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Electoral Sentencing Cycles*

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Abstract

We add to our understanding of the optimal method of judicial selection by exploiting an unusual feature in North Carolina: judges rotate location every six months. This allows us to identify the existence and source of sentencing variation over the electoral cycle. We show that when elections approach, felony sentences rise. This increase is found exclusively when judges are sentencing in their district of election, and only when elections are contested. When judges hear cases outside their home district, sentences do not significantly vary over the electoral cycle. Our results show that electoral sentencing cycles can be explained by strategic sentencing by judges in an attempt to please voters. The unique setting allows us to reject alternative behavioral or contextual explanations for the rise in sentences as elections approach.

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1 Introduction

The process of selection and retention of judges is a key determinant of the quality of the justice system. Yet, no consensus exists on the most desirable system, some arguing in favor of appointments, others for elections, and others preferring the random selection of judges. The proponents of judicial elections argue that being subject to electoral review creates incentives to reach prompt decisions and to overcome personal biases. Opponents on the contrary point out that elections push judges to pander to local preferences, particularly during election periods. This, in turn, can cause sentencing electoral cycles, a potentially costly uncertainty in sentences.

While electoral sentencing cycles have been identified in several settings (see for instance Berdejo and Yuchtman 2013; Dippel and Poyker 2019), determining the cause of these variations is challenging. Indeed variations in sentences during electoral periods are not necessarily driven by the effort of judges to meet their voters' expectations, but can reflect behavioral or contextual factors. First, pressure and job insecurity can affect judicial decisions. An emerging literature in legal and health studies has pointed out that judges are subject to stress in particular when their position is in jeopardy due to the risk of losing elections (Chamberlain and Miller, 2009; Lebovits, 2017; Maroney, 2019). Some studies suggest that judges' emotions can affect their decisions (for instance Eren and Mocan (2018) for the case of juvenile sentencing). Stress and psychological conditions are widely recognized as critical determinants of decision-making (Hammond, 2000) and some recent research has shown that behavioral biases in sentences might emerge at particular moments in the day where judges feel more stressed (Danziger et al. 2011). Second, sentencing variations could capture changes in the context in which decisions are made. This contextual effect could be due to the fact that several elections are often concomitant, for instance sheriff elections that may affect the type of crimes targeted and prosecuted. Determining whether sentencing cycles are driven by the strategic incentives of judges or behavioral and contextual factors is essential to understand how judges respond to elections and ultimately to assess the desirability of electoral scrutiny.

In this paper, we exploit several unique features of the criminal justice system in North Carolina that allow us to address the questions outlined above. First, we use an original identification strategy to document sentencing cycles. Second, and more importantly, this identification method allows us to determine the role of strategic behavior in determining the cycle. The North Carolina superior-court system is characterized by both judicial elections and forced judicial rotation. The state is divided into eight divisions, in turn divided into a variable number of districts. Judges are elected in a district (henceforth home-district)

but they cannot stay there permanently. Every six months, they are forced to rotate across districts according to a schedule determined by the state chief justice. This rotation takes place within electoral cycles lasting eight years. Another important feature is that elections are not held at the same time across districts.

We exploit these institutional features in our main empirical strategy. We use an event study where we measure the evolution of sentences over the electoral cycle. For each judge, we first distinguish between decisions made in her home-district and those made outside this district. The idea is that when elections approach, the behavioral factors (e.g. stress) faced by a judge do not depend on where she is currently working, whereas purely electoral incentives are present only in home-districts. We then distinguish between contested and non contested elections. Contextual effects, due to other elections being simultaneously held, should not depend on whether a judge's election is contested.

We use the universe of criminal decisions made in North-Carolina superior courts between 1998 and 2010.¹ For each sentence, we know the identity of the judge who made the decision, a set of characteristics of the defendant (including demographics and crimes committed) and the outcome of the trial. We find that judges increase sentences the year before the election, but only when sentencing in their home district. Thus, we do not find evidence of behavioral factors driving judicial decisions during pre-electoral periods. Exploiting differences between contested and retention elections, we find that there is no change in sentences when a judge's position is not contested. There does not seem to be evidence for a contextual effect. Overall, these results are consistent with an explanation of electoral sentencing cycles driven by judges' efforts to match what they perceive to be the voters local preferences, i.e. a demand for longer sentences.

The effect is sizable: the year before elections, sentences increase by about 10%. This result lies in between the effects of sentencing cycles estimated in previous literature. Our effect is slightly larger than that estimated for the state of Washington where Berdejo and Yuchtman (2013) found an increase of approximately 10% in sentencing over the electoral cycle focusing only on severe crimes (while we also consider smaller crimes), and smaller than Pennsylvania where Huber and Gordon (2004) found an increase of approximately 25% in sentencing over the electoral cycle.

Our interpretation of the result as a strategic reaction to electoral incentives is reinforced by the study of media coverage of these decisions. Indeed, using judges' citation in 38 North Carolinian local newspapers, we show that journals distributed in a judge's district of election are twice as likely to cover her decisions in this district than decisions made further away. This result suggests that voters are more likely to hear about judges' decisions in their

¹The same data is used in Abrams et. al. 2019 to study the question of adaptation to local norms.

district.

Finally, we show that the sentencing cycle is driven by judges who are less severe than the average and that it is concentrated on the most severe crimes. These results are consistent with a desire to use visible cases to signal toughness before an election (as in Cohen, Klement and Neeman 2013).

Our paper joins a recent literature studying the effects of judicial elections on lower courts sentencing decisions.² Early studies in this literature (Besley and Payne, 2005; Helland and Tabarrok 1999) used cross-state variation in electoral scrutiny to provide evidence consistent with judicial response to electoral pressure. Using cross-state evidence is, however, problematic to isolate the causal impact of electoral timing when the electoral cycle can be correlated with other time-varying factors.³ More recently, using data from Pennsylvania (Huber and Gordon 2004) look at the variation of sentencing over the electoral cycle exploiting within state variation. Their findings point to the existence of electoral cycles with sentences becoming more severe when elections approach. In another study Gordon and Huber (2007) exploit data from Kansas to investigate whether systems with retention versus nonpartisan elections show different patterns finding that electoral cycles are driven by counties with partisan elections.⁴ Also focusing on the difference between partisan and nonpartisan elections, Lim et al. (2015) show that greater media pressure increases sentences but only in districts where judges are elected in nonpartisan election.

Three recent papers are particularly related to our work. Using an event study analysis around the date of election, Berdejo and Yuchtman (2013) show that sentences are longer at the end of a judge's cycle compared to the beginning. Given that elected judges do not rotate they cannot however distinguish the strategic reaction from the other effects that coincide with the electoral cycle. Boston and Silveira (2019) also use sentencing data from North-Carolina's Superior Courts and study the effect of the transition from state-wide to district-level elections in 1996. They find that judges from liberal districts were more lenient than before, while those from moderately conservative districts assigned harsher sentences suggesting that elected judges strategically adapt their sentencing behavior to retain office. Dippel and Poyker (2019) investigate the generality of electoral sentencing cycles across US states. The authors replicate the findings of previous papers and found electoral cycles in NC as well as other previously studied states, but not in a number of previously unstudied

²A related literature has focused on state supreme court judges. See for instance Shepherd, 2009

³Looking at murder cases, Brooks and Raphael (2002) show that defendants are 15% more likely to be sentenced to death when the judge is in an election year.

⁴These results are consistent with Lim(2013) who studies differences in sentencing patterns among judges retained via partisan or retention elections, estimating a structural model with data from Kansas Lim(2013) shows that differences in the patterns of judges selected with different types of elections can be attributed both to selection on preferences and judges' responses to elections

states. The authors show that the strength of the electoral cycle depends on the degree of competitiveness of the judicial elections.

Our study complements this literature by providing evidence on the mechanisms driving electoral cycles and by showing how elections and rotation interact. In particular the fact that the electoral cycle is only present when the judge is sentencing in her home district, rules out behavioral factors such as stress, as a determinant of the cycle. While other studies (Danziger et al. 2011, Chen and Philippe, 2019) have shown that emotions may affect judicial decisions under particular circumstances, our findings show that rational strategic behavior can explain consistent variation of sentencing over the electoral cycle.

This paper is also connected to Abrams et. al. (2021) who, using the same data, provide evidence on local sentencing norms. Abrams et. al. (2021) show the existence of local sentencing norms by districts and gradual convergence to them of judges newly arrived in a district where they had never worked before. It also draws implications on the optimal mix of instruments to discipline judges, in particular elections and rotations. The current paper provides an additional element in that debate. We show that judicial rotation has the benefit of decreasing electoral sentencing cycles.

Our paper is more broadly connected to the literature on political business cycles, which shows how policy (for instance tax policy or public spending) appears to vary when elections approach.⁵ The papers in the literature show extensive evidence of a cycle for a variety of outcomes. However, establishing that this is due to a strategic political choice by politicians to maximize votes is, as in our context, difficult to show. While behavioral factors are unlikely to play a role in major political decisions, the context of elections can have an effect on policies other than pushing politicians to try and attract voters. For instance, local politicians might spend more before the end of their mandate to guarantee that some of their preferred policies are implemented before a possible electoral defeat. Alternatively, increased media focus might affect the incentives of politicians in electoral periods. What our paper shows, admittedly in the specific context of judicial elections, is that the main driver of the cycle does appear to be electoral incentives.

2 Institutional Setting

In North Carolina, the superior court has general trial jurisdiction on criminal matters for felony cases. The state is partitioned into 50 districts aggregated into eight divisions (see Figure 4 in Appendix A). Superior Court judges are elected officials who rotate districts every

⁵The literature is too large to be cited, starting from the seminal paper by Nordhaus (1975) and reviewed in Drazen (2001) and Franzese (2002).

six months (see Figure 5 in Appendix A that documents the patterns of rotation).⁶ They are elected for an eight year term. Elections take place at the district level and judges are administratively attached to their district of election (home-district henceforth). There is no term limit and a judge is potentially allowed to run in a different district in the next election, although only 2 judges have done so in our data. When a vacancy arises in the middle of a term, the governor fills the vacancy by appointment. The appointment is effective until the next general election.

During our observation period, elections were nonpartisan, even though observers point out that party affiliation is often evident during the campaign. The filing deadline for candidates is between 9 and 12 months before the election date. However, presumably, rumors of who might be candidates would circulate before filing, particularly in smaller or more rural districts. Primary election may take place 6 months before the general election. However, in our data, we observe that, when the incumbent is running, primaries virtually never happen.⁷

Elections are not held simultaneously across the state (see Figure 7 in Appendix A). In a given year some judges in certain districts will be up for reelection while other districts in the same division won't be running elections. Among districts running elections, some but not all elections will be contested. We exploit this feature when we estimate models with semester fixed effects.

Another actor of the judicial system is the district attorney who is also elected (with a four year term). Each district has a main district attorney who is in charge of a team of assistant district attorneys who prosecute the different cases. As in the rest of the U.S., the vast majority of criminal cases in North Carolina are resolved via plea bargain, in which the sentence is agreed to between the prosecution and defense. Even though the judge is often not directly involved in plea bargains, she is required to approve the sentence, and thus still exerts a great deal of influence over the outcome. Recent work in Abrams and Fackler (2018) shows that plea bargaining is often to the advantage of the defendant, although this varies dramatically by geography, crime type and race. Judicial decisions in North-Carolina have been subject to sentencing guidelines since 1994. To establish a sentence, the judge first determines the type of offense to which the case belongs. The judge then takes into account the offender's prior criminal record and the aggravating and mitigating factors of the case. Given these elements a case falls into a cell of a sentencing grid. The cells provides the boundaries of the minimum sentencing that the judge has to apply to the case.⁸ After having

⁶This rotation rule was established in the North Carolina Constitution of 1868. The main motivation for the rotation system is to avoid explicit or implicit capture by the local community (Bobbit, 1948)

⁷We only observe two such cases in our data.

⁸In practice, the judge has a quite large range of options, for instance an offender convicted for an

determined the sentence range, the judge determines the case disposition and whether the sentence is active, implying that the minimum sentence must be served, or not in which case the sentence can be suspended.

3 Data

Our data come from the North Carolina Administrative Office of the Courts and include the universe of felony cases decided in North-Carolina superior courts from 1998 to 2010. For each case, we know the week of the sentencing decision (i.e. the disposition date), the identities of the defendant, the judge and the assistant district attorney prosecuting the case. The data also include the main demographic characteristics of the defendant as well as the categories of crime for which the defendant is charged. The dataset includes 343,776 sentencing decisions with a final disposition date between 1998 and 2010.⁹

In the main analysis, the sentencing variable we focus on is the minimum sentence chosen by the judge following the procedure described in the previous section. If multiple cases are settled at the same time for the same defendant, we consider the maximum of the minimum sentences, i.e the decision made for the most severe offense, since in the case of multiple sentences the default is to have the sentences run concurrently. In Appendix B we provide further details on the dataset construction, variable definition and treatment of extreme values.

We also make use of the master schedule produced by the Chief Justice that prescribes which court each judge is officially assigned to each week. To guarantee that we correctly identify the judge dealing with a case, we restrict the data to observations where the judge is in the division recorded in the master schedule provided by the Chief Justice's administration. The identity of the judge is then used to match with the result of judicial elections. Finally, for each individual judge in our data, we collect information on the date of election and the electoral results, which is obtained from North Carolina's Board of Elections.

As we describe in the next section, our identification strategy is an event study using an eight years window around election dates. Thus, most of our analysis will only take into account elections that took place in 2002, 2004 or 2006 (for an election occurring in 2000 we would observe only 2 years of data before that). In the data we use to estimate our main

armed robbery and having a prior record of two offenses faces a minimum sentencing range between 79 and 97 months. The range can be shifted upward to a 97-121 month range if the judge finds aggravating circumstances or downward, to 58-78 months, if she finds mitigating circumstances.

⁹It is worth noting that our main unit of analysis (a case) is defined by aggregating all outstanding charges for a defendant that are disposed of at one time.

specification, we observe 95 elections, 60 of which are contested (63%), and 35 noncontested. In Table 1 we report some descriptive statistics, in the left columns for the entire sample, in the middle columns for the restricted sample used in our main analysis and in the last columns for the restricted sample, further limited to contested elections. In Panel A, we present descriptive statistics at the case level, while in Panel B, we give statistics at the judge level. Focusing on the middle column we observe that the average sentence is 386 days and that among the defendants, 53% are black and 16% women. Regarding judges, we have 157 judges, out of which 60 will face contested elections. Judges make approximately 300 decisions per year in the sample, slightly less around the time of elections (276 in general, 259 around contested elections).

4 Identification strategy

As discussed in the introduction, the goal of our empirical exercise is to provide evidence on the existence of electoral sentencing cycles and to determine what mechanisms drive these cycles if they exist. Our analysis exploits two features of the institutional setting in North Carolina. First, elections are not simultaneously run in all districts. Second, judges must rotate across districts. This allows us to compare a judge’s sentencing decisions in her home district with her decisions made in nonhome districts.

As a first step of our analysis, we estimate the following model restricting our data to the 8 years around judges’ election (4 years before and 4 years after):

$$\begin{aligned}
 S_{ijdt} = & \sum_{k=-3}^3 \alpha_k * 1_{Year=k} + \sum_{k=-3}^3 \beta_k * 1_{Year=k} * 1_{Home} \\
 & + \zeta_1 * 1_{Home} + \nu_{ElecPeriod,j} + \nu_d + \nu_c + \sigma * X_i + \tau * Z_d + \epsilon
 \end{aligned} \tag{1}$$

where S_{ijdt} is the sentence of offender i decided by judge j in district d at time t . $1_{Year=k}$ is a dummy equal to one if the decision is made in the year k before/after the judge’s election. k varies from -3 to $+3$ (excluding 0), -1 being the 12 months just before the election and $+1$ the 12 months immediately after. 1_{Home} is a dummy equal to one if the decision is made in the home district of judge j (i.e. the district where she runs for election). All parameters are measured with respect to comparison periods composed of sentences decided 4 years before/after the elections (baseline periods).¹⁰ ζ_1 captures the differences between

¹⁰Note that this specification does not include time fixed effects. Indeed, as the sample is restricted to electoral periods, time fixed effects would absorb a large share of the identifying variation.

baseline periods in home district with respect to nonhome district. $\nu_{ElecPeriod,j}$, ν_d and ν_c are fixed effects for judge*electoral periods (i.e. we include two fixed effects for one judge if we observe her during two different electoral periods), district and crime type respectively. X_i is a set of sociodemographic characteristics of the defendant (age, gender, race and number of past convictions). Z_d is a set of variables controlling for the distance to district attorney's elections in the district. Standard errors are clustered at the judge*district level.

The main parameters of interest are α_k and β_k . α_k measures the behavioral effect which should be present regardless of where the judge is sentencing. The underlying assumption is that the stress of potentially losing the judgeship in the election does not depend on where the judge is working but rather on the fact that a contested election is approaching. Coefficient β_k measures the strategic effect, under the assumption that voters pay less attention to what happens outside their district. We provide below some evidence on newspaper coverage of judges at home and away. Moreover, in cases decided in the home district, the victims and their network will be potential voters in elections where typically less than 20000 people cast a vote. Finally we point out that we are able to test this assumption in the data. Indeed the fact that we estimate α_k to be zero and β_k to be positive shows that strategic concerns are not present outside the home district.

We thus estimate equation 1 on all elections, contested elections only, i.e. where at least two candidates are running, and noncontested elections only. After estimating the effect separately for contested and noncontested elections we combine all the elections in the following single model.

$$\begin{aligned}
S_{ijdt} = & \sum_{k=-3}^3 \alpha_k * 1_{Year=k} + \sum_{k=-3}^3 \beta_k * 1_{Year=k} * 1_{Home} \\
& + \sum_{k=-3}^3 \gamma_k * 1_{Year=k} * 1_{Contested} + \sum_{k=-3}^3 \delta_k * 1_{Year=k} * 1_{Contested} * 1_{Home} \\
& + \zeta_1 * 1_{Home} + \nu_{ElecPeriod,j} + \nu_d + \nu_c + X_i + Z_d + \epsilon
\end{aligned} \tag{2}$$

where $1_{Contested}$ is a dummy equal to one if the election is contested and the other parameters are similar to those in equation 1.

In equation 2, α_k captures the overall cyclicity while β_k captures additional cyclicity in home districts. In addition, γ_k and δ_k capture the additional cyclicity, in general and in home district respectively, when the election is contested. In this model, behavioural response driven by the stress of the elections would be captured by γ_k with $k < 0$. Indeed we would expect to observe changes in sentences before contested elections in any district.

Strategic reactions driven by electoral motives would be captured by δ_k with $k < 0$. Indeed, this mechanism is only expected in home district before contested elections.

Contextual effects, particularly that judicial elections might be coordinated with other elections at the district level, should matter regardless of whether the election for the district judge is contested.¹¹ Thus finding $\beta_k = 0$ and $\delta_k > 0$ and for $k < 0$, would be evidence favoring the absence of contextual effects and arguing in favor of strategic motives for the sentencing cycles.

The main potential challenge to our identification strategy is linked to strategic case assignment. Suppose that judges are in a better position to influence their case assignment in their home district and, for some reason, have a preference for more severe cases.¹² In this case α_k would capture case selection rather than the judicial response to electoral pressure. Table 2 provides some reassuring evidence. For each case, we first compute the predicted sentence using the characteristics of the accused (age, sex, race, prior offenses) and crime dummies. We then estimate equation (1) and equation (2) using the predicted sentence as a dependent variable. Our main result will be to reproduce this analysis but using the real sentence as a dependent variable. We see in Table 2 that there are no coefficients significantly different from zero. In particular the coefficient β_{-1} is not significantly different from zero and tends to be smaller than β_{-2} .

The second potential challenge could be linked to the sorting of judges during electoral periods. We believe that this is unlikely to be a concern for two reasons. First, there is no indication that judges can affect their rotation schedule. This schedule is determined by the chief justice and special requests need to be made in order to change it. Second and more importantly, to affect the results we would need to have a very specific pattern of influence on the schedule: tougher judges would need to be the ones who manage to return to their home district just before election. Figure 6 provides some supportive evidence. The figure represents the probability of being in the home district over the electoral cycle. We see that judges are indeed more likely to be in their home district 1 semester before the election, but the pattern is exactly the same for noncontested election (for which the strategic motive should be absent) and for contested elections. This strongly suggests that strategic sorting over the cycle is not an issue.

The final potential challenge is the possibility that other factors influence sentencing away from home. One such factor is that the judge is less aware of the local sentencing practice and might learn over time, as shown in Abrams et al. (2021). However, this should not affect our identification strategy since the results in Abrams et al.(2021) show that there is

¹¹We do control in Z_i for the distance to district attorney's elections.

¹²This could fit with the idea that the judge wants visible cases to signal ability.

an effect of the time spent in a non-home district by judges on the absolute value of the distance between the sentence and the local norm, but not on the distance itself. The idea is that certain judges will converge from below to the local sentencing practice and others from above, so this does not affect the average sentence. Another effect is a behavioral effect depending on the higher distance between home and workplace when judges are away from their home district, but this is unlikely to matter specifically around election time.

5 Results

5.1 Sentencing variation over the electoral cycle

The results of our event study are presented in Figure 1. The figure plots the coefficients of the estimation of equation (2). We plot separately δ_k (black line), our main coefficients of interest, measuring how sentences given when the judge is sentencing at home vary in the years before/after judges' elections when this election is contested, as well as α_k , β_k and γ_k .

The figure clearly shows a significant spike in sentencing one year before the election. This spike is present only when the judge is facing a contested election and only if she is sentencing in her home district. This graphical representation has two main implications. First, it confirms that electoral cycles do exist. Second, as highlighted in the previous section, these results rule out certain mechanisms that could explain the sentencing cycle. The fact that when the judge is sentencing away from her home district, facing an election at home has no impact on sentencing, suggests that behavioral effects such as the stress generated by the election, should not play a role. Second, the fact that sentencing cycles are present only when the election is contested is incompatible with contextual effects, such as other elections occurring at the same time. Overall the most natural mechanism appears to be that the judge increases sentences to please voters.

These graphical results are confirmed in Table 3. In columns (1) to (3) we report the coefficients of the estimation of equation (1), on all observations in column (1), and separately for contested elections in column (2) and non-contested ones in column (3). Taken together, the results confirm what is visible in Figure 1: sentences increase the year before elections, but only when elections are contested and only when the judge is assigned to her home district.

In column (4) we present our preferred specification, estimating equation (2), with the full set of interactions between home and contested elections. We see that the only significant coefficient is one year before contested elections, when the judge is sentencing in her home district. In terms of magnitudes, sentences at that point are 29 days longer than those

outside electoral periods, representing approximately an increase of 10% in sentences.

The results are very robust. In column (5) we introduce semester fixed effects. In column (6) we avoid any composition effect by further restricting the sample to the events for which we observe decisions both before and after the election. This means that we exclude decisions made by newly elected judges, judges who lost the election or judges who only handle civil cases before or after the election. In contrast, in column (7), we keep all the observations in the sample. In practice, this means that all parameters are measured with respect to sentences decided 4 years before/after the elections and sentences decided by appointed or unidentified judges. The magnitude of our coefficients of interest barely changes.

Overall, the evidence reported above shows that criminal sentencing in North Carolina varies along the judicial electoral cycle and that the increase in sentencing in the months before the election date is mainly driven by strategic motives of judges and can thus be explained by voters' preferences (real or perceived) for tougher sentences. This type of preference is consistent with survey evidence (NCSC, 2006) suggesting that voters prefer tougher sentences, research in sociology showing that people tend to perceive actual sentencing as too lenient (Roberts, 2003; Stalans, 1993) and with other papers showing that elected officials spend more in enforcement before elections (Levitt, 1997). In Lim, Snyder and Stromberg (2015), the authors state that "there is substantial survey evidence that ordinary voters believe that criminal sentences are too lenient" and list some cases where controversies emerged on lenient sentences.

5.2 Mechanisms

Media coverage The difference in cyclicity between home and nonhome districts is only rational if voters have more chances to hear about decisions made in the former. To test for this assumption we provide some evidence on newspaper reporting relative to home versus nonhome districts. We obtained the portion of the data used in Lim, Snyder and Stromberg (2015) specific to North Carolina.¹³ The data measure by year, for the years 2004 and 2005, and by judge, the number of articles mentioning that particular judge, for a sample of 38 newspapers. In addition, we collected for each of these newspapers, the districts in which the newspaper circulates. Note that out of these 38 newspapers, 27 are sold in a single district. The combination of these data allows us to measure the number of articles from what we call "home newspapers", i.e. newspapers circulating in the district of election of the particular judge.

In terms of aggregate statistics, on average a judge has 12.23 articles per year that mention

¹³We thank in particular Claire Lim for helping us with this data.

her, out of which 6.48 are from home newspapers. This seems to indicate that voters indeed have access to information on their local judge. Moreover, as judges also made approximately 50% of their decisions in home districts, it seems that coverage is more related to the place of the case than to the origin of the judges. To more precisely examine the determinants of coverage we estimate the following specification¹⁴

$$N_{jnt} = \alpha \text{ casecovered}_{jnt} + \beta \text{ homepaper}_{nj} * \text{ casecovered}_{jn} + \zeta \text{ homepaper}_{nj} * \text{ casenotcovered}_{jnt} + \epsilon \quad (3)$$

where N_{jnt} is the number of articles in newspaper n mentioning judge j , $\text{ casecovered}_{jnt}$ are the number of cases sentenced by judge j in a district that is part of the coverage of newspaper n and homepaper_{nj} is a variable taking value 1 if judge j is elected in a district that is part of the coverage of newspaper n . We include judge*year fixed and newspaper fixed effects that capture, for instance, the total number of cases handled by a judge in a year. We estimate this model using a Poisson maximum likelihood estimation procedure.

The results are presented in Table 4. In columns (1) to (3) the outcome variable is N_{jnt} , the total number of articles in newspaper n mentioning judge j in year t , while in columns (4) to (6) the outcome variable is a restriction of N_{jnt} to the articles relative to murders. The results are very similar for the two outcome variables. We will therefore only comment on the results in columns (1) to (3). In column (1) we include only the variable $\text{ casecovered}_{jnt}$. We see that an increase in the number of cases covered by judge j in the zone of coverage of the newspaper significantly increases the number of articles on judge j written by that newspaper. This first result shows that newspapers tend to report more on local cases, an intuitive result. In column (2) we take into account the fact that the newspaper is a home newspaper. We therefore estimate equation (3). We see that coefficient β is not significantly different from 0. A case within the zone of coverage of the newspaper is treated the same way, regardless of whether the judge is a home judge or not. In contrast, coefficient ζ is positive and significantly different from 0. This implies that a judge is more

¹⁴In the ideal data set, observations would be at the case level. We could define the variable covered_{njt} that takes the value 1 if district d (district where judge j is sentencing at date t) is covered by newspaper n , $A_{jndt} \in \{0, 1\}$ measures whether an article was written in newspaper n on judge j for that particular case. In this case we would estimate the following model

$$A_{jndt} = \alpha \text{ covered}_{jnd} + \beta \text{ homepaper}_{ij} * \text{ covered}_{jnd} + \zeta \text{ homepaper}_{ij} * (1 - \text{ covered}_{jnd}) + \epsilon_{jndt}$$

the excluded categories are cases that are in districts not covered and sentenced by judges who are not elected in the coverage zone of the newspaper. Our current specification can be seen as an aggregation of the specification above at the level judge*newspaper*year. Given that we include judge*year fixed effects, $\text{ casenotcovered} * \text{ nothomepaper}$ is the excluded category.

likely to be reported on by her home newspaper, even if she is not sentencing at home. The key however is that ζ is smaller than α . We confirm this in column (3) where we restrict the data to observations jnt where newspaper n is the home newspaper for judge j (i.e. judge j is elected in a district that is covered by newspaper n). We estimate equation (3), but of course including only the variable $casecovered_{jnt}$ (recall that we have judge*year fixed effects, so that we cannot identify separately $casecovered_{jnt}$ and $casenotcovered_{jnt}$). We find that coefficient α is positive and significantly different from 0, showing that the judge is more likely to be covered by the home newspaper when she is sentencing at home.

Judges’ characteristics We now explore more in depth, what characteristics of the judge, make the sentencing cycle more likely. Cohen et. al. (2015) show that strategic sentencing should be more likely for judges who are usually lenient, as it becomes a way to signal toughness. We explore this link in Figure 2, where on the left panel we restrict the sample to judges below the median in severity, while in the right panel we focus on those above the median.¹⁵ Consistent with the results in Cohen et. al. (2015), we find that the cycle is only present for judges who are usually less severe.

Targeted crimes The second dimension we explore is how the results vary by severity of the crime. In Figure 3 we separate crimes by quartiles of the predicted sentence and present the coefficient for one year before election and at home for contested elections. We see that the effect is fully concentrated on the top quartile representing more serious offenses. Those crimes are more likely to be covered: 49% of the articles in the newspapers we had access to were about murdered while they represent only 1.6% of court cases. Overall this draws a picture where judges use more salient cases to signal their toughness.

Heterogeneity In Appendix Table 6, we explore the heterogeneity in the results based on observable case characteristics. We separately estimate equation (2) for different subgroups. We present the full set of coefficients. We show that the increase in sentences before elections is stronger for black and male defendants.¹⁶ We also observe some heterogeneity with respect to the type of offense: while sentences for property crimes are not affected over the electoral cycle, sentences for violent crime and drugs vary more over the electoral cycle. The sentences for violent crimes increase by 80 days the month before contested elections in the home

¹⁵A judge’s severity is proxied by its fixed effect in a regression of sentences on crime fixed effects and defendants’ characteristics using a sample restricted to decisions made more than 3 years from the time of elections (i.e. excluding the period between $t-3$ and $t+3$).

¹⁶This is consistent with Park (2017) who shows that sentences increase more for black defendants.

district (compared to an average sentence of 740 days in that category) and the sentences for drugs by 30 days (compared to an average of 313 days).

Other judicial actors. In Appendix C we explore electoral cycles among other key judicial actors: district attorney. We could expect electoral pressure to be less intense for DAs as their actions are less visible and, more importantly, the assistant DAs, who make most decisions, are not themselves elected. As DAs are not rotating we could only compare contested vs non-contested elections. Results (presented in Appendix Table 8) indicate that sentences slightly increase one semester before the election but that there is no difference between contested and non-contested elections.

6 Conclusions

In this paper, we exploit the unique features of the North Carolina justice system to show that electoral sentencing cycles exist and to explain why this is the case. Sentences increase on the order of 10% at the end of the electoral cycle, and that this increase is driven by judges' strategic attempt to attract voters. The fact that sentences given outside the home district do not react to the electoral cycles also indirectly shows that voters do not use (or are not perceived to use) sentencing decisions made away from their jurisdiction to form beliefs about the judges. This could be due to the structure of media reporting or to the cognitive biases of voters.

This result feeds into the discussion of what is the best method to discipline judges: rotation, elections or sentencing guidelines. Our findings, showing that increases in sentences are concentrated in home districts and in the months preceding the elections, suggest that the effect of elections on sentencing inflation can be mitigated by allocating judges outside their electoral district in the months just before elections. However rotation comes at the cost of potentially decreased accountability. More generally, our findings can be informative for a large literature on political cycles among elected officials. As in the case of judges, reducing the impact of the decisions made close to election time by elected officials and directly affecting their constituencies may mitigate increases in spending typically observed at the end of electoral cycles.

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7 Figures and tables

Figure 1: Main effects

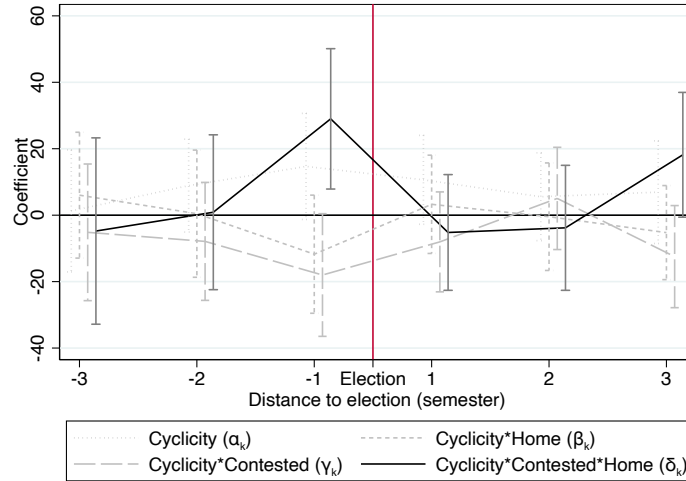
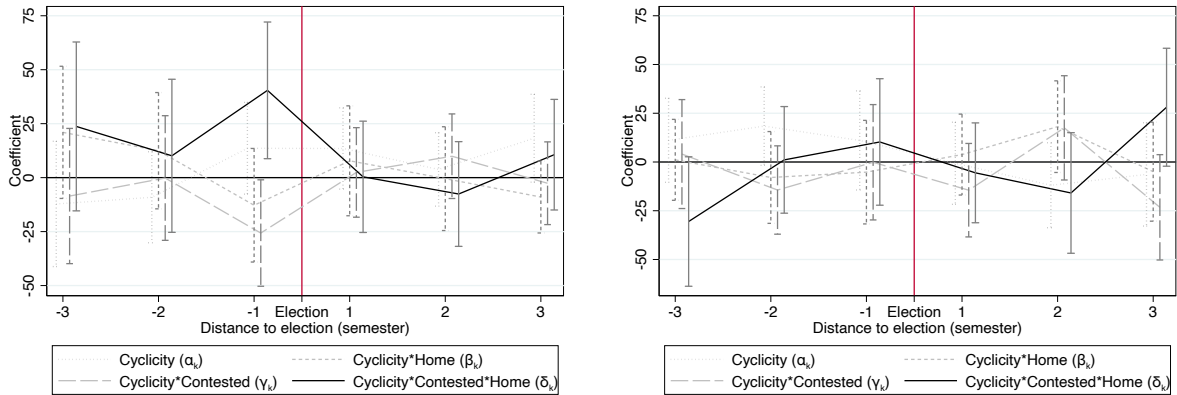


Figure 2: Main effects by judges' severity



Note: Judges less severe than average on the left, more severe than average on the right.

Figure 3: Effect of election on crime by severity

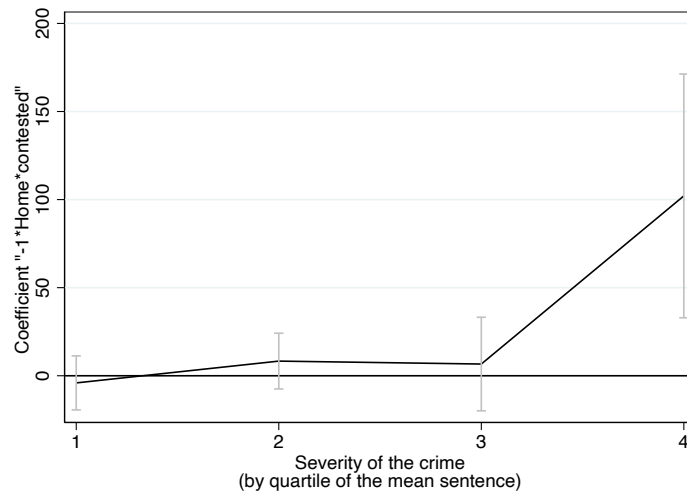


Table 1: Descriptive statistics

	Original dataset		4years before-after All elections		4years before-after Contested elections	
	Mean	Sd	Mean	Sd	Mean	Sd
Panel A: by judicial decision						
Home district	.51	.5	.54	.5	.51	.5
Tenure	9.83	6.92	9.82	6.98	8.92	6.18
Prison (days)	382.3	505.69	386.25	507	384.37	500.22
First offense	.46	.5	.45	.5	.45	.5
Woman	.16	.37	.16	.36	.16	.37
Black	.53	.5	.53	.5	.52	.5
Minor	.04	.19	.04	.19	.04	.19
Age	30.99	10.43	31.1	10.39	31.05	10.34
Charge category	8.21	2.03	8.19	2.04	8.2	2.03
Property	.22	.41	.21	.41	.22	.41
Violent	.22	.42	.22	.42	.23	.42
Drugs	.27	.44	.27	.44	.27	.44
Fraud	.13	.33	.13	.33	.13	.33
Other	.13	.33	.12	.33	.12	.32
N	343660		172033		95005	
Panel B: by judge						
Nb case	2189	1802	1842	1200	1592	1170
Nb case per year	298	171	276	182	259	179
Nb judge	157		94		60	

Table 2: Balancing check: effect on predicted sentence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	Contested	Non-Contested	All	All-Semester fe	All-No loser/New	All-All observations
-3	11.1 (9.0)	3.8 (13.1)	11.4 (10.2)	13.2 (10.2)	13.2 (10.2)	15.0 (10.2)	4.8 (9.6)
-2	7.6 (8.1)	-0.7 (9.8)	11.6 (13.1)	11.2 (13.0)	11.2 (13.0)	11.9 (13.1)	3.4 (12.3)
-1	6.2 (8.5)	1.9 (12.6)	5.3 (11.3)	5.5 (10.8)	5.5 (10.8)	4.7 (10.8)	-1.4 (9.2)
+1	9.0 (6.5)	4.0 (9.3)	4.1 (9.5)	8.3 (8.7)	8.3 (8.7)	4.9 (9.0)	0.5 (8.0)
+2	7.0 (6.9)	1.7 (9.8)	8.8 (8.8)	8.4 (8.5)	8.4 (8.5)	5.5 (9.0)	0.7 (7.9)
+3	6.5 (7.5)	2.1 (10.5)	6.6 (10.4)	6.5 (10.0)	6.5 (10.0)	6.5 (10.9)	-1.9 (9.1)
-3 * Home	-3.8 (11.0)	5.6 (15.1)	-7.9 (15.1)	-6.8 (14.3)	-6.8 (14.3)	-8.9 (14.4)	4.7 (13.8)
-2 * Home	13.3 (10.5)	19.5 (13.0)	10.0 (16.9)	12.4 (15.7)	12.4 (15.7)	11.0 (15.9)	20.3 (16.3)
-1 * Home	10.7 (10.6)	12.3 (15.2)	14.5 (15.0)	16.5 (13.3)	16.5 (13.3)	16.1 (13.5)	25.2** (12.5)
+1 * Home	5.9 (8.4)	8.4 (11.2)	11.6 (13.0)	10.7 (11.1)	10.7 (11.1)	16.9 (11.5)	19.1 (12.3)
+2 * Home	5.2 (9.0)	18.7 (12.6)	-4.2 (12.6)	-1.4 (10.7)	-1.4 (10.7)	-0.9 (11.6)	7.1 (9.6)
+3 * Home	-0.5 (9.2)	-0.6 (12.0)	7.1 (13.9)	8.0 (12.9)	8.0 (12.9)	3.3 (14.2)	16.8 (13.5)
-3 * Contested				-3.8 (15.3)	-3.8 (15.3)	-1.8 (16.4)	1.7 (15.0)
-2 * Contested				-6.3 (15.5)	-6.3 (15.5)	-9.4 (15.8)	0.2 (15.2)
-1 * Contested				1.2 (15.0)	1.2 (15.0)	2.3 (15.3)	5.1 (13.5)
+1 * Contested				1.1 (11.8)	1.1 (11.8)	7.9 (13.1)	6.4 (11.6)
+2 * Contested				-2.4 (11.2)	-2.4 (11.2)	0.6 (12.9)	3.0 (10.0)
+3 * Contested				-0.2 (12.5)	-0.2 (12.5)	1.4 (14.2)	4.7 (11.1)
-3* Home * Contested				5.9 (18.8)	5.9 (18.8)	5.2 (19.5)	1.3 (19.1)
-2* Home * Contested				1.0 (18.5)	1.0 (18.5)	4.5 (18.5)	-0.1 (20.0)
-1* Home * Contested				-11.0 (17.4)	-11.0 (17.4)	-11.0 (17.5)	-12.8 (17.6)
+1* Home * Contested				-9.0 (13.9)	-9.0 (13.9)	-15.4 (14.5)	-9.8 (15.3)
+2* Home * Contested				12.9 (13.4)	12.9 (13.4)	4.8 (14.8)	8.5 (13.4)
+3* Home * Contested				-15.1 (15.3)	-15.1 (15.3)	-16.3 (17.7)	-16.7 (15.4)
Observations	163525	90808	72717	163525	163525	144058	246589

Note: This table present in columns (1) to (3) the coefficients α_k (first 6 rows) and β_k (next 6 rows) of the estimation of equation (1). In column (1) the full sample is used, column (2) we restrict to the contested elections while in column (3) we restrict to non-contested elections. In columns (4)-(7) we present the estimation of equation (2). We present the coefficients α_k (first 6 rows), β_k (next 6 rows), γ_k (next 6 rows) and δ_k (last 6 rows). We add semester fixed effects in column (5). In column (6) we restrict the sample to the events for which we observe decisions both before and after the election. In column (7), we keep all the observations in the sample. *Home* is an indicator variable taking value 1 if the judge is sentencing in her home district and *Contested* is an indicator variable taking value 1 if the judge is facing a contested election. Standard errors are clustered at the judge*district level and reported in parentheses. Main coefficients of interest in bold.

Table 3: Main results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	Contested	Non-Contested	All	All-Semester fe	All-No loser/New	All-All observations
-3	-1.5 (9.1)	-3.8 (12.4)	-0.7 (14.7)	1.3 (11.2)	2.7 (11.9)	9.0 (12.8)	1.1 (7.5)
-2	4.5 (6.3)	4.5 (9.0)	8.4 (10.0)	8.9 (8.5)	6.1 (8.6)	9.5 (8.8)	5.5 (7.8)
-1	5.2 (7.4)	-0.5 (9.7)	14.7 (12.7)	14.7 (9.7)	13.5 (10.5)	18.3* (10.7)	14.9* (7.7)
+1	6.5 (6.9)	0.2 (9.4)	9.4 (11.5)	10.6 (8.2)	9.1 (8.8)	6.5 (9.5)	10.3 (6.5)
+2	8.8 (5.5)	7.6 (7.2)	6.7 (8.8)	5.5 (8.1)	3.2 (8.5)	4.6 (8.3)	7.6 (7.2)
+3	-0.1 (8.7)	-8.9 (11.9)	9.6 (13.8)	6.9 (9.4)	7.0 (10.5)	3.5 (11.3)	4.2 (6.5)
-3 * Home	3.4 (9.4)	0.0 (14.3)	8.5 (11.7)	6.0 (11.6)	4.8 (11.4)	2.5 (11.5)	2.7 (10.0)
-2 * Home	1.3 (9.2)	1.8 (12.2)	-0.2 (12.9)	0.4 (11.7)	3.4 (11.4)	-3.4 (12.0)	-1.1 (11.8)
-1 * Home	3.2 (8.3)	17.4 (11.2)	-12.1 (13.7)	-11.7 (10.8)	-10.2 (10.7)	-14.3 (11.3)	-13.3 (10.1)
+1 * Home	0.7 (7.2)	-3.2 (9.9)	4.3 (10.7)	3.3 (9.0)	5.8 (9.2)	2.7 (9.7)	0.8 (8.7)
+2 * Home	-3.4 (7.4)	-6.7 (10.1)	-2.2 (10.4)	-0.5 (9.9)	-1.4 (10.2)	-3.5 (10.4)	-2.3 (9.6)
+3 * Home	5.1 (7.3)	15.0 (10.6)	-6.1 (9.1)	-5.2 (8.6)	-4.3 (8.9)	-11.0 (8.9)	-5.7 (8.6)
-3 * Contested				-5.2 (12.5)	-5.3 (13.1)	-0.0 (13.0)	-3.9 (11.9)
-2 * Contested				-7.9 (10.8)	-7.7 (10.8)	-4.6 (10.9)	-8.2 (10.2)
-1 * Contested				-18.0 (11.3)	-17.7 (11.6)	-15.3 (11.4)	-20.1* (10.6)
+1 * Contested				-8.0 (9.2)	-8.7 (9.5)	-7.1 (10.0)	-13.1 (8.6)
+2 * Contested				5.0 (9.4)	3.0 (9.8)	3.4 (10.0)	0.7 (8.5)
+3 * Contested				-12.5 (9.4)	-13.0 (9.8)	-9.6 (10.4)	-15.0* (8.7)
-3* Home * Contested				-4.8 (17.1)	-2.3 (17.1)	-8.1 (17.3)	-3.2 (17.0)
-2* Home * Contested				0.9 (14.2)	-3.4 (13.9)	-2.4 (14.3)	2.9 (14.5)
-1*Home*Contested				29.0** (12.9)	26.7** (13.0)	28.0** (13.0)	34.4*** (12.9)
+1* Home * Contested				-5.2 (10.6)	-8.4 (10.7)	-6.0 (11.4)	-0.3 (11.1)
+2* Home * Contested				-3.8 (11.5)	-1.1 (11.9)	5.8 (12.8)	-2.1 (11.7)
+3* Home * Contested				18.2 (11.5)	17.4 (11.5)	13.9 (12.7)	20.0* (11.7)
Observations	163438	90733	72642	163438	163438	143975	246494

Note: This table present in columns (1) to (3) the coefficients α_k (first 6 rows) and β_k (next 6 rows) of the estimation of equation (1). In column (1) the full sample is used, column (2) we restrict to the contested elections while in column (3) we restrict to non-contested elections. In columns (4)-(7) we present the estimation of equation (2). We present the coefficients α_k (first 6 rows), β_k (next 6 rows), γ_k (next 6 rows) and δ_k (last 6 rows). We add semester fixed effects in column (5). In column (6) we restrict the sample to the events for which we observe decisions both before and after the election. In column (7), we keep all the observations in the sample. *Home* is an indicator variable taking value 1 if the judge is sentencing in her home district and *Contested* is an indicator variable taking value 1 if the judge is facing a contested election. Standard errors are clustered at the judge*district level and reported in parentheses. Main coefficients of interest in bold.

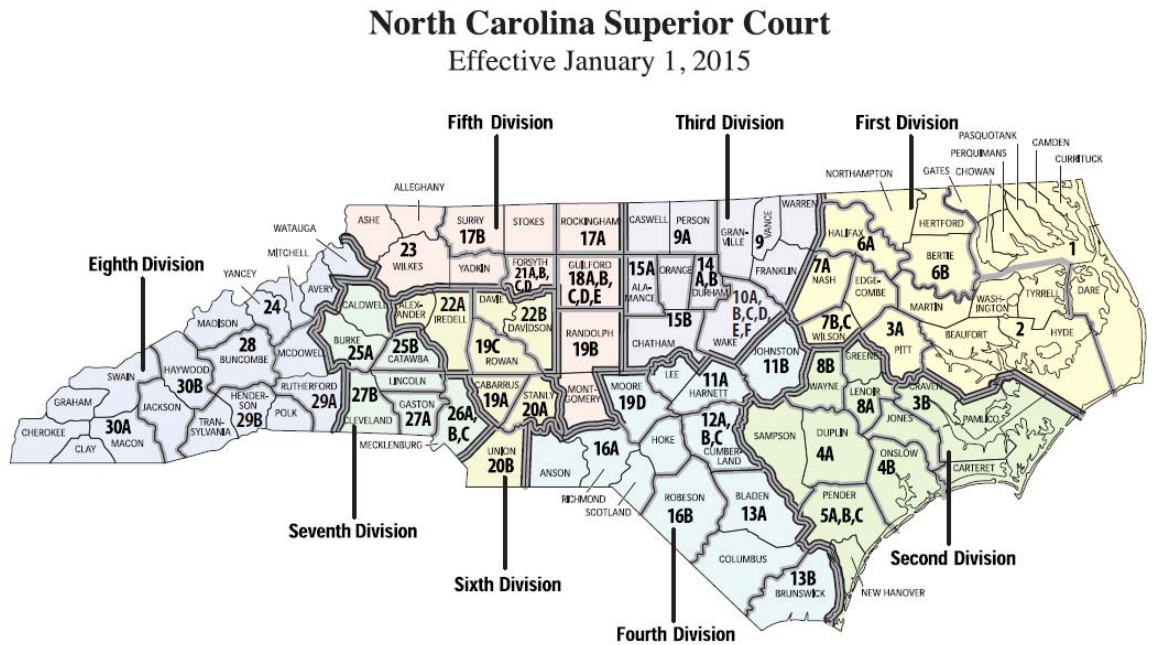
Table 4: Coverage by newspapers of sentencing

	(1)	(2)	(3)	(4)	(5)	(6)
	articles	articles	articles	art. murder	art. murder	art. murder
casecovered	0.009*** (0.001)	0.011*** (0.002)	0.007*** (0.002)	0.013*** (0.002)	0.018*** (0.004)	0.007*** (0.002)
casecovered * homepaper		-0.002 (0.002)			-0.006* (0.003)	
casenotcovered * homepaper		0.005*** (0.002)			0.005*** (0.002)	
Observations	6270	6270	920	6270	6270	920
Restriction to home newspapers			✓			✓
Mean dep variable	1.1	1.1	2.8	.54	.54	2.3

Note: This table presents the results of the estimation of equation 3, using a Poisson maximum likelihood estimator. In columns (1) to (3) the dependent variable is the number of articles in newspaper n mentioning judge j . In columns (4) to (6) the dependent variable is the number of articles on murders. The variable $casecovered_{jnt}$ are the number of cases sentenced by judge j in a district which is part of the coverage of newspaper n , $homepaper_{nj}$ is a variable taken value 1 if judge j is elected in a district which is part of the coverage of newspaper n . We include judge*year and newspaper fixed effects and cluster standard errors at the newspaper level.

Appendix A (Online Appendix not for publication)

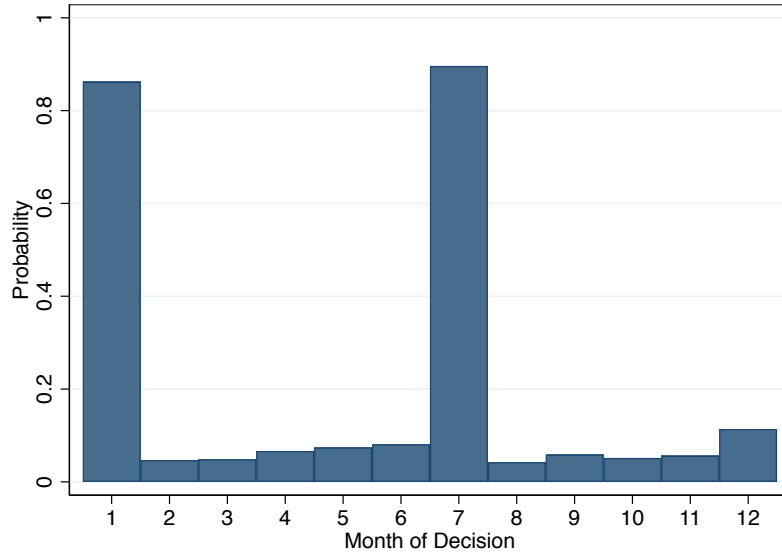
Figure 4: Map of North Carolina Judicial districts



Note: Districts that have more than one letter associated with the district number (i.e., 10A, B, C, D) are divided into separate districts for electoral purposes. For administrative purposes, they are combined into a single district.

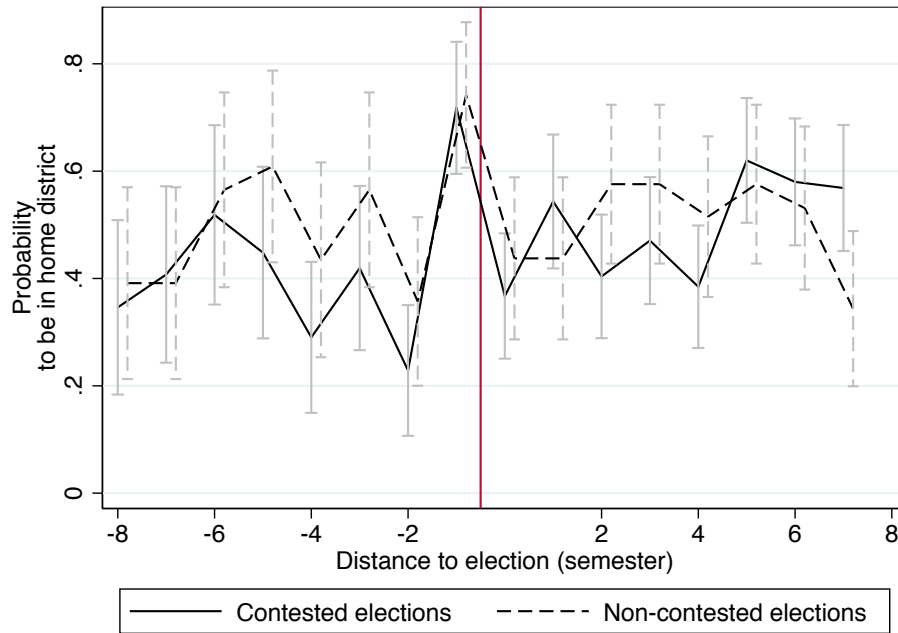
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Figure 5: Probability of moving per month, using data from the master schedule.



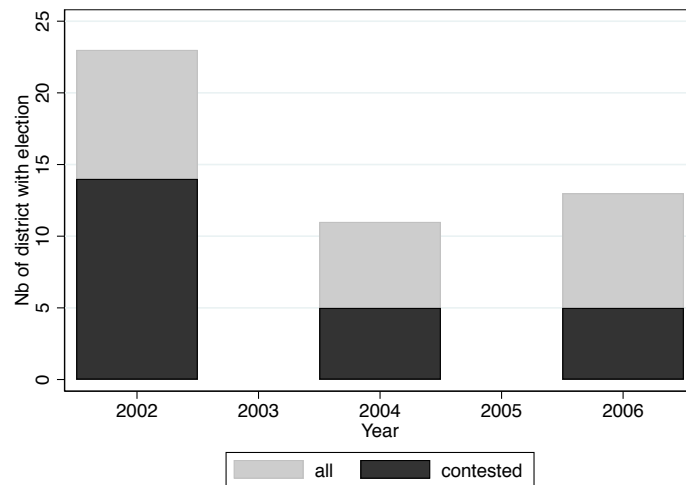
Note: A move is defined as a change in the district where the majority of decisions are made by a judge in a month.

Figure 6: Probability of being in home district



Note: This figure plots the probabilities that judges take the majority of their decisions in their home district by semester and election type. The sample is restricted to 4 years before and 4 years after judges' elections.

Figure 7: Number of districts holding elections in any given year.



Note: A district is considered as holding an election during one year if at least one identified judge in the database is running for it.

Table 5: Effect on the probability of conviction

	(1) All	(2) Contested	(3) Non-Contested	(4) All	(5) All-Semester fe	(6) All-No loser/New	(7) All-All observations
-3	0.000 (0.004)	-0.005 (0.005)	0.010 (0.007)	0.004 (0.005)	0.001 (0.005)	0.007 (0.005)	0.001 (0.004)
-2	-0.000 (0.003)	-0.004 (0.004)	0.004 (0.004)	0.002 (0.004)	0.003 (0.004)	0.003 (0.004)	0.002 (0.004)
-1	-0.004 (0.003)	-0.010** (0.004)	0.002 (0.005)	0.000 (0.004)	-0.004 (0.005)	0.002 (0.005)	-0.003 (0.004)
+1	-0.005* (0.003)	-0.009** (0.004)	0.001 (0.005)	0.001 (0.003)	0.001 (0.004)	-0.001 (0.004)	0.002 (0.003)
+2	0.005** (0.002)	0.003 (0.003)	0.011*** (0.004)	0.010*** (0.003)	0.008** (0.003)	0.011*** (0.004)	0.008*** (0.003)
+3	-0.000 (0.004)	-0.003 (0.005)	-0.000 (0.007)	0.002 (0.004)	0.005 (0.005)	0.001 (0.005)	0.003 (0.003)
-3*Home	-0.002 (0.004)	-0.002 (0.006)	-0.005 (0.005)	-0.003 (0.005)	-0.001 (0.005)	-0.003 (0.005)	-0.002 (0.004)
-2*Home	-0.003 (0.004)	-0.000 (0.005)	-0.009* (0.005)	-0.007 (0.005)	-0.008 (0.005)	-0.008* (0.005)	-0.006 (0.005)
-1*Home	0.001 (0.003)	0.005 (0.005)	-0.002 (0.004)	-0.003 (0.004)	-0.001 (0.004)	-0.003 (0.004)	-0.002 (0.004)
+1*Home	-0.001 (0.003)	0.003 (0.005)	-0.006 (0.005)	-0.005 (0.004)	-0.005 (0.004)	-0.005 (0.004)	-0.005 (0.004)
+2*Home	-0.006** (0.003)	-0.004 (0.004)	-0.011*** (0.004)	-0.011*** (0.004)	-0.010*** (0.004)	-0.010** (0.004)	-0.010*** (0.004)
+3*Home	-0.001 (0.003)	-0.000 (0.004)	-0.003 (0.004)	-0.003 (0.004)	-0.002 (0.004)	-0.004 (0.005)	-0.003 (0.004)
-3*Contested				-0.007 (0.006)	-0.004 (0.006)	-0.009 (0.006)	-0.004 (0.006)
-2*Contested				-0.006 (0.005)	-0.005 (0.005)	-0.007 (0.005)	-0.004 (0.005)
-1*Contested				-0.008 (0.005)	-0.009* (0.005)	-0.009* (0.005)	-0.007 (0.005)
+1*Contested				-0.011** (0.004)	-0.010** (0.004)	-0.009** (0.005)	-0.009** (0.004)
+2*Contested				-0.009** (0.004)	-0.008** (0.004)	-0.012*** (0.004)	-0.007* (0.004)
+3*Contested				-0.006 (0.004)	-0.005 (0.004)	-0.006 (0.005)	-0.004 (0.004)
-3*Home*Contested				0.001 (0.007)	-0.001 (0.007)	0.003 (0.007)	-0.001 (0.007)
-2*Home*Contested				0.008 (0.006)	0.009 (0.006)	0.009 (0.006)	0.008 (0.007)
-1*Home*Contested				0.007 (0.005)	0.008 (0.006)	0.007 (0.005)	0.008 (0.006)
+1*Home*Contested				0.008 (0.005)	0.008 (0.005)	0.004 (0.006)	0.008 (0.005)
+2*Home*Contested				0.009** (0.004)	0.009** (0.004)	0.010* (0.005)	0.010** (0.005)
+3*Home*Contested				0.004 (0.005)	0.002 (0.005)	0.005 (0.006)	0.003 (0.005)
Observations	163438	90733	72642	163438	163438	143975	246494

Note: This table present the effect of elections on the probability that an offender is found guilty. Columns (1) to (3) present the coefficients α_k (first 6 rows) and β_k (next 6 rows) of the estimation of equation (1). In column (1) the full sample is used, column (2) we restrict to the contested elections while in column (3) we restrict to non-contested elections. In columns (4)-(7) we present the estimation of equation (2). We present the coefficients α_k (first 6 rows), β_k (next 6 rows), γ_k (next 6 rows) and δ_k (last 6 rows). We add semester fixed effects in column (5). In column (6) we restrict the sample to the events for which we observe decisions both before and after the election. In column (7), we keep all the observations in the sample. *Home* is an indicator variable taking value 1 if the judge is sentencing in her home district and *Contested* is an indicator variable taking value 1 if the judge is facing a contested election. Standard errors clustered at the judge*district level and reported in parentheses. Main coefficients of interest in bold.

Table 6: Heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	First-offender	Recidivist	Female	Male	Black	Not Black	Property	Violence	Drug	Other
-3	8.6 (13.1)	3.8 (15.8)	15.1 (18.5)	-0.1 (11.7)	6.0 (16.3)	0.6 (15.2)	-24.2 (20.9)	12.1 (38.0)	32.2** (15.9)	-13.5 (14.8)
-2	11.4 (11.3)	14.2 (10.9)	-16.0 (11.6)	12.6 (10.0)	13.6 (11.6)	3.1 (12.7)	0.6 (14.3)	51.4** (25.0)	-9.8 (11.7)	3.4 (11.9)
-1	32.5** (12.8)	5.6 (11.8)	3.0 (14.2)	15.3 (10.7)	14.9 (10.7)	15.8 (13.2)	-21.6 (18.2)	40.7 (28.3)	45.9*** (13.5)	-13.6 (11.6)
+1	2.9 (9.7)	21.8** (10.7)	14.8 (13.9)	9.0 (9.5)	26.8** (10.6)	-4.8 (11.5)	-9.0 (15.0)	37.7 (25.1)	3.0 (10.9)	4.8 (11.6)
+2	12.8 (11.9)	3.9 (10.0)	6.0 (14.5)	5.9 (9.0)	14.2 (9.3)	-1.9 (11.9)	-9.1 (13.8)	11.8 (19.5)	20.7* (11.2)	-4.4 (9.5)
+3	-24.2* (12.8)	31.7** (12.6)	4.7 (18.5)	6.1 (10.2)	11.3 (12.3)	3.2 (13.3)	2.4 (16.7)	15.6 (29.9)	-7.6 (14.9)	14.7 (14.6)
-3*Home	9.9 (13.6)	3.0 (15.6)	-18.9 (18.3)	9.0 (12.9)	-1.2 (18.3)	3.7 (14.1)	14.7 (21.4)	9.9 (37.9)	-3.9 (15.0)	-8.0 (14.9)
-2*Home	-11.5 (14.7)	1.9 (15.6)	20.8 (13.7)	-3.0 (13.0)	-7.3 (13.0)	11.3 (17.7)	1.5 (17.7)	-37.6 (31.0)	22.8 (13.9)	-2.7 (13.3)
-1*Home	-11.8 (13.2)	-12.7 (14.6)	8.0 (15.6)	-13.9 (12.5)	-28.5** (12.9)	9.5 (13.1)	20.0 (18.9)	-41.8 (31.1)	-27.8** (15.9)	14.0 (10.5)
+1*Home	6.2 (11.4)	-6.5 (12.6)	6.3 (12.6)	3.4 (10.3)	-11.9 (11.1)	15.9 (13.4)	30.2* (17.3)	-42.5* (23.6)	1.3 (13.1)	23.4* (12.6)
+2*Home	14.9 (13.1)	-13.5 (12.7)	16.0 (16.7)	-4.1 (11.2)	-14.2 (11.9)	13.5 (13.2)	-3.8 (18.6)	12.6 (24.0)	-7.6 (15.1)	11.4 (11.1)
+3*Home	17.8 (11.4)	-24.3** (12.0)	-6.5 (17.1)	-5.3 (9.4)	1.7 (11.6)	-14.9 (11.9)	-5.1 (14.5)	5.1 (24.0)	-20.4 (14.3)	-1.9 (14.1)
-3*Contested	1.6 (14.9)	-16.7 (17.8)	-13.5 (17.6)	-3.0 (13.6)	-9.9 (19.1)	-3.3 (15.5)	1.5 (20.6)	9.0 (42.5)	-36.6** (17.6)	5.5 (13.3)
-2*Contested	-17.3 (14.5)	-8.6 (15.3)	7.9 (15.2)	-9.4 (12.5)	-13.7 (15.9)	-1.3 (15.8)	-7.1 (18.3)	-46.5 (31.8)	-6.2 (14.0)	17.4 (15.8)
-1*Contested	-25.1* (14.0)	-17.0 (14.2)	-1.0 (15.8)	-19.3 (12.7)	-21.3 (13.7)	-15.1 (14.7)	17.4 (21.1)	-51.0 (35.0)	-38.8** (15.5)	5.5 (14.4)
+1*Contested	-5.8 (10.8)	-21.8* (12.8)	-19.5 (14.5)	-6.0 (10.7)	-17.7 (12.2)	0.1 (12.2)	10.8 (15.7)	-54.4** (25.6)	-14.9 (11.8)	18.8 (12.7)
+2*Contested	0.5 (13.5)	3.5 (12.0)	-15.3 (15.7)	7.2 (10.4)	-0.3 (11.7)	8.2 (13.2)	3.5 (15.0)	6.3 (24.9)	-11.2 (13.6)	7.9 (10.8)
+3*Contested	3.1 (12.2)	-23.3* (12.3)	-10.8 (17.4)	-11.6 (10.3)	-2.6 (12.9)	-25.0** (12.6)	-5.0 (16.0)	-36.9 (24.4)	-15.7 (13.5)	0.6 (13.1)
-3*Home*Contested	-0.7 (19.2)	-8.2 (22.2)	11.5 (23.0)	-6.7 (18.3)	1.7 (23.7)	1.3 (20.9)	7.7 (25.7)	-49.8 (52.1)	22.2 (19.7)	-0.1 (18.5)
-2*Home*Contested	7.1 (18.7)	3.7 (19.5)	-17.2 (18.7)	3.0 (15.6)	-2.0 (18.8)	2.0 (20.2)	16.8 (21.1)	29.2 (40.7)	-15.9 (16.4)	-10.7 (18.8)
-1*Home*Contested	25.3 (15.9)	30.8* (17.2)	-15.1 (19.8)	35.7** (14.8)	39.5** (16.8)	14.9 (16.0)	3.7 (24.1)	80.6** (38.4)	30.3* (17.8)	-8.4 (15.7)
+1*Home*Contested	-9.0 (14.2)	11.0 (16.4)	-18.9 (16.8)	-2.8 (11.9)	5.3 (13.6)	-15.5 (14.6)	-24.7 (18.5)	47.6 (29.6)	3.0 (15.2)	-37.5** (15.0)
+2*Home*Contested	-8.4 (15.2)	3.9 (15.0)	2.4 (19.0)	-2.9 (13.1)	-5.3 (14.1)	-1.6 (15.4)	17.7 (20.0)	-30.9 (30.9)	-5.3 (16.2)	0.1 (13.4)
+3*Home*Contested	3.5 (14.5)	24.8* (13.9)	17.3 (20.3)	18.3 (12.7)	-2.2 (15.2)	41.5*** (14.2)	22.1 (17.1)	5.8 (31.7)	30.4* (16.4)	11.4 (16.4)
Observations	72940	90431	25658	137717	87412	75947	34712	36879	44853	40310
Mean outcome	294	461	242	413	408	361	292	742	313	224

Note: This table present in columns (1) to (3) the coefficients α_k (first 6 rows) and β_k (next 6 rows) of the estimation of equation (1). In column (1) the full sample is used, column (2) we restrict to the contested elections while in column (3) we restrict to non-contested elections. In columns (4)-(7) we present the estimation of equation (2). We present the coefficients α_k (first 6 rows), β_k (next 6 rows), γ_k (next 6 rows) and δ_k (last 6 rows). We add semester fixed effects in column (5). In column (6) we restrict the sample to the events for which we observe decisions both before and after the election. In column (7), we keep all the observations in the sample. *Home* is an indicator variable taking value 1 if the judge is sentencing in her home district and *Contested* is an indicator variable taking value 1 if the judge is facing a contested election. Standard errors are clustered at the judge*district level and reported in parentheses. Main coefficients of interest in bold.

Appendix B (Online Appendix not for publication): Variable Description and Data Set Construction

This contains the same material as Appendix E in Abrams et. al. (2019) where we use the same data.

Case Definition: Charge and Sentence

The first step of our analysis is a case definition. Since a criminal case is often comprised of multiple charges for a single defendant, and our focus is on overall sentencing for a case, we build on the procedure described in Abrams and Fackler (2018) in identifying cases with the same criminal and disposition date, and we define a case as a unique person-date of disposition. Treating a multiple charges case as a single unit implies that we must decide which charge to keep. We proceed as follows: we first define the lead charge of an incident as the charge with the highest associated sentence length. Our main sentencing variable is then defined as the minimum sentence determined by the judge for the lead charge. As we have specified in the main text, when a defendant is found guilty to a felony, North Carolina imposes a sentencing range. If the judge determines the sentence should be active, the defendant is required to serve the full minimum of the range, and may serve less than the maximum with good behavior. The final active sentence is the main variable used in our analysis. It is worth noting that, in order to deal with the outliers in sentences, in our analysis we winsorize this variable at the 5 percent level.

Judge identity

The second step to conduct our analysis is to identify the judge dealing with each case. North Carolina sentencing data reports a judge acronym (with two or three letters) for each case. In order to identify a specific judge based on these acronyms, we use the Master Schedules recording in which district and division a judge is in a given week. Using this information, we construct one, two, and three letter acronyms for each judge in the schedule and match this with our case data. Using disposition date data and the acronyms, we are able to match the 84 percent of judges in the master schedule to cases in the sentencing data. We only keep these observations in our working sample. For judges elected or nominated after the 1998, we observe the whole history of decisions.

District level demographic variables

We collect various demographic and other district level variables that we use in different steps of the analysis. District level demographic characteristics are constructed starting from the US Bureau of Census data. We use variables for the year 2010 (the most recent census fully available). These variables (listed in descriptive statistics table of the paper) are collected at the county level and are then aggregated at the district level since each district usually includes more than one county.

District level administrative and political variables

Prison population data are collected from the National Census of Jails and the Annual Survey of Jails and are used to construct a crowding metric as the ratio between the value of the total population of inmates at that county's jail facilities at the survey date and the rated capacity of the jail, which measures the maximum number of beds (and therefore overnight inmates) that could fit into the facility on the date the survey was taken.

Finally, we collect data on referenda about justice. We identified four referenda that took place in 1996, 2004, 2010, and 2014. The 1996 referendum asked voters about the expansion of alternative punishments to be used on convicted criminals, such as probation and community service. The 2004 referendum aims at clarifying and defining several areas of jurisdiction of the courts, and changed the term of office of magistrates to provide for an initial term of 2 years and subsequent terms of 4 years. The 2010 is intended to prohibit convicted felons for running as sheriffs in the state and finally the 2014 introduces the possibility for felons to waive a trial by jury. We collect data about county level votes in these referenda from North Carolina's Board of Elections and then aggregate them at the district level to compute the percentage of votes in favor or against the main question asked in the referendum.

Figure 8: Sentencing guidelines

Figure A

***** Effective for Offenses Committed on or after 10/1/13 *****

**FELONY PUNISHMENT CHART
PRIOR RECORD LEVEL**

	I 0-1 Pt	II 2-5 Pts	III 6-9 Pts	IV 10-13 Pts	V 14-17 Pts	VI 18+ Pts	
A	Death or Life Without Parole Defendant Under 18 at Time of Offense: Life With or Without Parole						
B1	A	A	A	A	A	A	DISPOSITION Aggravated Range PRESUMPTIVE RANGE Mitigated Range
	240 - 300	276 - 345	317 - 397	365 - 456	Life Without Parole	Life Without Parole	
	192 - 240	221 - 276	254 - 317	292 - 365	336 - 420	386 - 483	
	144 - 192	166 - 221	190 - 254	219 - 292	252 - 336	290 - 386	
B2	A	A	A	A	A	A	
	157 - 196	180 - 225	207 - 258	238 - 297	273 - 342	314 - 393	
	125 - 157	144 - 180	165 - 207	190 - 238	219 - 273	251 - 314	
	94 - 125	108 - 144	124 - 165	143 - 190	164 - 219	189 - 251	
C	A	A	A	A	A	A	
	73 - 92	83 - 104	96 - 120	110 - 138	127 - 159	146 - 182	
	58 - 73	67 - 83	77 - 96	88 - 110	101 - 127	117 - 146	
	44 - 58	50 - 67	58 - 77	66 - 88	76 - 101	87 - 117	
D	A	A	A	A	A	A	
	64 - 80	73 - 92	84 - 105	97 - 121	111 - 139	128 - 160	
	51 - 64	59 - 73	67 - 84	78 - 97	89 - 111	103 - 128	
	38 - 51	44 - 59	51 - 67	58 - 78	67 - 89	77 - 103	
E	I/A	I/A	A	A	A	A	
	25 - 31	29 - 36	33 - 41	38 - 48	44 - 55	50 - 63	
	20 - 25	23 - 29	26 - 33	30 - 38	35 - 44	40 - 50	
	15 - 20	17 - 23	20 - 26	23 - 30	26 - 35	30 - 40	
F	I/A	I/A	I/A	A	A	A	
	16 - 20	19 - 23	21 - 27	25 - 31	28 - 36	33 - 41	
	13 - 16	15 - 19	17 - 21	20 - 25	23 - 28	26 - 33	
	10 - 13	11 - 15	13 - 17	15 - 20	17 - 23	20 - 26	
G	I/A	I/A	I/A	I/A	A	A	
	13 - 16	14 - 18	17 - 21	19 - 24	22 - 27	25 - 31	
	10 - 13	12 - 14	13 - 17	15 - 19	17 - 22	20 - 25	
	8 - 10	9 - 12	10 - 13	11 - 15	13 - 17	15 - 20	
H	C/I/A	I/A	I/A	I/A	I/A	A	
	6 - 8	8 - 10	10 - 12	11 - 14	15 - 19	20 - 25	
	5 - 6	6 - 8	8 - 10	9 - 11	12 - 15	16 - 20	
	4 - 5	4 - 6	6 - 8	7 - 9	9 - 12	12 - 16	
I	C	C/I	I	I/A	I/A	I/A	
	6 - 8	6 - 8	6 - 8	8 - 10	9 - 11	10 - 12	
	4 - 6	4 - 6	5 - 6	6 - 8	7 - 9	8 - 10	
	3 - 4	3 - 4	4 - 5	4 - 6	5 - 7	6 - 8	

A – Active Punishment I – Intermediate Punishment C – Community Punishment
Numbers shown are in months and represent the range of minimum sentences

Appendix C (Online Appendix not for publication): District attorney electoral cycle

Another key actor of the judicial system is also subject to electoral review, namely the district attorney. While the previous literature has focused on election of judges, no paper to the best of our knowledge has focused on elections of DAs. As discussed in the introduction, we could expect electoral pressure to be less intense in this case since the action of the DA is less visible than that of the judge and more importantly the assistant DAs, who make most decisions, are not themselves elected. We show that electoral cycles are also observed for DAs. We exploit the fact that elections of DAs do not necessarily coincide with elections of judges. In particular elections occur every 4 years for DAs while they happen every 8 years for judges. Moreover, they do not happen simultaneously across all districts.

We thus estimate similar specifications to equations (1) and (2), with several distinctions. First, since the district attorneys don't rotate there is no distinction between home and non-home districts, so we impose $\beta_k = 0$ and $\delta_k = 0$. Second, since the electoral term is 4 years for DAs, we use 3 semesters before the DA election and three semesters after. Finally, we control in Z_i for the distance to the local judge's elections and differentiate contested and non contested elections. We present in Appendix Table 7 the balancing checks that show that there is no indication of strategic case selection. This was anyway less of a concern for DAs since there is only one DA per district and the strategic allocation thus cannot be done across individuals (like for judges) but only across time.

The results are presented in Appendix Table 8. In column (1) we estimate the modified equation (1) for the entire sample and in columns (2) for non-contested elections and column (3) for contested elections. In column (4) we present the estimation of the modified version of equation (2), adding semester fixed effects in column (5).

All the results convey the same message. Sentences increase one semester before the election. The magnitude of the effect is an increase of 15 days, slightly less than in the case of judges, as shown in our preferred specification in column (4). None of the other coefficients are significant. The surprising effect is however that there is no difference between contested and non-contested elections. A potential explanation is that in the case of DAs, for most trials, the DA facing reelection is not a direct actor, but is represented by an assistant DA. These assistant DAs might want to demonstrate toughness as election time approaches, in particular if there is turnover of teams in the DAs office even if the DA is renewed. It could also be the case that even when the election is non contested, assistant DAs might feel overall more scrutinized during election periods.

Table 7: Balancing checks for elections of DAs: effect on predicted sentences

	(1)	(2)	(3)	(4)	(5)	(6)
	All	Contested	Non-Contested	All	All-Semester fe	All-All observations
-3	4.454 (4.456)	3.483 (6.873)	4.979 (5.787)	5.122 (5.050)	10.55* (5.740)	14.57** (5.983)
-2	4.956 (4.541)	11.24 (6.902)	0.728 (5.938)	1.905 (5.282)	3.907 (6.795)	7.953 (6.824)
-1	9.820** (4.536)	10.56 (6.749)	9.446 (5.952)	8.948* (5.149)	10.82 (6.832)	13.77** (6.890)
+1	4.926 (4.597)	11.67* (7.021)	0.604 (5.898)	3.113 (5.339)	3.461 (6.882)	6.909 (6.538)
+2	9.107** (4.220)	12.41* (6.560)	7.063 (5.386)	8.242* (4.969)	5.134 (7.012)	9.496 (6.712)
+3	14.06*** (4.104)	17.39*** (6.203)	12.00** (5.352)	11.51** (4.817)	5.119 (5.930)	9.233 (5.805)
-3 * Contested				-2.718 (10.51)	0.0358 (10.58)	-5.747 (11.37)
-2 * Contested				12.53 (10.28)	13.22 (10.31)	7.135 (11.18)
-1 * Contested				3.904 (10.76)	4.568 (10.65)	-0.935 (11.50)
+1 * Contested				7.776 (9.942)	9.672 (9.993)	2.412 (10.62)
+2 * Contested				3.974 (9.668)	4.815 (9.544)	-4.189 (10.78)
+3 * Contested				11.45 (9.173)	10.76 (9.030)	0.243 (9.278)
Observations	181526	71174	110352	181526	181526	262536

Note: This figure plots the coefficients corresponding to the estimation of a modified version of equation (2) applied to DAs. The sample is restricted to 3 semesters before and 3 semesters after election of a DA. The controls include the distance to the election of the judge and whether this election is contested. Standard errors are clustered at the DA level and reported in parentheses. Main coefficients of interest in bold.

Table 8: DA elections

	(1)	(2)	(3)	(4)	(5)	(6)
	All	Contested	Non-Contested	All	All-Semester fe	All-All observations
-3	11.82* (6.873)	31.68*** (11.40)	3.223 (9.105)	7.730 (7.095)	12.46 (8.209)	2.779 (6.350)
-2	11.24 (7.313)	14.86 (10.40)	16.21 (10.53)	9.246 (7.727)	16.49* (8.843)	13.95** (6.885)
-1	11.09** (4.723)	19.13** (8.107)	8.985 (6.196)	10.56** (4.957)	14.42** (7.077)	9.625 (6.786)
+1	-0.978 (4.104)	9.544 (6.175)	-6.006 (6.133)	-2.966 (4.655)	-7.487 (7.004)	-3.885 (5.934)
+2	10.71 (6.622)	24.87** (10.54)	4.172 (9.527)	10.14 (6.921)	9.641 (8.733)	4.924 (6.630)
+3	6.087 (7.357)	18.77* (10.61)	4.749 (10.48)	4.047 (7.515)	3.886 (8.776)	5.227 (6.069)
-3 * Contested				17.82* (9.432)	18.19* (9.486)	16.28* (9.014)
-2 * Contested				9.952 (8.254)	10.96 (8.332)	9.336 (7.883)
-1 * Contested				3.330 (9.029)	3.686 (9.064)	2.969 (8.403)
+1 * Contested				7.300 (7.249)	6.568 (7.458)	5.495 (6.744)
+2 * Contested				1.589 (7.440)	1.188 (7.467)	0.759 (7.015)
+3 * Contested				9.671 (7.130)	9.122 (7.184)	9.365 (6.310)
Observations	181452	71103	110277	181452	181452	262445

Note: This figure plots the coefficients corresponding to the estimation of a modified version of equation (2) applied to DAs. The sample is restricted to 3 semesters before and 3 semesters after election of a DA. The controls include the distance to the election of the judge and whether this election is contested. Standard errors are clustered at the DA level and reported in parentheses. Main coefficients of interest in bold.