

# Electoral Cycles in Criminal Sentencing: Evidence from California Prosecutor Elections

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Current Draft: November 20, 2021

## Abstract

Prosecutors in the United States wield immense discretionary power over the outcome of criminal cases. Despite this, there has been relatively little research concerning the effect that electoral cycles might have on their sentencing behavior. Conventional wisdom dictates that prosecutors will likely pursue harsher sentences on average, in an attempt to appear “tough on crime”. To test this, I construct a novel dataset of California prosecutors and electoral outcomes. Using criminal sentencing data, I then estimate the impact of electoral pressure, as measured by electoral proximity and competition, on criminal sentencing. I find that electoral pressure is associated with a decrease in the average severity of criminal sentences.

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# 1 Introduction

The United States is the only country in the world for which prosecutors are elected rather than appointed.<sup>1</sup> Indeed, for the vast majority of developed nations these positions are held by career civil servants as opposed to elected figures (Ellis (2012), Tonry (2007)). In spite of this, prosecutorial elections are far and away the most common means for determining sentencing officials in the United States.<sup>2</sup> While elections are meant to be a mechanism through which voters can hold prosecutors accountable, it remains an open question as to how effective of a mechanism they prove to be. Voters may not pay close attention to the sentencing behavior of elected officials, instead focusing primarily on high-profile or controversial cases. This contributes to a system where the majority of prosecutors run unopposed, and those who do run opposed overwhelmingly win reelection. This is particularly notable due to the immense power that prosecutors have in determining charges and sentencing. Nearly 95% of all convictions occur as the result of plea-bargaining, and there is little to no oversight as to what charges are brought or what sentences are proposed (Pfaff, 2017).<sup>3</sup>

The efficacy of elections as a mechanism for influencing prosecutor behavior aside, it is somewhat ambiguous as to how prosecutors will interpret and respond to voters' punishment preferences. Although recent years have seen broad movements supporting the reduction of mass incarceration, voters have historically demonstrated preferences for harsher sentences for criminals.<sup>4</sup> Thus it is possible that, in an attempt to be more politically viable, prosecutors will pursue harsher than necessary charges or sentences. Indeed even if prosecutors face little to no electoral pressure in their district attorney elections, they may pursue more "electable" sentencing behavior to bolster their electoral profile for other, more competitive political offices (Pfaff, 2017). On the other hand, as crime rates fall and decarceration move-

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<sup>1</sup>Note that prosecutor and district attorney (DA) can be used somewhat interchangeably.

<sup>2</sup>Currently 46 states elect their local prosecutors.

<sup>3</sup>As Pfaff notes, while judges do technically need to approve the sentences proposed by prosecutors in the plea-bargaining process, they generally acquiesce to the prosecutor's proposal.

<sup>4</sup>According to the General Social Surveys (GSS), a nationally representative set of surveys conducted from 1972-2018, 66% of respondents believe that courts are "not harsh enough" with criminals, while only 7% believe that courts are "too harsh" (Smith & Morgan, 1972-2018).

ments gain political ground, prosecutors may try and give more lenient sentences to seem more “progressive” on crime. Prosecutors may also pursue alternative signals of their quality, such as conviction rates or high profile jury trials, which have a theoretically ambiguous relationship with sentence severity.

In this paper I analyze the relationship between prosecutor electoral cycles and criminal sentence severity in California. To do so, I build a novel dataset of California prosecutors and elections. Using criminal sentencing data, I then develop both linear and non-linear regression models to test the impact of electoral pressure, as measured by electoral proximity and competition, on criminal sentencing. I find that electoral pressure is associated with a decrease in the average sentence length imposed, with a larger effect for serious, violent crimes. Specifically, I find that the average sentence imposed under maximal electoral pressure is 0.8282 months (2%) shorter than sentences imposed under no electoral pressure, while the average sentence for serious crimes decreases by 2.0124 months (3.2%). This is largely driven by a reduction in the probability of receiving a life sentence conditional on being convicted of a serious crime, which is 0.0016 percentage points (8.6%) lower when under maximum electoral pressure. Then, using data on arrests and filing decisions in San Francisco, I show that, holding the crime of arrest constant, defendants are 4.85 percentage points (7.8%) more likely to be charged with a misdemeanor, and 3.12 percentage points (4.6%) less likely to be charged with felony as an election nears. Similarly, I show that electoral pressure is associated with a relative increase in misdemeanor charges over felony charges, holding the type of crime constant. These results are consistent with the notion that prosecutors respond to electoral pressure by pursuing additional, weaker cases, and by engaging in additional charge bargaining, wherein they offer less severe charges in exchange for a guilty plea.

The rest of the paper is structured as follows: Section 2 explores the institutional background of prosecutor elections and California’s criminal justice system. Section 3 provides an overview of the related literature. Section 3 describes the data. Section 4 discusses the

data and empirical strategy. Section 5 presents the results. Section 6 concludes.

## **2 Institutional Background**

### **2.1 Historical Underpinnings**

Although the United States took its legal framework from the English tradition, local district attorneys appeared in America before England. As such, there was little precedent as to what system would be in place to elect or appoint prosecutors. Established after the Revolutionary War, most district attorneys were appointed officials, although who did the appointing varied widely across states. This remained the norm until the Jacksonian era, wherein waves of democratization swept the country, bringing with them a healthy skepticism of appointed officials. Jackson's supporters believed that the process of appointing district attorneys would lead only to corruption and patronage. If fair trials were to be guaranteed, the thinking went, then prosecutors should be elected by and held accountable to voters (Ellis, 2012). Thus, coinciding with the beginning of Andrew Jackson's second term in the early 1830's, states began passing legislation delegating the power to elect prosecutors, with nearly three quarters of states having done so by the onset of the Civil War in 1861.

### **2.2 California's Criminal Justice Systems**

Having been formally incorporated into the Union well after Jackson's presidency, California's criminal justice systems reflect many of the tenants of Jacksonian democracy. Both judges and prosecutors alike are subject to regular elections, with appointments only being used to fill vacancies which occur between elections. Broadly, California's judicial system is split into three levels, each with different responsibilities and selection processes. In order of authority, they are the Supreme Court of California, the Court of Appeals, and the Superior or Trial Courts. It is within the latter that the vast majority of civil and criminal trials are heard, and with whom prosecutors most often deal. There are currently 1,743 Superior

Court Justices, all of which are elected in non-partisan elections for six-year terms. The elections are staggered such that approximately one-third of judges are up for election every two years, with vacancies filled as described previously.

In contrast, each of California's 58 counties elect their own district attorney in non-partisan elections every four years, coinciding with midterm elections. As is the case all over the United States, the district attorney's office is responsible for representing the government in criminal trials, and choosing what does or does not go to trial, in addition to what charges are being levied. The DA and the prosecutor's office also play the significant role of the primary negotiator in plea bargains. In fact, judges are not even allowed to participate in the plea-bargaining process. After plea agreements are struck between a prosecutor and a defendant, the deal is then sent to the respective Trial Court judge for approval. The corresponding position for the state, the Attorney General, is elected in popular elections every four years.

## **2.3 California Sentencing Reform**

Having established the basic criminal justice framework within which sentencing occurs, it is important to look at how criminal sentencing has developed within California. After all, while judges and prosecutors play significant roles in determining sentences, they are ultimately bound to enforce the laws and basic sentences inherited from the state constitution and the legislature. For the purposes of this analysis it will be important to discuss the impact which the Determinate Sentencing Law (DSL) of 1977 had on determining sentence lengths. The DSL had the stated goal of the “[E]limination of disparity and the provision of uniformity of sentences...by determinate sentences fixed by statute in proportion to the seriousness of the offense as determined by the Legislature to be imposed by the court with specified discretion” (Bailey, 2008). In practice this created three tiers of sentencing for each crime: lower, middle, and upper, with escalating sentence lengths respectively. The middle, or intermediate sentence served as a sort of baseline, with the lower or upper sentences being

levied according to the evidence and a degree of discretion. Thus, DSL created a system in which sentencing officials choose between fixed, discrete sentencing lengths for a given crime. This system has become to be known as the sentencing triad. The consequence of the DSL was similar to other mandatory minimum sentencing laws, on average increasing sentence lengths and leading to a quintupling of California's prison population between DSL's passing in 1977 and 2007 (Weisberg, 2019).

The DSL and the exploding prison populations in California created a variety of different problems for the state to contend with. Chief among these problems was the issue of overcrowding. California simply did not have the prison infrastructure available to continue imprisoning people at the same rate as before. Ultimately the federal government stepped in and mandated that California cut down on its prison population. This led the California legislature to enact to the 2011 corrections realignment, also known as Assembly Bill 109 (AB 109). By lowering the rate at which parolees were sent back to prison and sentencing many lower-level offenders to county jails instead of state prisons, AB 109 lead to a substantial reduction in state prison populations (Lofstrom & Raphael, 2013). Thus, while the DSL and the sentencing triad still dictate how sentencing works in California, AB 109 has lead to changes in sentencing behavior which will need to be accounted for when analyzing sentence behavior.

### **3 Literature Review**

This study seeks to contribute to a growing body of research surrounding the determinants of sentencing lengths. The literature as it exists, however, focuses primarily on judicial discretion: both when and how they choose to exercise it when sentencing criminals. Previous research has examined the role of race (Depew *et al.* (2017), Alesina & La Ferrara (2014), Depew *et al.* (2017), Lim *et al.* (2016)), gender (Butcher *et al.* (2017), Lim *et al.* (2016), Knepper (2018)), news coverage Lim *et al.* (2015), and even the performance of local sports teams (Eren & Mocan, 2018) in influencing how judges decide to sentence. Each of these

is relevant when considering how prosecutors choose to charge and sentence as well. For example, prosecutors who exhibit animus towards certain racial groups or genders may decide to be less lenient in their proposed plea-bargains, either with sentence length or with what charges they decide to file. Even if their discrimination is implicit, or “statistical”, it is easy to imagine a situation in which a prosecutor will seek additional prison time for a defendant. If a prosecutor believes people of a certain group to be inherently prone to crime, they may seek harsher sentences in order to keep said person off the streets for longer, regardless of the specific circumstances of the alleged crime.<sup>5</sup>

Indeed there is evidence that prosecutor characteristics play a role in sentencing. Krumholz (2019), using a unique database of national prosecutor elections, shows that sentences and admissions increase in response to the election of a Republican DA, and decline when non-white DAs are elected. This effect, the author shows, is primarily driven by changes with respect to drug crimes. Arora (2018), exploiting quasi-experimental variation in close elections, similarly finds that Republican DAs sentence more harshly than their counterparts.

Perhaps more closely related to this paper is the research which explores the relationship between sentencing and elections. Much of the existing literature has focused on the state of Kansas, which, conveniently for identification, uses a mixture of elections and appointment for their trial court judges. Lim (2013) and Gordon & Huber (2007) each find significant differences in sentencing behavior across these districts. Gordon and Huber find that judges facing partisan elections do in fact sentence more harshly than their counterparts, and they claim that this is due to electoral pressure rather than selection. Going a bit further, Lim (2013) finds that the harshness of sentencing is closely related to the majority political ideology in their respective districts, with no corresponding effect in the districts with appointed judges.

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<sup>5</sup>For each hypothetical, one could of course imagine that prosecutors could seek lighter sentences for preferred groups, or in response to positive emotional shocks or news coverage.

While both Gordon & Huber (2007) and Lim (2013) find evidence of electoral pressure affecting sentencing in partisan elections, there is reason to believe that the response to non-partisan elections may be different. Lim & Snyder Jr (2015) note that in partisan elections voters largely just vote along party lines, giving judges less of an incentive to pay attention to voters' wishes. In contrast, they find that in non-partisan elections much more attention is paid to candidate quality, crowding out the partisan effect. To my knowledge, Berdejó & Yuchtman (2013) conduct the only study directly examining electoral pressure in a non-partisan setting. Using Washington State sentencing data, the authors use distance to election as an independent variable in both linear and nonlinear regression analyses. They find that sentences are about 10% longer at the end of a judge's electoral cycle relative to the beginning, implying that judges are responding to electoral pressure. In addition, they show that this discrepancy only appears for judges who actually face competition in their election.

Despite the important role which prosecutors play in the criminal justice system, there has been relatively little research done examining prosecutors and sentencing. Bjerk (2005) looks at how prosecutors respond to the introduction of mandatory minimum laws, including California's DSL. They find that, when felonies become beholden to mandatory minimum sentences, prosecutors become significantly less likely to charge people with felonies when given the choice between a felony and a misdemeanor. Influencing the decision to prosecute, and the charges levied extends to elections as well. Bandyopadhyay & McCannon (2014) build a theoretical model to predict the effect an election may have on a prosecutor's decision to prosecute, and then test their model using administrative data from North Carolina. Both the model and the data suggest that prosecutors seek to increase the number of convictions made prior to elections. In order to do this prosecutors begin taking more cases to trial. The authors find that reelection pressure leads to nearly a 10% increase in the number of cases brought to trial, with an additional 14.7% increase if they have a challenger in the election. Interestingly, this has the effect of reducing the average sentence obtained, as

presumably they start bringing weaker cases to trial to try and increase the absolute number of convictions.

The study proposed in this paper contributes to the aforementioned literature in several ways. First, to my knowledge no other papers have empirically explored the possibility of a dynamic electoral pressure effect throughout the prosecutors term, focusing instead on either averages based on prosecutor characteristics or looking at pressure as binary. In addition, I construct a novel dataset of California district attorneys and election outcomes. Finally, few other papers have had access to as rich a set of prosecutor, offender, and case characteristics, nor have any of the current papers focused on the dynamics in a nonpartisan setting.

## 4 Data and Empirical Methodology

Sentencing data for this analysis comes from the National Corrections Reporting Program (NCRP)<sup>6</sup>, which collects offender-level prison administrative data. States voluntarily offer this data to the Bureau of Justice Statistics (BJS). 48 states have participated at some point, providing prison admission and release records dating back to 1971 and continuing through 2016. Each observation in the data represents one prison sentence. Inmates have been de-identified and provided unique ID numbers to enable matching across multiple incarceration spells within the same state. Each observation details the month and year of admission and release, the type of release, the county of conviction, the type of offense committed, and information about how long the defendant has been in jail. Each record also includes demographic information for the inmate. Observed characteristics include race, age, sex, and prior conviction history.

Because the NCRP data consists primarily of people entering and exiting state prisons, AB 109 impacts the quality of the data beginning in 2011. Namely, because people who were previously sent to prison are being sent to jail instead they will not appear in the data.

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<sup>6</sup>United States Bureau of Justice Statistics (2019)

As such, for my analysis I will only analyze the pre-2011 data. To create my sample, I make a number of changes to the data. First, I drop all observations from States other than California, and all observations of offenders admitted before 1995, as there is no electoral information available. Second, I drop all observations in which the county of conviction is unknown, as it is impossible to link them to a prosecutor. Third, I drop all observations for which the sentence length is missing, and I right censor life sentences at 720 months. Thus, the final dataset consists of 2,050,464 unique prison spells, covering the years from 1995-2010.

This paper also utilizes the California Elections Data Archive (CEDA), which documents all local California elections beginning in 1995 and going through 2016. In addition to documenting location, returns, and date of each election, CEDA contains information about each candidate running. Candidate information includes their name, former occupation, and their incumbency designation. When election information was missing, or when DA's were replaced mid-term, candidate and election information was gathered via local news reports and case filings. Ultimately I observe 177 unique elections, and unique 119 district attorneys.

In addition, I use arrest, filing, and trial data provided by the San Francisco District Attorney's office in order to provide additional evidence of changes in prosecutor filing behavior. Data is provided in three separate files that that can be connected via unique case identifiers. In general, observations include data on the type of offense, and the severity of the offense and cover cases from 2011 through the present.

For my primary specification I follow Berdejó & Yuchtman (2013) and estimate a linear regression with a normalized distance to the next election filing date as the independent variable of interest. In particular, I will estimate the following model:

$$Sentence_{i,p,c,q,t} = \alpha + \beta_1 Pressure_{i,p,t} + \beta_2 X_i + \beta_3 Z_p + F(t) + \gamma_c + \delta_p + \epsilon_{i,p,c,q,t} \quad (1)$$

where  $i$ ,  $p$ ,  $c$ ,  $q$ , and  $t$  denote case, prosecutor, county, quarter-of-year, and time respectively.  $Sentence_{i,p,c,q,t}$  is the sentencing outcome in months for the most severe sentence associated with case  $i$ , Pressure is a normalized measure of electoral proximity.  $X_i$  is a vector of case-specific controls,  $F(t)$  includes both year and quarter fixed effects, while  $\gamma_c$  and  $\delta_p$  are county and prosecutor fixed effects respectively. To construct the electoral pressure variable, I first calculate the linear distance in months until the next filing date. I then get electoral pressure by normalizing linear distance in the following way:

$$Pressure_{i,p,c,t} = 1 - \frac{LinearDistance_{i,p,c,t}}{\# \text{ of Months in Electoral Cycle}_{p,c,t}}$$

and by providing adjustments based on electoral competitiveness. Specifically, a candidate is assumed to face maximum electoral pressure between the filing date and the election when they run opposed, and no electoral pressure in that time frame if they are not running for re-election. Accordingly,  $Pressure_{i,p,c,t}$  is a continuous variable equal to 1 when there is maximal electoral pressure, and 0 when there is minimal, or no pressure.

Finally, in alignment with Berdejó & Yuchtman (2013) and related literature, I will use as my baseline specification the subset of serious, visible offenses as classified by the FBI: assault, murder, rape, and robbery. These are the crimes that are both most salient to voters, and are most often covered by local news. I explore alternative samples of offenses as a robustness check, and to explore effect heterogeneity.

## 5 Results

To begin, Figure 1 plots the estimated coefficients and 95% confidence intervals from a regression of sentence length on quarter-by-year dummy variables, and the set of control variables as described by equation (1). The figure shows that, relative to the first quarter after the previous election, there seems to be a steady increase in average sentence lengths for several quarters, followed by a sharp decline approximately two years after. This is likely

the result of other political election cycles, as two years into most DA terms coincides with one-third of judicial elections, presidential elections, and certain local elections. In general, however, it does seem to be the case that sentences in the quarters following a prosecutor election are noticeably more severe than those sentences immediately before.

Table 1 presents linear distance estimates of electoral pressure across several different samples and outcomes. Column (1) presents the results for the baseline specification. I find, contrary to conventional wisdom, that the average sentence length for these crimes decreases by approximately 2.1 months on average, corresponding to a 3.2% decline relative to the start of an electoral cycle, when the candidate faces the least pressure. Although perhaps counterintuitive, relatively lighter average sentences when electoral pressure is greatest is consistent with the model and results from Bandyopadhyay & McCannon (2014). Their model suggests that severity will in fact go down near elections as prosecutors, wanting to increase their number of convictions, will bring worse cases to trial, resulting in lighter sentences. Similarly, because prosecutor resources are finite, an increase in the number of jury trials will necessarily decrease the amount of resources devoted to plea-bargaining. Prosecutors may be incentivized to engage in charge or sentence bargaining, offering lighter charges or sentences in exchange for a guilty plea, in order to increase their clearance rate and free up resources for jury cases.

Columns (2) through (5) present the same regression analysis, but with alternative offense samples, including any offense, and offenses categorized by the BJS as violent, drug, and property offenses respectively. In contrast to the FBI definition, each of these includes any offense that might fall into each category, not just the most severe ones. I continue to find evidence of electoral pressure being associated with less severe sentences in the aggregate sample and with violent offenses, but fail to detect any effect for drug or property offenses. The effect being concentrated among violent offenses is consistent with the argument that violent cases get more attention and are more important to voters, making them the cases that would be most sensitive to electoral pressure.

It is possible, however, that the effect is being unduly driven by outlier cases with the largest sentences, namely life sentences. Column (6) presents results from the baseline sample, but with the exclusion of all life sentences. I find that, although still statistically significant, the affect does attenuate some, corresponding to only a 1.96% decline in sentence length. Given that life sentences are universally capped at 720 months, the difference in effect across these samples is likely the result of a electoral pressure causing a change in the probability of receiving a life sentence. Column (7) tests this by showing the results of regressing equation (1) on the baseline sample, but with a binary variable equal to 1 if the defendant received a life sentence as the outcome of interest. I find that that moving from no electoral pressure to maximum electoral pressure is associated with a 0.0016 percentage point (8.6%) decline in the likelihood of receiving a life sentence. While prosecutors may be taking more of these types of cases to trial, which is usually associated with longer sentences, plea-bargaining is still the dominant mechanism for clearing cases. When restricted by time and resources, prosecutors may be more inclined to forego pursuing a life sentence for marginal cases, instead offering a determinate sentence which is more likely to be accepted.

In order to test for potential differential effects based on defendant characteristics, I run each of the regressions from Table 1 separately for white and black offenders. Panel A of Table 2 shows the results for black offenders, while Panel B presents them for white offenders. Broadly, the results correspond with the pooled regressions, with the notable exception of the sample which includes violent offenses of any type. The estimated effect of electoral pressure attenuates and loses significance for white offenders, while the opposite occurs for black offenders. In particular, I find that the average sentence for black ex-offenders convicted of a violent offense decreases by 2.589 months (3.6%). Thus, while the results are similar across many of the specifications, there does seem to be some evidence of a differential effect for certain black offenders.

In order to evaluate the validity of these findings, Table 3 presents a variety of robustness checks designed to rule out alternatives. First, in order to rule out the possibility of other

election cycles causing my results, I run my primary specification separately for elections with and without competition. If other political cycles, such as judicial elections, are creating the effect, then one would expect that the competitiveness of the prosecutor election wouldn't matter. I find that, when an incumbent runs unopposed, the estimated effect of electoral proximity attenuates and loses significance, although it is fairly noisy. Figure 2 conducts a similar analysis, instead using the non-linear specification of quarter-by-year dummies to plot the dynamics of each group throughout the cycle. The estimated coefficients for unopposed elections are consistently larger than those for opposed elections, likely reflecting compositional differences between the groups. Elections are more likely to be opposed in counties with larger populations, which also tend to be politically more liberal. However, the sharp decline in average sentencing immediately prior to the following election does seem to attenuate, if not disappear for the unopposed elections, which suggests that electoral pressure is altering prosecutor behavior.

Column (3) of Table 3 presents results for my primary specification, with the exception that murder is excluded from the sample. Murder convictions result in, on average, the longest sentences, raising the possibility that murder convictions could have an outsized influence on the estimator. While the actual point estimate does significantly shrink, the relative effect is qualitatively and quantitatively similar to my primary estimate. As such, it doesn't seem likely that murder offenses are driving the result. Similarly, it is important to check that my results aren't sensitive to the choice of right-censor for sentencing. Following Berdejó & Yuchtman (2013), I test this by running an alternative specification with a higher right-censor of 1200 months. I find that the choice of censor does not seem to be impacting my results.

There might also be endogeneity concerns with my electoral proximity measure, as a prosecutor's sentencing behavior early in their term could impact the likelihood that they ultimately face a challenger, and thus the pressure they face. To get around this, I re-estimate equation (1) with linear distance as the electoral proximity metric, as unaltered distance until

the next filing deadline is exogenous. Column (5) presents these results. I find that, as the distance to the next election increases, so to does the severity of the sentence. Using 48 month terms as the baseline, this estimate would imply that moving from no electoral pressure to full pressure would cause a 1.92 percentage month decrease in the average sentence length, which is similar to my baseline estimate. In addition, I test the sensitivity of my estimates to the choice of outcome variable. Column (6) uses the total sentence of a given prison spell as the outcome of interest, rather than the sentence from most severe offense. I do not find any evidence that this is affecting my results.

Finally, because I am unable to directly observe the filing behavior of the prosecutor in the NCRP sentencing data, I supplement my primary analysis by using arrest and filing data from the San Francisco District Attorney's office. Specifically, I test whether there is evidence that prosecutors are altering the quantity or severity of charges filed in response to electoral pressure. To give an example, suppose that there is someone who has been arrested for driving under the influence. Depending on what they believe they can prove, a prosecutor might charge this person with a misdemeanor DUI, a felony DUI, or they might dismiss the case, filing no charges. This decision could change with electoral pressure, depending on what signal the prosecutor is trying to send voters. Table 4 presents estimates for the change in the likelihood of being charged with a misdemeanor or felony, conditional on the specific crime of arrest. I find that electoral pressure is associated with a 4.85 percentage point (7.8%) increase in the likelihood of being charged with a misdemeanor, and a 2.07 percentage point decline in the probability of being charged with a felony. Thus, holding the crime of arrest constant, it seems that prosecutors are more likely to actually file misdemeanor charges when there is an upcoming election. This is consistent with previous literature, which has found evidence that prosecutors seek to increase their total number of convictions by prosecuting weaker cases. Finally, column (3) of Table 4 shows that, holding the type of crime constant, electoral proximity is associated with an increase in the relative likelihood that a filed charge is a misdemeanor, rather than a felony. While I can't observe the case outcomes, each

of these findings is consistent with the proposed mechanisms of prosecutors choosing to prosecute weaker cases and engaging in more charge bargaining.

## 6 Discussion

In this paper I use California election and sentencing data to test for the presence of electoral cycles in prosecutor sentencing. I provide evidence that electoral proximity is associated with, on average, less severe sentences, with a particularly large decrease in the probability of receiving a life sentence. Using San Francisco arrest and filing data, I also show that electoral proximity is associated with, holding offense constant, an increase in the likelihood of being charged with a misdemeanor, a decrease in the likelihood of being charge with a felony, and increase in the likelihood of being charged with a misdemeanor relative to a felony. This is consistent with several other studies of prosecutor behavior, which have found that, in response to an upcoming election, prosecutors prefer to increase their number of convictions, and take more cases to trial. Even though trials tend to result in more severe sentences for defendants, the increase in sentence severity associated with more jury trials is likely dwarfed by the reduction in severity associated with increased reliance on charge bargaining, and prosecuting weaker cases, resulting in a net decrease.

This paper contributes to our understanding of the forces which influence prosecutor behavior, and draws additional attention to the impact which prosecutor discretion plays in determining criminal justice outcomes. While there is a robust literature on the effects of electoral pressure on other criminal justice officials, most notably judges, there has been far too little examining prosecutors, despite their pivotal role influencing criminal sentencing. Future research should continue to explore the impact of prosecutor elections on criminal justice outcomes, particularly given the wide range of institutional differences across states and jurisdictions.

## 7 Tables and Figures

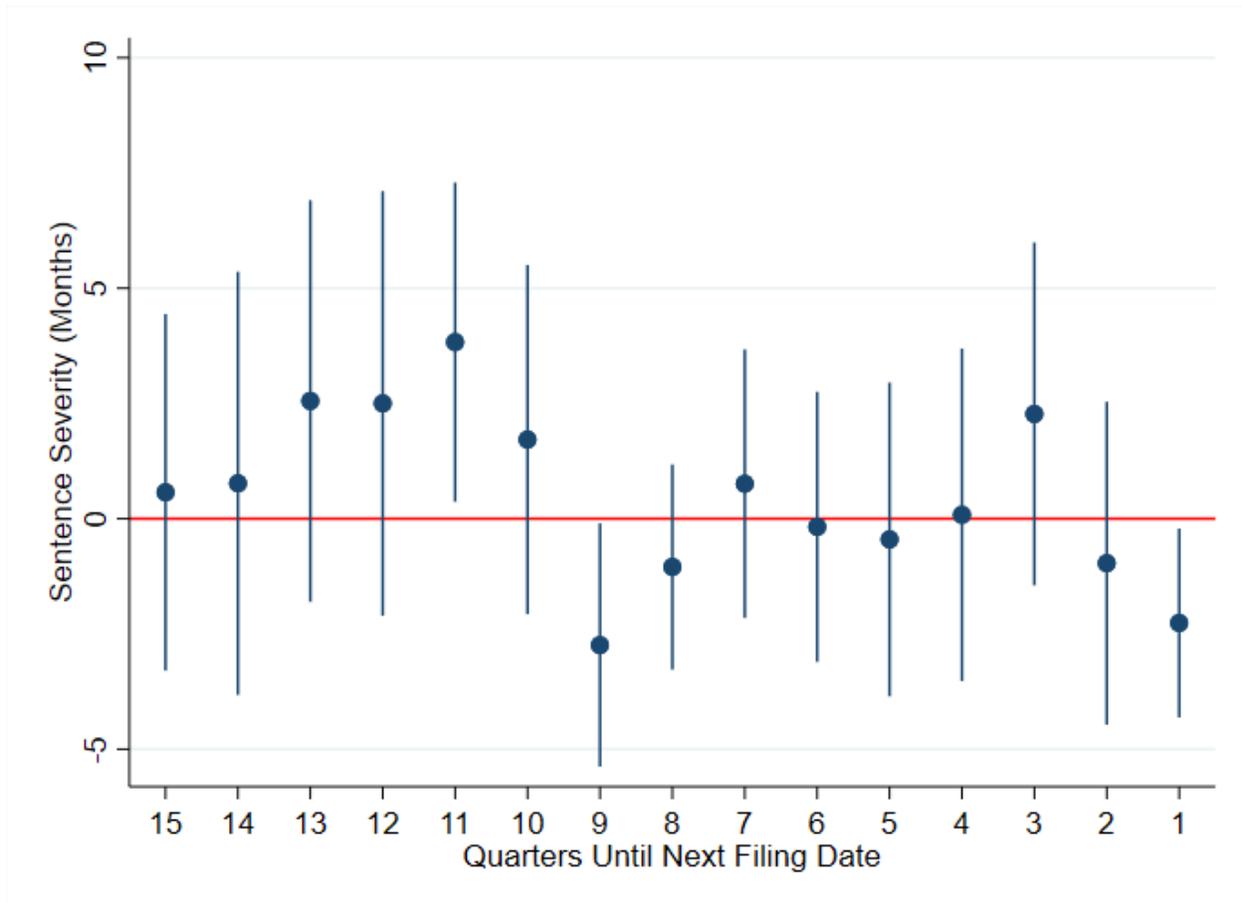


Figure 1: Sentence Severity by Quarter to Election

The figure plots the estimated coefficients (relative to the omitted 16-quarters-to-filing-deadline category) and 95% confidence intervals from a regression of sentence length in months, capped at 720, on dummy variables indicating the number of quarters until the next filing deadline, in addition to the set of controls described in equation (1).

Table 1: Effects of Electoral Pressure on Sentence Lengths (Months)

	Baseline Offenses (1)	Any Offense (2)	Violent Offense (3)	Drug Offense (4)	Property Offense (5)	Excluding Life Sentences (6)	Life Sentence (Outcome) (7)
Electoral Pressure	-2.0124*** (0.6592)	-0.8282** (0.4044)	-1.4090** (0.6052)	0.0900 (0.1834)	-0.1285 (0.2631)	-1.0058** (0.3919)	-0.0016** (0.0007)
Observations	302,620	2,050,464	505,375	650,747	657,917	296,961	302,620
Mean	63.8700	41.1097	71.6476	31.6483	30.2296	51.3687	0.0187

Notes: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . For columns (1)-(7) the outcome variable is the sentence length of the most severe offense in months, capped at 720. For column (8) the outcome variable is a binary variable equal to 1 if the offender received a life sentence. Each regression controls for sex, age at admission, race, Hispanic ethnicity, prior felony convictions, the most severe offense, and indicator variables for missing control variables. Fixed effects include a set of quarter fixed effects, a set of DA fixed effects, a set of year-of-admission fixed effects, and county fixed effects. Standard errors robust to correlation at the quarter-year level are reported in parentheses.

Table 2: Effects of Electoral Pressure on Sentence Lengths (Months): By Race

<i>Panel A. Black Offenders</i>							
	Baseline Offenses (1)	Any Offense (2)	Violent Offense (3)	Drug Offense (4)	Property Offense (5)	Excluding Life Sentences (6)	Life Sentence (Outcome) (7)
Electoral Pressure	-2.3010** (0.9640)	-0.8775* (0.4941)	-2.5890*** (0.9200)	0.1393 (0.2626)	-0.0096 (0.4695)	-1.2727* (0.6596)	-0.0017 (0.0010)
Observations	103,928	597,433	152,628	218,112	174,852	102,442	103,928
Mean	64.0937	44.2073	72.8495	35.5446	32.3255	54.5793	0.0143
<i>Panel B. White Offenders</i>							
	Baseline Offenses (1)	Any Offense (2)	Violent Offense (3)	Drug Offense (4)	Property Offense (5)	Excluding Life Sentences (6)	Life Sentence (Outcome) (7)
Electoral Pressure	-2.7430* (1.5011)	-0.8733* (0.4752)	-1.2036 (1.0145)	-0.1309 (0.1832)	-0.1868 (0.2629)	-1.7151*** (0.6428)	-0.0017 (0.0019)
Observations	84,374	774,244	159,056	240,731	281,111	82,716	84,374
Mean	60.6128	37.1890	66.7892	28.6538	29.8413	47.4035	0.0196

Notes: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . For columns (1)-(7) the outcome variable is the sentence length of the most severe offense in months, capped at 720. For column (8) the outcome variable is a binary variable equal to 1 if the offender received a life sentence. Each regression controls for sex, age at admission, race, Hispanic ethnicity, prior felony convictions, the most severe offense, and indicator variables for missing control variables. Fixed effects include a set of quarter fixed effects, a set of DA fixed effects, a set of year-of-admission fixed effects, and county fixed effects. Standard errors robust to correlation at the quarter-year level are reported in parentheses.

Table 3: Effects of Electoral Pressure on Sentence Lengths: Robustness Checks

	Opposed (1)	Unopposed (2)	Excluding Murder (3)	Higher Censor (4)	Exogenous Linear Distance (5)	Total Sentence (6)
Electoral Pressure	-2.7662*** (0.8193)	-1.9811 (1.3235)	-1.2922** (0.5037)	-2.8249*** (0.0963)	0.0427*** (0.0139)	-2.3920*** (0.8423)
Observations	176,526	112,894	289,366	302,620	302,620	302,620
Mean	64.9506	62.2954	51.9654	72.9212	63.8700	74.0895

Notes: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . For columns (1)-(7) the outcome variable is the sentence length of the most severe offense in months, capped at 720. For column (8) the outcome variable is a binary variable equal to 1 if the offender received a life sentence. Each regression controls for sex, age at admission, race, Hispanic ethnicity, prior felony convictions, the most severe offense, and indicator variables for missing control variables. Fixed effects include a set of quarter fixed effects, a set of DA fixed effects, a set of year-of-admission fixed effects, and county fixed effects. Standard errors robust to correlation at the quarter-year level are reported in parentheses.

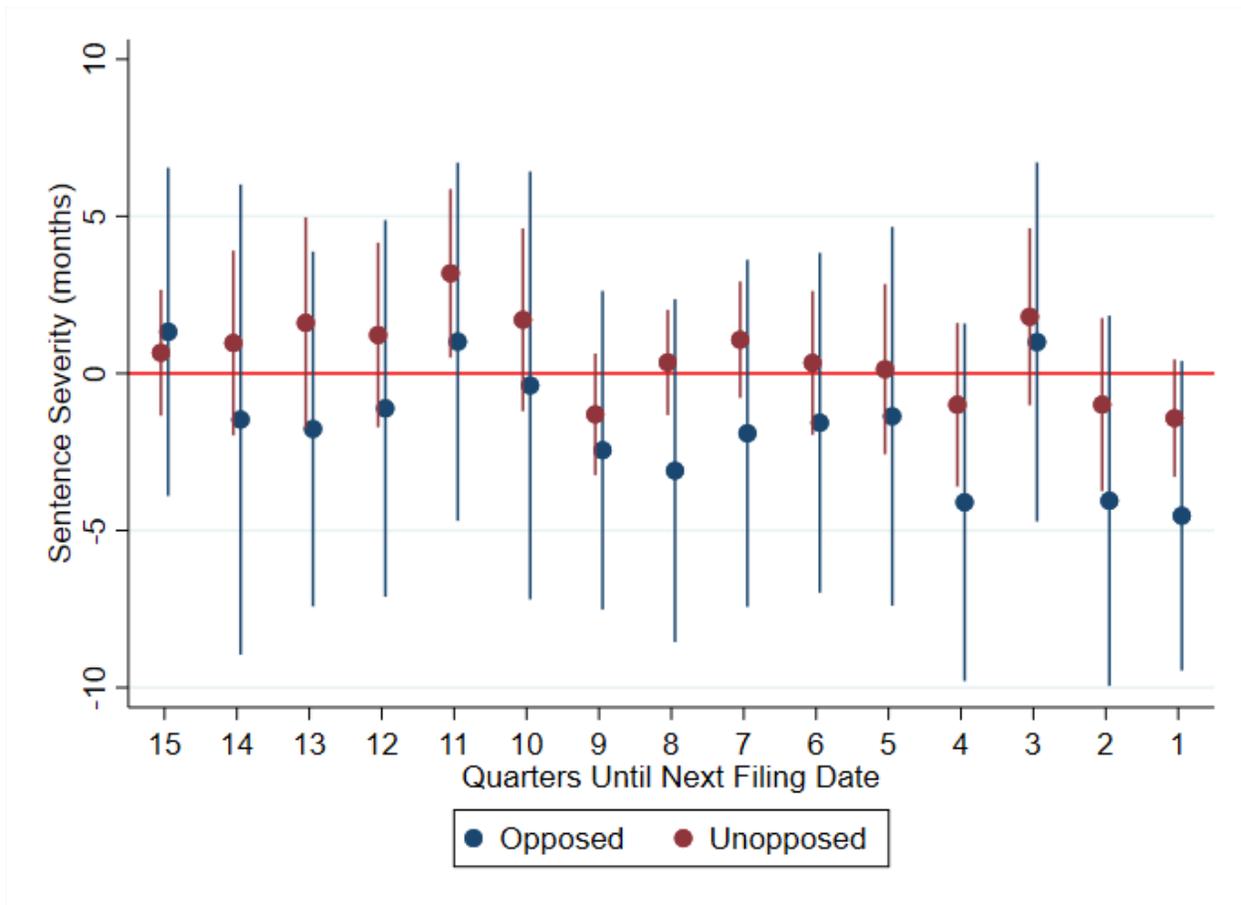


Figure 2: Sentence Severity by Quarter to Election: Heterogeneity by Competition

The figure plots the estimated coefficients (relative to the omitted 16-quarters-to-filing-deadline category) and 95% confidence intervals from a regression of sentence length in months, capped at 720, on dummy variables indicating the number of quarters until the next filing deadline, in addition to the set of controls described in equation (1), for opposed and unopposed elections respectively.

Table 4: Effects of Electoral Pressure on Filing Behavior

	Misdemeanor Charges Filed (1)	Felony Charges Filed (2)	Percentage of Felonies (3)
Electoral Pressure	0.0485*** (0.0104)	-0.0312* (0.0161)	-0.0207** (0.0100)
Observations	25,619	52,687	60,089
Mean	0.6245	0.6815	0.5535

Notes: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . For columns (1)-(2), the sample is the universe of cases presented to SF prosecutors for a filing decisions. The outcome variables are binary variables equal to 1 if the defendant is charged with a misdemeanor or felony respectively. Control variables include the most serious charging offense, a set of quarter fixed effects, a set of DA fixed effects, and a set of year fixed effects. Standard errors robust to correlation at the quarter-year level are reported in parentheses.

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