DISCUSSION PAPER SERIES

ELECTORAL SENTENCING CYCLES

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PUBLIC ECONOMICS

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Abstract

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Electoral Sentencing Cycles[*]

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Abstract

Exploiting features of the North-Carolina judicial system, elections and forced rotation of judges, we overcome major challenges hampering the identification of the existence and source of sentencing variation over the electoral cycle. We show that when elections approach, sentencing for felonies increase. This increase is driven by decisions taken by judges present in their district of election, and only when elections are contested. When judges operate outside their district of elections, sentencing decisions do not significantly vary over the electoral cycle. Our results demonstrate the existence of strategic sentencing by judges in an attempt to please voters and allow us to discard alternative explanations for the rise along the cycle, such as behavioral motives or contextual explanations.

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 appointment, others for elections, while others prefer 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, in particular during election periods. This in turn can cause sentencing electoral cycles, a potentially costly uncertainty in sentences.

But do election actually have an impact on sentencing and, if so, is it due to judges strategically reacting to attract voters? In answering these questions the literature has faced two main challenges. First, finding environments where the judicial sentencing cycle can be identified. Second, if sentences do vary during electoral periods, determining the actual cause of this variation. Indeed variations during electoral periods are not necessarily driven by the effort of judges to meet their voters' expectations, but can reflect confounding factors. First judges' increased stress due to the risk of losing their job that could affect their sentencing behavior.[1] Second, and more importantly, the variations could capture changes in the context in which decisions are taken. This contextual effect could be for instance due to a more intense media coverage or to an increasingly politicized working environment. It could also correspond to a change in the types of cases seen during electoral periods, for instance due to a change in incentives of the police.[2] Distinguishing between these different mechanisms 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. The system is characterized by both judicial elections and forced judicial rotation. The State is divided in eight divisions, in turn divided in 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

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[1]As recently documented by Eren and Mocan (2018) for juvenile justice, sentencing decisions can reflect emotional shocks. This idea is also consistent with recent studies such Aghion et al. (2016) showing that jobs destructions are related to significative losses in subjects' well being. Some research has also shown that behavioral biases in sentences might emerge also at particular moments in the day where judges feel more stressed (Danziger et al. 2011)

[2]Recent studies have investigated judicial responses to preferences of the constituency and political pressure. For instance, Berdejo and Chen (2017) study US Courts' of Appeals decisions showing that judicial decisions respond to presidential electoral cycles. Before presidential elections dissent rates significantly increase as well as judges' voting along partisan and ideological lines

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 distinguish between decisions taken in her home-district and decisions taken outside this district, focusing on contested elections, where the judge faces a challenger in the electoral competition. 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. Among non-home districts we distinguish between decisions taken in districts where no judicial elections are held and those where there are elections. The comparison between these two types of districts allows us to isolate the effects of the contextual changes due to elections.

We use the universe of criminal decisions taken in North-Carolina superior courts between 1998 and 2011.[3] For each sentence, we know the identity of the judge that took the decision, a set of characteristics of the defendant (including demographics and crimes committed) and the outcome of the trial. We find that judges modify their behavior over the electoral cycle, moreover changes in sentences present some tale-telling heterogeneity: sentences increase in the order of 10% before the elections, but only for decisions taken in a judge's home-district. Thus, we do not find evidence of behavioral or contextual 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 judges' position is not contested. These results are consistent with an explanation of electoral sentencing cycles driven by judges' effort to match what they perceive voters local preferences to be, i.e. a demand for longer sentences. Interestingly, we also show that the increase in sentences' severity before elections is driven by more serious crimes while juveniles and women are not affected.

This paper joins a recent literature studying the effects of judicial elections on lower courts sentencing decisions. Early studies in this literature (Besley and Payne, 2005; Helland and Tabarrok 1999) have 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. 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 Huber and Gordon (2007) exploit data from Kansas to investigate wether systems with retention

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

versus non partisan elections show different patterns finding that electoral cycles are driven by counties with partisan elections.[4] Also focusing on the difference between partisan and non-partisan elections, Lim et al. (2015) show that more media pressure increase sentences but only in district where judges are elected in non partisan election.

A couple of 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.[5] Our main innovation is to exploit the forced rotation of judges in North-Carolina, allowing us to observe them both in home-districts and outside over the electoral cycle and exploiting the district/election composition we are able to provide clean evidence about the effect of electoral incentives and disentangle it from other drivers of decisions varying over 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. Our study complement this findings by exploiting forced rotation and the differential location of judges during the electoral cycle.

Our paper is more broadly connected to the literature on political business cycles, that shows how policy (for instance tax policy or public spending) appears to vary when elections approach.[6] 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, like 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 to push 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

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[4]These results are consistent with Lim(2008) studying differences in sentencing patterns among judges retained via partisan or retention elections, estimating a structural model with data from Kansas Lim(2008) shows that differences in the patterns of judges selected with different type of elections can be attributed both to selection on preferences and judges' responses to elections

[5]Dippel and Poyker (2019), using data from 11 States finds that existence of electoral cycles depends on the level of competition in judicial elections, which varies considerably across states.

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

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 in 50 districts aggregated into eight divisions (see Figure 6 in Appendix A). Superior Court judges are elected officials who rotate district every six months (see Figure 7 in Appendix A that documents the patterns of rotation). They are elected for a term of eight years. Elections take place at district level and judges are administratively attached to their district of election that is identified as district of "residence" (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 do 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.[7]

An important feature that we will exploit for identification is that elections are not held simultaneously across the state (see Figure 8). 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. The other feature is that these judicial elections are often organized at the same time as other local elections, such as elections for county sheriff. This could be a factor that contributes to the contextual effect. For instance, the sheriff running for reelection might enforce the law differently when the election approaches.

Superior Court judges are required to rotate every six months within the division where their district of election is located. This 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). Rotation takes place according to a master schedule established by the Chief Justice of the North Carolina Supreme Court and the rotation occurs in January and July.[8] Judges return to their home district at least once every two years.

As in the rest of the U.S., the vast majority of criminal cases in North Carolina are resolved via plea bargain, where 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 ad-

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[7]Elections were non partisan before 2016 and have now been changed to partisan elections.

[8]Figure 7 (reported in appendix), shows that indeed the forced rotation policy is implemented by Chief-Judges and each judge in a given division is forced to move, generally in January and July.

vantage of the defendant, although this varies dramatically by geography, crime type and race. Judicial decisions in North-Carolina are subject to sentencing guidelines since 1994. In order to establish a sentence, the judge has first to determine the kind of offense the case belongs. Then the judge has to take into account the offender's prior criminal records 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.[9] After having determined the sentence range, the judge determines the case disposition and wether 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 comes from the North Carolina Administrative Office of the Courts and includes 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 and of the judge. The data also includes the main demographic characteristics of the defendant as well as categories of crime for which the defendant is charged. The dataset includes 343,776 sentencing decisions with final disposition date between 1998 and 2010.[10]

During the time period under consideration, we observe the results for 169 elections. Out of those, 54% of elections are contested. In those contested elections, judges face stark competition. Indeed they win only 44% of the contested elections and even when they do win, it is typically not by a large margin, with the 90th percentile of vote percentage in contested elections established around 60%. This suggest that electoral concerns are of first order for judges facing contestants.

In the main analysis, our main sentencing variable is the minimum active sentence chosen by the judge as in 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 data construction.

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[9]In practice, the judge has a quite large range of options, for instance an offender convicted for an 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 months range if the judge finds aggravating circumstances or downward, to 58-78 months, if she finds mitigating circumstances.

[10]It 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.

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. In order 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, data obtained from North Carolina's Board of Elections.

In table 1 we report some descriptive statistics, in the left columns for the entire sample and on the right, restricting to the 4 years around election time. The average sentence is 382 days and 97% of the cases are resolved by plea bargains. Among the defendants, 50% are black and 16% women.

4 Identification strategy

The paper aims at identifying whether judges strategically adapt their sentences when elections approach. As highlighted in the introduction, other confounding factors potentially affecting sentences vary during election periods. First, behavioral factors, such as the stress of judges under pressure of potentially losing their job. Second contextual factors that imply that during electoral periods, crimes committed or dedication of police forces might be different.

We therefore exploit two features of the institutional setting in North Carolina. First, judges need to rotate across districts. Second, elections are not held at the same time in all districts. The first feature allows us to compare a judge sentencing in her home district versus her decisions taken in non-home districts. Under the assumption that voters only pay attention to sentences given in their district[11], the strategic effect is only present in the home district. The second feature can be used to further distinguish judges operating outside their home district, comparing judges in districts where an election is held (but where they are not running by definition) from districts where no election is held. The difference between these two cases allows us to identify the contextual effect.

To implement this idea, we estimate the following model, restricting our data to the 8 years around judges' election (4 years before, 4 years after) and using only judges running in contested elections at home (i.e where at least two candidates are running), so that the behavioral effect is always present.

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[11]The assumption is coherent with evidence about how voters' respond to crime control policies (Drago, Galbiati and Sobbrio, 2019).

$$S_{ijdt} = \sum_{k=-4}^{4} \alpha_k * 1_{Semester=k} * 1_{Home} + \underbrace{\sum_{k=-4}^{4} \beta_k * 1_{Semester=k} * 1_{No-Home} * 1_{Election}}_{}$$

$$\underbrace{\phantom{\sum_{k=-4}^{4} \alpha_k * 1_{Semester=k} * 1_{Home}}}_{\text{Strategic+Context+Behavioral Effects}} \qquad \underbrace{\phantom{\sum_{k=-4}^{4} \beta_k * 1_{Semester=k} * 1_{No-Home} * 1_{Election}}}_{\text{Context+Behavioral Effects}}$$

$$+ \underbrace{\sum_{k=-4}^{4} \gamma_k * 1_{Semester=k} * 1_{No-Home} * 1_{No-Election}}_{\text{Behavioral Effect}}$$

$$+ \quad \zeta_1 * 1_{Home} + \zeta_2 * 1_{No-Home} * 1_{Election} + \nu_{ElecPeriod,j} + \nu_d + \nu_c + \epsilon \qquad (1)$$

where S_{ijdt} is the sentence of offender i decided by judge j in district d at time t. $1_{Semester=k}$ is a dummy equal to one if the decision is taken in the semester k before/after the judge's election. 1_{Home} is a dummy equal to one if the decision is taken in judge's j home district (i.e. the district where she runs for election) and $1_{No-Home}$ is a dummy when the judge is not sentencing in her home district. $1_{Election}$ and $1_{No-Election}$ are dummy variables measuring whether a contested election is being held in the district where the judge is currently sentencing. The main parameters of interest are α_k, β_k and γ_k. All parameters are measured with respect to comparison periods composed of sentences decided 5, 6, 7 and 8 semester before/after the elections (baseline periods).[12] ζ_1 and ζ_2 capture the differences between baseline periods in home district (ζ_1) or outside home district when there is a simultaneous contested election (ζ_2) with respect to the third situation (outside home district in a district without simultaneous contested election). $\nu_{ElecPeriod,j}$, ν_d and ν_c are fixed effect for judge*electoral periods (i.e. we include two fixed effects for one judge if we observe him during two different electoral periods), district and crime type respectively.

Given that we restrict our analysis to elections that are contested in the home district, the behavioral effect always affects the judge during the electoral period ($k = -4$ to 0) regardless of where she is sentencing. The underlying assumption is that the stress of potentially losing the job in the election does not decrease when the judge is not at home.[13] γ_k thus measures the behavioral effect. In addition, if the judge is away from home, but in a district where an election is being held, the context of the election might directly affect sentences. The difference $\beta_k - \gamma_k$ thus measures the contextual effect. Finally, in addition to the behavioral and contextual effects, when the judge is sentencing in her home district, the strategic consideration of wanting to influence voters comes into play. The difference $\alpha_k - \beta_k$ measures

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[12]Note 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.

[13]This is not directly testable but appears natural. One potential issue is that the risk of losing the election might be more salient when at home, thus increasing the stress.

this strategic effect.

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 have for some reason a preference for more severe cases.[14] In this case α_k would capture case selection rather than the judicial response to electoral pressure. Figure 1 provides some reassuring evidence. We estimate equation (1) using as dependent variables defendant characteristics (sex, race, minor, prior offenses). Reassuringly, we do not observe any systematic difference in observable case characteristics across different districts/elections combinations over the electoral cycle.

The second potential challenge could be linked to sorting of judges during electoral periods. We believe 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 2 provides some supportive evidence. The figure represents the probability of being in the home district over the electoral cycle. We see that judges are more likely to be in their home district 1 semester and 3 semesters before the election, while the effects we find on sentences are in $t-1$, $t-2$ and $t-3$. Most importantly the pattern is exactly the same for non contested election (for which the strategic motive should be absent) and for the other elections. This strongly suggests that strategic sorting over the cycle is not an issue.

5 Results

5.1 Sentencing variation over the electoral cycle

The results of our event study are presented in Figure 3. The figure plots the coefficients of the estimation of equation (1), coefficient are presented in table 2 column (1). We plot separately α_k (black line), measuring how sentences vary in the 4 semesters before/after judges' elections for decisions taken in the judge's home-district, β_k (dashed line) for sentences given outside the home district but in a district where an election is held and γ_k (dotted line) for sentences outside the home district and in a district without elections.

We find that both β_k and γ_k are not significantly different from zero, regardless of the

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[14]This could fit with the idea that the judge wants visible cases to signal ability.

point in the electoral cycle, showing that both behavioral and contextual effects do not play a role in influencing sentencing. On the contrary, α_k is significantly different from zero and particularly high 2 and 3 semesters before the elections. In terms of magnitudes, sentences 1 year before a contested election, given by a judge in her home district, are 30 days higher than outside electoral periods, representing more or less an increase of 10% in sentences.

The results presented in Figure 3 are robust to the inclusion of control variables for defendant's characteristics (table 2 column (2)). They are unchanged if we compare decisions in home district to decisions in non home district with any election (instead of contested ones) and decisions in non home district with no election (table 2 column (3)). They are also similar if we use active prison time instead of prison time (table 2 column (4)). The increase in sentence time observed in home district before elections only occurred when elections are contested. Indeed, no judicial fluctuation is observed around non contested elections (Table 2 column (5)).

We further confirm these results using an alternative identification strategy. We focus on decisions taken in a judge's home district and compare decisions taken under contested or retention elections (in which the judge does not have competitors). In the second type of elections, while the changes to the context due to elections may still be present, the judge should have smaller electoral concerns. Specifically we estimate:

$$
\begin{aligned}
S_{ijdt} &= \underbrace{\sum_{k=-4}^{4} \alpha_k * 1_{Semester=k} * 1_{Home} * 1_{Contested}}_{\text{Strategic+Context+Behavioral Effect}} + \underbrace{\sum_{k=-4}^{4} \beta_k * 1_{Semester=k} * 1_{No-Home} * 1_{Contested}}_{\text{Context+Behavioral Effect}} \\
&+ \underbrace{\sum_{k=-4}^{4} \gamma_k * 1_{Semester=k} * 1_{Home} * 1_{Not-Contested}}_{\text{Context}} \tag{2} \\
&+ \zeta_1 * 1_{Home} * 1_{Contested} + \zeta_2 * 1_{Home} * 1_{Not-Contested} + \nu_{ElecPeriode,j} + \nu_d + \nu_c + \epsilon \tag{3}
\end{aligned}
$$

where variables are defined as in equation (1).

As in our main specification, we observe an increase in sentences in the period before the elections for decisions taken in home-districts when elections are contested while we do not find a significant increase in sentences for decisions taken in the district of election when elections are not contested or decisions taken outside the district of elections (Figure 4). Moreover the effect is of similar magnitude as in the main specification.

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5.2 Discussion

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 kind of preferences 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). Hereafter we further investigate this issue.

We first explore in Table 3 heterogeneity in the results based on observable case characteristics. We estimate separately equation (1) for different subgroups. We show that the increase in sentences before elections is stronger for white than for black defendants.[15] Moreover, we do not find an increase in sentencing before elections when defendants belong to groups usually considered as more vulnerable: women and juveniles. If judges are determining sentences in such a way as to signal toughness, the political reward may be higher for being tough on less vulnerable people, such as white males.

We explore further the idea of signalling toughness. If their constituents are asking for higher sentences, judges' subject to electoral scrutiny need to prove their toughness. To this end they have different possible strategies. Judges can increase sentences overall or could focus on some more visible case. We study these possibilities by running a battery of quantile regressions allowing us to single out what sentence range is more affected by the increase in sentencing along the judicial electoral cycle.

In order to achieve convergence of the quantile regression we proceed in two steps: first we regress sentences on crime fixed effects, district fixed effects and a set of fixed effect for one electoral period for one judge. Then, we use the residual of this first regression as the outcome in a quantile regressions of our main specification. The effects of contested elections on decisions taken in home district are presented in Figure 5 for the different quantiles. We find that while the lower quantiles of the sentences' distribution do not vary over the electoral cycle, the upper quantiles (80th and 90th) peak two semesters before the election. This suggests that judges needing to send a signal to their voters about their toughness choose the second strategy and focus on more severe and thus visible cases.

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[15]Note that this contrasts with the results of Park (2017) who shows that sentences increase more for black defendants.

6   Conclusion

In this paper we exploit the unique features of the North Carolina justice system to show that sentencing electoral cycles exist, sentences increase in the order of 10% at the end of the cycle, and that this increase is driven by the strategic attempt by judges 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) decisions taken away from their jurisdiction to form beliefs about the judges. This could be due to the structure of media reporting or to cognitive biases of voters.

This result feeds in the discussion of what is the best method to discipline the judges, rotation, elections or sentencing guidelines. It appears that rotation of judges has the effect of dampening electoral sentencing cycles, what can be considered to be socially beneficial. However it comes at the cost of decreased accountability. Whether the mix between the two systems may actually be a good way to balance the tradeoff between these two effects remains an open question.

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7 Figures and tables

Figure 1: Effect of the distance to judges' election on defendant's characteristics
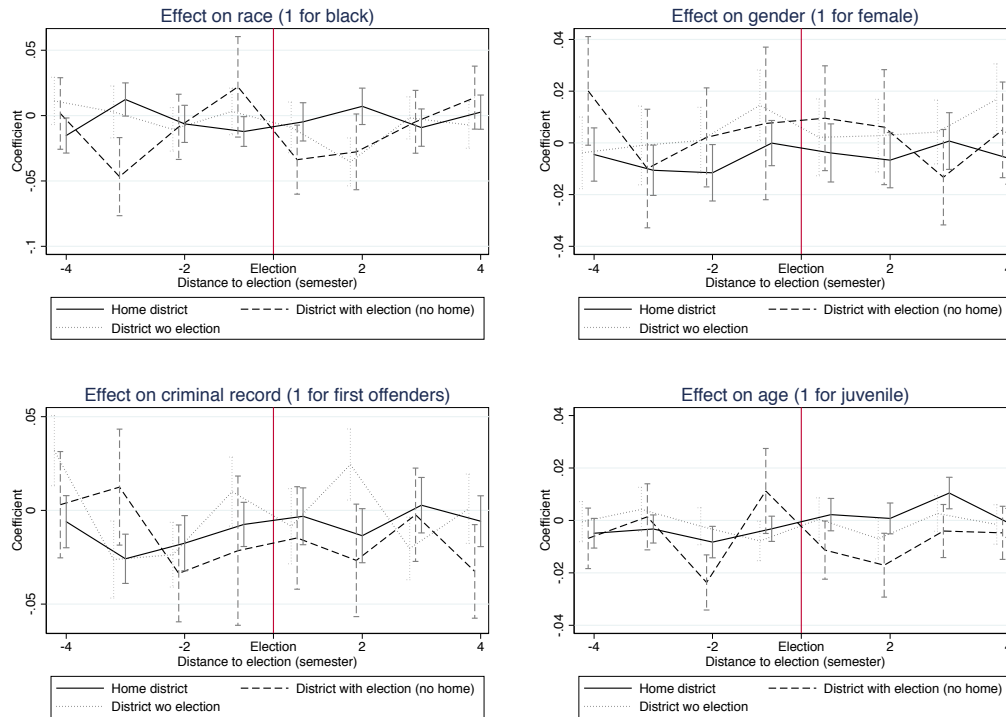
Note: This figure plots the coefficients α_k (black line), β_k (dashed line) and γ_k (dotted line) corresponding to the estimation of equation (1) restricting the sample to 4 years before and 4 years after a contested election for judge j using as dependant variable the different characteristics of the defendant. In the top left panel, dummy variable for black defendants, top right for female defendant, bottom left for defendant committing her first offense and bottom right for defendants who are minor.

Figure 2: Probability of being in home district
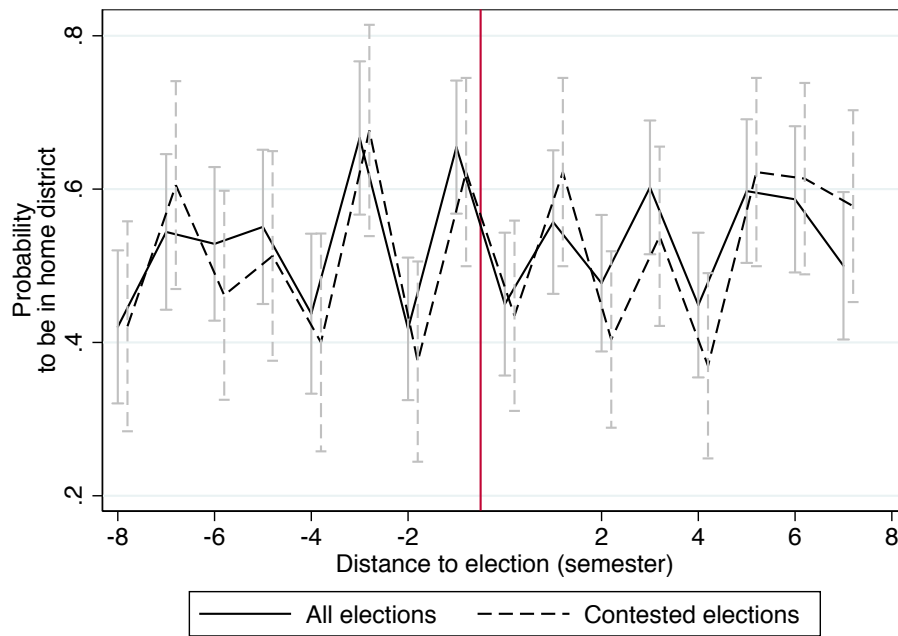
Note: This figure plots the probabilities that judges take the majority of their decisions in their home district by semester and election type. The sample is restricted to 4 years before and 4 years after judges' elections

Figure 3: Effect of the distance to judges' election on sentencing

Note: This figure plots the coefficients α_k (black line), β_k (dashed line) and γ_k (dotted line) corresponding to the estimation of equation (1). The sample is restricted to 4 years before and 4 years after a contested election for judge j. Sentences decided 5-8 semesters before/after elections are the baseline periods. The coefficients used to plot this figure can be found in Table 2, Column 1.

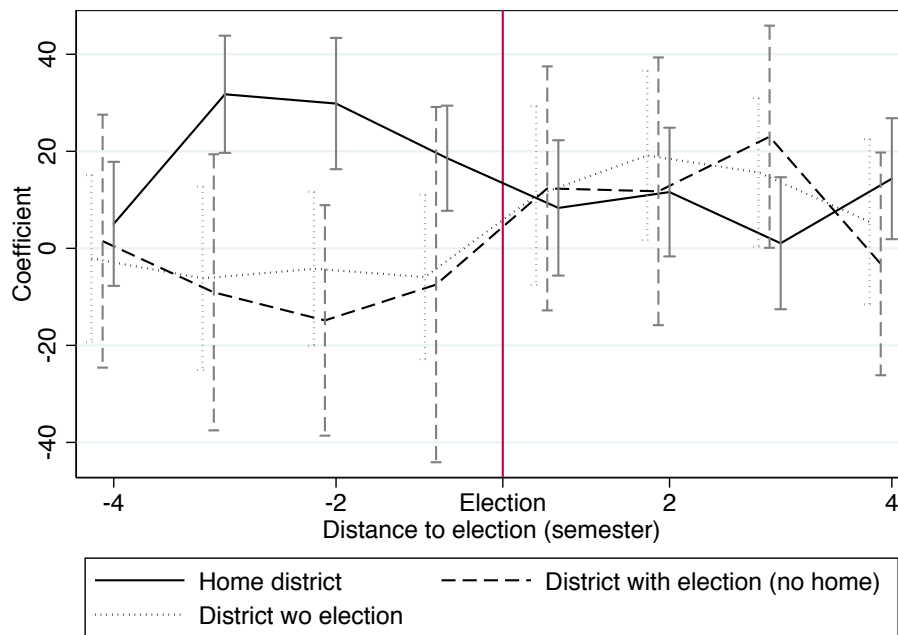Figure 4: Effect of the distance to judges' election on sentencing
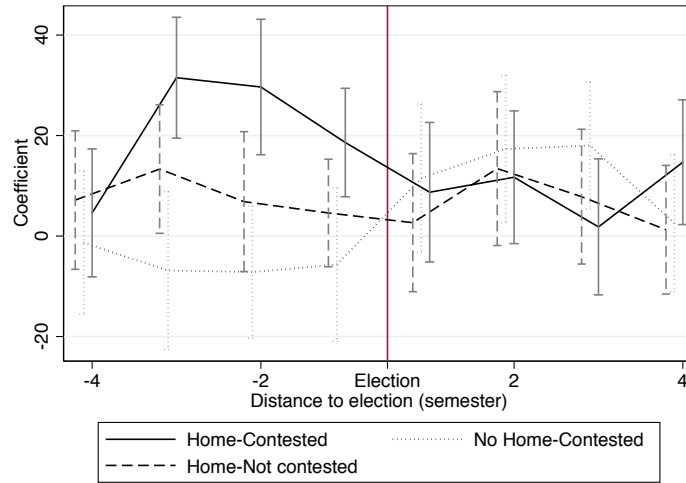
Note: This figure plots the coefficients α_k (black line), β_k (dashed line) and γ_k (dotted line) corresponding to the estimation of the alternative specification (2) restricting the sample to 4 years before and 4 years after an election for judge j.

Figure 5: Effect of the distance to judges' elections on sentencing by quantile
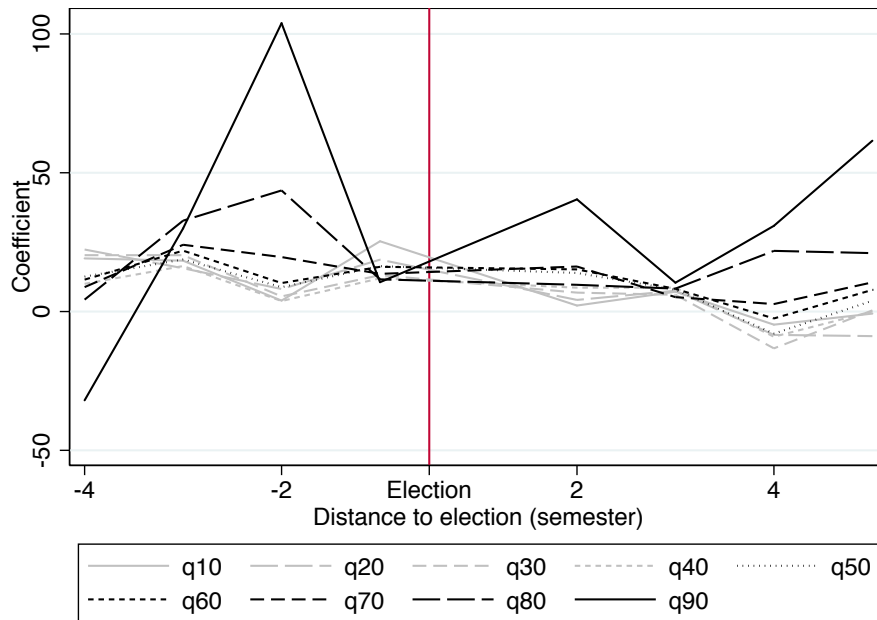
Note: This figure plots the coefficients α_k corresponding to the estimation of equation (1). Coefficients β_k and γ_k are not presented even though they are estimated. The sample is restricted to 4 years before and 4 years after a contested election for judge j. Sentences decided 5-8 semesters before/after elections are the baseline periods. Each line represent one separate quantile regression.

Table 1: Descriptive statistics

	Original dataset		4years before-after contested election	
	Mean	Sd	Mean	Sd
District election	.51	.5	.55	.5
Tenure	9.83	6.92	9.34	6.72
Prison (days)	382.3	505.69	380.43	498.86
Active prison	241.67	525.47	238.33	518.45
Probation	362.75	492.12	355.22	480.45
Woman	.16	.37	.16	.37
Black	.53	.5	.53	.5
Minor	.04	.19	.04	.19
Age	30.99	10.43	31.12	10.44
Nb past offense	1.67	1.14	1.71	1.16
First offense	.46	.5	.45	.5
Property	.22	.41	.22	.41
Violent	.22	.42	.22	.42
Drugs	.27	.44	.27	.44
Fraud	.13	.33	.13	.33
Other	.13	.33	.12	.33
N	343,660		111,670	

Table 2: Coefficients of the estimation of specification 1

	(1)	(2)	(3)	(4)	(5)
Event:			Contested election		Non-contested
Outcome:			Prison time	Active prison time	Prison time
Specification:	Home vs. No-Home with Contested Election vs. No-Home No-Election		Home vs. No-Home with ANY Election vs. No-Home No-Election	Home vs. No-Home with Contested Election vs. No-Home No-Election	
-4 * Home	5.05	4.34	4.89	-2.23	7.81
	(7.80)	(7.65)	(7.79)	(8.31)	(8.59)
-3 * Home	31.8***	23.6***	31.3***	31.8***	14.8*
	(7.37)	(7.24)	(7.37)	(7.86)	(7.97)
-2 * Home	29.8***	24.5***	29.5***	28.1***	6.16
	(8.25)	(8.11)	(8.26)	(8.80)	(8.68)
-1 * Home	18.6***	15.0**	18.5***	27.6***	4.41
	(6.61)	(6.50)	(6.61)	(7.05)	(6.67)
1 * Home	8.33	5.95	8.36	3.54	1.61
	(8.50)	(8.33)	(8.50)	(9.07)	(8.61)
2 * Home	11.6	6.64	11.2	10.4	14.0
	(8.09)	(7.94)	(8.09)	(8.62)	(9.61)
3 * Home	1.05	-1.24	1.16	-4.91	9.44
	(8.30)	(8.12)	(8.29)	(8.85)	(8.44)
4 * Home	14.4*	8.89	14.8*	10.8	2.09
	(7.60)	(7.45)	(7.60)	(8.11)	(8.04)
-4 * No-Home - Election	1.48	0.64	12.7	-11.9	24.2*
	(15.9)	(15.6)	(13.5)	(17.0)	(13.4)
-3 * No-Home - Election	-9.07	-3.43	4.80	-24.6	25.6
	(17.3)	(17.0)	(13.9)	(18.5)	(19.2)
-2 * No-Home - Election	-14.9	-19.2	-5.05	-8.78	27.2**
	(14.5)	(14.2)	(11.6)	(15.4)	(13.4)
-1 * No-Home - Election	-7.48	-1.01	-28.1**	4.04	25.7*
	(22.3)	(21.9)	(13.6)	(23.8)	(15.2)
1 * No-Home - Election	12.4	11.0	17.2	9.95	33.0**
	(15.3)	(15.0)	(13.8)	(16.4)	(15.4)
2 * No-Home - Election	11.8	0.98	31.5**	23.9	34.7**
	(16.8)	(16.4)	(14.5)	(17.9)	(16.5)
3 * No-Home - Election	23.0*	21.9	34.0***	32.7**	15.8
	(14.0)	(13.7)	(11.6)	(14.9)	(19.7)
4 * No-Home - Election	-3.20	-9.58	8.13	-2.24	-8.15
	(14.0)	(13.7)	(12.4)	(14.9)	(19.1)
-4 * No-Home - No-Election	-2.09	-1.32	-12.7	0.11	5.38
	(10.5)	(10.3)	(11.4)	(11.2)	(15.3)
-3 * No-Home - No-Election	-6.18	-12.7	-17.8	-5.69	-9.08
	(11.5)	(11.3)	(13.3)	(12.3)	(14.0)
-2 * No-Home - No-Election	-4.22	-8.17	-8.82	-5.14	8.47
	(9.67)	(9.53)	(11.2)	(10.3)	(12.1)
-1 * No-Home - No-Election	-5.91	0.43	12.6	-6.98	-11.1
	(10.3)	(10.2)	(12.8)	(11.0)	(15.3)
1 * No-Home - No-Election	10.9	6.80	5.22	9.53	-8.82
	(11.2)	(11.1)	(12.0)	(12.0)	(14.8)
2 * No-Home - No-Election	19.1*	21.6**	5.70	16.1	15.3
	(10.6)	(10.5)	(11.5)	(11.3)	(12.7)
3 * No-Home - No-Election	15.6*	12.6	4.77	8.79	-3.80
	(9.35)	(9.19)	(10.4)	(9.97)	(12.5)
4 * No-Home - No-Election	5.48	0.27	-2.60	-8.35	10.3
	(10.4)	(10.2)	(11.4)	(11.1)	(13.6)
Event, crime and district fixed effects	x	x	x	x	x
Control judges' tenure	x	x	x	x	x
Control offenders' characteristics		x			
Observations	109,274	107,669	109,274	109,274	91,554

Note: This table present the coefficients α_k (first rows), β_k (middle rows) and γ_k (bottom rows) corresponding to the estimation of equation (1). The sample is restricted to 4 years before and 4 years after a contested election for judge j. Sentences decided 5-8 semesters before/after elections are the baseline periods. Column 1 present the main specification. Column 2 includes control for age, sex, race and criminal history. In Column 3, coefficients β_k measure the effect of any election in non home district (instead of measuring the effect of contested elections) Column 4 present the results when using active prison time instead of all prison time. Lastly, Column 5, presents the estimation of equation (1) around non contested election (instead of contested ones).

Table 3: Differential effects: main model

Distance election	(1) White	(2) Black	(3) Male	(4) Female	(5) First offense	(6) Recidivist	(7) Minor	(8) Adult	(9) Judge's tenure <median	(10) >median	(11) Democratic	(12) Republican
-4	18.6*	-9.61	1.99	12.8	5.57	-2.07	-103***	7.43	1.41	8.91	-30.1	62.2*
	(10.5)	(11.5)	(8.97)	(12.9)	(10.2)	(11.1)	(32.9)	(8.03)	(12.3)	(10.2)	(27.1)	(33.8)
-3	48.6***	14.8	38.1***	-10.2	18.7*	34.2***	-23.6	30.0***	26.1**	33.7***	13.2	98.7***
	(10.4)	(10.4)	(8.43)	(12.5)	(9.76)	(10.4)	(30.6)	(7.61)	(10.9)	(10.1)	(23.0)	(25.0)
-2	42.9***	19.0	34.1***	4.31	36.0***	20.6*	-54.2	30.5***	29.1***	28.4**	52.0**	84.5***
	(11.3)	(12.0)	(9.43)	(14.1)	(10.9)	(11.7)	(37.2)	(8.50)	(11.1)	(12.8)	(20.3)	(32.0)
-1	29.3***	7.54	21.9***	0.16	2.46	30.0***	-17.3	20.5***	28.8***	7.82	-0.042	44.2
	(9.08)	(9.54)	(7.61)	(10.8)	(8.65)	(9.42)	(26.6)	(6.84)	(9.90)	(9.06)	(19.0)	(29.9)
1	13.3	2.25	13.2	-18.8	-1.50	16.6	9.95	8.26	4.68	10.7	-5.47	63.4*
	(12.1)	(11.9)	(9.75)	(14.2)	(11.1)	(12.1)	(33.1)	(8.78)	(12.6)	(11.8)	(21.2)	(37.4)
2	2.13	18.4	18.9**	-25.0*	5.49	13.4	25.3	10.7	15.6	7.01	-3.03	61.4
	(11.6)	(11.2)	(9.25)	(13.7)	(10.6)	(11.5)	(33.3)	(8.35)	(11.9)	(11.3)	(18.2)	(39.4)
3	11.0	-8.25	-1.07	10.8	-4.46	8.09	17.5	1.33	-14.8	6.30	-9.03	76.7*
	(11.3)	(12.0)	(9.53)	(13.7)	(11.0)	(11.7)	(30.7)	(8.58)	(13.3)	(11.0)	(17.0)	(43.7)
4	30.7***	-0.072	14.2	12.8	18.8*	13.1	33.1	13.5*	19.3*	3.83	-36.1**	57.9
	(10.6)	(10.8)	(8.67)	(13.1)	(9.98)	(10.8)	(31.3)	(7.84)	(10.1)	(11.9)	(14.5)	(44.1)
Obs	50,756	58,517	91,760	17,512	49,450	59,823	4,155	103,510	56,896	52,377	26,775	8,120

Note: This table present the coefficients α_k corresponding to the estimation of equation (1) for the subgroups mentioned in the header. Coefficients β_k and γ_k are not presented even though they are estimated. The sample is restricted to 4 years before and 4 years after a contested election for judge j. Sentences decided 5-8 semesters before/after elections are the baseline periods. Column 1 present the main specification.

Appendix A

Figure 6: Map of North Carolina Judicial districts

North Carolina Superior Court
Effective January 1, 2015
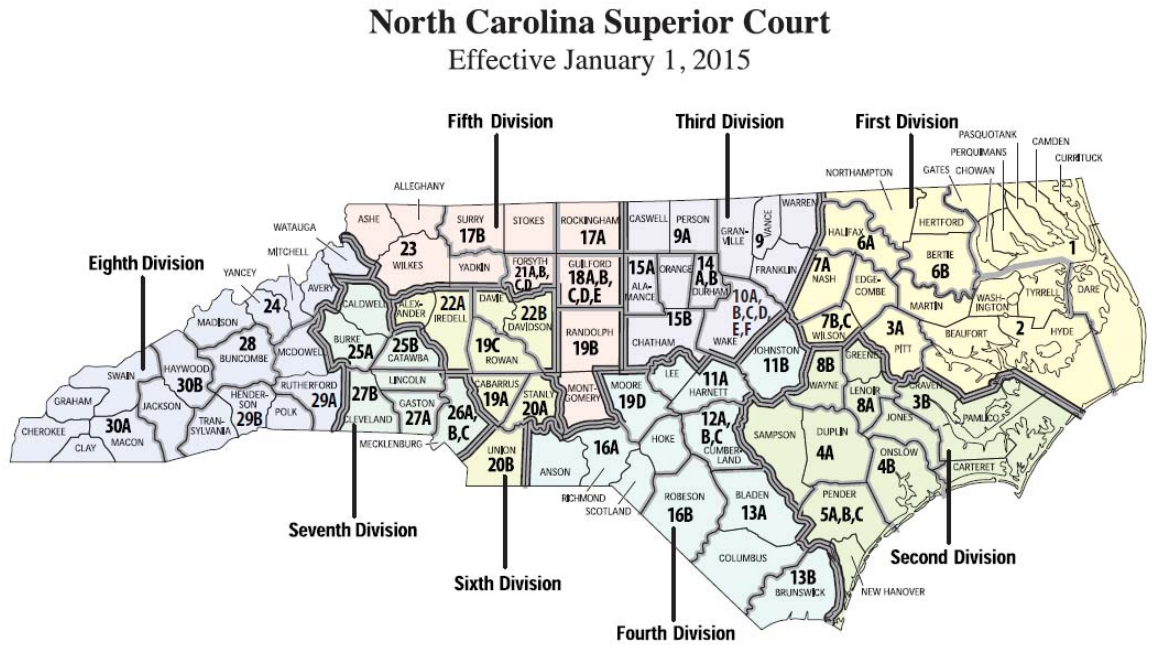
Note: Districts that have more than one letter associated
with the district number (i.e., 10A, B, C, D) are divided into
separate districts for electoral purposes. For administrative
purposes, they are combined into a single district.

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Figure 7: Probability of moving per month, using data from the master schedule.
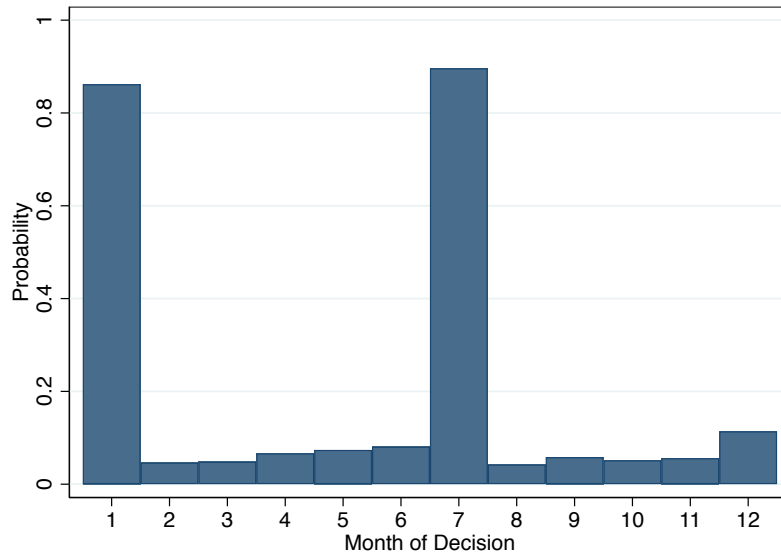
Note: A move is defined as a change in the district where the majority of decisions are made by a judge in a month.

Figure 8: Number of districts holding elections in any given year.
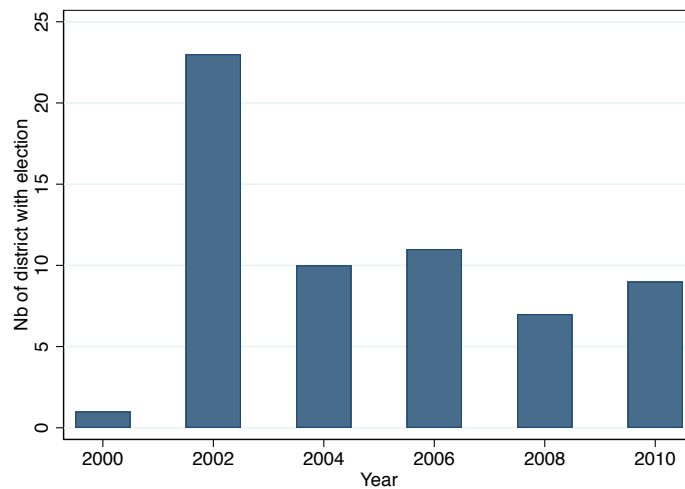
Note: A district is considered as holding an election during one year if at least one identified judge in the database is running for it.

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

(Left margin label: OFFENSE CLASS)

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

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Revised: 09-09-13

4