargains, perhaps in order to boost their re-election probability. Pfaff (2011) suggests that the convex increase in incarceration figures in the U.S. in the 1980s and 1990s is connected to an increase in the total number of felony filings per arrest (a discretionary power of the DA), as opposed to increases in arrest rates (decided by the police), increased time served per sentence (sometimes decided by a judge in the absence of a plea deal when the case goes to trial), and increased crime rates (often connected to much larger social, political, and economic factors). Neal and Rick (2016) provides a contrasting perspective, finding that much of the growth in prison populations can be attributed to changes in sentencing policies. Krumholz (2019) investigates the impact of district attorney political affiliation, finding that a Republican district attorney leads to an increase in prison admissions and new sentenced months per capita in the four years following their election, driven by higher incarceration of drug offenses.

Prior studies have explored the relationship between federal prosecutors and sentencing outcomes. For instance, Rehavi and Starr (2014) finds that case and defendant characteristics can explain much, though not all, of disparities in federal criminal sentencing. The remaining gaps in outcomes can be attributed to differences in prosecutors' initial charging decisions, particularly the decision to charge with crimes that involve mandatory minimum sentences. Other studies have similarly looked at national data on federal criminal suspects to identify racial disparities (see, e.g., Mustard 2001; Shermer and Johnson 2010).

The sparse research on district attorneys does relate to a larger literature on how judges respond to political pressures—yet district attorneys and judges occupy distinct roles in the criminal justice system so may face different incentives. Huber and Gordon (2004) mentions how voters may be more likely to perceive instances of underpunishment by judges than

overpunishment, which plausibly also applies to the prosecutorial discretion that district attorneys exercise. In addition, many judges themselves previously served as district attorneys, introducing another source of similarity through selection.

Evidence that judges respond to political incentives has been found in various contexts.⁶ Lim, Snyder Jr, and Strömberg (2015) finds that newspaper coverage has impacted judicial sentencing for violent offenses by nonpartisan elected judges; Berdejó and Yuchtman (2013) finds that Washington State judges have given 10% longer sentences at the end of their political cycle than at the beginning; and Abrams et al. (2019) finds that sentencing for felonies in North Carolina increases as elections approach. Yet a recent study has found that although the severity of criminal sentencing from judges does follow electoral cycles in some states, there is much heterogeneity (Dippel and Poyker 2019). In fact, the impact of electoral cycles on sentencing outcomes is not found in two-thirds of states studied after expanding the sample. Furthermore, election year effects only appeared for competitive judicial elections.

This paper proceeds as follows. Section 2 provides institutional background on the role of the district attorney. Section 3 describes the data collected for this study. Section 4 explains the empirical strategy I employ. Section 5 describes the main results. Section 6 explores mechanisms behind the results that relate to public sentiment, and Section 7 concludes.

2 Institutional Background

2.1 Description of District Attorney Role

The district attorney is the most common term for the chief local prosecutor, who can also be referred to as a county prosecutor, a city prosecutor, a state prosecutor, a state attorney, a prosecuting attorney, a Commonwealth's attorney, a circuit solicitor, an attorney general, or a district attorney general, depending on the state. DAs can be the representative for either a

6. Other research that has explored the impact on judicial decisions in response to federal guidelines becoming less binding include Stith (2007), Scott (2010), Fischman and Schanzenbach (2012), and Nowacki (2015).

single county, numerous counties, or an entire state in legal matters involving the government. While DAs have complete authority to prosecute within their districts, the direct day-to-day tasks involved in prosecution of felonies is performed by their assistant district attorneys (ADAs) and/or deputy district attorneys (DDAs). Yet DAs are often responsible for the actual hiring and promotion of ADAs and DDAs in their offices. Furthermore, DAs decide the charging and sentencing policies for their office (within the discretion allowed by state law): just as they can decide to follow more punitive policies in their district, they can also decline to prosecute some crimes, they can narrow the range of sentencing dictated by the law for certain crimes, and they can choose rehabilitative sentences over incarceration.⁷ In short, in their respective offices, district attorneys can set the tone of prosecution, the culture of the office, and the decision-making policies.

2.2 Selection Process

Currently, chief local prosecutors are elected in every U.S. state but three—Alaska, Connecticut, and New Jersey, where they are appointed respectively by their Attorney general, a commission, or the governor. After being elected or appointed, DAs in most states serve four-year terms—those in Alabama, Kentucky, and Louisiana serve six-year terms, DAs in Tennessee serve eight-year terms, and DAs in New Hampshire serve two-year terms. In every state but Colorado, there is no limit to the number of terms that one can hold the office of district attorney. There is also not an equal distribution of the population that a given DA district serves—the DA’s office in Los Angeles oversees over 10 million people while the office in Alpine County, California, has jurisdiction over just below 1000 people (Hessick and Morse 2020).

7. For example, on March 20, 2020, it was reported that Kim Foxx, the DA in Cook County IL, decided that her office would not prosecute low-level drug offenses during the COVID-19 pandemic.

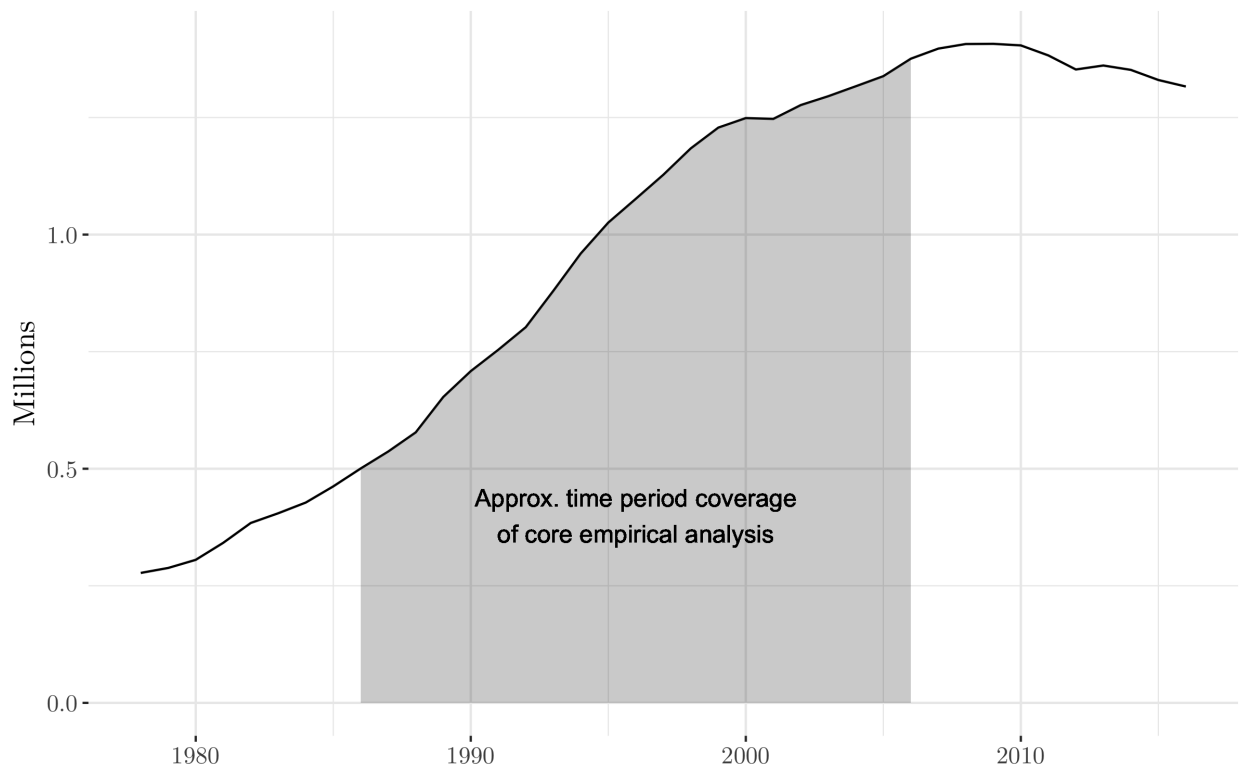
3 Data

3.1 Sentencing Data

National Directory of Prosecuting Attorneys—I compiled the names of all district attorneys in the United States using the the National Directory of Prosecuting Attorneys (administered by the National District Attorneys Association) for all years in which the directory could be retrieved: 1987, 1989, 1992, 1994, 1998, 1999, 2000, 2003, and 2005.⁸ For the core empirical analysis of this paper, I include the full term in office for the district attorneys

8. No issues of the National Directory of Prosecuting Attorneys could be found after 2005, suggesting it has been out of print for several years.

Figure 1: United States Prison Population (State & Federal)



Note: Source of the prison population data is the United States Department of Justice Office of Justice Programs (Bureau of Justice Statistics). Core empirical analysis in this study includes entire term of all district attorneys listed in the issues of the National Directory of Prosecuting Attorneys found between 1987 and 2005. Consequently, the coverage years indicated in the graph are approximate values with only slight variation by district, because DAs have different start/end years for their 4-year terms based on the respective district.

identified through this process. Since election years tend to fall in even-numbered years, the core empirical analysis thus spans roughly 1986–2006. The advantages of analyzing this time period are numerous; two include: (1) the time period corresponds with the largest recorded increase in total U.S. prison population (see Figure 1); and (2) the period corresponds with the greatest state participation in sentencing outcome data from the National Corrections Reporting Program (as described immediately below).

National Corrections Reporting Program (NCRP)—This represents the primary source for sentencing data used in this study. NCRP collects offender-level data on admissions and releases from state and federal prison. The NCRP records include offender-level information such as: BJS offense category, state offense category, total sentence, race, gender, age, prior felony status, county, and state where sentence imposed. The analysis includes the full universe of records on prison admissions.⁹ Sentenced months corresponds with the longest length of time as stated by the court that the offender could be required to serve for all offenses of which they have been convicted.¹⁰

3.2 Election Data

Election data was compiled from various sources. Election results of district attorneys were compiled from states, counties, and other sources (see dataset indicated in Hessick and Morse (2020)). The Database on Ideology, Money in Politics, and Elections (DIME) contains over 130 million political contributions made by individuals and organizations for local, state, and federal elections between 1979 and 2014; it also includes information on candidates and committees for state and federal elections (Bonica 2015).

9. At the time of writing the manuscript, the datasets include [NCRP-1983](#), [NCRP-1984](#), [NCRP-1985](#), [NCRP-1986](#), [NCRP-1987](#), [NCRP-1988](#), [NCRP-1989](#), [NCRP-1990](#), [NCRP-1991](#), [NCRP-1992](#), [NCRP-1993](#), [NCRP-1994](#), [NCRP-1995](#), [NCRP-1996](#), [NCRP-1997](#), [NCRP-1998](#), [NCRP-1999](#), [NCRP-2000-2016](#).

10. Life sentences and death penalties are coded as 1200 months, or 100 years.

Table 1: Annual State Prison Sentencing Outcomes, By County

	Mean	Median	Standard Deviation
Admissions/1000 Population			
All Offenses	1.68	1.21	2.83
Violent Offenses	0.42	0.31	0.70
Drug Offenses	0.50	0.30	1.21
Property Offenses	0.63	0.48	0.87
Sentenced Months/1000 Population			
All Offenses	141.73	99.32	215.02
Violent Offenses	65.61	42.86	115.62
Drug Offenses	37.03	18.81	74.35
Property Offenses	40.40	27.75	52.37

Notes: Data cover roughly 1986–2006 and 42,500 county-years.

4 Empirical Strategy

In this paper, I adopt a quasi-experimental research design to estimate the effects of election cycles on the decision-making of district attorneys. County-level variation in the timing of district attorney elections resulted in substantial variation across counties that I exploit using a series of dynamic difference-in-differences specifications:

$$\log(Y_{cst}) = \gamma_s + \lambda_t + \sum_{k=-T}^{T-1} \beta_k \mathbb{1}\{R_{ct} = k\} + \Gamma_{cst} + \varepsilon_{cst} \quad (1)$$

The variable Y_{cst} corresponds to one of two outcome variables—(1) admissions/ capita or (2) months sentenced/capita—evaluated for county c in state s in time period t .¹¹ Time period t is evaluated in either years or in months. On the right-hand-side, let $R_{ct} = t - E_c$ denote the “relative time”—the number of periods relative to the nearest election period for the district attorney in county c in time period t . T equals half the length of a district attorney’s term in

¹¹ Following the approach of Lim, Snyder Jr, and Strömberg (2015), all life sentences and death penalties are coded as a 1200-month sentence.

office.¹² γ_s are state fixed effects, λ_t are year fixed effects, and Γ_{cst} is a vector of controls.¹³ The standard errors are corrected for correlation across district attorneys and over time in a given county by clustering at the district level.

The coefficients of interest are β_k for $k \neq$ normalized time period. I indicate in each section below which election cycle time period corresponds with the normalized time period. For example, if I normalize such that $\beta_0 = 0$, that would mean for a district attorney serving in county c in time period t , all β_k coefficients for $k \neq 0$ would estimate the sentencing outcomes *relative to* the sentencing outcomes during the election period. I measure the dependent variable in logs, which yields regression estimates that can approximate the percentage difference in sentencing outcomes.¹⁴

I first estimate these models using the NCRP data for which I also have data on the corresponding prosecutor from the National Directory of Prosecuting Attorneys.¹⁵ The NCRP data contain offender-level information, such as BJS offense category, total sentence, county, and state where sentence imposed. The sample for each year consists of all states that reported data who had four-year election cycles.¹⁶

The key identifying assumption for difference-in-differences estimation strategies is the common trends assumption, which in this case is that any differential change in outcomes in district attorney election years is the result of the district attorney election. Perhaps the greatest challenge to this identifying assumption is the confounding effects of other relevant

12. The length of a district attorney term in office is determined by the state and usually equals four years (since -2 and +2 are functionally equivalent in this specification, the index of summation ends at $T - 1$). Furthermore, only one state was found that had their district attorney term length change during the time period under analysis, Arkansas. Arkansas Prosecuting Attorneys were elected for 2-year terms until about 2001; Amendment 80 §20 of the Arkansas Constitution (passed in 2001) modified the Prosecuting Attorneys' term limits to 4 years.

13. The main results persist even with inclusion of controls such as white share of the population and per capita income.

14. A log point estimate of a approximates a $(100 \cdot a)\%$ effect when the magnitude of a is small. The formal definition of log points holds that an estimate of a corresponds with a multiplicative effect of e^a .

15. The years of coverage correspond to the full term length for all prosecutors who were in office between 1987 and 2005. The states of coverage consist of the entire United States, except for the states with non-elected DAs—Alaska, Delaware, Connecticut, and New Jersey.

16. See Appendix Table A.1 for the number of total reporting states by year in the NCRP dataset. All years from 1986–2006 include data on over 44 states in the United States, with over 1/3 including all 50 states. Limiting to states with four-year election cycles yields a sample of 39 states.

elections with the same timing as DA election cycles, for example of other offices related to law enforcement (e.g., sheriff elections). These alternative elections may have direct (and indirect) effects on sentencing outcomes. I address this issue by re-running the analysis only including election cycles in which the district attorney elections are not synchronous with those of other offices. As discussed later in Section 5.4, the results are largely unchanged.

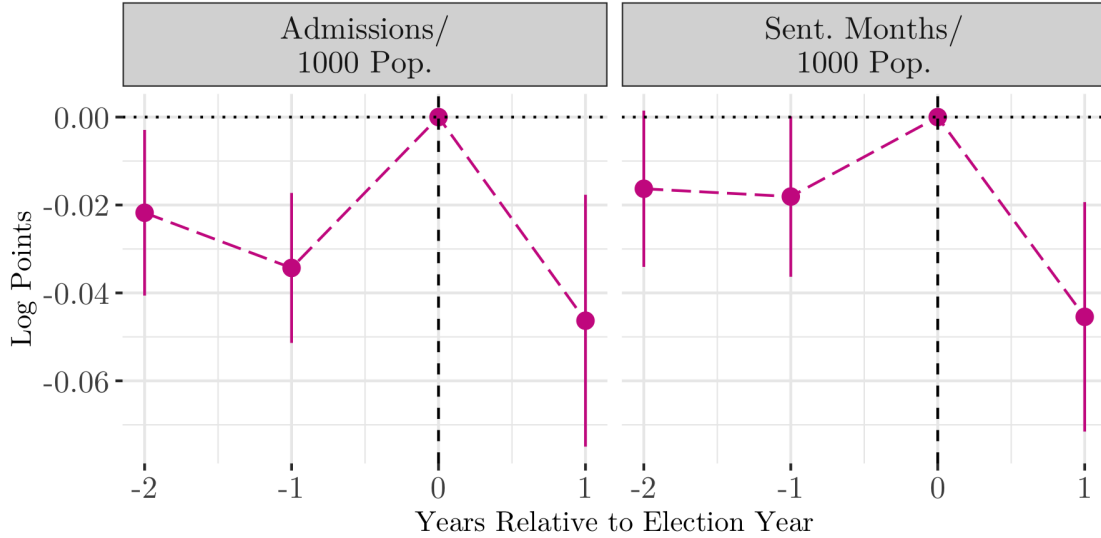
5 Results

5.1 Election Effects on Criminal Sentencing – Outcomes by Year

Figure 2 graphically depicts estimates of the effects of election cycles on two separate criminal sentencing outcomes across all offenses: (1) admissions per 1000 population; and (2) total sentenced months per 1000 population. These graphs follow the regression specification from Equation 1. All estimates are calculated relative to the election year value, which is normalized to 0. Hence, if the estimates at time $t = -1$ and $t = 1$ are both negative, this can be interpreted as the election year (when $t = 0$) having a relatively higher magnitude of the corresponding criminal justice outcome compared with the year prior and the year after. All estimates are in log points. Vertical lines mark the 95% confidence interval.

The graphs provide evidence that election years have effects on the admissions rate per 1000 population and the sentenced months per 1000 population. The estimated effects of an election year on the admissions rate are statistically significant and positive, compared to all non-election years. The estimated effects of an election year on months sentenced are also positive compared to all other years, with the effect being statistically significant for the year immediately following the election year.

Figure 3 again illustrates estimates of the effects of election cycles on criminal sentencing outcomes using regression specification Equation 1, this time by criminal offense subcategory. This analysis provides evidence that similar effects persist across offense subcategories, with the impact on sentenced months relative to adjacent years being less pronounced for drug

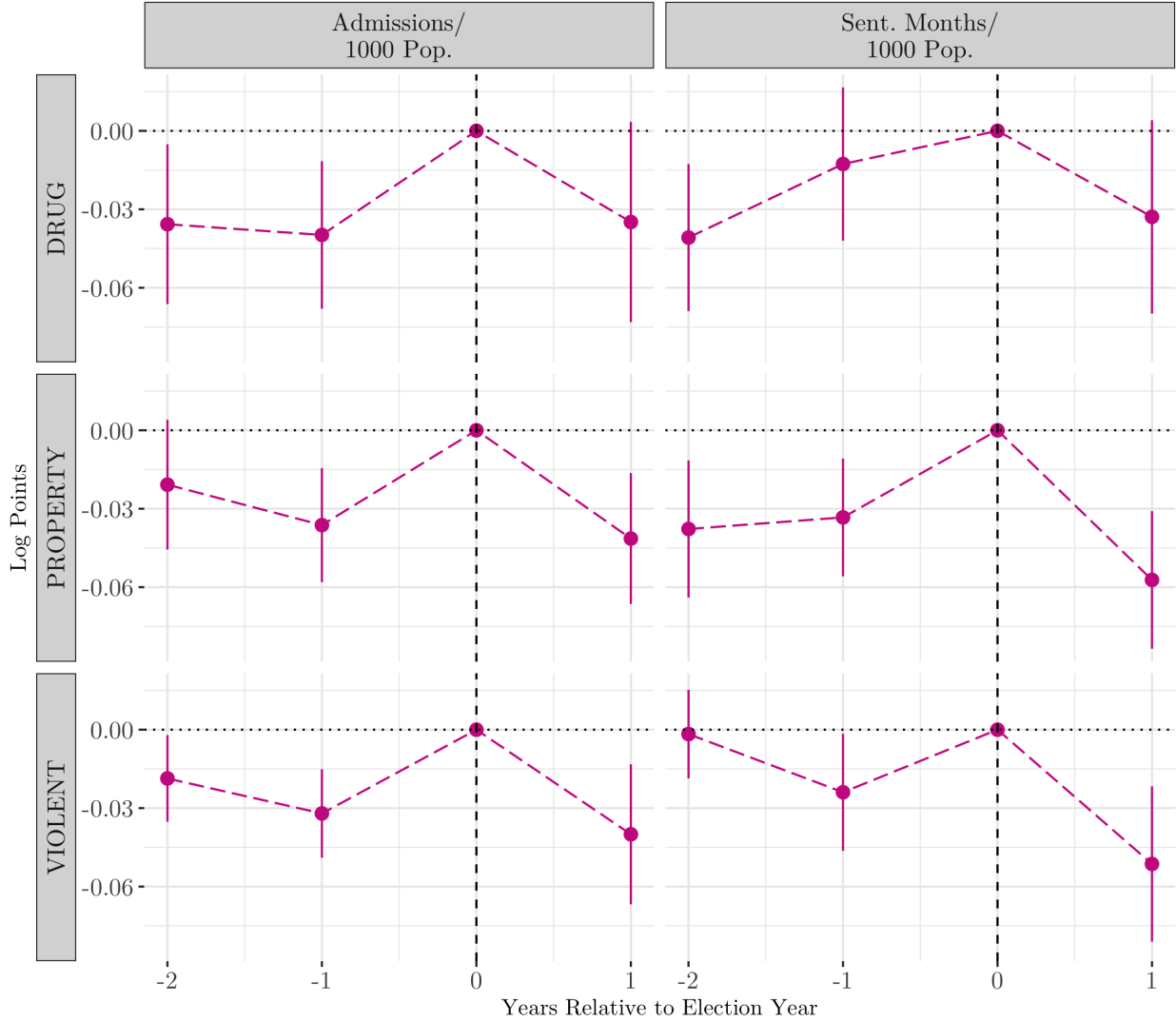
Figure 2: Criminal Sentencing Outcomes,
Relative to District Attorney Election Year

Notes: Graph depicts dynamic difference-in-differences model estimates using Equation 1 (weighted to adjust for differences in sampling probabilities across districts and across time) for counties with 4-year election cycles. Estimates are at the county level by year. The standard errors are corrected for correlation across district attorneys and over time in a given county by clustering at the district level. Estimates include state- and year- fixed effects. Vertical lines mark two standard errors. Estimates calculated relative to the election year value, which is normalized to 0. Corresponding regression table is in the Appendix Table C.2.

offenses compared to property and violent offenses. Not only is the effect of election years on admissions rate positive and statistically significant across most non-election years for each crime subcategory, but also the magnitude of this impact on admissions rate appears smallest for violent offenses.

Heterogeneity of election year effects across crime categories could be affected by various potential factors, including differences in political risk from lenient behavior, as well as varying levels of discretionary power DAs possess for different crime categories. First, there could be greater political risk associated with being lenient on certain crime categories than for others (e.g., consider the political incentives for being harsh on a high-profile murder vs. being harsh on low-level drug possession). In addition, mandatory minimum sentencing is more prominent for certain crime categories than others, which could impact the discretion prosecutors have in plea bargaining negotiations. This in turn might be a factor in explaining

Figure 3: Criminal Sentencing Outcomes,
Relative to District Attorney Election Year – Offense Subcategories



Notes: Graph depicts dynamic difference-in-differences model estimates using Equation 1 (population weighted to adjust for differences in sampling probabilities across districts and across time) for counties with 4-year election cycles. Estimates are at the county level by year. The standard errors are corrected for correlation across district attorneys and over time in a given county by clustering at the district level. Estimates include state- and year- fixed effects. Vertical lines mark two standard errors. Estimates calculated relative to the election year value, which is normalized to 0. Corresponding regression tables are in Appendix Tables C.3, C.4, and C.5.

heterogeneity in election year effects across crime categories.

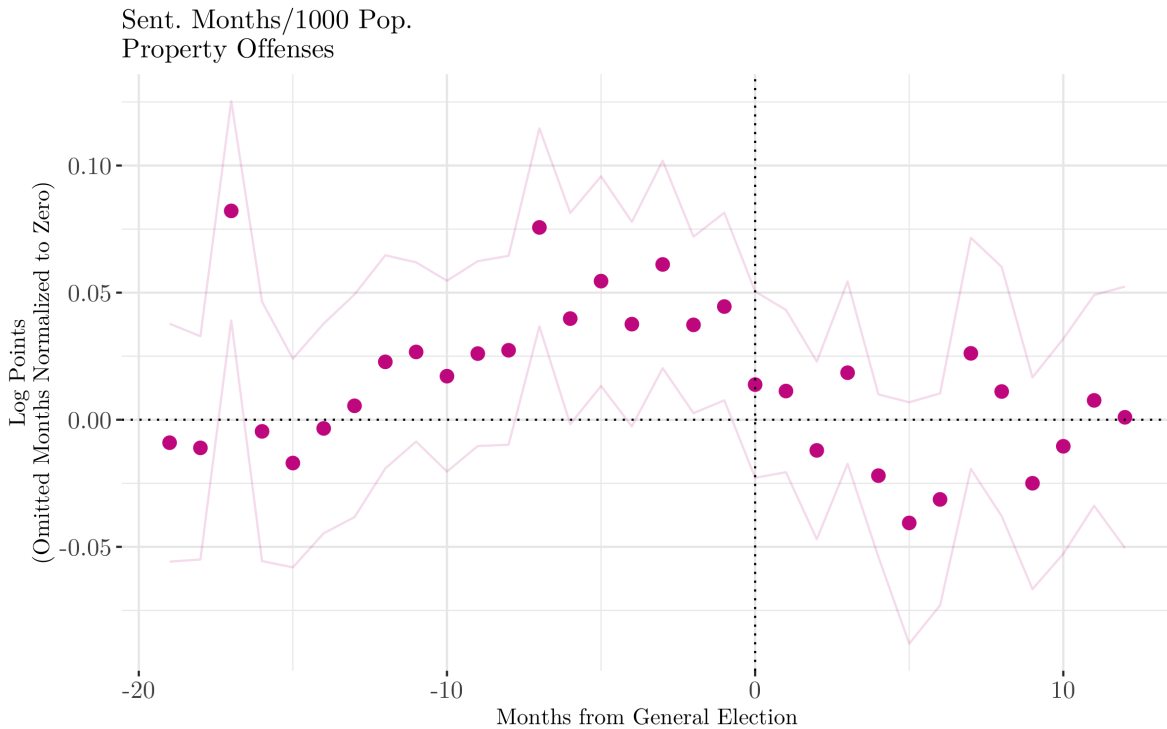
5.2 Election Effects on Criminal Sentencing – Outcomes by Month

This subsection presents another analysis of Equation 1, this time with the time period observed in months and all coefficients measured relative to the months omitted from the graph (the months which are furthest from the election year).¹⁷

Figure 4 illustrates the estimates for total months sentenced for property offenses during roughly the one year period before an election year, during the election year itself, and

17. Omitting this volume of months from the specification sufficiently reduces the dimensionality of the dependant variables for convergence. This analysis also is a modification from Equation 1 because year fixed effects are replaced by year-month fixed effects.

Figure 4: Monthly Total Sentenced Months in Election Cycle (Property Offenses)



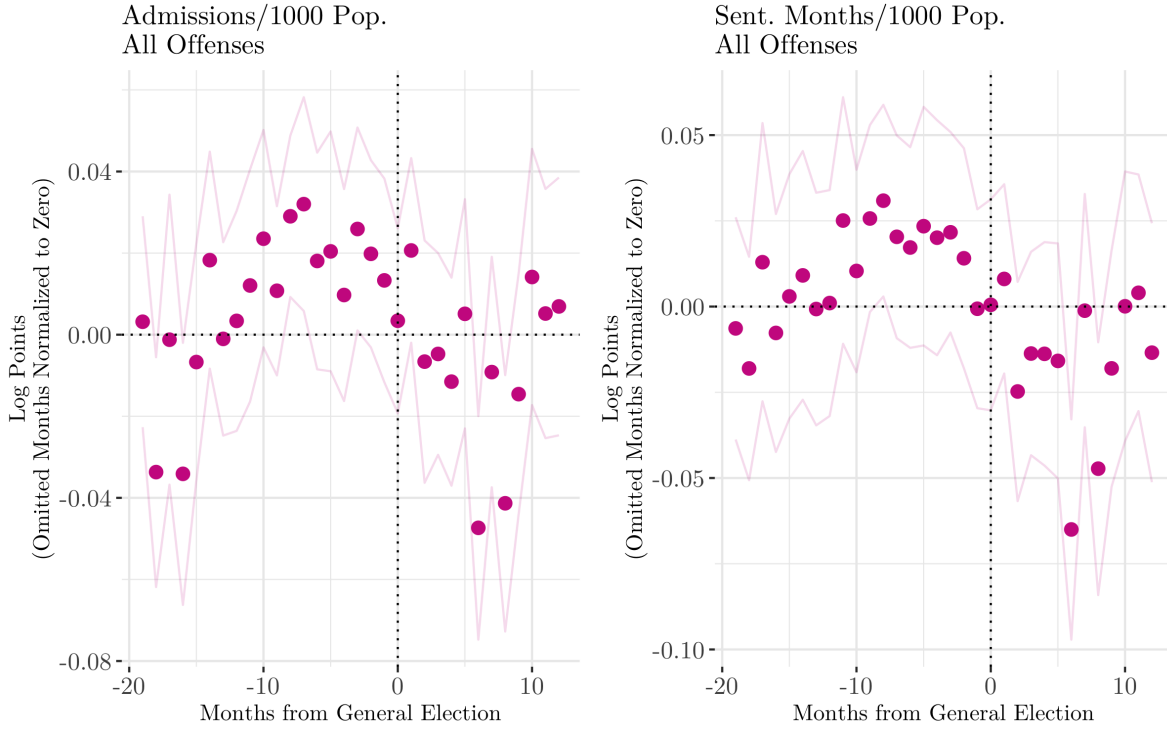
Notes: Graph depicts dynamic difference-in-differences model estimates using Equation 1 (population weighted to adjust for differences in sampling probabilities across districts and across time). Estimates are at the county level by month. Estimates include state- and year-month- fixed effects. Lines demarcate 95% confidence intervals. Estimates calculated relative to omitted months in election cycle, which are all normalized to 0. Corresponding regression table is in Appendix Table C.6.

during the one year period after the election year. A corresponding regression table is in the Appendix. The plot shows perhaps the most clear consistent upward trend in the outcome variable from about 15 months prior to the election through the middle of the election year. Across months in the same jurisdiction, there may be a common district attorney and similar regional conditions, yet individual criminal cases are generally unrelated to each other. Thus, the fact that the dots are close together and increasing in the period immediately preceding a general election suggests that there is in fact an increasing trend occurring during this time period.

One can clearly see that all the estimates during the election year are greater than zero, which is not the case for the time periods before and after the election year—and is also not true for the time period omitted from the graph, since that period has been normalized to zero. Also, one can see that the estimates immediately following the election year have the greatest concentration of estimates below zero (followed by the period preceding the election year), which is consistent with the prediction from the theoretical model that the sentencing intensity of district attorneys increases in the election year. Political incentives and their impact on prosecution may be lowest just after an election concludes.

Figure 5 illustrates a similar plot as Figure 4, except looking at the average across all offense categories. Again, the corresponding regression tables are in the Appendix. It is apparent that the sentencing outcomes for both admissions per capita and sentenced months per capita is higher during the election year period than during the time periods before and after, which is consistent with the property offense plot from Figure 4. Furthermore, one can see that just about all the estimates during the election year are greater than zero, which is not the case for either of the two other time periods included in the graph. Also, one can see that the estimates immediately preceding the election year are more frequently lower than the election year period, which is again consistent with the theoretical model from Equation 5. Specifically, this plot provides evidence that the estimates for both admissions per capita and sentenced months per capita increase over the election cycle on average. In

Figure 5: Monthly Criminal Sentencing Outcomes in Election Cycle



Notes: Graph depicts dynamic difference-in-differences model estimates using Equation 1 (population weighted to adjust for differences in sampling probabilities across districts and across time). Estimates are at the county level by month. Estimates include state- and year-month- fixed effects. Lines demarcate 95% confidence intervals. Estimates calculated relative to omitted months in election cycle, which are all normalized to 0. Corresponding regression table is in Appendix Table C.7.

other words, the period immediately following the election has the lowest estimates (with a negative mean), then the estimates increase in the second year after the election (which has been normalized to zero), then they increase again in the period immediately preceding the election year, and then increase again during the election year itself. This is consistent with the perspective that DAs may respond to political incentives associated with the election cycle.

One slight puzzle is that there seems to be a single lagging positive estimate that extends into the last month of the election year (at $t = 1$) for the admissions rate plot. This slight jump is not present in the months sentenced plot nor in the property offenses plot from Figure 4, suggesting it might be due to an alternative offense subcategory. Yet this jump does not meaningfully represent an outlier, given the confidence intervals of the adjacent

estimates.

5.3 Magnitude of Election Year Effects

To interpret the magnitude of the election year effects, I adopt a similar methodology for signal variance as described in various papers (see, e.g., Chetty and Hendren 2018). Signal variance (χ^2) allows one to interpret the magnitude of election year effects in units of standard deviations under basic assumptions (based on the underlying distribution of district attorney sentencing behavior).

I estimate χ^2 by subtracting the average sampling variance across district attorneys ($E[s_{ps}^2]$) from the variance of the residuals obtained by regressing $\hat{\mu}_s$ (state fixed effects) on \bar{y}_{ps} (district attorney sentencing outcomes):

$$\chi_p^2 = \text{Var}(\hat{\mu}_s - \gamma_p(\bar{y}_{ps} - \bar{y}_s)) - E[s_{ps}^2] \quad (2)$$

where $\hat{\mu}_s$ denotes the causal effect of an election year on sentencing outcomes in state s , \bar{y}_{ps} denotes the mean sentencing outcomes for district attorney p in state s , $\bar{y}_s = E[\bar{y}_{ps}]$ is the mean of \bar{y}_{ps} across district attorneys within state s , and $\gamma_p = \frac{\text{Cov}(\hat{\mu}_s, \bar{y}_{ps})}{\text{Var}(\bar{y}_{ps})}$ is the coefficient obtained from regressing $\hat{\mu}_s$ on \bar{y}_{ps} .¹⁸

This procedure yields $\chi_{admissions}^2 = 0.0323$ for the admissions rate per capita and $\chi_{months}^2 = 0.0364$ for the total months sentenced per capita. I then take the election year estimates calculated from the static difference-in-differences specification in Equation 4 and compare it to the (square root) signal variance. Under an additional assumption of normality in district attorney sentencing outcomes, the estimates for χ^2 tell us that, on average, the election year

18. In practice, the basic steps I employ to calculate χ^2 are as follows. First, I estimate the mean sentencing outcomes for each district attorney (\bar{y}_{ps}). Second, I compute an estimate of how much variation there is across district attorneys in sentencing outcomes (within state). I accomplish this by regressing state fixed effects ($\hat{\mu}_s$) on the set of district attorney sentencing outcomes (\bar{y}_{ps}), then calculate the Mean Squared Error from this regression. It is important to realize that this measure of variation across district attorneys within state is partly driven by noise (e.g., from sampling variation). Consequently, as a third step, I subtract the amount of variation coming from the noise ($E[s_{ps}^2]$). To do so, I subtract the average standard error of the district attorney fixed effects squared.

effects on admissions are akin to moving 0.85 standard deviations along the distribution of prosecutor sentencing behavior within state. This means that with respect to the admissions rate, being in an election year is akin to going from the 50th percentile of district attorney sentencing intensity to the 80th percentile. Under similar normality assumptions, the election year effects on months sentenced are akin to moving 0.62 standard deviations along the distribution of prosecutor sentencing behavior, on average. This means that with respect to the months sentenced, being in an election year is akin to going from the 50th percentile of district attorney sentencing intensity to the 73rd percentile.

5.4 Robustness Checks

5.4.1 Alternative Weighting for Two-Way Fixed Effects (TWFE) Estimator

Recent research has highlighted that in settings with variation in treatment timing across units—such as in election cycles—the estimated coefficients in two-way fixed effects regressions can be contaminated by effects from other periods (De Chaisemartin and d’Haultfoeuille 2020; Sun and Abraham 2021). Given this potential problem in the estimation of regression coefficients in the two-way fixed effects model, in Appendix Section B.0 I adapt an alternative estimator introduced by Sun and Abraham (2021), which can be applied to my setting despite some superficial differences with the event-study setting they consider. This alternative estimator is robust to treatment effect heterogeneity. Under this alternative two-way fixed effects estimator, the magnitude and direction of the election year effects largely remain.

5.4.2 Common Trends Assumption

The key identifying assumption for the difference-in-differences empirical approach used throughout this Section is common trends. Given the cyclical nature of elections, the common trends assumption in this context is different than in standard settings with an absorbing treatment (i.e., where units remain treated once initially treated). In particular, in this paper’s setting, for the common trends assumption to hold I require that in the absence of

treatment (i.e., without being in a DA election time period), the expectation of the outcome (i.e., per capita admissions rate and per capita months sentenced) follows the same evolution over time in every county (De Chaisemartin and d’Haultfoeuille 2020).

Perhaps the greatest threat to the common trends assumption is potential confounding effects from other elections—in other words, if there are other local and state elections systematically occurring at the same time as DA elections that meaningfully impact sentencing outcomes. The leading contenders for such confounding elections are mayoral elections, judicial elections, and sheriff elections. Research has found that mayoral elections can increase local police presence (Levitt 2002), which may impact sentencing outcomes via changes in the arrest rate. Judicial elections may have an impact on sentencing outcomes due to judicial discretion in sentencing for certain cases, particularly some cases where defendants are convicted via trial. And sheriff elections may have an impact on sentencing outcomes, given the fact that sheriffs can serve as the principal police force within a county (and may thus exhibit different law enforcement behavior as elections approach). Below, I describe each of these threats in more detail, as well as provide empirical evidence for why the common trends assumption still holds despite these potential threats.

McCrary (2002) and Levitt (2002) find that municipal police forces tend to vary over local and state electoral cycles. This finding might seem to threaten common trends. Yet even if higher police forces did coincide with mayoral election years—and such mayoral elections did in turn coincide with district attorney elections—this would not necessarily violate the common trends assumption. Owens (2020) shows that, in the case of one program, increasing the number of police officers did not appear to increase arrest rates, suggesting the potentially higher arrest frequency from having more officers may have been offset by greater deterrence of potential offenders from the expanded police presence. This deterrent effect is consistent with Levitt (1998), which finds that arrest rates appeared to reduce the frequency of many different types of crimes, not just ones likely to be committed by the person arrested.

Particularly because evidence suggests larger election year police forces may primarily

impact crime through greater deterrence, then if election effects on sentencing were in fact caused by increases in municipal police forces, one would not expect an increase in the admissions rate or total sentenced months during the election period. To the contrary, one would expect the election period estimates to have the opposite sign: the deterrent effect would lead to lower admissions and lower total sentenced months. As such, it is difficult to ascribe the DA election year increases in admissions and months sentenced to changes in police presence associated with other local and state elections. In addition to this descriptive evidence, I provide further evidence through an empirical test at the end of this subsection (see Figure 6) that the potential confounding factor of increased police presence during DA election years does not threaten the interpretation of the findings.

Another potential challenge to the common trends assumption are judicial elections, especially given the role judges can play in determining sentencing for those convicted by trial. Yet as was described earlier in this paper, in recent times plea bargaining has been the mode of conviction for over 95 percent of convicted felons in counties, meaning individual sentences were strongly influenced by negotiations with the district attorney and not by the outcomes of judge or jury trials. Although judges have the power to not approve a plea bargain, they rarely do so (Wright and Miller 2002). In addition, the transition from indeterminate sentencing to determinate sentencing in the late 1970s caused state legislatures to pass laws that constrained the discretion of judges as well as parole boards (Neal and Rick 2016). Sentencing policies, such as mandatory minimums, presumptive sentencing ranges, truth-in-sentencing laws, and two- and three-strike laws, provide determinate guidelines judges must follow in sentencing decisions, greatly limiting the extent to which they can affect criminal sentencing outcomes (Pfaff 2011; Neal and Rick 2016).¹⁹ In contrast, policies analogous to sentencing guidelines for judges do not exist for district attorneys. DAs have unreviewable discretion on whether to bring charges, and can strategically select the collection of charges for a criminal

19. Though *Blakely v. Washington* (2004) may have resulted in the relaxation of some sentencing guideline systems, the decision occurred after the vast majority of elections included in the core empirical analysis of this paper. Furthermore, the actual interpretation and application of the Supreme Court ruling by state judicial system came even later.

suspect that incorporates sentencing guidelines, based on how severe or lenient they wish to be. Thus, district attorneys have much control over admissions—through choosing whether or not to charge an individual with a crime—and sentence length, through choosing which collection of charges to bring against a criminal suspect. In contrast, the channel through which judicial elections would meaningfully increase the extensive margin—the number of individuals actually admitted to state prisons—is less convincing. These facts provide evidence that the common trends assumption holds with regards to the potential confounding factor of judicial elections. Furthermore, I provide an additional empirical test at the end of this subsection (see Figure 6) by analyzing the district attorney election year effects when the district attorney elections do not coincide with judicial elections.

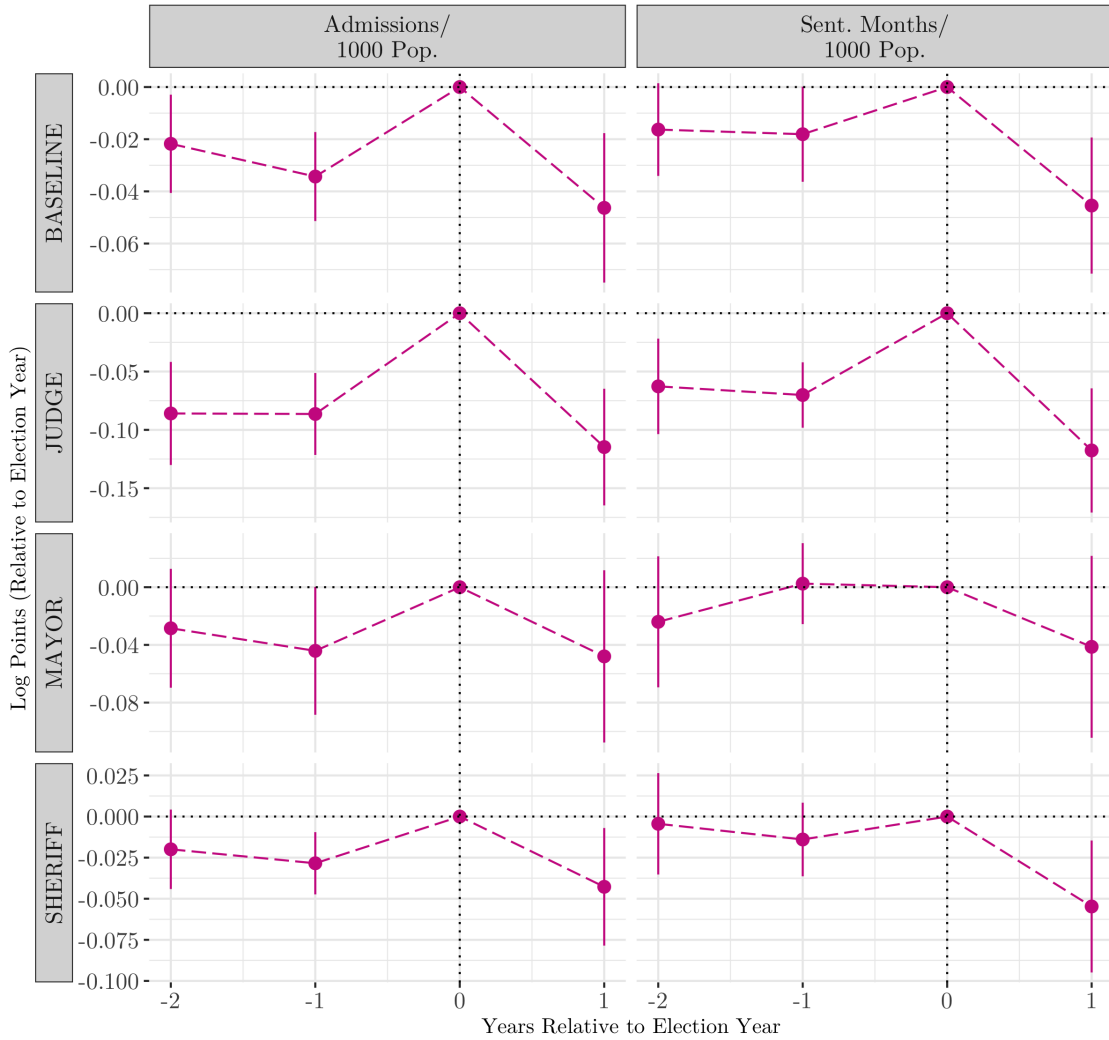
A third challenge to the common trends assumption is sheriff elections. The scope of the sheriff role varies across states and counties. The sheriff is most often an elected officer, and in some counties where urban areas have their own police departments, a sheriff may be restricted to civil procedure enforcement duties; in other counties, the sheriff may serve as the principal police force with jurisdiction over all the county’s municipalities. In the latter case, the sheriff may be responsible for enforcing criminal law. The earlier facts I cite regarding the deterrent impact of police presence similarly provides evidence the common trends assumption holds for sheriff elections. To provide further evidence, I also test empirically this assumption below (see Figure 6) by analyzing the district attorney election year effects when the district attorney elections do not coincide with the sheriff elections.

To test for potential confounding effects of synchronous judicial, mayoral, and sheriff elections, I rely on data from Lim and Snyder Jr (2015), Benedictis-Kessner (2018), and Thompson (2020) for judges, mayors, and sheriffs, respectively. The Lim and Snyder Jr (2015) data consists of over 58,000 state judicial elections—at the trial, appellate and supreme court level. I manually map which elections by state correspond to trial court judicial elections, and only use those corresponding elections in this analysis.²⁰ The Benedictis-Kessner (2018)

20. Trial courts are where cases are tried in the first place—as opposed to an appeals or supreme court—and represent the most plausible challenge to the common trends assumption.

data consists of 9,131 mayoral elections, and includes data from several different sources on elections between 1950 and 2014 in cities of all sizes. I merge the city election data with the corresponding county, and use the gap between two consecutive election years to determine the historical incidence of elections for a particular municipality. This allows me to employ

Figure 6: Criminal Sentencing Outcomes, Relative to District Attorney Election Year – Non-Synchronicity with Other Elected Office



Notes: Judge, mayor, and sheriff non-synchronous analysis omits areas where no data found to determine synchronicity with district attorney election cycle. Baseline analysis includes all district attorney election cycles. Graph depicts dynamic difference-in-differences model estimates using Equation 1 (weighted to adjust for differences in sampling probabilities across districts and across time). Estimates are at the county level by year. The standard errors are corrected for correlation across district attorneys and over time in a given county by clustering at the district level. Estimates include state- and year- fixed effects. Vertical lines mark two standard errors. Estimates calculated relative to the election year value, which is normalized to 0.

the same two-way fixed effects model I performed in the previous subsection—illustrated in Figure 6—this time removing county-years in which the district attorney election year was synchronous with a local mayoral election. For this analysis, I only include counties in which one can identify whether or not there was a synchronous mayoral election; counties without corresponding mayoral election data were not included in the analysis. I perform a similar analysis with the sheriff election data from Thompson (2020), which includes over 3,200 partisan sheriff elections, as well as the judicial data from Lim and Snyder Jr 2015.

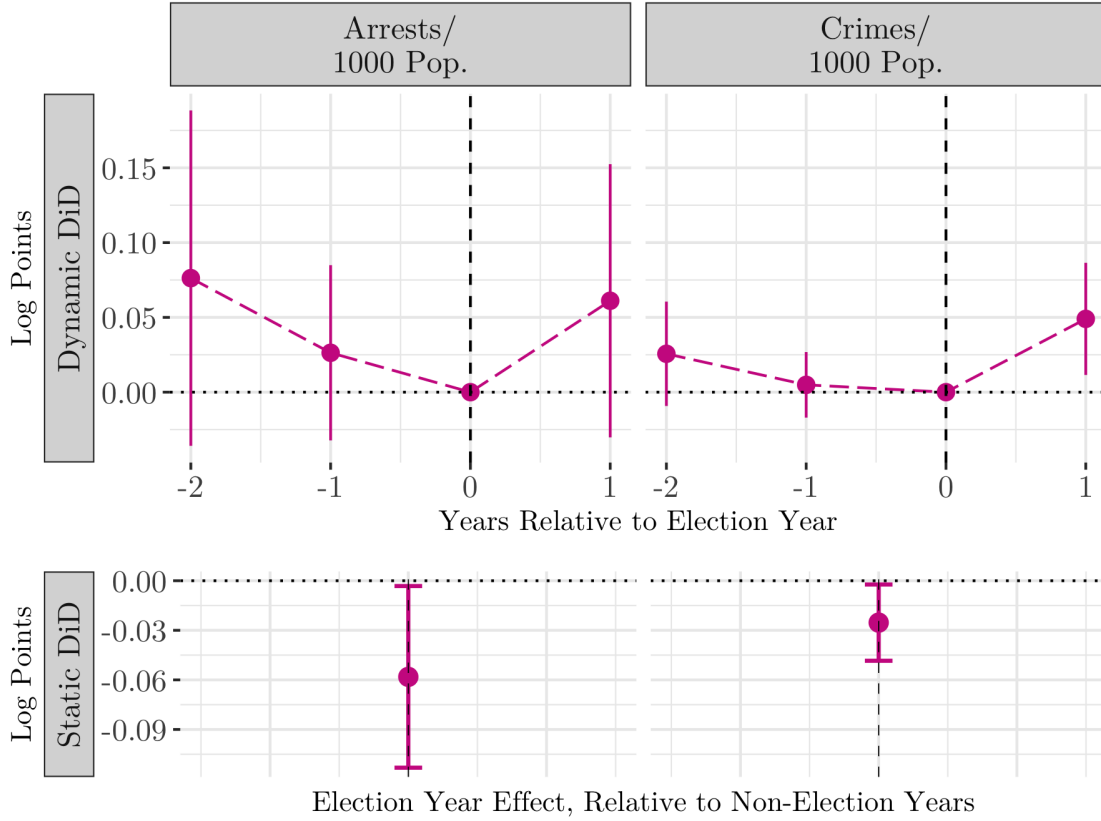
Figure 6 illustrates that after removing county-years in which the district attorney election was synchronous with the judicial, mayoral, and sheriff elections, the same direction of the election year effects remain. This provides further evidence that the common trends assumption holds. In Appendix Figure C.1, I perform an additional analysis using the alternative two-way fixed effects estimator, which provides additional support in favor of this paper’s interpretation of the underlying DA election treatment effects.

5.4.3 Results Not Explained by Changes in Arrest Rates or Crime Rates

Figure 7 graphically depicts estimates of the effects of DA election cycles on per capita arrest rates and per capita crime rates,²¹ via the regression specification from Equation 1. The plots show there is a statistically significant *negative* election year effect on the volume of arrests and crimes (which is the opposite direction of the core election effects on criminal sentencing outcomes). Hence, Figure 7 does not support the notion that the main election effects on criminal sentencing outcomes can be explained by changes in the volume of arrests or crimes around elections. In other words, this analysis undercuts a perspective that election year increases in the admissions rate or total months sentenced might be attributable to election year increases in arrests or crime. Thus, it appears DA election effects are not attributable to changes in criminal behavior; this evidence suggests criminal activity does

21. Arrest and crime data for this analysis comes from the Uniform Crime Reporting (UCR) Program. Though there are important caveats to using the county-level statistics (see https://www.icpsr.umich.edu/web/pages/NACJD/guides/ucr.html#desc_cl), it may represent the best available source for historical county-level arrest and crime data spanning the United States.

Figure 7: Arrest & Crime Rates,
Relative to District Attorney Election Year



Notes: Graphs depicts dynamic or static difference-in-differences model estimates using Equation 1 or Equation 4, respectively (weighted to adjust for differences in sampling probabilities across districts and across time). Estimates are at the county level by year. The standard errors are corrected for correlation across district attorneys and over time in a given county by clustering at the district level. Estimates include state- and year- fixed effects. Vertical lines mark two standard errors. Dynamic DiD estimates calculated relative to the election year value, which is normalized to 0. Regression tables are in Appendix Tables C.9 and C.10.

not spike in or near DA election years. Furthermore, the earlier discussion on mayoral and sheriff elections in Section 5.4.2 describes how those election years can increase police presence and thereby increase arrest rates. Given this relationship—and since arrest rates are not increasing in DA election years—Figure 7 suggests the relative paucity of synchronicity between DA elections and those of mayors/sheriffs, and/or the relatively low impact of mayor/sheriff elections on the election effects found in this study when they are synchronous.

5.4.4 Other Robustness Checks

I also performed additional robustness checks. For example, the findings in this study are robust to specifications in which the set of controls—white share of the population and income—are not included. The findings are also robust to county (instead of state) fixed effects. Also, the shape of the curve depicting the relationship between the election cycle and criminal sentencing outcomes is preserved under a log transformation— $\log(1+x)$ —of the data; the utility of this transformation is that it prevents zeros from being omitted from the regression analysis. See the Appendix for graphs of the primary election cycle analyses with log transformations.

6 Mechanisms on Public Sentiment

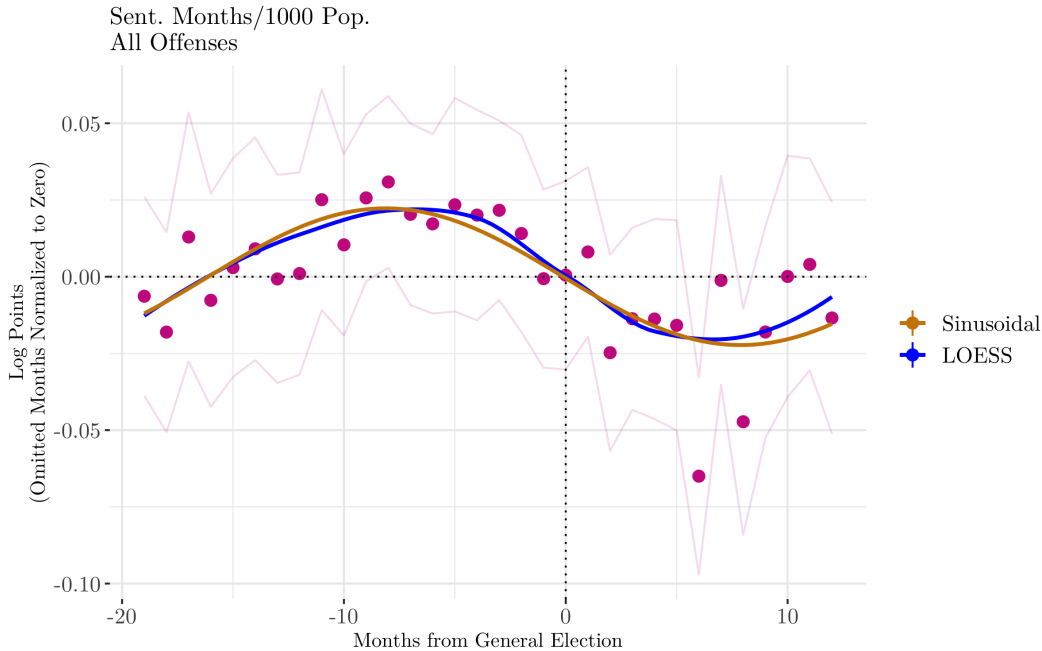
The results thus far document that the admissions rate per capita and the rate of total months sentenced per capita increase in the lead up to general elections during an era of rising incarceration (roughly 1986 to 2006). In this section, I perform additional analysis to shed light on various mechanisms that underlie the relationship between DA election year sentencing outcomes and public sentiment. The analysis uncovers five key facts, described in more detail below: (1) DA election effects vary by region and are concentrated in the South Central U.S.; (2) DA election effects depend on county political ideology more than DA ideology, with effects larger in contested elections; (3) anti-Black/pro-White county-level bias associated with greater punitiveness on black prisoners throughout the entire election cycle; (4) DA election effects declined in the era of rising incarceration, coinciding with softening public opinion on punishment; and (5) DA election effects disappeared at the national level after the era of rising incarceration ended. Taken together, these facts provide evidence of a nexus between public sentiment and sentencing outcomes.

6.1 DA Election Effects Vary by Region and are Concentrated in South Central U.S.

To begin our spatial analysis of the election year effects, let us return to the monthly sentencing outcome analysis from Section 5.2. For this spatial analysis, I seek to also use a second modeling approach since there is comparatively less data available for any given region. Hence, the objective here is to estimate the magnitude of the election period effect given the relative time from an upcoming election. To capture potential non-linearities, I first fit a LOESS regression to the non-parametric conditional expectation functions plotted previously in Figure 5 to find a transformation of relative time from an election that renders the relationship between sentencing outcomes and relative time linear.²² Through

²² As described in (Chetty et al. 2018), this estimation approach is analogous to a Box-Cox transformation. The main assumption underlining this estimator is that the shape of the conditional expectation of the outcome is preserved in each county up to an affine transformation.

Figure 8: Parameterizing Sentencing Outcomes



Notes: Graph depicts a dynamic difference-in-differences model (population weighted to adjust for differences in sampling probabilities across districts and across time). Estimates are at the county level by year. Jagged lines demarcate 95% confidence intervals. Estimates include state- and year- fixed effects. Plot includes non-parametric LOESS best-fit line and parametric sinusoidal model.

observation of Figure 8, an appropriate transformation appears to be sinusoidal. Hence, one approach I use in this section is to estimate partial effects on the amplitude A from the following specification evaluated with time t in years (since differences in the A parameter roughly correspond to differences in the magnitude of election year effects):

$$\log(Y_{cst}) = \gamma_s + \lambda_t + A \cdot \sin(2\pi R_{ct}/4 + \phi) + \varepsilon_{cst} \quad (3)$$

A second approach I use is the static difference-in-differences model from Equation 4 below, evaluating the β parameter:

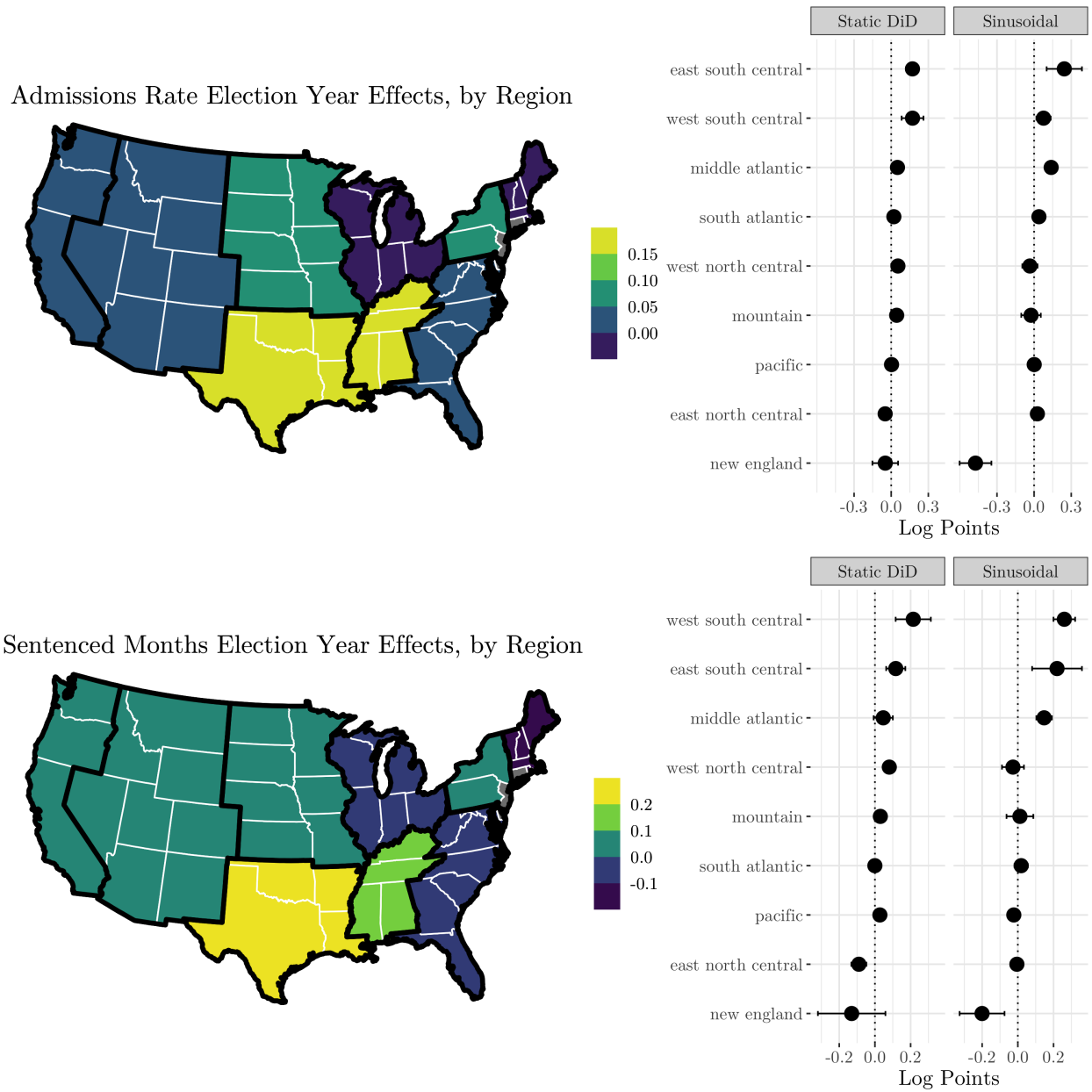
$$\log(Y_{cst}) = \gamma_s + \lambda_t + \beta D_{ct} + \Gamma_{cst} + \varepsilon_{cst} \quad (4)$$

The dependent variable Y_{cst} corresponds to one of two outcome variables—(1) admissions/capita or (2) months sentenced/capita—evaluated for county c in state s in year t . On the right-hand-side, let D_{ct} denote the treatment, which is an indicator variable denoting whether county c is in an election year in year t . γ_s are state fixed effects, λ_t are year fixed effects, and Γ_{cst} is a vector of controls.²³ The standard errors are corrected for correlation across district attorneys and over time in a given county by clustering at the district level.

Based on the two approaches from Equation 3 and Equation 4, I examine how election effects vary by geographic region. Figure 9 shows election year effects by region for both outcome variables, with the maps on the lefthand side shaded according to partial effects from the static difference-in-differences model (Equation 4). The righthand side of the figure shows the corresponding partial effects based on both the static diff-in-diff model and the sinusoidal model (Equation 3), evaluated with the time period in years. One can see that the largest election year effects on admissions rate is consistently in the South Central region of the U.S. (east and west). Furthermore, these effects significantly differ from zero for both modeling

²³. Again, the main results persist even with inclusion of controls such as white share of the population and per capita income.

Figure 9: Election Year Effects By Region



Notes: Map shading depicts a static difference-in-differences model (population weighted to adjust for differences in sampling probabilities across districts and across time). Estimates are at the county level by year. The standard errors are corrected for correlation across district attorneys and over time in a given county by clustering at the district level. Estimates include state- and year- fixed effects. Horizontal lines mark two standard errors. Heterogeneity across regions calculated via a static difference-in-difference model and a sinusoidal model, respectively.

approaches. New England and the East North Central regions have negative election year effects, with the estimate from the sinusoidal model being significant and largely negative. The election year effects in many other geographic regions are not statistically different than zero.

The plot for the months sentenced outcome show similar results, with the largest estimated election year effects for the South Central region (east and west). Again, New England and East North Central—which is the region that includes Chicago—are negative, with each region’s estimate significant in just one of the two models.

6.2 DA Election Effects Depend on County Political Ideology More than DA Ideology; Effects Larger in Contested Elections

Figure 10 shows the association of county, prosecutor, and election traits with DA election year effects on criminal sentencing outcomes. Whereas the political ideology of the district has a statistically significant association with the election year effects, evidence suggests the political ideology of the district attorneys themselves do not. Across both outcomes, the association between being a conservative DA and the election year effects is weakly positive.²⁴ Counties that voted Republican in the most recent Presidential election had statistically significant and positive association with election year effects for both outcome variables. Counties that voted Republican in more than 6 out of 10 elections from 1980 to 2016 had a weaker association with election year effects, with the association only being significant for the admissions rate. One interpretation of these findings is that it is the most recent (Republican) political dynamics that most influence election year effects on sentencing outcomes, a conclusion which in turn is consistent with the perspective (and with the theoretical model from Equation 5) that political incentives from reelection concerns

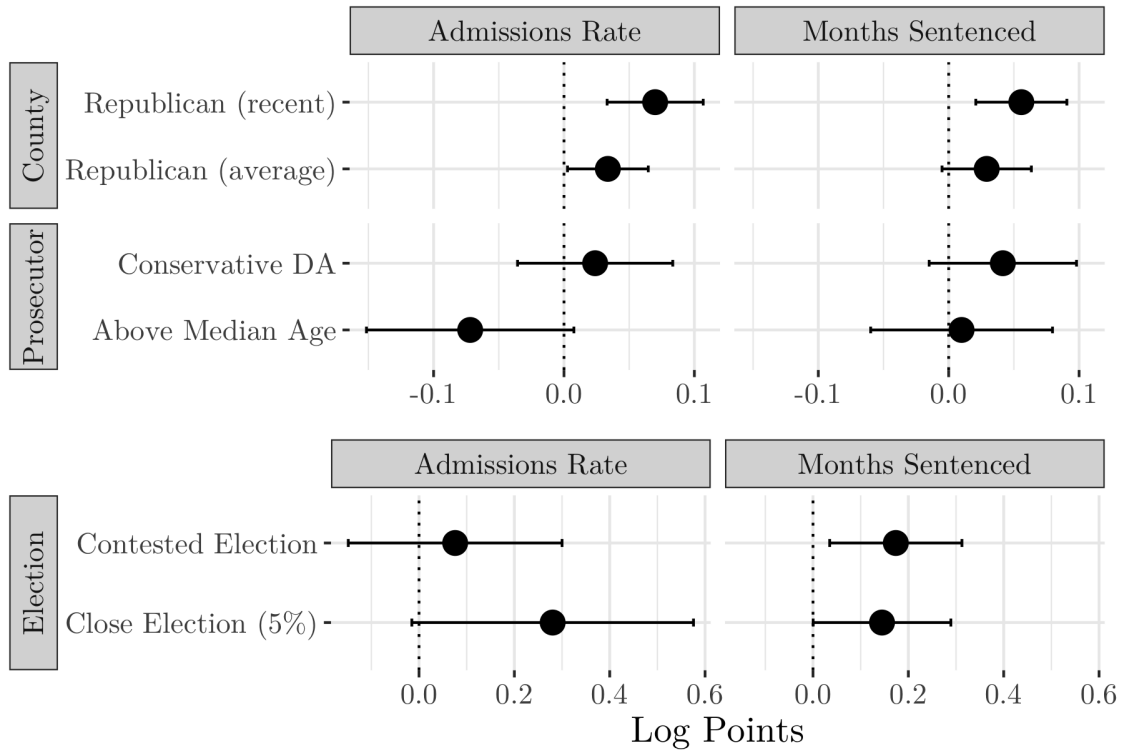
24. Ascribing the political ideology of individual district attorneys was performed by linking the district attorney names with donors with the Database on Ideology, Money in Politics, and Elections (DIME) (Bonica 2015). Individuals with an ideology score below 0 were coded as liberal, while those with a score above 0 were coded as conservative. The coverage of the DIME database does not include all district attorneys; hence, Figure 10 includes analysis on DAs who were successfully matched.

impact district attorney behavior.²⁵

The salience of reelection concerns is reinforced by the bottom plot in Figure 10. The plot demonstrates that being in a contested or close election is associated with greater election year effects (though only the effect on the months sentenced is statistically significant). This election analysis is based on a subset of the total county-years, reflecting those county-years in which corresponding election data were successfully compiled (from state and county data). This election data covers approximately 4,000 district attorney general elections from

25. There may be other plausible interpretations; for example, an uptick in local crime may lead to both more admissions and a Republican shift in voting.

Figure 10: Association of County, Prosecutor, & Election Traits with Election Year Effects



Notes: Graph depicts a static difference-in-differences model (population weighted to adjust for differences in sampling probabilities across districts and across time), calculated by running a pooled regression of Equation 4 and interacting with DA and county characteristics. Estimates are at the county level by year. The standard errors are corrected for correlation across district attorneys and over time in a given county by clustering at the district level. Estimates include state- and year- fixed effects. Horizontal lines mark two standard errors. “Republican (recent)” corresponds with counties that voted for a Republican Presidential candidate in the most recent election. “Republican (average)” corresponds with counties that voted for a Republican Presidential Candidate in more than 6 out of 10 elections from 1980 to 2016. District attorney and election characteristics analysis based on subset of overall counties where data was found.

the 1980s onward (out of over 30,000).

6.3 Anti-Black/Pro-White County-Level Bias Associated with Greater Punitiveness on Black Prisoners Throughout Election Cycle

In this section, we estimate the magnitude of county-level racial bias and analyze its relationship with election and non-election year sentencing outcomes. To estimate racial bias, we rely on the Implicit Association Test (IAT). The IAT was developed by Tony Greenwald, Mahzarin Banaji, and Brian Nosek, and attempts to measure attitudes and beliefs that people may be unwilling or unable to report. The IAT is intended to measure the strengths of associations between concepts (e.g., black people, white people) and evaluations (e.g., good, bad). More description of the methodology can be found on the Project Implicit website (<https://implicit.harvard.edu/implicit/aboutus.html>). The IAT has been widely used to estimate both implicit bias (bias that is relatively inaccessible to conscious awareness and/or control) and explicit bias (bias that one deliberately thinks about and reports) (see, e.g., Greenwald et al. 2009).

Apart from ongoing debates surrounding the IAT,²⁶ several considerations must be remembered when interpreting analyses using the Project Implicit IAT data. First, the people who have taken the IAT at the Project Implicit website are not a random sample of Americans; rather, they are individuals who have self-selected to take the online test measuring their implicit biases. These volunteers tend to be younger, more educated, more politically liberal, and more female than the U.S. population as a whole (Mooney 2014). Yet, if selection into the testing sample is similar by county, then it may still be helpful to interpret the data as an approximation for *relative* differences in biases between geographic areas. The analysis below (and in Table 2) exploits the variation in estimated racial bias at the county level. Another consideration to remember is that the analysis below assumes relative differences

26. There has been scholarly and public debate surrounding the IAT (see, e.g., Singal 2017). Proponents of the IAT have responded to some of the critiques, and the debate continues (see, e.g., Lane et al. 2007; Tierney 2008; Sleek 2018).

in estimated racial bias for counties does not differ meaningfully between the time period covered by the Project Implicit data (the 2000s) and the time period of this paper’s election year effects (1980s to early 2000s).

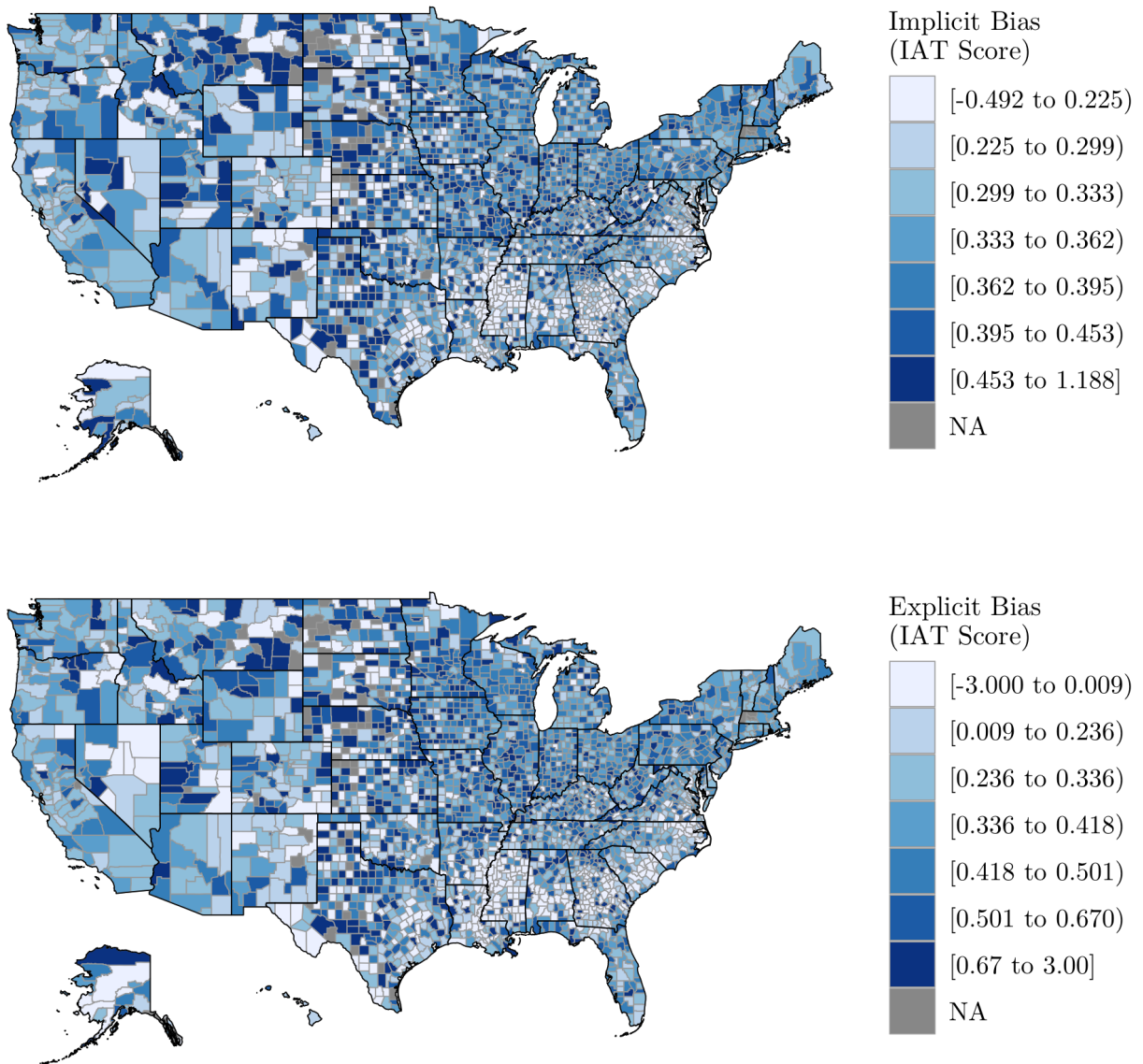
The Race IAT attempts to measure the preference for white people vs. black people (or vice versa) of those who take the test. The IAT score can be interpreted as an estimate for racial bias, where negative values correspond with pro-black bias and positive values correspond with pro-White bias. The magnitude of the score reflects the strength of the racial bias. Figure 11 illustrates there is substantial variation in the estimated racial bias—not only across states, but also across counties within the same state. One important observation to note is that there is pro-White bias in the vast majority of counties in the country. However, though county-level bias is nearly-universally pro-White, there is meaningful variation in the estimated strength of pro-White bias across counties.

Table 2 shows the relationship between sentencing outcomes, election year effects, and county-level racial bias using the static difference-in-differences model from Equation 4. This analysis provides evidence that greater county-level pro-White racial bias is associated with higher baseline levels of punitiveness toward black prisoners. The coefficients on both the implicit bias and explicit bias variables—across both outcome variables—is positive and statistically significant at the 1% level. In contrast, the coefficient of the biases interacted with the election year effect is consistently *negative* and mostly statistically significant for black prisoners.²⁷ In short, the increase in punitiveness toward black prisoners across the entire election cycle far outweighs the associated decline in short-term election year effects. Simply put, the “sea level” of punitiveness toward black prisoners rises with pro-White racial bias, and more than offsets the reduced height of each “wave” of sentencing outcomes from election year effects.

One possible explanation for the findings about black prisoners in Table 2 is linked to

27. The only exception to the statistical significance of the coefficients is the impact on Admissions/1000 population from implicit bias interacted with election year effects, which is still negatively correlated but is instead significant at the 10% level.

Figure 11: Estimated County-Level Racial Bias (Based on IAT Assessment)



Notes: Map shading depicts a randomized sample of 1 million observations in Project Implicit’s publicly-available data for the Race Implicit Association Test. Lighter blues indicate less pro-White bias. Details on the dataset can be found on a Github site associated with the Project Implicit blog (<https://github.com/lizre/map-iat>). Explicit Bias IAT scores are normalized from -3 to +3. For both Implicit Bias and Explicit Bias IAT scores, -3 to 0 corresponds with pro-black bias, while 0 to +3 corresponds with pro-White bias. The IAT Scores are increasing in the extent of pro-White attitudes (and decreasing in the extent of pro-black attitudes).

Table 2: Sentencing Outcomes and Estimated County-Level Racial Bias

	Admissions/1000 Pop. (in Log Points)					
	Black Prisoners			White Prisoners		
	(1)	(2)	(3)	(4)	(5)	(6)
Election Year Effect	0.024** (0.009)	0.070** (0.029)	0.362*** (0.127)	0.031*** (0.011)	0.050 (0.038)	0.206 (0.188)
Implicit Bias (Pro-White) – County-Level		2.021*** (0.319)			0.070 (0.252)	
Election Year Effect x Implicit Bias		-0.143* (0.084)			-0.059 (0.109)	
Explicit Bias (Pro-White) – County-Level			0.732*** (0.098)			0.007 (0.089)
Election Year Effect x Explicit Bias			-0.079*** (0.029)			-0.041 (0.043)
Observations	28,851	28,722	28,722	41,624	41,079	41,079

	Months Sentenced/1000 Pop. (in Log Points)					
	Black Prisoners			White Prisoners		
	(7)	(8)	(9)	(10)	(11)	(12)
Election Year Effect	0.031*** (0.010)	0.110*** (0.035)	0.419*** (0.160)	0.036*** (0.010)	0.082** (0.042)	0.190 (0.202)
Implicit Bias (Pro-White) – County-Level		1.322*** (0.299)			-0.127 (0.251)	
Election Year Effect x Implicit Bias		-0.245** (0.107)			-0.142 (0.120)	
Explicit Bias (Pro-White) – County-Level			0.491*** (0.092)			-0.072 (0.084)
Election Year Effect x Explicit Bias			-0.090** (0.037)			-0.036 (0.046)
Observations	27,210	27,103	27,103	39,325	38,821	38,821

*p<0.1; **p<0.05; ***p<0.01

Notes: Table depicts static DiD model estimates using Equation 4 (population weighted to adjust for differences in sampling probabilities across districts and across time). Estimates are at the county level by year. The standard errors are corrected for correlation across district attorneys and over time in a given county by clustering at the district level. Estimates include state- and year- fixed effects. Biases are measured based on a randomized sample of 1 million observations in Project Implicit’s dataset of the Race Implicit Association Test (more details can be found here: <https://github.com/lizre/map-iat>). Bias (IAT scores) are increasing in the extent of pro-White attitudes (and decreasing in the extent of pro-black attitudes). The county-level bias estimated from the data are assumed to equal that of the time period covered in this study.

the fact that the higher the level of baseline punitiveness across the entire election cycle, the more the baseline punitiveness approaches the highest-possible level that district attorneys can impose given the criminal activity committed in their district. Approaching this ceiling naturally restricts the capacity for district attorneys to become even more punitive toward black prisoners in election years. A ceiling on punitiveness restricts behavior more if the ceiling is binding. This may explain why higher measures of county-level bias against a racial group is associated with lower election year effects for black prisoners, whereas it is associated with *higher* election year effects for white prisoners.²⁸ In short, the ceiling on punitiveness might be more binding for black prisoners than for white prisoners, restricting DAs' capacity to be more punitive as elections near.²⁹

This analysis does not identify whether the association between estimated racial bias and election effects is causal.³⁰ If it is causal, it can be interpreted along at least two dimensions: first, DAs and their offices may themselves exhibit pro-White biases that influence how they respond to black defendants. Second, DAs may instead be responding to voter preferences—to the appetites of their respective constituency as elected officials—even if they themselves do not exhibit pro-White racial biases. Also, if the association between estimated racial bias and election effects is causal, then this analysis suggests differences in pro-White racial bias may influence sentencing outcomes for black prisoners much more than white ones—a finding particularly relevant given the significant over-representation of black people in U.S. prisons.³¹

28. Increases in the bias measures from Table 2 represents higher bias against black prisoners, and lower bias against white prisoners.

29. Many have documented how the criminal justice system is more punitive for black defendants. In the criminal sentencing data used for the analysis in this paper, the mean admissions rate per 1000 population is 16.1 for black prisoners and 1.2 for white prisoners—though this does not account for differences in criminal activity between demographic groups. Similarly, the mean months sentenced per 1000 population is 1369 for black prisoners and 95 for white prisoners.

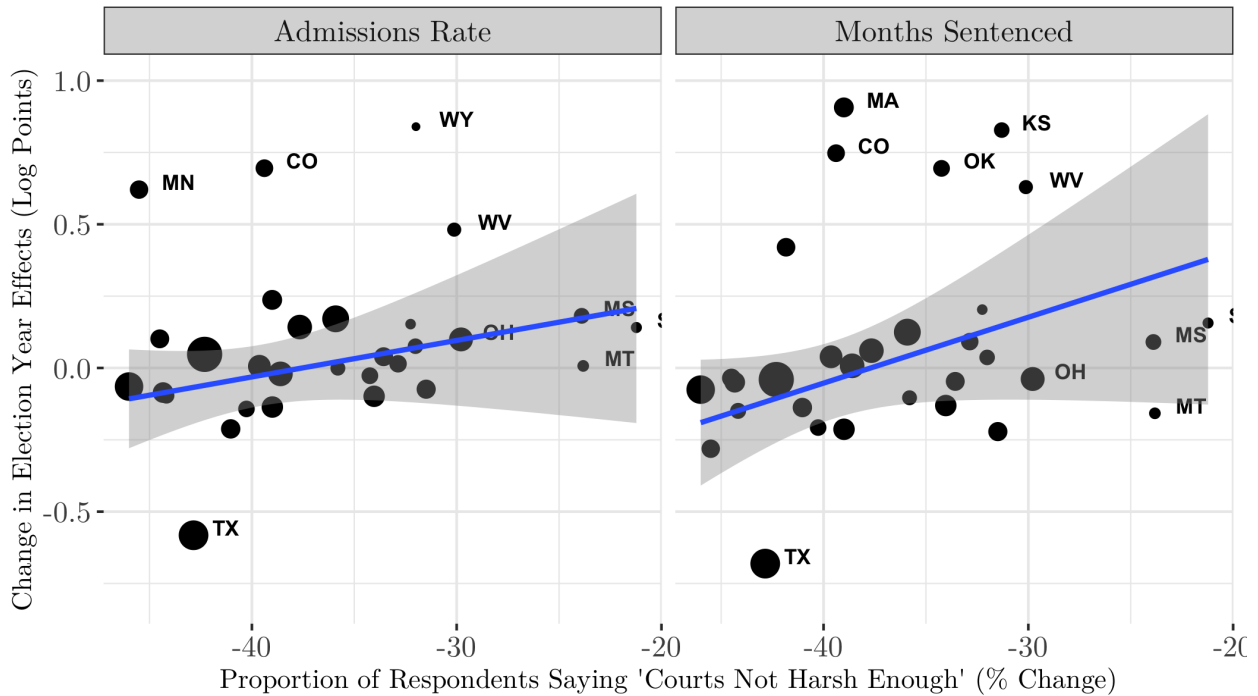
30. Basic regression analysis, in the absence of experimental or quasi-experimental designs, may yield correlations at best instead of causal estimates.

31. Given the dichotomous construction of the IAT score, pro-White sentiment can also be interpreted as sentiment against black people. If the relationship between bias and sentencing is causal, one interpretation consistent with the Table 2 findings is that only bias *against* a racial group influences sentencing outcomes—not bias in favor of a racial group. As such, since so few counties exhibit pro-black bias (i.e., bias against white people), this could explain why no statistically significant effects are found for white prisoners.

6.4 DA Election Effects Declined in Era of Rising Incarceration, Coinciding with Softening Public Opinion on Punishment

Another key question—which our data allows us to consider—is how election year effects have evolved over time. This question is particularly relevant in light of several recent developments: the emergence of the term “mass incarceration” to characterize the prison trends since the 1980s; the emergence of “progressive prosecutors” who explicitly run on a platform counter to being “tough-on-crime,” and the recent bipartisan efforts toward criminal justice reform. These developments, among others, suggest that the political incentives that many prosecutors face may be different than those in the 1980s and 1990s.

Figure 12: Change in Election Year Effects, by State (Pre-2000 Period to Post-2000 Period)



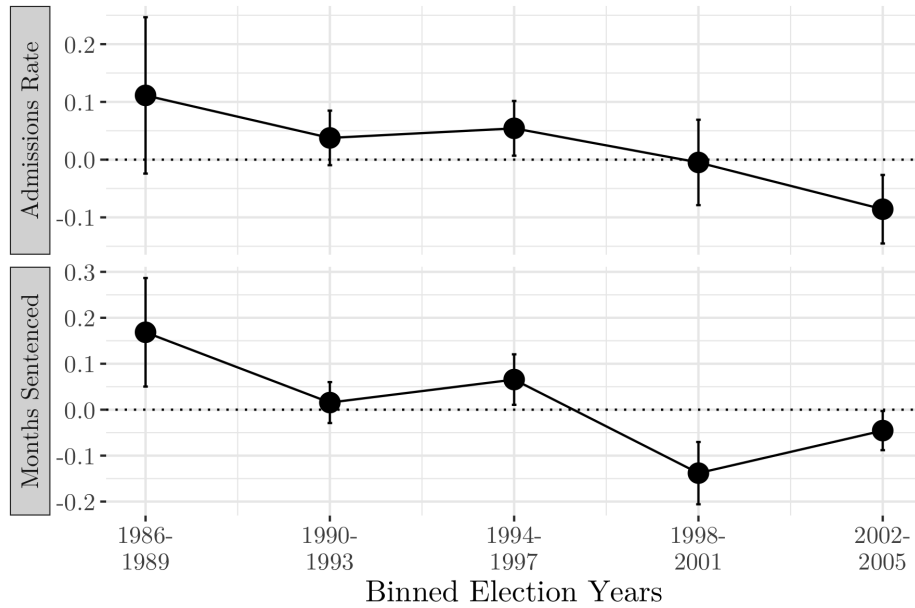
Notes: Change in election year effects calculated using a static difference-in-differences model (population weighted to adjust for differences in sampling probabilities across districts and across time). Estimates include year- fixed effects. Size of dots corresponds with mean state population between 1983 and 2016. Proportion of respondents saying “Courts Not Harsh Enough” taken from the General Social Survey (GSS) question: “Do you think courts in this area deal too harshly or not harshly enough with criminals?” (Smith, Davern, Freese, et al. 2018). The GSS is a nationally-representative survey (and not so at a sub-national or state-level of analysis).

In Figure 12, I compare the relationship between changes in public opinion and changes in election year effects—comparing the 16 years before 2000 with the 16 years after 2000. The y-axis is the change in election year effects between these two periods. The x-axis is a proxy for the change in public opinion—namely, the percent change in respondents from the General Social Survey (a longitudinal survey administered yearly since the 1970s) who say courts are not harsh enough. So in short, the further left you are on the x-axis, the more lenient the corresponding public opinion has become over time. Each dot represents a state, with the size corresponding to population. Of note, across all states, the public has increasingly viewed courts as too harsh. The relationship between the level of change in public opinion and the level of change in election year effects is weakly positive for the admissions rate. The relationship is more positive for months sentenced. These findings provide suggestive evidence that the election year effects are related to public opinion toward criminal punishment, with states who have had the greatest decline in respondents viewing the courts as “not harsh enough” also having the greatest decline in election year effects.

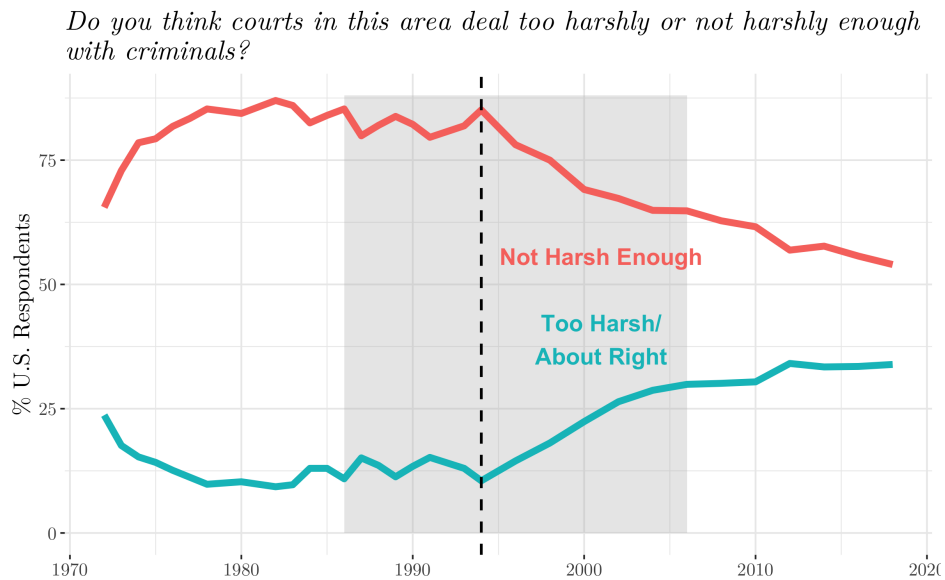
The change in election year effects over time in the top plot of Figure 13 is also consistent with the view that an evolution in public sentiment over time impacted DA behavior. In the 1980s and 1990s, anecdotally there was a lot more discussion about—and perhaps perceived benefit from—being a politician who was viewed as being “tough-on-crime.” This was the period of the spike in crack-related drug convictions, the passing of the 1994 crime bill, and the establishment of minimum sentencing laws around the country. The GSS shows that in fact the vast majority of the population believed courts were not harsh enough. The bottom plot of Figure 13 shows that only after 1994 did this fraction of the population begin to decline. In contrast, those who believed courts were too harsh or about right have become a growing share of the population since 1994. In short, public sentiment toward punishment has been shifting over the last 25 years or so. This shift coincides with lower election year effects over time, and plausibly impacted the political incentives faced by district attorneys.

There is plausibly a nexus between (1) changes in public opinion over time, and (2)

Figure 13: Election Year Effects Over Time vs. Public Opinion Over Time



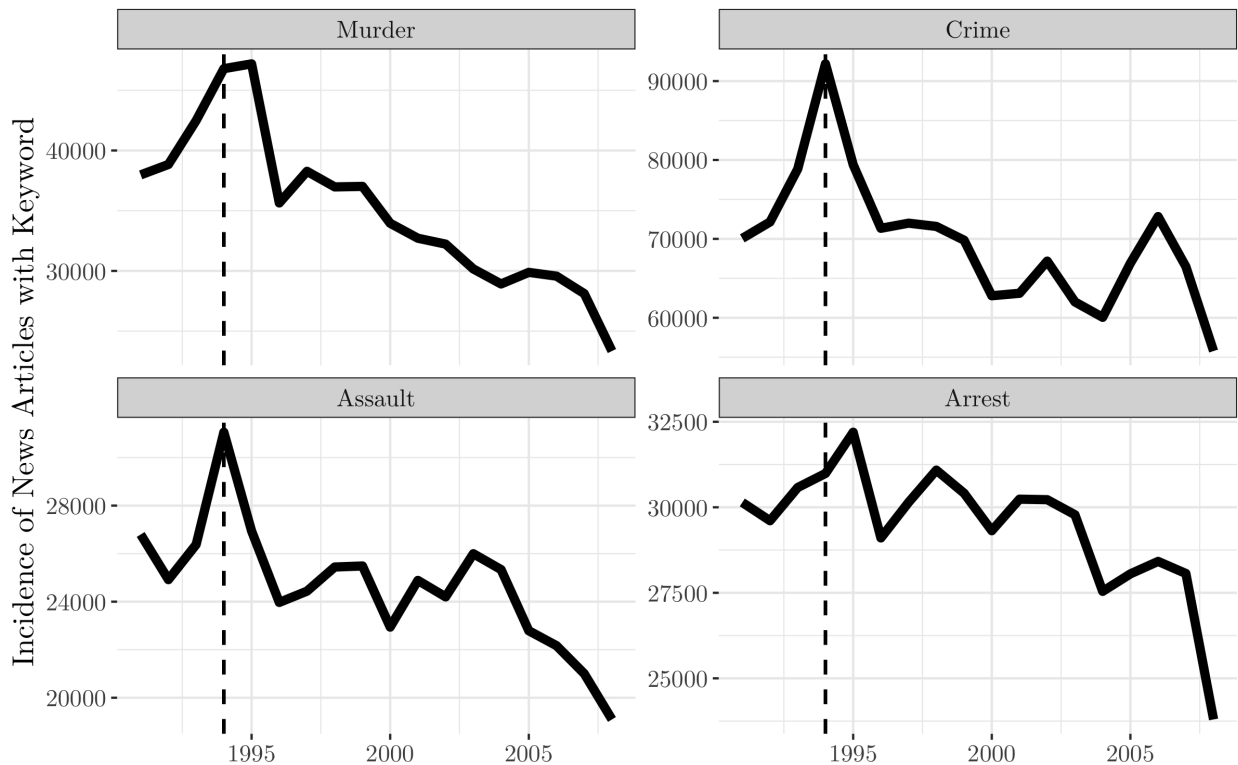
Notes: Graph depicts a static difference-in-differences model (population weighted to adjust for differences in sampling probabilities across districts and across time). Estimates are at the county level by year. The standard errors are corrected for correlation across district attorneys and over time in a given county by clustering at the district level. Estimates include state- and year- fixed effects. Horizontal lines mark two standard errors.



Notes: Source is the GSS (Smith, Davern, Freese, et al. 2018). “Don’t Know” responses excluded. Vertical dashed line corresponds with the year 1994. Shaded region corresponds with the approximate coverage of core empirical analysis illustrated in Figure 1 and explained in Section 3 (an era of rising incarceration).

changes in the salience of criminal activity over time. In other words, the public may adopt a more lenient stance toward criminal punishment in periods in which crime is less prominent. The guiding hypothesis is clear: the *salience* of crime is plausibly more impactful on public sentiment—and in turn the political incentives district attorneys face—than the actual crime rate, since salience more proximately influences voter perceptions than the crime levels themselves. Under this hypothesis—and based on the public sentiment trends described in Figure 13—one would expect that the softening of public opinion toward punishment would coincide with (and potentially be caused by) a declining salience of criminal activity in the public eye. The GSS figure at the bottom of Figure 13 illustrates a clear shift in the public

Figure 14: Salience of Leading Local Newspaper Articles with Crime-Related Language, 1991–2008



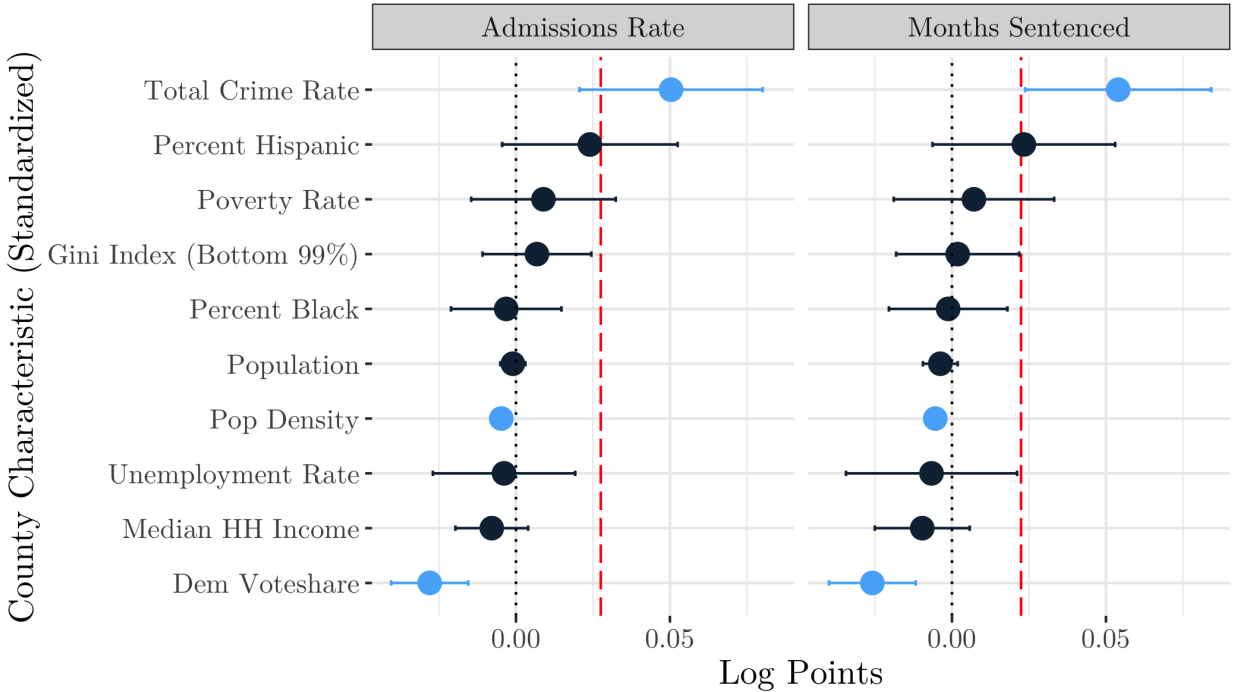
Notes: Article counts taken from one of the top three newspapers in each respective state. Searches performed via Factiva, Nexisuni, Hollis, or WestlawNext. Given data availability, analysis begins from 1991 onward and includes the following states: Alaska, Arizona, California, Colorado, Connecticut, District of Columbia, Florida, Georgia, Kansas, Kentucky, Louisiana, Maryland, Massachusetts, Minnesota, Missouri, Nevada, New Hampshire, New York, North Carolina, Ohio, Oklahoma, Oregon, South Carolina, Texas, Utah, Virginia, Washington, and Wisconsin. Newspapers used in analysis can be found in Appendix C.11.

opinion trend line after the year 1994. Now compare that shift in trends with Figure 14. Figure 14 shows the incidence of top local newspaper articles that include various crime-related keywords; in short, it can be viewed as proxy for the salience of crime in the public eye. Strikingly, again we see a clear break in the trend line around the year 1994, after which the incidence of articles mentioning various crime-related language begins to decline. This general decline occurs for articles that include the word “murder,” “crime,” “assault,” and “arrest.” Yet, the downward trend is most apparent for articles that contain the word “murder.” Murders may possess the greatest nexus with public sentiments toward criminal punishment (given the extreme severity and grotesqueness of the offense). Hence, the sharp decline in local newspaper coverage of murder (and other crime-related terms) after 1994 is consistent with the perspective that the softening of public sentiment toward punishment is in response to the declining salience of criminal activity in the public eye.

To deepen our understanding of potential mechanisms explaining this relationship between public sentiment and election year effects, I calculate the association between various standardized county characteristics and election year effects. Figure 15 shows the result of this analysis, based on the static difference-in-differences model (Equation 4). The red vertical line represents the main effect—the election year effect in the absence of interactions with county characteristics. The figure illustrates that a one standard deviation increase in the total crime rate is associated with a five log points increase in election year effects for both outcome variables. The magnitude and significance of total crime rate (compared with other county characteristics) is consistent with the perspective that election year effects are responsive to public opinion. Greater crime rates may impact public opinion, perhaps by galvanizing the public against those convicted of crimes, which in turn might increase the political pressure prosecutors might face in being harsher on crime.

Figure 15 also shows that democratic voteshare, which measures the fraction of votes going to the Democratic Presidential candidate in the prior election, is the second most influential county characteristic in the plot. It is negatively associated with the election year

Figure 15: Association of County Characteristics with Election Year Effects



Notes: Source of county characteristics data is Opportunity Insights (www.opportunityinsights.org). Estimates reflect a static difference-in-differences model (population weighted to adjust for differences in sampling probabilities across districts and across time). Estimates are at the county level by year. The standard errors are corrected for correlation across district attorneys and over time in a given county by clustering at the district level. Estimates include state- and year- fixed effects. County characteristics are standardized, so estimates reflect change in election year effect from a one standard deviation change in corresponding characteristic. Horizontal lines mark two standard errors. Vertical red dashed line represents the main election year effect. Dem Voteshare is vote share in previous Presidential election going to Democratic candidate. All county characteristics calculated from the mean value between 1986–2006.

effects and it is statistically significant for both outcomes. Again, we see that the political ideology of the district is associated with election year effects, which is consistent with the notion that it is public opinion, or the public’s general sentiment toward crime, that is creating political incentives for district attorneys.

Proposition 2 from the theoretical model in the Appendix illustrates why DA election year effects might have declined in response to softening public sentiment:

6.5 DA Election Effects Disappeared at the National Level After the Era of Rising Incarceration Ended

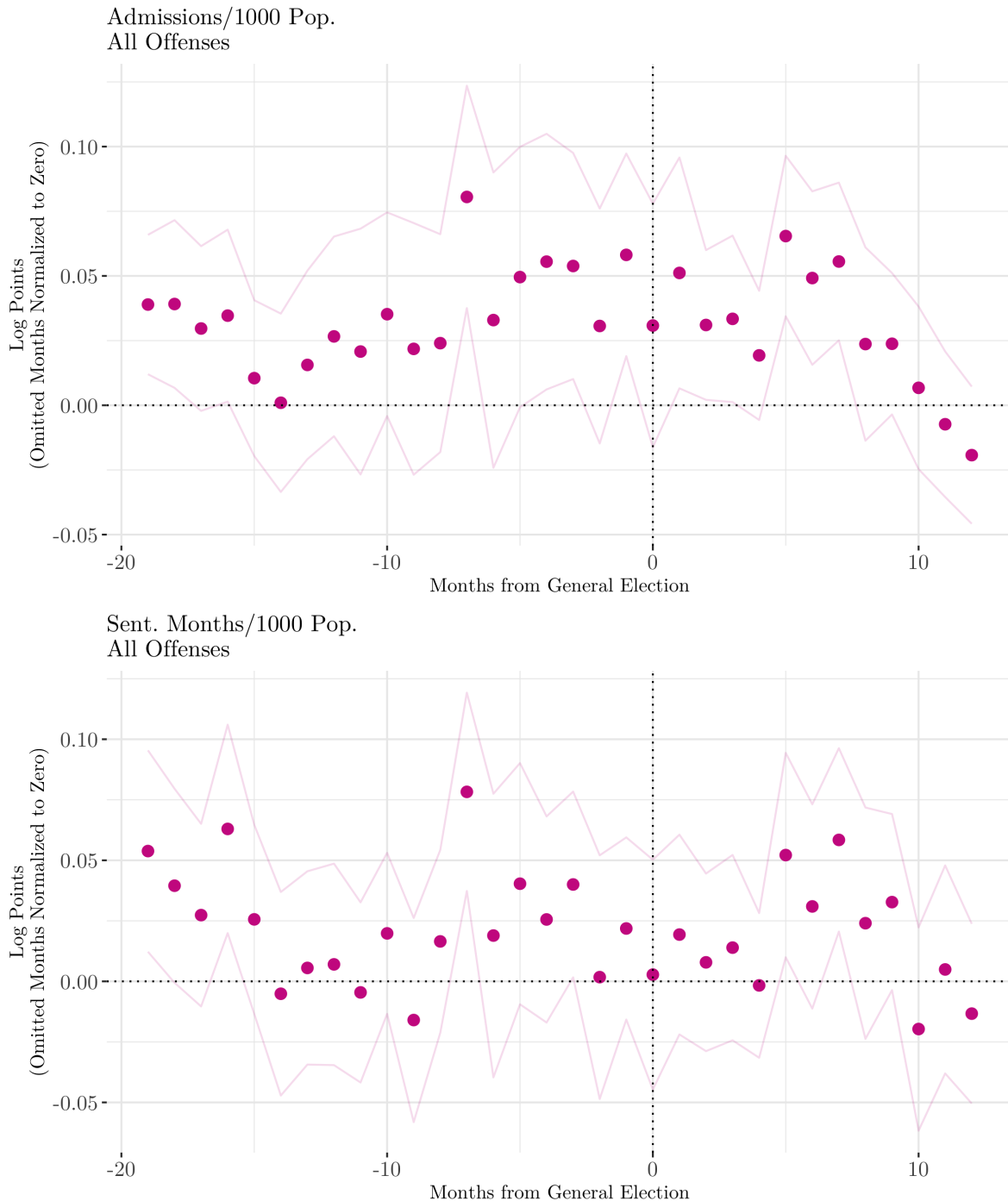
Given the data collected on DAs and the timing of their elections during the era of rising incarceration (roughly 1986–2006), one can project the election cycles for the time period afterward. Based on the assumption term lengths and timing of elections are consistent between both time periods, one can perform an analysis similar to what was performed in Section 5.2 to determine whether the election effects observed before persisted in the more recent time period.

Figure 16 illustrates the monthly estimates for both the admissions rate and the total months sentenced during the one year period before an election year, during the election year itself, and during the one year period after the election year. Unlike the corresponding plots in Section 5.2 for the era of rising incarceration, there is no observable increase in the total months sentenced as DA elections approach. And although there is still a slight increase in the admissions rate leading up to DA elections, the relationship appears much weaker. This positive relationship between the admissions rate and being in a DA election year may in fact be spurious, as the increase in admissions extends for several months after the election has passed.

7 Conclusion

For this paper, I compiled a new comprehensive dataset on the election cycles and offices sought for all district attorneys in office during the steepest rise in incarceration in U.S. history (roughly 1986–2006). Using quasi-experimental methods, I found that being in a DA election year increased total admissions per capita to state prisons and total months sentenced per capita. I found evidence that sentencing outcomes are associated with public sentiment: (1) election effects are higher in Republican counties; (2) election effects depend on county political ideology more than DA ideology, with effects larger in contested elections;

Figure 16: Monthly Criminal Sentencing Outcomes in Election Cycle (Roughly 2006–2016)



Notes: Graph depicts dynamic difference-in-differences model estimates using Equation 1 for the time period after the core empirical analysis of the paper (population weighted to adjust for differences in sampling probabilities across districts and across time). Estimates are at the county level by month. Estimates include state- and year-month- fixed effects. Lines demarcate 95% confidence intervals. Estimates calculated relative to omitted months in election cycle, which are all normalized to 0. Corresponding regression table is in Appendix Table C.8.

(3) anti-Black/pro-White county-level racial bias is associated with greater punitiveness on Black prisoners throughout the entire election cycle; (4) election effects declined in the era of rising incarceration, closely coinciding with softening public opinion on punishment as measured through the General Social Survey; and (5) election effects disappeared at the national level after the era of rising incarceration ended.

If sentencing outcomes were solely determined by the DA’s personal philosophy toward punishment, one might not expect there to be much cyclicity in the severity of outcomes. One would simply expect the districts with DAs having harsher theories of punishment to have *consistently* more severe sentences. Instead, we see greater election year effects in Republican counties, which is consistent with the perspective that DA behavior incorporates political incentives, such as voters’ perceived preferences for harshness. Future analysis might explore “progressive prosecutors”—based on the finding that political incentives for sentencing intensity differ markedly between liberal and conservative districts, this may translate to different behaviors in the run-up to elections for various DA offices. Other legal scholars have already touted the potential for progressive prosecutors to help reform the system (Hessick and Morse 2020). One would expect not only that areas with progressive prosecutors would have lower overall sentences, but based on this study one would also expect there to be no election year effects—or perhaps election year effects that are less pronounced—for districts where progressive prosecutors are politically viable.

There are at least two obvious interpretations of the election year effects this study finds: (1) that the election year level of sentencing is too high, based on prevailing theories of justice; (2) that the election year level of sentencing is adequate, and instead the level for *non*-election years is too low. Given the fact that the United States has the highest incarceration rate in the world, many might find the second interpretation less plausible than the first one. Under the first interpretation, politics may have encouraged overly severe sentencing outcomes. In short, under this first interpretation, the argument for reducing political incentives of district attorneys becomes more salient—particularly when the public’s desire for harshness diverges

from what may be considered “fair” or socially optimal.

Accordingly, the findings relate to broader legal debates on the efficacy of “democratizing” the criminal justice system (see, e.g., Kleinfeld 2016). On one side of the argument are those who attribute many problems with the criminal justice system to an ill-informed (or particularly vengeful) American public; this side tends to call for the justice system to be more greatly influenced by professionals and experts. On the other side of the argument are those who attribute the problems with the justice system to bureaucratic attitudes and other institutional/structural problems. This side tends to call for more control of the system from local communities, with greater responsiveness to the lay public. For those skeptical of democratizing the criminal justice system, a potential corrective response to the election year effects could be to transition the DA selection process from an electoral system to appointment. Several states already operate under this model, such as Delaware, New Jersey, Alaska, and Connecticut.³²

Some legal scholars, however, question the political feasibility of accomplishing this shift (see, e.g., Kress 1976; Wright 2014). For those who support increased democratization of the criminal justice system, appropriate corrective responses to the election year effects found in this paper are less clear. As scholars like Rappaport (2020) argue, even if criminal justice actors were made more accountable to local communities, it remains unclear whether the lay public in the local communities most afflicted by crime would necessarily encourage more leniency. There could be unintended consequences, with more democratization potentially yielding political incentives to *increase* severity of sentences. Similarly, proposals to increase the competitiveness of DA elections (see, e.g., Hessick and Morse 2020) may exacerbate the impact of election cycles on sentencing outcomes. Ultimately, the myriad consequences of further democratization in the DA selection process may remain an empirical question—one that I leave for future research.

Of note, this paper only evaluates the short-term cyclical impact of being in an election

32. One future analysis could be to test whether there remains a cyclical relationship in severity of sentencing outcomes in these locations.

year on sentencing outcomes. It does not capture potential longer-term political considerations that might impact how district attorneys approach their work at *all* periods in the election cycle. This means the full impact of political incentives on the criminal justice system and the unprecedented rise in incarceration may be larger than what is found in this paper alone: not only (1) might DAs be responding to political incentives in ways above and beyond what can be captured through this paper’s empirical strategy alone, but also (2) the findings may signal that *other* elected officials, particularly legislators, were also responding to public sentiment in making policy choices during the era of rising incarceration. So much work on criminal justice reform to date has focused on policy improvements to the system itself and to the laws that govern it. If the political incentives of those entrusted to administer justice are shaped by public sentiment regarding criminal punishment, then this paper highlights that focusing on “hearts and minds”—on shifting public opinion toward punishment independent of shifts to laws or to the structure of the criminal justice system itself—may prove not only beneficial but also instrumental in improving sentencing outcomes and stemming mass incarceration.

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Appendix A – Theoretical Model

Several models have suggested that more frequent elections of politicians induce less shirking of responsibilities (see the classic model in Barro (1973); also see Ferejohn (1986) and Schultz (2008)). Below, I postulate a very simple model adapted from Dal Bó and Rossi (2011) that is designed to explore the impact of district attorney election cycles on criminal sentencing outcomes.

I consider a two-period model ($t = 1$ and $t = 2$) with a class of district attorneys who faces reelection after the second period. The value of reelection is denoted by V . Since reelection is desired, $V > 0$. The exposition below focuses on the effort choices and sentencing intensity of the district attorney at times $t = 1$ (the period before the election year) and $t = 2$ (the election year).

District attorneys choose prosecuting effort for the corresponding time period—denoted by e_1 and e_2 —at the start of the period, facing a quadratic cost for the effort exerted in each period (e_1^2 and e_2^2).³³ The prosecuting effort across both time periods affects sentencing intensity $s(\cdot)$,³⁴ which in turn affects a district attorney’s probability of reelection $P(s(e_1), s(e_2), \pi) = \psi p(s(e_1), s(e_2), \pi) \in [0, 1]$. This probability depends on a scalar parameter $\psi \geq 0$ and an electoral safety shifter π .³⁵ I will frequently suppress dependence on π and represent $s(e_1)$ as s_1 for brevity. The function $p(s_1, s_2)$ satisfies $p_{s_1}(\cdot, \cdot) > 0$, $p_{s_2}(\cdot, \cdot) > 0$, $p_{s_1 s_1}(\cdot, \cdot) \leq 0$, $p_{s_2 s_2}(\cdot, \cdot) \leq 0$, $p_{s_1 s_2}(\cdot, \cdot) \geq 0$, $p_\pi(\cdot, \cdot) \geq 0$, $s'(\cdot) > 0$, $s'_1(\cdot) = s'_2(\cdot)$, and $p_{s_2}(\cdot, \cdot) > p_{s_1}(\cdot, \cdot)$. In short, I assume positive and weakly decreasing marginal returns from sentencing intensity, weak complementarity between sentencing intensity in period $t = 1$ and $t = 2$, positive and equal marginal returns to sentencing intensity from effort in both periods³⁶, and larger marginal returns to reelection probability from sentencing intensity s_2 compared to s_1 .

The assumption that $p_{s_2}(\cdot, \cdot) > p_{s_1}(\cdot, \cdot)$ is consistent with conclusions drawn from political science, in which voter decision-making weighs performance in election years more than performance in non-election years (see, e.g., Healy and Lenz 2014). Different explanations for this phenomenon abound, including: that voters may simply fail to remember earlier conditions (Sarafidis 2007); that voters perceive the election year as particularly informative (MacKuen, Erikson, and Stimson 1992); and that voters avoid the cognitively laborious effort of discerning total performance by simply substituting end conditions for overall conditions (Healy and Lenz 2014). These findings taken together illustrate why there are larger marginal returns to reelection probability from the election period sentencing intensity s_2 compared to the non-election period sentencing intensity s_1 (i.e., $p_{s_2}(\cdot, \cdot) > p_{s_1}(\cdot, \cdot)$).

Each unit of prosecuting effort yields a unitary return in the period the effort is ex-

33. Prosecuting effort represents tangible personnel resources—or simply the institutional focus of the office led by the DA—devoted to prosecuting criminal suspects.

34. Sentencing intensity can refer to either or both the extensive margin (the volume of admissions to prison) or the intensive margin (the total months sentenced to prison).

35. The electoral safety shifter π captures the fact that safer district attorneys with respect to electoral competition should be less sensitive to sentencing intensity $s(e_1)$ and $s(e_2)$ than those at risk.

36. The assumption of positive and equal marginal returns to sentencing intensity from effort is especially plausible during the time period covered by this paper’s core empirical analysis, the “tough-on-crime” era of the late 20th century.

pended.³⁷ Assuming district attorneys earn wages w per period, the respective objective functions for time period $t = 1$ and $t = 2$ are:

$$\begin{aligned} DA_{t=1} &: \max_{e_1} \{-e_1^2 + w + e_1 + \delta^2 P(s(e_1), s(e_2))V\} \\ DA_{t=2} &: \max_{e_2} \{-e_2^2 + w + e_2 + \delta P(s(e_1), s(e_2))V\} \end{aligned} \quad (5)$$

I illustrate the main solution as follows:

Proposition 1 *Both in the environment where the more distant future is discounted more heavily ($\delta \in (0, 1)$) and in the environment where the more distant future is not discounted at all ($\delta = 1$), district attorneys exert more sentencing intensity in the election year than in the period preceding it ($s(e_2) > s(e_1)$).*

Proof of Proposition 1. The first-order conditions for the problems in Equation 5 are:

$$\begin{aligned} e_1 &: -2e_1 + 1 + \delta^2 \cdot P_{s(e_1)}(s(e_1), s(e_2)) \cdot s'(e_1) \cdot V = 0 \\ e_2 &: -2e_2 + 1 + \delta \cdot P_{s(e_2)}(s(e_1), s(e_2)) \cdot s'(e_2) \cdot V = 0 \end{aligned}$$

The assumptions made on prosecuting effort, sentencing intensity, and reelection probability functions guarantee that (a) $e_1, e_2 > 0$; (b) $P_{s(e_2)}(\cdot), P_{s(e_1)}(\cdot) > 0$; (c) $P_{s(e_2)}(\cdot) > P_{s(e_1)}(\cdot)$; and (d) $s'(e_1) = s'(e_2)$ in all scenarios. Combining first-order conditions yields the expression: $e_1 = e_2 - \frac{\delta}{2}(P_{s(e_2)}(\cdot)s'(e_2) - \delta P_{s(e_1)}(\cdot)s'(e_1))V$. Since $\delta \in (0, 1]$, the expression implies $e_2 > e_1$. Since $s'(\cdot) > 0$ by assumption, this implies $s(e_2) > s(e_1)$. ■

This proposition tells us that the comparison of sentencing intensity between election and non-election periods is unambiguous: district attorneys should exert more prosecuting effort in the election year than in the period preceding it, driving up sentencing intensity. The discounting of more distant rewards discourages effort by district attorneys who are further away from reelection. Yet even in a scenario in which there is no discounting of more distant rewards, there is still comparatively larger marginal returns to reelection probability from effort expended in the election year. This, in turn, represents a second—and likely the key—mechanism that encourages district attorneys to increase sentencing intensity during the election period than in the period preceding it.

Proposition 2 *As the difference decreases between the marginal returns to reelection probability from sentencing intensity ($\frac{\partial P(s(e_1), s(e_2))}{\partial s(e_2)} - \frac{\partial P(s(e_1), s(e_2))}{\partial s(e_1)}$), the difference also decreases between the sentencing intensity between periods ($s(e_2) - s(e_1)$).*

Proof of Proposition 2. The first-order conditions for the problems in Equation 5 are:

$$\begin{aligned} e_1 &: -2e_1 + 1 + \delta^2 \cdot P_{s(e_1)}(s(e_1), s(e_2)) \cdot s'(e_1) \cdot V = 0 \\ e_2 &: -2e_2 + 1 + \delta \cdot P_{s(e_2)}(s(e_1), s(e_2)) \cdot s'(e_2) \cdot V = 0 \end{aligned}$$

37. The unitary return may take the form of recognition, personal fulfillment, or legacy, and represents the present discounted value of the future stream of such returns.

Let $k = P_{s(e_2)}(\cdot) - P_{s(e_1)}(\cdot)$. Substituting for $P_{s(e_1)}(s(e_1), s(e_2))$, combining first-order conditions, and simplifying yields:

$$k = P_{s(e_2)}(\cdot) - \frac{P_{s(e_2)}(\cdot)}{\delta} + \frac{2}{\delta^2 s'(e_1) V} (e_2 - e_1)$$

The assumptions guarantee that $\delta > 0$, $s'(\cdot) > 0$, and $V > 0$. Hence, on inspection one can see that k is positively correlated with the difference $e_2 - e_1$. Since $s'(\cdot) > 0$, in equilibrium k is also positively correlated with the difference $s(e_2) - s(e_1)$. ■

Recall that Proposition 1 holds that more prosecuting effort will be expended in the election year than in the period preceding it ($e_2 > e_1$), yielding greater sentencing intensity during election periods ($s(e_2) > s(e_1)$). This is because: (1) sentencing intensity is positively correlated with the probability of reelection ($\frac{\partial P(s(e_1), s(e_2))}{\partial s(e_1)}, \frac{\partial P(s(e_1), s(e_2))}{\partial s(e_2)} > 0$), combined with (2) the election period sentencing intensity $s(e_2)$ has comparatively larger marginal returns to reelection probability ($\frac{\partial P(s(e_1), s(e_2))}{\partial s(e_2)} > \frac{\partial P(s(e_1), s(e_2))}{\partial s(e_1)}$). Historical trends on public sentiment toward crime help elucidate how the relationship between sentencing intensity and reelection prospects may evolve over time. The political science literature generally views elections as a means of accountability for public officials. Hence, as was consistent in the 1980s and 1990s, the public perception that courts were not harsh enough suggests reelection probability would clearly be positively correlated with sentencing intensity. Since public sentiment toward crime softened from 1994 onward, the correlation between sentencing intensity and reelection probability plausibly may have softened as well. Formally, this means the difference between $\frac{\partial P(s(e_1), s(e_2))}{\partial s(e_2)}$ and $\frac{\partial P(s(e_1), s(e_2))}{\partial s(e_1)}$ would decrease, as sentencing intensity in both time periods would increasingly be providing less information to voters on district attorney job performance; in short, severity is becoming an increasingly less-useful performance metric since voters increasingly view courts as already too harsh. As a result, Proposition 2 predicts the election year effects on sentencing intensity would flatten out (i.e., the difference $s(e_2)$ minus $s(e_1)$ would narrow). This prediction is borne out through the empirical analysis provided in the main body of the paper.

Appendix B

B.0 Alternative Weighting for Two-Way Fixed Effects (TWFE) Estimator

Recent research has highlighted that in settings with variation in treatment timing across units—such as in election cycles—the estimated coefficients in two-way fixed effects regressions can be contaminated by effects from other periods (De Chaisemartin and d’Haultfoeuille 2020; Sun and Abraham 2021). Given this potential problem in the estimation of regression coefficients in the two-way fixed effects model, here I construct an alternative estimator introduced by Sun and Abraham (2021). The authors explain that this alternative estimation approach is robust to treatment effect heterogeneity.

Sun and Abraham (2021) is designed for settings with staggered adoption, where treatment turns on and stays on, which on the surface is different than my setting in which the election period treatment is cyclical. As such, I adapt their approach to my setting such that the goal is to estimate a weighted average of the cohort average treatment effects on the treated ($CATT_{e,k}$); this average represents the average treatment effect k periods from an election year for the cohort of all counties with election years at time e . We want to estimate $CATT_{e,k}$ with more reasonable weights than would occur if the original two-way fixed effects specification had negative weights (i.e., we estimate the effects with weights that are non-negative and sum to one). In particular, I estimate the following weighted average of $CATT_{e,k}$, where the weights are shares of cohorts that experience at least k periods relative to treatment, normalized by the size of g (the collection of all periods included in the regression expression)³⁸:

$$v_g = \frac{1}{|g|} \sum_{k \in g} \sum_e CATT_{e,k} \Pr\{E_c = e\} \quad (6)$$

where E_c is the time of the treatment (i.e., an election year), and e is the time of treatment for observations belonging to the same cohort.³⁹ I estimate the weights $\Pr\{E_c = e\}$ by sample shares of each cohort in the relevant periods $k \in g$.

Per Sun and Abraham (2021), I estimate $CATT_{e,k}$ using a linear two-way fixed effects specification that interacts indicators for the relative year with indicators of the corresponding cohort:

$$Y_{cst} = \gamma_s + \lambda_t + \sum_e \sum_{k \neq 0} \delta_{e,k} (\mathbb{1}\{E_c = e\} \cdot D_{c,t}^k) + \varepsilon_{cst} \quad (7)$$

The coefficient estimator $\hat{\delta}_{e,k}$ from Equation 7 is a difference-in-difference estimator for

38. Since we are estimating all coefficients in relation to the election year, g is the collection of all periods except when $k = 0$.

39. Given the cyclical nature of elections and the truncation of the observations to counties with 4-year terms for district attorneys, the four cohorts can be represented as those counties who had a previous election year in 1986, 1987, 1988, and 1989, respectively.

$CATT_{e,k}$. $D_{c,t}^k$ are indicator variables corresponding to being k years from an election year in county c at year t . Similar to Equation 1, I exclude interactions with $D_{c,t}^0$, so that the estimated coefficients are relative to the sentencing outcomes in the election year.⁴⁰ Plugging $\hat{\delta}_{e,k}$ from Equation 7 in place of $CATT_{e,k}$ from Equation 6 allows us to estimate v_g for various sentencing outcomes. In normalizing the election year estimate to zero, negative values for v_g correspond with a positive relative effect from being in an election year compared to non-election periods.

Table B.1: Coefficients for Alternative TWFE Weighting

	$CATT (v_g)$ (log points)	Bootstrap Standard Error
Admissions / 1000 Pop.		
All Offenses	-0.045	0.0117
Violent Offenses	-0.032	0.0107
Property Offenses	-0.020	0.0119
Drug Offenses	-0.076	0.0146
Sentenced Months / 1000 Pop.		
All Offenses	-0.037	0.0116
Violent Offenses	-0.012	0.0105
Property Offenses	-0.015	0.0109
Drug Offenses	-0.072	0.0162

Table B.1 illustrates that the re-weighted coefficients for the ATE (v_g) remain negative for both admissions per capita and sentenced months per capita, with standard errors estimated via a bootstrap sampling procedure. Furthermore, these coefficients are negative for each crime subcategory (i.e., violent, property, and drug), and consistently statistically significant for the All Offenses and Drug Offenses categories. The sign of these coefficients—and the average magnitude across all offenses—are consistent with the findings from the original specification Equation 1, providing evidence that the original specification does in fact show the correct sign of the underlying average treatment effect.

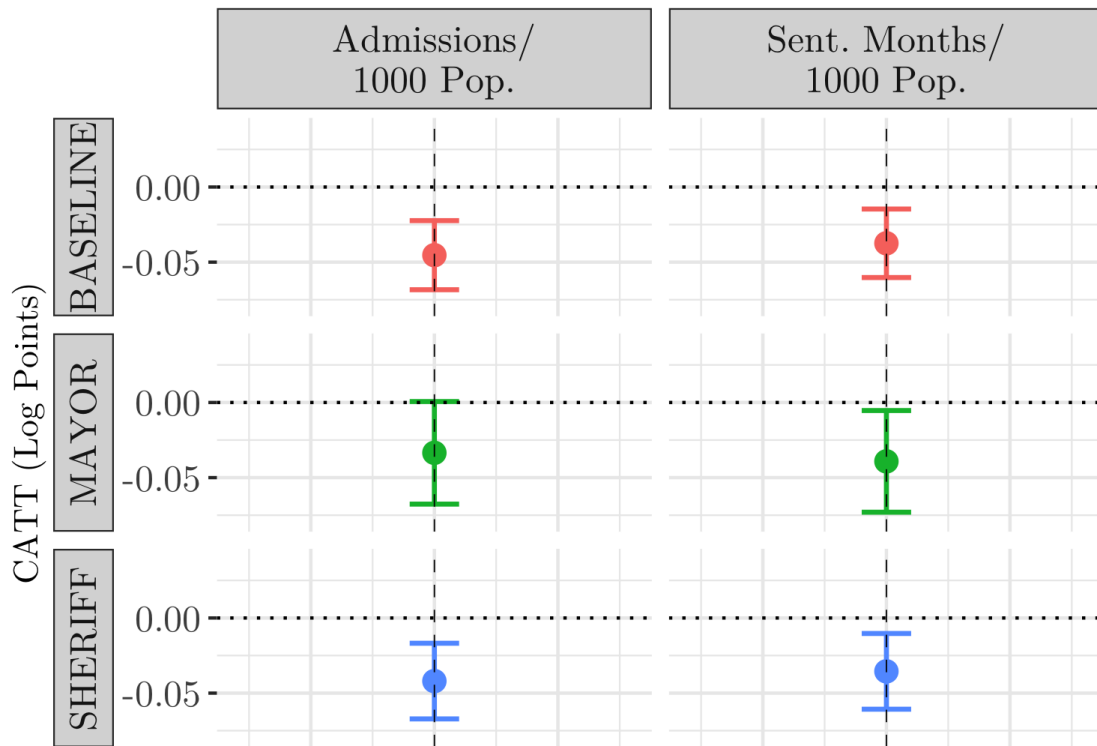
40. Since all cohorts have elections in future time periods beyond what is included in the dataset, I estimate Equation 6 on all counties c . Observations are weighted by county population in estimating $\hat{\delta}_{e,k}$.

Appendix C

Table C.1: Number of States Included in National Corrections Reporting Program, by Year

Year	# States Reporting
1983	26
1984	42
1985	38
1986	50
1987	50
1988	50
1989	50
1990	50
1991	50
1992	46
1993	46
1994	44
1995	45
1996	44
1997	45
1998	45
1999	48
2000	49
2001	48
2002	50
2003	48
2004	48
2005	48
2006	48
2007	46
2008	48
2009	48
2010	47
2011	47
2012	48
2013	46
2014	47
2015	43
2016	43

Figure C.1: Criminal Sentencing Outcomes, Relative to District Attorney Election Year – Non-Synchronicity with Other Elected Office (Alternative TWFE Estimator)



Notes: Mayor and sheriff non-synchronous analysis omits areas where no data found to determine synchronicity with district attorney election cycle. Baseline analysis includes all district attorney election cycles. Estimates tabulated by county and by year, and calculated in accordance with alternative two-way fixed effects estimator (see Sun and Abraham 2021). Observations weighted by county population. Vertical lines mark two standard errors. Estimates calculated relative to the district attorney election year value, which is normalized to 0.

Table C.2: Sentencing Outcomes, by Years from Election Year (All Offenses)

	Dependent variable (in Log Points)	
	Admissions/1000 Pop.	Months/1000 Pop.
	(1)	(2)
-1	-0.034*** (0.009)	-0.018* (0.009)
1	-0.046*** (0.015)	-0.045*** (0.013)
-2	-0.022** (0.010)	-0.016* (0.009)
Joint Test p-value	2.2e-16	2.2e-16
Observations	41,950	39,620

*p<0.1; **p<0.05; ***p<0.01

Notes: Table depicts dynamic difference-in-differences model estimates using Equation 1 (population weighted to adjust for differences in sampling probabilities across districts and across time). Estimates are at the county level by year. The standard errors are corrected for correlation across district attorneys and over time in a given county by clustering at the district level. Estimates include state- and year-month-fixed effects. Estimates calculated relative to election year, which is normalized to 0.

Table C.3: Sentencing Outcomes, by Years from Election Year (Violent Offenses)

	Dependent variable (in Log Points)	
	Admissions/1000 Pop.	Months/1000 Pop.
	(1)	(2)
-1	-0.032*** (0.009)	-0.024** (0.011)
1	-0.040*** (0.014)	-0.051*** (0.015)
-2	-0.019** (0.008)	-0.002 (0.009)
county_population	0.00000** (0.000)	0.00000*** (0.000)
Joint Test p-value	2.2e-16	2.2e-16
Observations	38,314	36,174

*p<0.1; **p<0.05; ***p<0.01

Notes: Table depicts dynamic difference-in-differences model estimates using Equation 1 (population weighted to adjust for differences in sampling probabilities across districts and across time). Estimates are at the county level by year. The standard errors are corrected for correlation across district attorneys and over time in a given county by clustering at the district level. Estimates include state- and year-month-fixed effects. Estimates calculated relative to election year, which is normalized to 0.

Table C.4: Sentencing Outcomes, by Years from Election Year (Property Offenses)

	Dependent variable (in Log Points)	
	Admissions/1000 Pop.	Months/1000 Pop.
	(1)	(2)
-1	-0.036*** (0.011)	-0.033*** (0.011)
1	-0.041*** (0.013)	-0.057*** (0.013)
-2	-0.021 (0.013)	-0.038*** (0.013)
county_population	0.000 (0.000)	0.000 (0.000)
Joint Test p-value	5.057e-06	8.201e-10
Observations	38,734	36,296

*p<0.1; **p<0.05; ***p<0.01

Notes: Table depicts dynamic difference-in-differences model estimates using Equation 1 (population weighted to adjust for differences in sampling probabilities across districts and across time). Estimates are at the county level by year. The standard errors are corrected for correlation across district attorneys and over time in a given county by clustering at the district level. Estimates include state- and year-month-fixed effects. Estimates calculated relative to election year, which is normalized to 0.

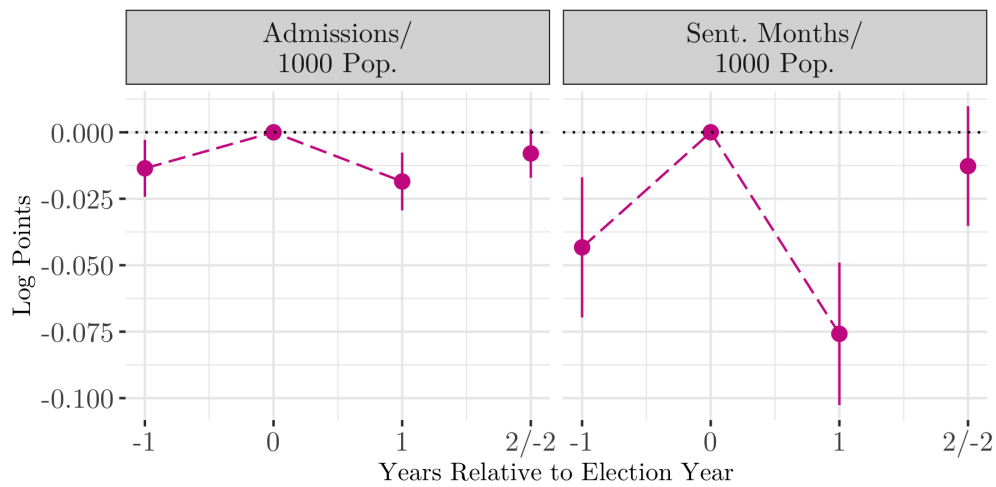
Table C.5: Sentencing Outcomes, by Years from Election Year (Drug Offenses)

	Dependent variable (in Log Points)	
	Admissions/1000 Pop.	Months/1000 Pop.
	(1)	(2)
-1	-0.040*** (0.014)	-0.013 (0.015)
1	-0.035* (0.020)	-0.033* (0.019)
-2	-0.036** (0.016)	-0.041*** (0.014)
county_population	0.00000** (0.00000)	0.00000*** (0.000)
Joint Test p-value	2.2e-16	2.2e-16
Observations	33,816	32,010

*p<0.1; **p<0.05; ***p<0.01

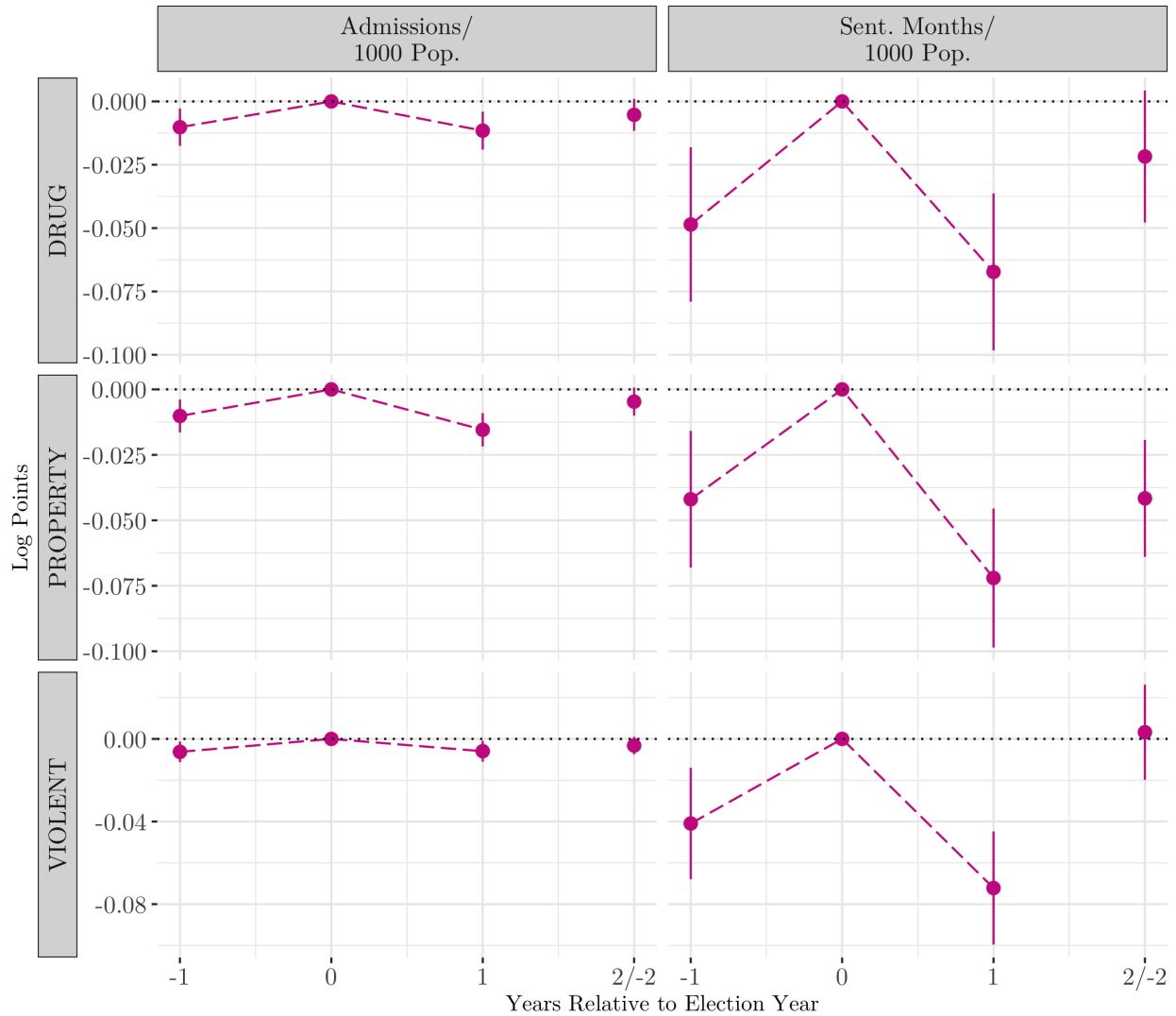
Notes: Table depicts dynamic difference-in-differences model estimates using Equation 1 (population weighted to adjust for differences in sampling probabilities across districts and across time). Estimates are at the county level by year. The standard errors are corrected for correlation across district attorneys and over time in a given county by clustering at the district level. Estimates include state- and year-month-fixed effects. Estimates calculated relative to election year, which is normalized to 0.

Figure C.2: Criminal Sentencing Outcomes,
 Relative to District Attorney Election Year
 – with Log Transformation ($\log(1+x)$)



Notes: Graph depicts dynamic difference-in-differences model estimates using Equation 1 (weighted to adjust for differences in sampling probabilities across districts and across time). Estimates are at the county level by year. The standard errors are corrected for correlation across district attorneys and over time in a given county by clustering at the district level. Estimates include state- and year- fixed effects. Vertical lines mark two standard errors. Estimates calculated relative to the election year value, which is normalized to 0. Log Transformation performed where each county’s outcome variable is incremented by 1 before taking the log.

Figure C.3: Criminal Sentencing Outcomes,
 Relative to District Attorney Election Year – Offense Subcategories
 – with Log Transformation ($\log(1+x)$)



Notes: Graph depicts dynamic difference-in-differences model estimates using Equation 1 (population weighted to adjust for differences in sampling probabilities across districts and across time). Estimates are at the county level by year. The standard errors are corrected for correlation across district attorneys and over time in a given county by clustering at the district level. Estimates include state- and year- fixed effects. Vertical lines mark two standard errors. Estimates calculated relative to the election year value, which is normalized to 0. Log Transformation performed where each county's outcome variable is incremented by 1 before taking the log.

Table C.6: Total Months Sentenced / 1000 Population, by Months from General Election (Property Offenses)

Dependent variable (in Log Points)	
Months/1000 Pop.	
-19	-0.009 (0.024)
-18	-0.011 (0.022)
-17	0.082*** (0.022)
-16	-0.005 (0.026)
-15	-0.017 (0.021)
-14	-0.003 (0.021)
-13	0.005 (0.022)
-12	0.023 (0.021)
-11	0.027 (0.018)
-10	0.017 (0.019)
-9	0.026 (0.019)
-8	0.027 (0.019)
-7	0.076*** (0.020)
-6	0.040* (0.021)
-5	0.055*** (0.021)
-4	0.038* (0.021)
-3	0.061*** (0.021)
-2	0.037** (0.018)
-1	0.045** (0.019)
0	0.014 (0.019)
1	0.011 (0.016)
2	-0.012 (0.018)
3	0.018 (0.018)
4	-0.022 (0.016)
5	-0.041* (0.024)
6	-0.031 (0.021)
7	0.026 (0.023)
8	0.011 (0.025)
9	-0.025 (0.021)
10	-0.010 (0.022)
11	0.008 (0.021)
12	0.001 (0.026)
Observations	256,764

*p<0.1; **p<0.05; ***p<0.01

Notes: Table depicts dynamic difference-in-differences model estimates using Equation 1 (population weighted to adjust for differences in sampling probabilities across districts and across time). Estimates are at the county level by month. The standard errors are corrected for correlation across district attorneys and over time in a given county by clustering at the district level. Estimates include state- and year-month-fixed effects. Estimates calculated relative to omitted months in election cycle, which are normalized to 0.

Table C.7: Sentencing Outcomes, by Months from General Election (All Offenses)

	Dependent variable (in Log Points)	
	Admissions/1000 Pop.	Months/1000 Pop.
	(1)	(2)
-19	0.003 (0.013)	-0.006 (0.017)
-18	-0.034** (0.014)	-0.018 (0.017)
-17	-0.001 (0.018)	0.013 (0.021)
-16	-0.034** (0.016)	-0.008 (0.018)
-15	-0.007 (0.015)	0.003 (0.018)
-14	0.018 (0.014)	0.009 (0.019)
-13	-0.001 (0.012)	-0.001 (0.017)
-12	0.003 (0.014)	0.001 (0.017)
-11	0.012 (0.015)	0.025 (0.018)
-10	0.024* (0.014)	0.010 (0.015)
-9	0.011 (0.011)	0.026* (0.014)
-8	0.029*** (0.010)	0.031** (0.014)
-7	0.032** (0.013)	0.020 (0.015)
-6	0.018 (0.014)	0.017 (0.015)
-5	0.020 (0.015)	0.023 (0.018)
-4	0.010 (0.013)	0.020 (0.017)
-3	0.026** (0.013)	0.022 (0.015)
-2	0.020* (0.012)	0.014 (0.016)
-1	0.013 (0.013)	-0.001 (0.015)
0	0.003 (0.012)	0.001 (0.016)
1	0.021* (0.012)	0.008 (0.014)
2	-0.007 (0.015)	-0.025 (0.016)
3	-0.005 (0.013)	-0.014 (0.015)
4	-0.012 (0.013)	-0.014 (0.017)
5	0.005 (0.014)	-0.016 (0.017)
6	-0.047*** (0.014)	-0.065*** (0.016)
7	-0.009 (0.014)	-0.001 (0.017)
8	-0.041*** (0.016)	-0.047** (0.019)
9	-0.015 (0.015)	-0.018 (0.018)
10	0.014 (0.016)	0.0001 (0.020)
11	0.005 (0.016)	0.004 (0.018)
12	0.007 (0.016)	-0.013 (0.019)
Observations	370,104	346,945

*p<0.1; **p<0.05; ***p<0.01

Notes: Table depicts dynamic difference-in-differences model estimates using Equation 1 (population weighted to adjust for differences in sampling probabilities across districts and across time). Estimates are at the county level by month. The standard errors are corrected for correlation across district attorneys and over time in a given county by clustering at the district level. Estimates include state- and year-month-fixed effects. Estimates calculated relative to omitted months in election cycle, which are normalized to 0.

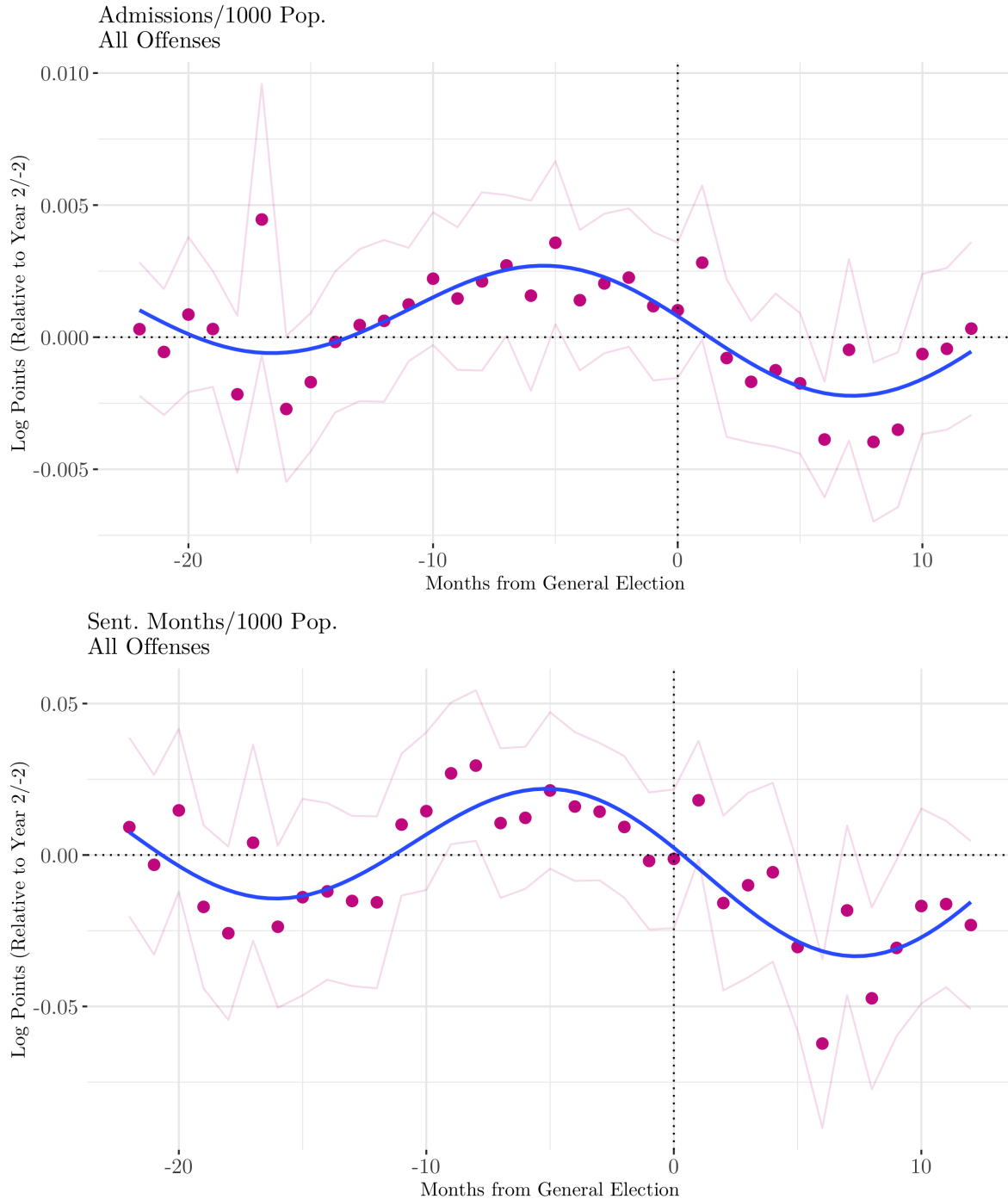
Table C.8: Sentencing Outcomes, by Months from General Election (All Offenses)—Roughly 2006–2016

	Dependent variable (in Log Points)	
	Admissions/1000 Pop.	Months/1000 Pop.
	(1)	(2)
-19	0.039*** (0.014)	0.054** (0.021)
-18	0.039** (0.017)	0.040* (0.020)
-17	0.030* (0.016)	0.027 (0.019)
-16	0.035** (0.017)	0.063*** (0.022)
-15	0.010 (0.015)	0.026 (0.020)
-14	0.001 (0.018)	-0.005 (0.021)
-13	0.016 (0.019)	0.006 (0.020)
-12	0.027 (0.020)	0.007 (0.021)
-11	0.021 (0.024)	-0.005 (0.019)
-10	0.035* (0.020)	0.020 (0.017)
-9	0.022 (0.025)	-0.016 (0.022)
-8	0.024 (0.021)	0.017 (0.019)
-7	0.081*** (0.022)	0.078*** (0.021)
-6	0.033 (0.029)	0.019 (0.030)
-5	0.050* (0.026)	0.040 (0.025)
-4	0.056** (0.025)	0.026 (0.022)
-3	0.054** (0.022)	0.040** (0.020)
-2	0.031 (0.023)	0.002 (0.026)
-1	0.058*** (0.020)	0.022 (0.019)
0	0.031 (0.024)	0.003 (0.024)
1	0.051** (0.023)	0.019 (0.021)
2	0.031** (0.015)	0.008 (0.019)
3	0.033** (0.016)	0.014 (0.020)
4	0.019 (0.013)	-0.002 (0.015)
5	0.065*** (0.016)	0.052** (0.022)
6	0.049*** (0.017)	0.031 (0.022)
7	0.056*** (0.016)	0.058*** (0.019)
8	0.024 (0.019)	0.024 (0.024)
9	0.024* (0.014)	0.033* (0.019)
10	0.007 (0.016)	-0.020 (0.021)
11	-0.007 (0.014)	0.005 (0.022)
12	-0.019 (0.014)	-0.013 (0.019)
Observations	208,933	207,215

Note: *p<0.1; **p<0.05; ***p<0.01

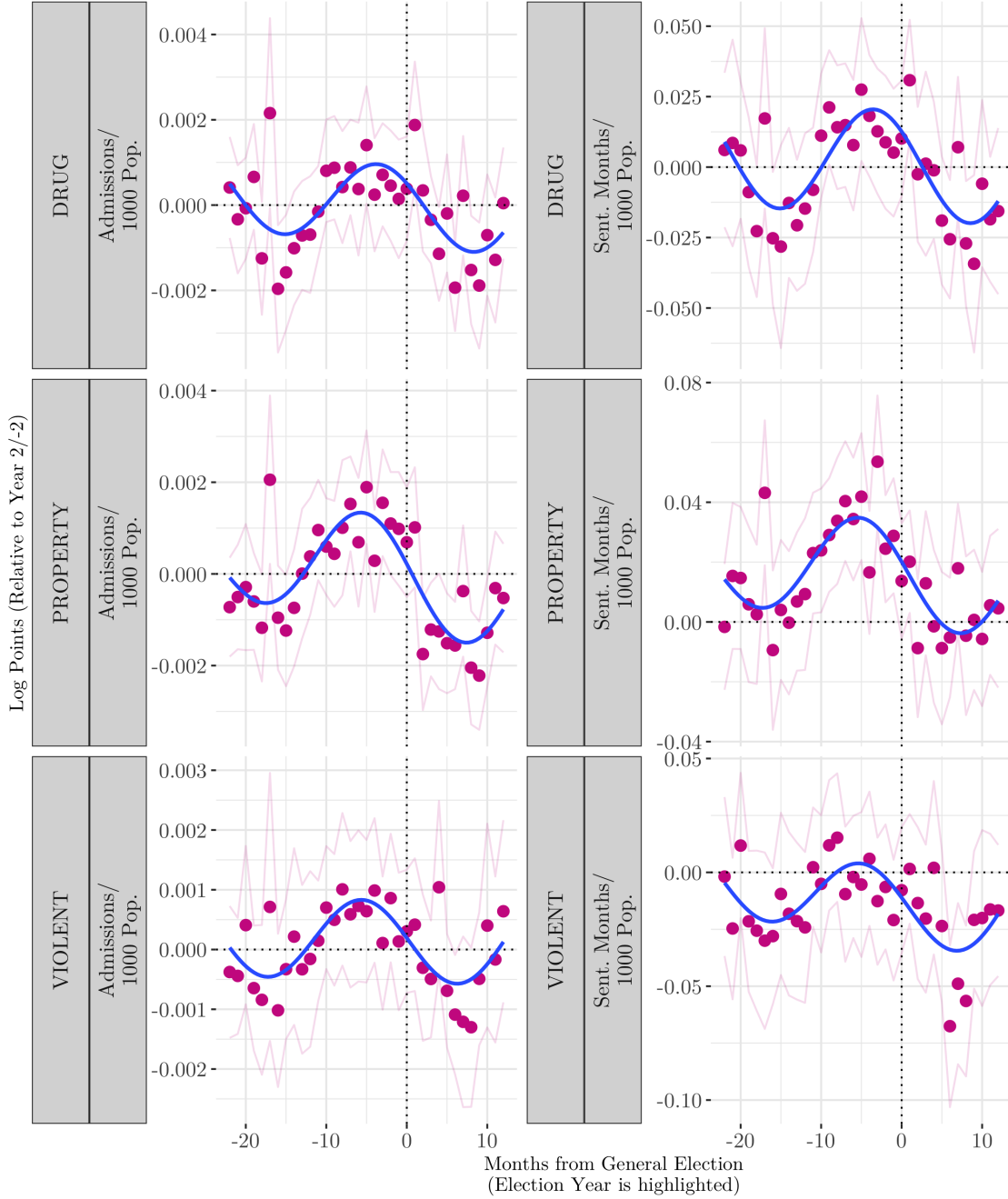
Notes: Table depicts dynamic difference-in-differences model estimates using Equation 1 for the time period after the core empirical analysis of this paper (population weighted to adjust for differences in sampling probabilities across districts and across time). Estimates are at the county level by month. The standard errors are corrected for correlation across district attorneys and over time in a given county by clustering at the district level. Estimates include state- and year-month- fixed effects. Estimates calculated relative to omitted months in election cycle, which are normalized to 0.

Figure C.4: Monthly Criminal Sentencing Outcomes in Election Cycle
 – with Log Transformation ($\log(1+x)$)



Notes: Graph depicts dynamic difference-in-differences model estimates using Equation 1 (population weighted to adjust for differences in sampling probabilities across districts and across time). Estimates are at the county level by month. Estimates include state- and year-month- fixed effects. Jagged lines demarcate 95% confidence intervals. Estimates calculated relative to the months in year 2/-2 from an election year, which are normalized to 0. Best-fit line estimated via a sinusoidal model. Corresponding regression table is in Appendix. Log Transformation performed where each county's outcome variable is incremented by 1 before taking the log.

Figure C.5: Monthly Criminal Sentencing Outcomes in Election Cycle – Offense Subcategories
 – with Log Transformation ($\log(1+x)$)



Notes: Graph depicts dynamic difference-in-differences model estimates using Equation 1 (population weighted to adjust for differences in sampling probabilities across districts and across time). Estimates are at the county level by month. Estimates include state- and year-month- fixed effects. Estimates calculated relative to the months in year 2/-2 from an election year, which are normalized to 0. Best-fit line estimated via a sinusoidal model. Jagged lines demarcate 95% confidence intervals. Corresponding regression tables are in Appendix. Log Transformation performed where each county’s outcome variable is incremented by 1 before taking the log.

Table C.9: Arrest and Crime Rates Election Year Effects, by Years from Election Year

	Dependent variable (in Log Points)	
	Crimes/1000 Pop.	Arrests/1000 Pop.
	(1)	(2)
-1	0.005 (0.011)	0.026 (0.030)
1	0.049** (0.019)	0.061 (0.047)
-2	0.026 (0.018)	0.076 (0.057)
Joint Test p-value	0.001309	0.2741
Observations	37,451	36,189

*p<0.1; **p<0.05; ***p<0.01

Notes: Table depicts dynamic difference-in-differences model estimates using Equation 1 (population weighted to adjust for differences in sampling probabilities across districts and across time). Estimates are at the county level by year. The standard errors are corrected for correlation across district attorneys and over time in a given county by clustering at the district level. Estimates include state- and year- fixed effects. Estimates calculated relative to the election year, which is normalized to 0.

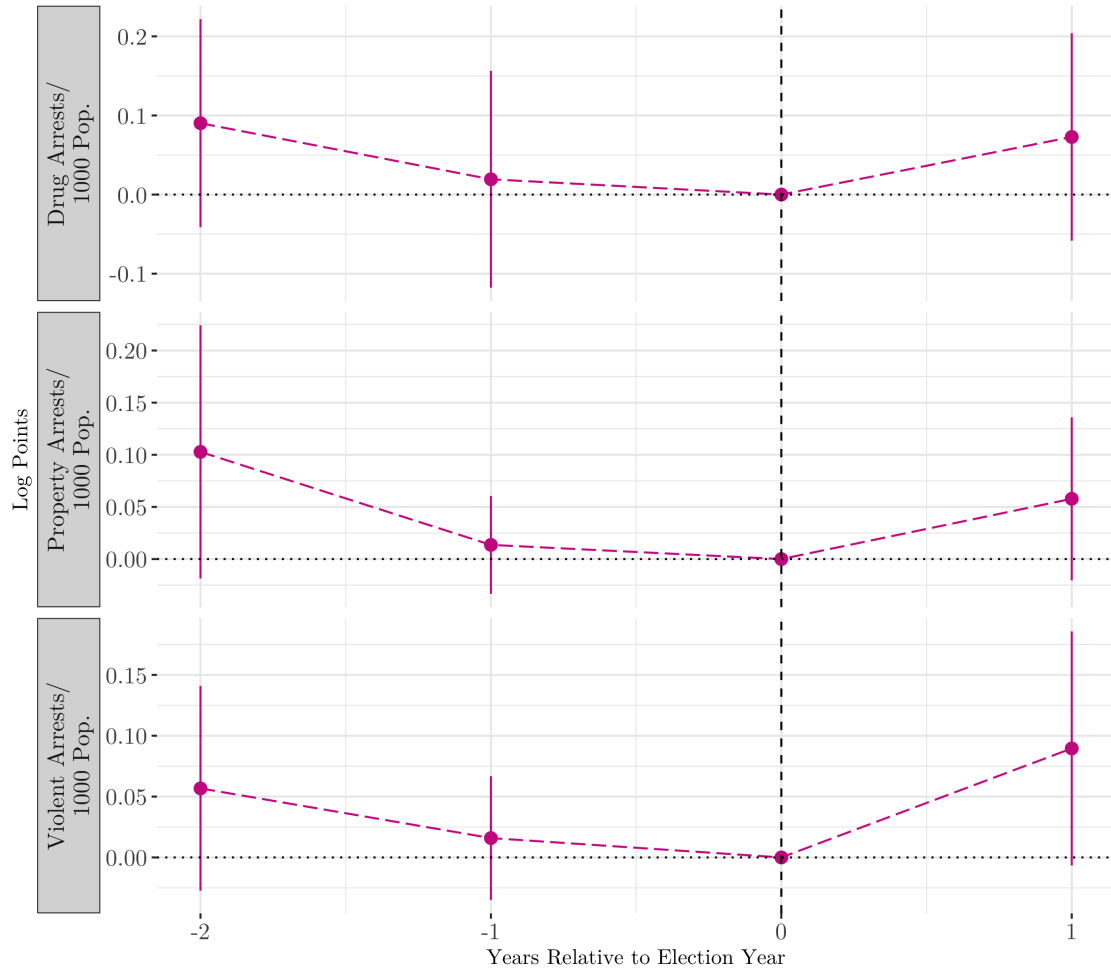
Table C.10: Arrest and Crime Rates Election Year Effects

	Dependent variable (in Log Points)	
	Crimes/1000 Pop.	Arrests/1000 Pop.
	(1)	(2)
Election Year	-0.025** (0.012)	-0.058** (0.028)
Observations	37,451	36,189

*p<0.1; **p<0.05; ***p<0.01

Notes: Table depicts static difference-in-differences model estimates using Equation 4 (population weighted to adjust for differences in sampling probabilities across districts and across time). Estimates are at the county level by year. The standard errors are corrected for correlation across district attorneys and over time in a given county by clustering at the district level. Estimates include state- and year- fixed effects.

Figure C.6: Arrest Outcomes in Election Cycle – Offense Subcategories



Notes: Graph depicts dynamic difference-in-differences model estimates using Equation 1 (population weighted to adjust for differences in sampling probabilities across districts and across time). Estimates are at the county level by year. Estimates include state- and year fixed effects. Vertical lines mark two standard errors. Estimates calculated relative to the election year, which is normalized to 0.

Table C.11: Leading Local Newspaper Included in Crime Keyword Analysis (Figure 14)

State	Newspaper	City
Alaska	Anchorage Daily News	Anchorage
Arizona	The Arizona Daily Star	Tucson
California	Los Angeles Daily News	San Diego
Colorado	The Denver Post	Denver
Connecticut	Hartford Courant	New Haven
N/A	The Washington Post	District of Columbia
Florida	Tampa Bay Times	Jacksonville
Georgia	The Atlanta Journal-Constitution	Atlanta
Kansas	The Wichita Eagle	Wichita
Kentucky	Lexington Herald-Leader	Lexington
Louisiana	New Orleans Times Picayune	Baton Rouge
Maryland	Baltimore Sun	Baltimore
Massachusetts	The Boston Globe	Boston
Minnesota	Star Tribune	Minneapolis
Missouri	St. Louis Post Dispatch	St. Louis
Nevada	Las Vegas Review-Journal	Las Vegas
New Hampshire	New Hampshire Union Leader	Manchester
New York	The New York Times	New York
North Carolina	The Charlotte Observer	Charlotte
Ohio	The Columbus Dispatch	Columbus
Oklahoma	The Oklahoman	Oklahoma City
Oregon	The Oregonian	Portland
South Carolina	The State	Charleston
Texas	Houston Chronicle	Houston
Utah	The Salt Lake Tribune	Salt Lake City
Virginia	The Virginian-Pilot	Virginia Beach
Washington	The Seattle Times	Seattle
Wisconsin	Milwaukee Journal Sentinel	Milwaukee