Rules Versus Norms: How Formal and Informal Institutions Shape Judicial Sentencing Cycles*

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Abstract

Existing research on electoral sentencing cycles consistently finds that elected judges levy longer sentences when they are up for re-election. However, this research finding had previously drawn exclusively on data from four states. Using newly collected sentencing data on seven additional states, we find substantial, and previously unnoted, heterogeneity in the strength of sentencing cycles. This heterogeneity appears to be explained by cross-state differences in informal norm of whether incumbent judges get challenged in judicial elections. We show that that variation is explain by the baseline probability of having a challenger and the number of donations per electoral race. That variation, in turn, is not well explained by observable formal electoral institutions.

Keywords: Judge Elections, Electoral Sentencing Cycles
JEL Codes: D72, H76, K41

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1 Introduction

The practice of electing judges is a distinctly American phenomenon (**?**). One argument in its favor is that it promotes policy congruence between judge and voter preferences, holding judges accountable to the public. A counter-argument is that policy congruence may not be desirable for its own sake since judicial decisions are meant to be based solely on the facts and the law (**?**). Furthermore, there is a concern that judicial elections may create inconsistent—and therefore unfair— sentencing behavior if judges give more weight to voter preferences or special interest groups closer to elections. This concern was evident when the Supreme Court ruled (in Williams-Yulee vs. Florida Bar, 575 U.S.) that states could prohibit judges from soliciting funds for their election campaign. Chief Justice Roberts wrote in the majority opinion of that ruling that "judges are not politicians, even when they come to the bench by way of the ballot. A state may assure its people that judges will apply the law without fear or favour, and without having personally asked anyone for money."[1]

The potential pitfalls of electing judges have motivated a body of empirical research that studies whether judges pass more punitive sentences when they are up for re-election. This hypothesis emerges from signaling models where voters have preferences for longer sentences than judges, especially for severe crimes like murder or rape. There are a number of studies that find evidence of precisely such electoral sentencing cycles, specifically in Pennsylvania (**?**), Washington (**?**), North Carolina (**??**), and Kansas (**??**).[2] Each of the aforementioned studies focuses on a single state in great detail. In contrast, we take a different approach by combining sentencing data that we collected from 10 different states (Pennsylvania, Washington, North Carolina, plus Alabama, Colorado, Georgia, Kentucky, Minnesota, Tennessee, and Virginia). These data are described in detail in Section 2.

In Section 3, we analyze each state's electoral cycles separately, and strongly confirm the presence of electoral sentencing cycles in those states considered in the existing literature (i.e., Pennsylvania, Washington, and North Carolina).[3] Among the other states, however, the evidence is

[1] As early as 1835, **?** had predicted that judicial elections "will sooner or later lead to disastrous results, and that some day it will become clear that to reduce the independence of magistrates in this way is to attack not only the judicial power but the democratic republic itself" (p310, ch8).

[2] Most research focuses on state trial courts because they handle by far the largest number of cases, including criminal cases, in the U.S. They can sentence defendants to long prison sentences and in some states to death.

[3] Trial court data are managed by each state's sentencing commission individually. We requested court sentencing

decidedly mixed and no other state displays statistically significant cycles. In Virginia, which forms a quasi-placebo as the one state in our data with purely appointed judges, sentencing cycle are actually negative, though insignificant.

This novel fact of heterogeneity in the presence of electoral sentencing cycles across U.S. states is important in part because sentencing cycles have so far often been viewed and discussed as being a pervasive feature of the U.S. judicial system.[4] This heterogeneity pattern is robust to an array of different specifications, including different approaches to inference, variations in how to control for sentencing guidelines, and recidivism, and to adding acquittals to the data. There is also heterogeneity across states in the direction and strength of racial differences in sentencing cycles, as we find qualitatively more pronounced sentencing cycle for whites in some states.[5] Other patterns found in previous research, e.g., gender and race biases, and a strong effect of recidivism show up consistently across all states in the data.

It is natural to ask what explains the heterogeneity in the presence of electoral sentencing cycles. In Section 4, We consider two broad explanations. First, heterogeneity in electoral sentencing cycles might be explained by variation in formal rules governing judge elections: judges can be chosen through non-partisan elections (with potentially many challengers), partisan elections, retention elections (where incumbents face only a confirmation vote and no challenger),[6] or—in the case of Virginia—by appointment and re-appointment. Second, heterogeneity in electoral sentencing cycles might be explained by the observed level of competitiveness of judicial elections, insofar as this is unexplained by the formal electoral rules. We measure competitiveness in two ways, either as the average number of donors who contributed to a judge's electoral campaign (**?**), or as the share of judicial elections in which incumbents face at least one challenger. We hypothesizethat these two measures capture local informal norms regarding how acceptable it is to challengea judge at the election.

data from all U.S. states. What determined the final sample of ten was (i) whether a state had digitized their sentencing data, and (ii) whether these data included judge identifiers in their data. The willingness to share the data was a third constraint in the case of Kansas, which is not in our study because its data processing fee was an order of magnitude larger than the next-most expensive state.

[4] For example, two articles in The Economist strongly argue this case: "The trouble with electing judges" (Aug 23rd 2014), and "New research confirms old suspicions about judicial sentencing" (April 27th 2019). Electoral sentencing cycles were also the subject of a February 23rd 2015 episode of John Oliver's popular TV show *Last Week Tonight*.

[5] In previous work, **?** finds a more pronounced sentencing cycle for minorities in Kansas, while **?** find the opposite in North Carolina, i.e., a more pronounced sentencing cycle for whites.

[6] **?** find *within* Kansas that sentencing cycles show up in districts with partisan elections, and not in districts with retention elections. (Kansas is unusual in having within-state variation in these rules.)

In pooled regressions, there is some evidence that states with partisan elections exhibit stronger estimated sentencing cycles, as well as some evidence that political donor activity correlate with stronger estimated sentencing cycles. However, by far the most significant predictor of sentencing cycles is whether the incumbent was being challenged at all.

Our paper contributes to the literature on judge behavior and court sentencing (**???**), and specifically builds on and expands the aforementioned work on electoral sentencing cycles.[7] Our first core finding—the heterogeneity in electoral sentencing cycles across U.S. states—enriches previous findings and opens avenues for future research in this literature. Our second core finding—that the strength of electoral sentencing cycles is a function of the underlying degree of electoral competition in judicial elections—provides evidence for the previously postulated mechanism that electoral cycles exist because judges internalize their voters' views more when they seek re-election. This finding also resonates with a broader literature showing that electoral accountability impacts the choices of elected officials (**??????**).

Our paper also contributes to the literature on the effect of cultural norms on sentencing outcomes. **?** find that judges moving within counties of their judicial district in Northern Carolina are more likely to adopt local sentencing norms over time. **?** show that counties with elected judges react to changes in voters' attitudes toward drug-related crimes due to slant media resulting in longer sentencing for drug offenses. We contribute to this literature by showing that counties, where challenging incumbent judges are more accepted, have stronger electoral cycles than those where it is not socially acceptable to challenge judges (i.e., making elections less competitive).

In Section 5, we discuss avenues for future research to explore the root cause of our second core finding: why are incumbent judges rarely challenged at all in some states but frequently challenged in others? Lastly, we revisit our opening paragraph to more broadly discuss how our findings speak to the trade-offs inherent in electing versus appointing local public officials.

2 Data

Section 2.1 discusses the sentencing data that was obtained separately from ten states. Section 2.2 discusses cross-state differences in the rules governing judicial elections, and in their observed

[7] A related line of research focuses on judge quality. **?** shows that elections may even reduce the quality of judges if re-election pressures deter highly qualified judges from entering.

competitiveness. Section 2.3 discusses how we the measure judicial electoral cycles (and, by extension, electoral sentencing cycles).

2.1 Sentencing Data

We contacted the majority of U.S. states' sentencing commissions with requests for access to their trial court data (alternatively referred to as circuit courts or lower courts in some states). In the end, 18 states had digitized their trial court sentencing data, and had processes in place for sharing these data. Of these, 10 states included judge identifiers in their sentencing data (a requirement for estimating electoral cycles). Online Appendix A.1 reports on the institutions in charge of the data in each state, the relevant contacts, and details the process of requesting the data. In total, these 10 states provided us with data on over three million sentencing decisions. Tennessee's data had the longest time coverage (1980–2017), Colorado the shortest (2010–2016). See Table 1. We consistently observe defendants' race and gender, except in Virginia, where the data does not include any defendant characteristics. Among the full sample of crimes, 18% of defendants are women and 30% are black. For the sample of severe crimes 11% of defendants are women and 38% are black. We also observe other non-white race groups (Asians, Native Americans, and Hispanics),[8] but neither previous research nor our own estimations display a consistent relation between these and sentence lengths (relative to the omitted white category). Recidivism is the defendant characteristic that has the most variability in how it is reported. Some states report counts of previous convictions, some report dummies for having been previously convicted, some report information on the severity of previously committed crimes, and four states do not report recidivism at all. Fortunately, different transformations of the recidivism measure have little bearing on our core results.

Empirical studies of judicial electoral cycles emphasize that electoral cycles should be expected primarily for more severe crimes because these are more visible to voters who may follow them in the media. These are also the cases where voters seem to prefer more severe punishments on average (relative to sentencing guidelines). **?** therefore censor their study of Pennsylvania sentencing to court cases of "aggravated assault, rape, and robbery convictions." Similarly, **?** restrict their study of Washington to severe crimes "as defined by the FBI ... assault, murder, rape, and

[8] The only exception is Alabama, which reports only Black, White, and Other.

Table 1: Sentencing Data

	WA	GA	KY	MN	NC	AL	TN	PA	CO	VA
Panel A: The Full Data, by State										
All Years	2004-2015	2010-2018	2002-2018	1991-2014	2006-2016	2002-2016	1980-2017	2001-2016	2010-2016	2006-2016
# Years	11	8	16	23	10	14	37	15	6	10
All Cases	132,940	39,853	80,261	76,255	250,503	59,925	185,877	267,160	51,668	23,325
Mean Sentence (All, in months)	17	50	56	33	17	70	70	12	74	26
Defendant Race	✓	✓	✓	✓	✓	✓	✓	✓	✓	-
Defendant Gender	✓	✓	✓	✓	✓	✓	✓	✓	✓	-
Defendant Recidivism	✓	-	✓	✓	✓	-	✓	✓	-	-
Panel B: Severe Crimes used in Electoral Cycles Analysis										
Severe Cases	13,124	2,433	4,150	11,888	34,906	10,701	20,515	12,866	6,395	2,412
Share: Severe Cases	9.9	6.1	5.2	15.6	13.9	17.9	11.0	4.8	12.4	10.3
Mean Sentence (Severe, in months)	74	69	166	88	43	134	139	70	159	70

Notes: This table reports on the number of cases and time span for which we have data from each state. In addition, the table reports on aggregate sentence length and whether the main defendant characteristics (race, gender, recidivism) are included in the data. States are sorted from left to right by their electoral institutions (reported in Table 2.) Washington, North Carolina, and Pennsylvania are set off visually as the states whose data was used in previous research on electoral cycles.

robbery." We follow this approach and only consider criminal cases involving assaults, murders, rapes, and robberies. Panel B of Table 1 shows the number of severe crimes in each state, as well as their share of the total number of cases. The average sentence length is confirmed to be significantly larger for severe crimes.

2.2 Cross-State Differences between Judicial Elections

Variation in Rules: There is considerable cross-state variation in the rules that govern judicial elections. The states in our data represent all possible sets of rules that exist. Nationwide, there are 9 states with partisan judge elections, 22 have non-partisan ones, 3 have partisan elections for entrants and retention elections for incumbents, 10 have appointments for entrants and retention elections for incumbents, and 11 have appointments only.[9] In our data, reported in Table 2, Washington, Georgia, Kentucky, and Minnesota have non-partisan elections. Alabama and Tennessee have partisan elections, where a judge has a party affiliation and may face a challenger from his or her own part in a primary. Pennsylvania has a unique mix whereby new judges initially face

[9] The numbers sum up to over 50 because 4 states have within-state variation in these rules. See **?**, Table.1 and **?** for excellent discussions of cross-state variation in judicial electoral/appointment rules.

partisan elections, but thereafter sit for a ten-year term at the end of which they stand for retention elections, i.e., they face only a yes/no vote and no challenger. Colorado and Virginia both appoint new judges. In Colorado, these initially appointed judges later face retention election, whereas in Virginia they are re-appointed on fixed cycles.[10]

Table 2 also displays considerable variation in judicial electoral cycle lengths: Two states have four-year cycles, three have six-year cycles, four have eight-year cycles. Newly appointed judges in Pennsylvania need to run for partisan re-elections at the next electoral cycle (i.e., within two years), upon which they serve for ten-year cycles that culminate in retention elections.[11] The combination of retention elections and unusually long election cycles would suggest it is ex ante less likely to find electoral sentencing cycles in Pennsylvania, a fact already noted by **?**, 250-251.

Competitiveness of Judicial Elections: The actual measured level of competitiveness of judicial elections is quite variable across states. The best measure of this is the average number of donors who contributed to a judge's electoral campaign, taken from **?**. As a secondary measure, we use `ballotpedia.org` to construct the share of judicial elections that faced any challenger in each state from 2012–2016 (when this data was consistently available). Both variables are reported at the bottom of Table 2. The two measures of judicial competitiveness have a surprisingly similar distribution: the average number of donors per race is 26, and the average share of contested races is 28%.[12] The two measures also display considerable cross-state variation in the average degree of electoral competition that judges face: In Washington and North Carolina, the average number of donors per race was 30 and 34 respectively, while in Kentucky this number was 6. These data do not cover Colorado or Tennessee. For Virginia, we set the number to zero. Similarly, in Washington and North Carolina, 32% and 38% of elections respectively were contested, while in Minnesota this share was 10%.[13] In Virginia, we set this value to zero since judges are almost always re-appointed, and there are no election donors.

The last four rows of the table show how dispersed judicial elections are over time within each

[10] **?** suggest that even appointed judges are not perfectly insulated from the electorate.

[11] In some states, newly appointed judges then have to run for electoral confirmation at the *next* election cycle (i.e., within two years). In other states, the electoral cycles is tied to the seat, and when a newly appointed judge needs to run for their first election depends on when in their cycle their predecessor retired.

[12] We treat the level of judicial competition as a time-invariant state-specific feature because information about challenges is very incomplete outside the 2012–2016 window.

[13] In Pennsylvania, we took a weighted average of the 28% of No votes in retention elections, and the much higher share of contested partisan elections for newly appointed judges.

state. The main point of asking this question is to clarify how much residual variation in electoral cycles there can be in the data once one conditions on time-controls. Because available data vary by state, we normalize all years as follows: judge elections that happen in the year of a presidential election (2000, 2004, 2008, etc) are dated as $t = 1$, elections the year after (2001, 2005, 2009, etc) are dated as $t = 2$, and so on for $t = 3$ and $t = 4$. The data show that the share of judges that is up for re-election is almost uniformly spread over time in most states, indicating that the identifying variation for the estimation of sentencing cycles in a state comes from all data, and not only from, say, the year of a presidential election.

Table 2: Judicial Elections and Electoral Cycles

	WA	GA	KY	MN	NC	AL	TN	PA	CO	VA
Initial Selection Rules:	Nonpartisan					Partisan			Appointment	
Re-Election Rules:	Nonpartisan					Partisan		Retent-Reel.		Reappt.
Most Common Entry Method	Nonpart.	Appointment								
Avg # donors per judge-race	30	26	6	20	34	32	-	35	-	0
Prob. electoral challenge	32	13	33	10	38	31	37	33.7	27	0
Cycles	4y.	4y.	8y.	6y.	8y.	6y.	8y.	10y.	6y.	8y.
Share Judges election-year t=1	20.6	33.1	24.2	17.4	32.6	14.1	26.9	20.2	36.0	27.1
Share Judges election-year t=2	28.7	15.3	27.7	36.4	25.6	15.9	22.9	21.2	16.8	24.6
Share Judges election-year t=3	27.7	25.4	25.0	33.0	22.4	37.7	24.3	31.4	22.9	24.1
Share Judges election-year t=4	23.0	26.2	23.1	13.2	19.5	32.3	25.9	27.3	24.3	24.1

Notes: This table reports on judicial electoral institutions in each state. Washington, North Carolina, and Pennsylvania are set off visually as the states whose data was used in previous research on electoral cycles. As well, it reports on the number of individual judges in each state whom we could merge to our judicial biography database. The rules of selection and re-election are well-know and have been reported in other sources. The 'most common entry method,' discussed in Section 2.3 under 'Judge Entry' is to our knowledge a novel fact, and we have not seen it discussed anywhere else in the literature.

2.3 Measuring Electoral Sentencing Cycles

We follow **?** and **?** in defining judge j's judicial election cycles as a linear running variable 'proximity to election' (PtE_{jt}) that is scaled from 0 to 1

$$PtE_{j(s)t} = t/T_s, \tag{1}$$

starts at 0 on the day after a general election, and increases by $1/T_s$ each day until it equals 1 on the day of the next general election. T_s is the length of state s's electoral cycle, i.e., $T_{WA} = 4 \times 365$

in Washington, and $T_{NC} = 8 \times 365$ in North Carolina.

Judge Entry and Establishing Re-Election Dates: In general, judicial elections are held on the general election cycle, i.e., in early November of every even-numbered year.[14] However, judicial elections are staggered in the same way as the elections of U.S. senators so that only a portion of judges is up for re-election in any given election year. The sentencing data contains no information on which judges are up for re-election in which election-year. Unfortunately, it is also not possible to infer electoral cycles from a judge's entry or exit in the sentencing data. This is because it turns out that judges mostly exit the profession at times that do not coincide with the electoral cycles.[15] The flipside of this fact is that, with seats needing to be filled, judges also mostly enter the profession outside of the regular electoral cycle, via gubernatorial appointment. Washington is the only state in our data where we found the majority of judges entered through elections. This is noted as the 'most common entry method' in Table 2. These facts mean that observed entry and exit in the sentencing data are not sufficient information for establishing in which year a judge is running for re-election. In order to construct judges' electoral cycles, we therefore had to code up individual judge biographies (entry/appointment dates, re-election dates, and retirement dates) from `www.ballotpedia.org`. See details in Online Appendix A.2.

Filing Dates, Primaries, and General Elections: There is considerable variation in when a judge is actually under electoral pressure:

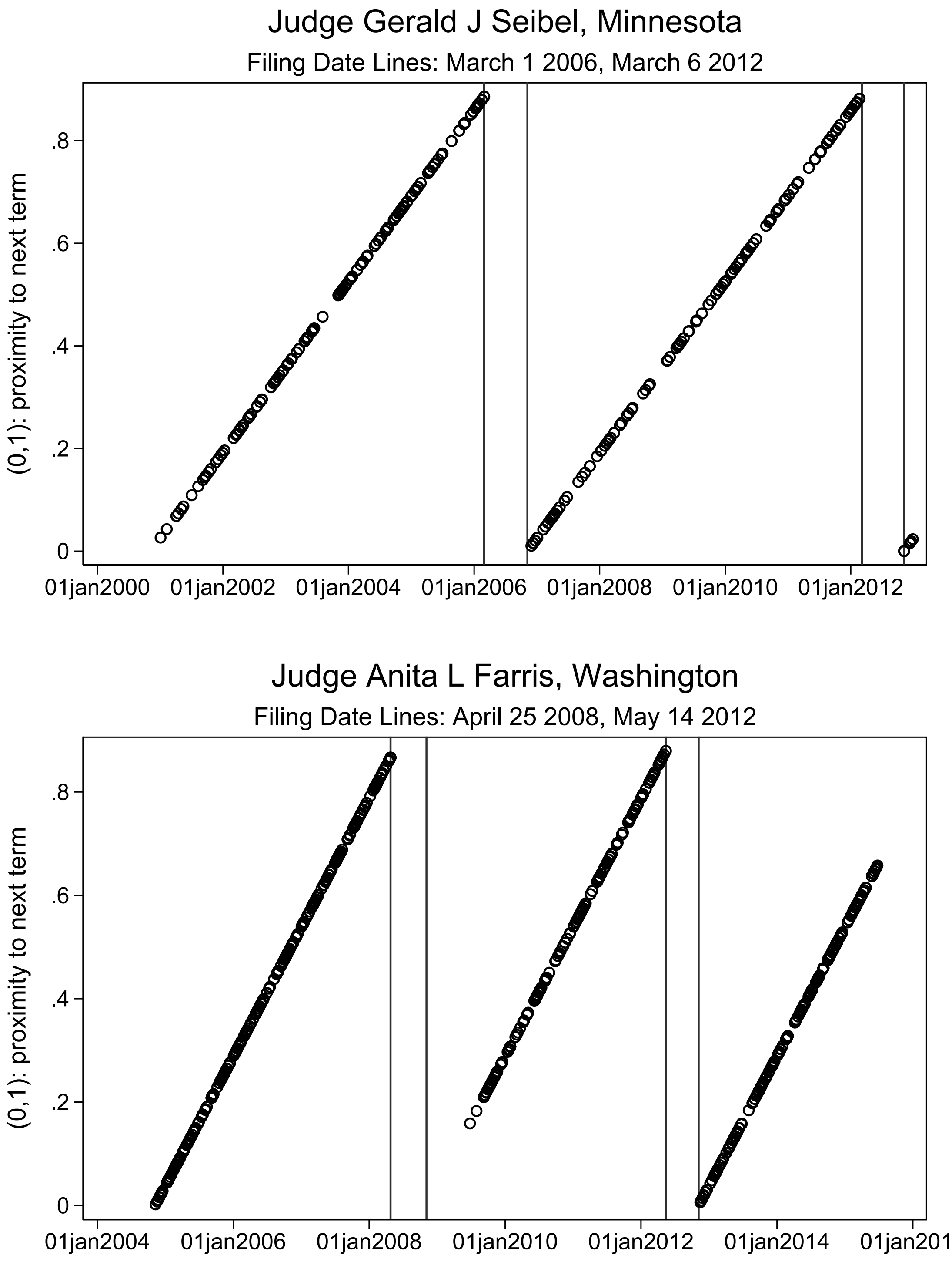
- First, there is an official filing date by which incumbents and challengers need to file their intent to (re-)run for the judgeship. Up to the filing date, all judges are under the threat of an electoral challenger. In the very frequent cases where no challenger files, all electoral competition on the incumbent effectively ends on the filing date.

- States with partisan elections have a primary between the filing date and the general election date. A common pattern in the data is to have a competitive primary election (say, between two Democratic Party candidates) that is followed by an uncontested general election, because all candidates are from the same party. In such cases, electoral pressure peaks

[14] Pennsylvania is the only state in the country to hold judicial elections solely in odd-numbered years. In Georgia elections happen in even years but the month varies and the election can take place as early as in May.

[15] Most exiting judges retire, relatively few die, and some move to their states' higher courts, or move to federal courts. The retiring judges may thereafter enter private practice, or continue as part-time 'senior judges'.

Figure 1: Examples of Proximity to Election (PtE_{jt}) over Time



Notes: (*a*) This figure shows two example judge bios and electoral cycles in our data. All data on electoral cycles is collected from ballotpedia.org. In Minnesota (top panel), judges are elected for six-year cycles. In Washington (bottom panel), judges are elected for four-year cycles. (*b*) Proximity on the vertical axis is defined on a $0, 1$ scale as in expression (1), where proximity equals 1 on the day of the general elections in early November. We trim the electoral cycles at the state-wide filing date, after which the electoral cycle effectively ends for the large majority of judges who have no challenger for their seat. The time between filing date and general election date is sandwiched between two vertical lines. The electoral cycle restarts with the general election date. An observation is a day in which a judge passed a sentence.

between the filing date and the primary date and then goes to zero between the primary and the general election. However, in partisan-election states, one can also have the opposite: no challenger in the primary but a challenger in the general election.

- Even states with non-partisan elections have a primary election if there is more than two candidates for a seat. In those cases, the general election is a run-off between the two candidates with the highest vote share in the primary. Electoral competition can peak in the primary (for example, if the challenger who comes third in the primary subsequently endorses the incumbent, making the general election less competitive), or it can peak in the general (for example, if the challenger who comes third in the primary subsequently endorses the remaining challenger).

In summary, the evolution of electoral pressure on an incumbent can be highly non-monotonic after the filing date, depending on the above scenarios. In Washington, **?** collected information on which races had a challenger and show that the sentencing cycle drops off after the filing date when there is no challenger. Unfortunately, in our broader sample information on challengers is very difficult to obtain in most cases; and attempting to collect it would result in a fragmentary and likely un-representative sample, with the availability of the information likely endogenous to the election. We therefore omit cases after the filing date in our baseline specification. This means PtE_{jt} usually peaks somewhere around 0.9 for judge j, before re-starting at 0 the day after the election date. Figure 1 illustrates this for two randomly drawn judges, one in Washington, one in Minnesota.[16] Figure 2 shows how close the filing date is to the general election date in each state. As a robustness check, we will also include cases after the filing date in some specifications.

3 Results

This section presents the core findings of our paper. Our approach is to go through a variety of specifications that test for the presence of electoral sentencing cycles, and to apply each specifica-

[16] There is a concern with this approach that judges may postpone contentious or visible cases until after the filing date. We check for this in Online Appendix B, but find no evidence of bunching of severe cases after the filing date.

tion separately to the ten states in the data. Our baseline estimation framework is

$$\text{SentenceLength}_{it} = \beta \cdot PtE_{jt} + \beta_{\text{X}} \cdot X_i + \mu_j + \mu_t + \mu_c + \epsilon_{ijt}, \tag{2}$$

where i identifies the court case, X_i are case characteristics, and PtE_{jt} is the 'proximity to election' defined in expression (1) for judge j. PtE_{jt} is our core regressor of interest, and it is evaluated relative to judge fixed effects μ_j that control for unobserved judge heterogeneity. For time controls μ_t we include year fixed effects and we also follow **?** and others in including quarter-of-year fixed effects to avoid spurious effects from other political cycles that coincide with election cycles. We also include county fixed effects μ_c to control for local characteristics; these are often co-linear with judge fixed effects, but not always because some judges switch district over the course of their tenure.[17]

Table 3 reports the results of estimating equation (2) separately for each state across the ten columns. The first five states have non-partisan elections, the next two states have partisan elections, Pennsylvania has a combination of initial partisan elections and later retention elections, Colorado has initial appointments and later retention elections, and in Virginia judges are appointed and re-appointed by the state legislature. Each panel of Table 3 reports on results of one specification. Panel A is our baseline specification, which focuses on severe crimes, includes as controls the defendant's race, gender, age, recidivism, and crime severity, and which two-way clusters standard errors by calendar-year (to account for trends in sentencing) and quarter-of-year (to account for cyclical patterns that could correlate with sentencing cycles). In Panel A, we cluster by year as well as by quarter to match the quarter-of-year fixed effects. Panel B is different only in using heteroscedasticity-robust standard errors, as in **?**'s study of Pennsylvania. (In Online Appendix Table 1, we alternatively cluster by calendar-quarter, replacing $4 + t$ fixed effects with $4 \times t$ fixed effects (for t years), and find similar results.) Taking these differences approaches to inference together, we confirm the existence of electoral sentencing cycles that previous research had found in the three states in our data that were covered by previous research: Washington, North Carolina, and Pennsylvania. Judges in these three states appear to issue sentences that are respectively 4.3, 3, and 15.2 months longer when a judge approaches their re-election than when

[17] In North Carolina, judges are required to rotate across districts, a fact that is exploited in a very nice identification strategy in **?**.

they are at the beginning of the electoral cycle.[18]

Table 3: Electoral Cycles in Judicial Sentencing in 10 States

	Dependent variable: Sentence (months)									
	WA	GA	KY	MN	NC	AL	TN	PA	CO	VA
Panel A: Baseline										
Proximity to election	4.318**	1.013	0.346	2.595	2.989**	4.375	3.822	15.214	-0.500	-7.739
	(1.1034)	(5.0592)	(8.9529)	(2.0438)	(0.7563)	(13.2667)	(6.1284)	(9.2765)	(15.3092)	(5.6663)
R-squared	0.527	0.737	0.420	0.386	0.423	0.119	0.656	0.389	0.465	0.323
Observations	13,124	2,433	4,150	11,888	34,906	10,701	20,515	12,866	6,395	2,412
Panel B: Robust s.e.										
Proximity to election	4.318	1.013	0.346	2.595	2.989**	4.375	3.822	15.214***	-0.500	-7.739
	(5.5215)	(3.7486)	(9.3843)	(2.4537)	(1.2102)	(9.0834)	(7.5038)	(5.1531)	(11.0688)	(7.3542)
R-squared	0.527	0.737	0.420	0.386	0.423	0.119	0.656	0.389	0.465	0.323
Observations	13,124	2,433	4,150	11,888	34,906	10,701	20,515	12,866	6,395	2,412
Panel C: All Crimes										
Proximity to election	0.432	1.945	-0.733	0.204	0.624**	5.033	-0.378	0.667	2.147	-2.578
	(0.3443)	(2.0679)	(1.2999)	(0.5960)	(0.1794)	(3.0149)	(1.1445)	(0.4487)	(3.9139)	(2.0439)
R-squared	0.569	0.243	0.805	0.565	0.445	0.129	0.503	0.363	0.491	0.504
Observations	132,940	39,853	80,261	76,255	250,503	59,925	185,877	267,160	51,668	23,325

Notes: *(a)* Each panel reports on results of a one specification, run for each state separately across columns. *(b)* All regressions include defendant's race, gender, age, age squared, and an indicator for recidivism. All regressions also include the case's severity, and the number of charges in each case. Finally, all regressions include judge fixed effects and year as well as three quarter-of-year fixed effects. *(c)* Panel B differs from A only in the treatment of standard errors. Panel C re-estimates Panel A for all crimes. *(d)* Standard errors are two-way clustered by calendar-year and quarter-of-year in all panels except B. We compute heteroscedasticity-robust standard errors in Panel B. *** p<0.01, ** p<0.05, * p<0.1

Panel C includes data on all crimes, not just severe crimes, and as expected this also generally weakens the existing evidence for sentencing cycles. Interestingly, **?**'s study of North Carolina is the only one we know of that uses all crimes in its base sample, and North Carolina is the only state where sentencing cycles are noticeable even when all crimes are considered.[19]

Strikingly, none of the other six states with judicial elections in our data display anything near statistically significant sentencing cycles in panels A and B, although the coefficient on PtE_{jt} is positive in all of them. Interestingly, this coefficient is negative in Virginia, which is the only state in our data where judges are appointed, and can therefore serve as somewhat of a counter-factual benchmark for the absence of electoral cycles.

In summary, the core observation in Table 3 is that there is far more heterogeneity in the presence of electoral cycles than previously thought. To state this another way, we employ a seemingly

[18] In Online Appendix Table 2, we report on specifications when electoral cycles are not normalized from 0 to 1 but instead start at 0 the day after an election and increases by 1 each day. this shrinks the Pennsylvania estimate (where cycles are particularly long) closer to the Washington estimate, but also shrinks the North Carolina estimate (where cycles are also particularly long) further away from the Washington one. The point of this exercise is to scale coefficients and standard errors down by T_s; statistical precision is naturally unaffected.

[19] **?**, **?**, and **?** all focus on severe crimes.

unrelated regression (SUR) framework to test whether electoral cycles in states with significant estimates are statistically different from those in states with insignificant coefficients. Table 4 contains the *p-values* from the state-by-state comparison of the coefficients using SUR. (We ranked the three states with statistically significant electoral cycles according to the size of the coefficient.) Pennsylvania's estimate does not statistically differ from those of Washington and North Carolina. Pennsylvania's cycle is statistically larger than cycles in Georgia, Minnesota, and Virginia. The difference is also close to significant for Kentucky, but not for Alabama, Tennessee, or Colorado. Virginia is again useful as a quasi-placebo in that it is the only state in our data that does not have judicial elections. Correspondingly, it displays the most significant difference relative to other states, particularly Pennsylvania, North Carolina, Washington, and Georgia (with the difference to each displaying a *p-value* of 0.12 or less).

Table 4: Differences between State-Specific Estimates

	PA	WA	NC	GA	KY	MN	AL	TN	CO	VA
PA	-	0.217	0.351	0.019**	0.189	0.015**	0.302	0.844	0.275	0.005***
WA		-	0.863	0.635	0.663	0.557	0.943	0.266	0.738	0.114
NC			-	0.262	0.547	0.122	0.691	0.537	0.986	0.051*
GA				-	0.843	0.820	0.800	0.487	0.908	0.120
KY					-	0.903	0.745	0.491	0.967	0.430
MN						-	0.736	0.374	0.960	0.151
AL							-	0.461	0.803	0.214
TN								-	0.601	0.182
CO									-	0.454
VA										-

Notes: *(a)* This matrix contains p-values of the state-by-state comparison of the coefficient $\hat{\beta}$. *(b)* We report p-values in square brackets, based on robust standard errors (Panel B of Table 3). *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Further Robustness: In **??**, we scrutinize the robustness of this pattern to data issues. One main data-quality concern is measuring recidivism, which is unavailable in some states' data, as reported in Table 1. It is encouraging that except for Alabama, the states without recidivism information do not stand out as having a lower *R-squared*. This is because a crime's measured severity in most states' coding already factors in recidivism. Alabama's low *R-squared* is explained by the (in our data, unique) combination of no recidivism information and a severity coding that does not factor in recidivism. In Panel A, we transform the recidivism categories into a single dummy to check whether a coarser measurement of recidivism matters. Encouragingly, it doesn't. In Panel B, we drop the recidivism control altogether. Thought there is some variation in how much the

R-squared is affected by this, our coefficient of interest is, encouragingly, never affected much.[20] In Panel C, we additionally control for sentencing guidelines, which we only observe in Washington, Alabama, Minnesota, and Pennsylvania. Controlling for sentencing guidelines makes sentencing cycles more pronounced in states where we find them (i.e., Washington and Pennsylvania) and moves the coefficient closer to zero for states where we don't find them in the baseline specification (i.e., Alabama and Minnesota). In short, this does not change the overall pattern. In **??**, we use a pooled-data estimation (replacing $competitiveness_s$ in equation (4) with measures of data quality) to show that differences in sentencing cycles are not driven by the state-specific differences in data quality, along the dimensions of recidivism, whether states allow concurrent sentences, and the quality of the measures of crime severity.

Time-District Varying Confounders: In **??**, we scrutinize the robustness of this pattern to confounding factors that time-vary in different ways across districts. One potential example of this could be that prosecutors run for re-election at different times in different districts in a state. We address this by adding wither county-quarter fixed effects or and county-year fixed effects. The main results are robust to these specifications, and in fact strengthen.

Acquittals: One might worry about acquittals (zero-sentences). These are omitted from the analysis because in our sample of severe crimes sentencing guidelines should prevent judge from acquitting unless the defendant was innocent, or there was insufficient evidence or procedural errors. As a robustness check, Panel A in Table 5 adds acquittals, and confirms that this marginally weakens results (as expected) while not affecting the overall pattern in the data. Another way of looking at the acquittal issue is that there should be no effect of sentencing cycles on the probability of being acquitted, since this should be determined by innocence or insufficient proof. Panel B replaces the outcome with an indicator for being convicted and confirms this is indeed the case.

Time-Path of Sentencing Cycles: The results thus far document pronounced heterogeneity in the presence of sentencing cycles across states. Another potentially interesting form of heterogeneity is the time path that the sentencing cycles takes over the electoral cycle (in the states where it exists). To investigate this, we re-estimate regression (2), but replace the linear regressor PtE_{jt}

[20] To provide one more piece of evidence on the comparability of the data quality, we also report the coefficients on defendant characteristics (race, gender, and recidivism) in **??**, and these also turn out to all have the expected signs and comparable magnitudes across states.

Table 5: Acquittals

	WA	GA	KY	MN	NC	AL	TN	PA	CO	VA
	Dependent variable: Sentence (months)									
Panel A: Add Zero Sentences										
Proximity to election	4.546*	1.850	0.046	1.157	2.987**	3.872	6.162	12.970	0.619	-8.575
	(1.5303)	(2.9155)	(9.2803)	(1.4153)	(0.7381)	(11.9983)	(6.5706)	(8.4785)	(14.9656)	(5.8958)
R-squared	0.529	0.572	0.420	0.369	0.420	0.104	0.639	0.420	0.468	0.327
Observations	13,630	6,540	4,209	13,181	35,033	12,849	22,787	16,109	6,624	2,728
	Dependent variable: D(Sentence > 0)									
Panel B: Alternative Outcome										
Proximity to election	0.0098	0.0292	-0.0014	-0.0078	0.0001	0.0130	0.0152	0.0067	0.0004	0.0023
	(0.0065)	(0.0172)	(0.0072)	(0.0134)	(0.0019)	(0.0168)	(0.0100)	(0.0089)	(0.0004)	(0.0175)
R-squared	0.130	0.327	0.274	0.168	0.071	0.168	0.374	0.374	0.996	0.164
Observations	13,630	6,540	4,209	13,181	35,033	13,181	16,109	16,109	6,624	2,728

Notes: *(a)* Each panel reports on results of a one specification, run for each state separately across columns. *(b)* All regressions include defendant's race, gender, age, age squared, and an indicator for recidivism. All regressions also include the case's severity, and the number of charges in each case. Finally, all regressions include judge fixed effects and year as well as three quarter-of-year fixed effects. *(c)* Panel A differs from the baseline estimation in Table 3 in that we add zero-sentences (acquittals). Panel B replaces the outcome with an indicator for any conviction. *(d)* Standard errors are two-way clustered by calendar-year and quarter-of-year in all panels. *** p<0.01, ** p<0.05, * p<0.1

with a schedule of quarterly fixed effects over the full electoral cycle. The estimated quarterly fixed effects are reported in Figure 2.[21] Figure 2 suggests that in Washington and Pennsylvania the effect of proximity to the next election really increases in particular in the last two quarters of the cycle. In contrast, the effect appears linear over the full cycle in North Carolina (and Minnesota, to the extent that it can be considered as having a sentencing cycle in Table 3).

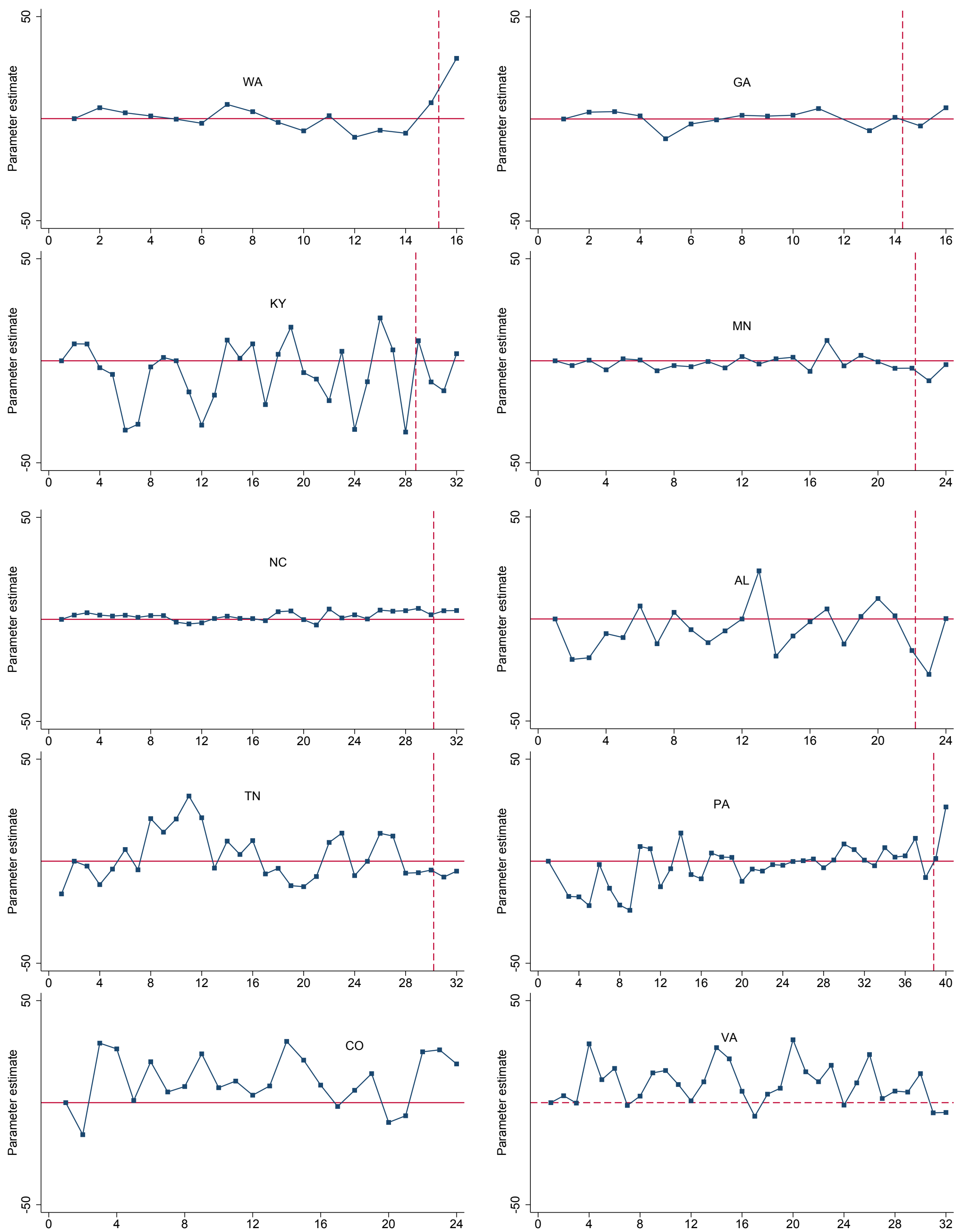
In order to estimate the full set of quarterly fixed effects for Figure 2, we include all data, including cases after the filing date. The filing date is depicted as a dashed vertical line near the end of the electoral cycle in Figure 2. Cases after the filing date are omitted on principle in Table 3 because we are unable to measure electoral pressure on judges in this time-window (see the discussion at the end of Section 2.3). However, because Figure 2 suggests that the sentencing cycle in Washington is most pronounced after the filing date, we also re-estimate the baseline estimation with these cases included. Panel A of **??** shows indeed that the baseline effect gets stronger in Washington when we include data after the filing date. Results in other states are qualitatively unaffected.[22]

There may also be a concern that judges could, instead of levying harsher sentences in the lead-up to the filing date, postpone contentious or visible cases until after the filing date to avoid a

[21] For visual clarity of the time-path of the cycle, we omit confidence bands from Figure 2. These are depicted in **??**.

[22] One may have expected the effect to also get stronger in Pennsylvania, but it is important to recall that in Pennsylvania the filing date only applies to judges running for their first election. See Table 2.

Figure 2: Quarter Fixed Effects



Notes: This figure depicts estimates from a regression that replaces PtE_{jt} with a full set of quarterly fixed effects, with the first quarter omitted (visually normalized to zero). The number of estimated quarter fixed effects varies with the length of a state's cycle. The dashed vertical line towards the end of a cycle marks the filing date. In Washington for example, this is about 5 month or 1.67 quarters before the general election.

challenger running against them on the basis of a contentious ruling. If this was the case we would expect some bunching of cases after the filing date, and we would expect this to be concentrated in the severe cases. In fact, what we find is the opposite: **??** shows no evidence of bunching either side of the filing date for severe-crime cases, but there is some evidence for bunching of non-severe cases before the filing date. For completeness, Panel B of **??** also reports on a specification where we replace the linear regressor from expression (1) with a count of quarters.[23]

Racial Biases in Electoral Sentencing Cycles: Another interesting and important potential form of heterogeneity in sentencing cycles pertains to race. Race-biased sentencing cycles have previously been investigated in the literature, with **?** finding a more pronounced sentencing cycle for minorities in Kansas, and **?** finding the opposite in North Carolina, i.e., a more pronounced sentencing cycle for whites. To isolate a race-based sentencing cycle, we estimate the following specification

$$\text{SentenceLength}_{it} = \beta \cdot PtE_{jt} + \beta_M \cdot PtE_{jt} \cdot Minority_i + \beta_X \cdot X_i + \mu_j \cdot Minority_i + \mu_t + \mu_c + \epsilon_{ijt}. \quad (3)$$

Aside from adding the interaction between PtE_{jt} and the race of the defendant, the noteworthy extension in specification (3) is that we estimate two separate sets of judge fixed effects that are specific to defendant's race. This is important because it allows judges' baseline sentencing attitudes to vary by the defendant's race, so that (3) mirrors a split-sample estimation strategy in which the core estimation (2) would be run separately for minorities and non-minorities, but at the same time imposes a common set of coefficients on other controls unrelated to race.[24]

Panel A of Table 6 reports the results of estimating specification (3). Panel B replaces $\beta \cdot PtE_{jt}$ with $\beta_{NM} \cdot PtE_{jt} \cdot Non\text{-}Minority_i$. Thus, while Panel A tests if there is a statistically significant difference in whites' and minorities' sentencing cycles, Panel B instead tests for each cycle separately if it is significantly different from zero. The difference between the sentencing cycles for whites and minorities is never itself statistically significant in Panel A. However, North Carolina's sentencing cycle is more precisely estimated when we consider only whites (in both panels, relative

[23] For example, Washington's electoral cycle is 4 years, i.e., 16 quarters, so that the coefficient in Panel B is roughly $1/16$ the baseline coefficient ($0.27 \approx 4.32/16$).

[24] We code Black, Hispanic, and Native Americans as minority, and Whites and Asians as non-minority. Results are similar when Asian defendants are re-classified as a minority.

Table 6: Race Differences

	Dependent variable: Sentence (months)									
	WA	GA	KY	MN	NC	AL	TN	PA	CO	VA
*Panel A: Baseline (with Minority*Judge-FE)*										
Proximity to election	2.251	3.094	-4.356	-0.542	4.434**	-3.536	-3.027	5.895	-10.096	-
	(1.9142)	(5.8960)	(9.3079)	(2.7860)	(1.0882)	(23.4451)	(8.9122)	(8.6420)	(15.9525)	-
Proximity to election x Minority	7.437	-4.106	14.397	7.897	-2.187	20.336	11.568	18.751	27.231	-
	(6.7837)	(6.6730)	(12.0746)	(4.4283)	(1.6402)	(24.3193)	(8.1850)	(8.5042)	(23.3266)	-
R-squared	0.534	0.752	0.429	0.395	0.427	0.140	0.658	0.398	0.477	
Observations	13,116	2,384	4,134	11,847	34,779	10,695	20,508	12,855	6,386	
Panel B: Separate Sentencing Cycles										
Proximity to election x Non-Minority	2.251	3.094	-4.356	-0.542	4.434**	-3.536	-3.027	5.895	-10.096	-
	(2.3496)	(5.9567)	(9.1994)	(2.7012)	(1.1273)	(23.3012)	(8.8653)	(8.5972)	(16.6314)	-
Proximity to election x Minority	9.688	-1.013	10.040	7.355*	2.247	16.800	8.541	24.646	17.135	-
	(4.7327)	(5.3926)	(8.9666)	(3.0257)	(1.2582)	(11.9546)	(4.2787)	(11.6019)	(25.8361)	-
R-squared	0.534	0.752	0.429	0.395	0.427	0.140	0.658	0.398	0.477	
Observations	13,116	2,384	4,134	11,847	34,779	10,695	20,508	12,855	6,386	

Notes: *(a)* Each panel reports on results of a one specification, run for each state separately across columns. *(b)* All regressions include defendant's race, gender, age, age squared, and an indicator for recidivism. All regressions also include the case's severity, and the number of charges in each case. Finally, all regressions include race-specific judge fixed effects and year as well as three quarter-of-year fixed effects. *(c)* Panel A reports on specification (3). *(d)* Panel B reports on specification (3) but replaces $\beta \cdot PtE_{jt}$ with $\beta_{NM} \cdot PtE_{jt} \cdot \textit{Non-Minority}_i$. *(e)* Standard errors are two-way clustered by calendar-year and quarter-of-year in all panels. *** p<0.01, ** p<0.05, * p<0.1

to Table 3); and Pennsylvania's sentencing cycle in Panel B is more precisely precisely estimated and economically large when we consider only minorities.

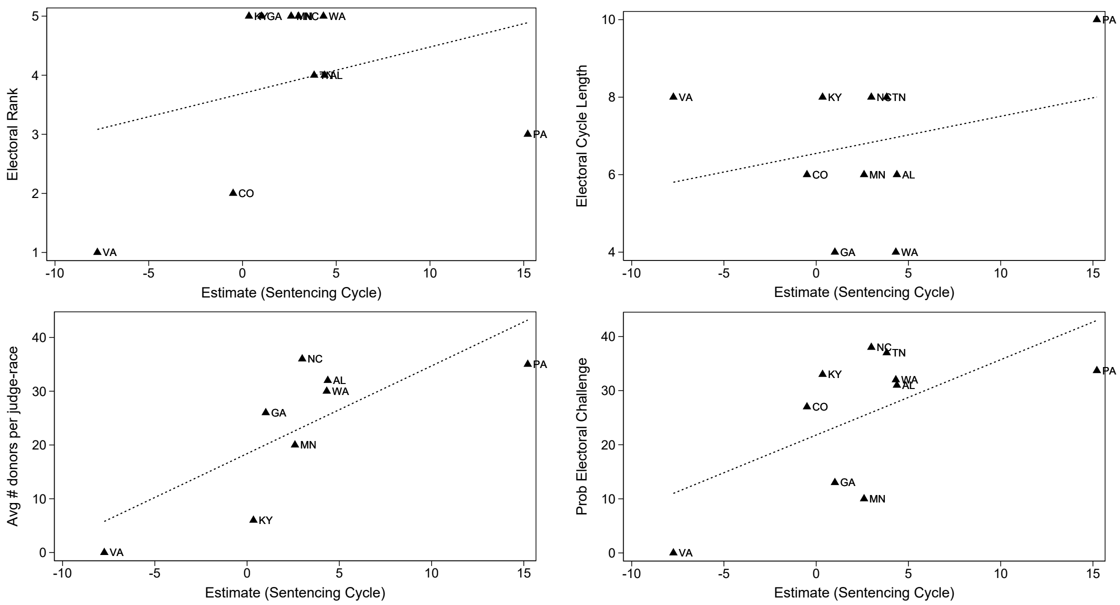
In summary, Figure 2 and Table 6 show that our core finding (sentencing cycles are heterogeneous) extends beyond mere heterogeneity in their presence. Even within the states that display significant evidence for sentencing cycles, these vary in their time-paths and in which population of defendants they most apply to.

4 Potential Explanations for the Presence of Sentencing Cycles

The core finding of Section 3 is that the strength and the "shape" of electoral sentencing cycles are heterogeneous across U.S. states. This naturally raises the question "what explains this heterogeneity?" Guided by the relevant literature that points to electoral competition as being at the heart of any political cycles (**???**), we hypothesize that the heterogeneity in the presence of sentencing cycles is driven by heterogeneity in the competitiveness of judicial elections.

To provide an answer to our question "what explains heterogeneity," we consider four determinants of $competitiveness_s$: first, variation in electoral rules (i.e. retention, partisan, non-partisan elections) likely determines the competitiveness of elections. Second, variation in the length of

Figure 3: Potential Correlates of Sentencing Cycles



Notes: (*a*) Each panel considers one of the four determinants of $competitiveness_s$ (see Table 2), and plots them against the point estimates of the sentencing cycle reported in Panel A of Table 3.

electoral cycles might also determine election competitiveness because longer cycles generate a stronger incumbency advantage. Third, donor activity will be a key determinant of a race's competitiveness. Fourth, differences in the baseline (state-wide) probability of having a challenger will determine the electoral pressures incumbents face.

Empirically, we can directly measure electoral cycle length, the level of donor activity, and the races in which incumbents face a challenger. All of these measures are reported and discussed in Section 2.2. Each electoral rules variation can be operationalized through indicator variables. For each measure, we rely on cross-state variation. This means that the evidence we provide next will necessarily be suggestive rather than causal.[25]

We begin by reporting simple visual correlations between each measure and the baseline estimate of the sentencing cycle in each state in Figure 3. For this illustrative purpose only, we rank-order electoral rules in order of their inherent competitiveness as follows: appointment (Virginia),

[25] The first two measures naturally vary only across states, while the third and fourth measure (donor and challenger activity) do vary by race within a state. However, we have data on these latter two only for a limited number of years (and therefore a limited share of judicial elections). Furthermore, measuring them at the level of the election would bias us towards finding them to be more important than state-level variation in rules (by reducing measurement error); without getting us closer to causal identification. For these reasons, we chose to aggregate these measures up to also treat them as cross-state sources of variation.

retention election (Colorado), initial election and later retention election (Pennsylvania), partisan election (Alabama and Tennessee), non-partisan election (Washington, Georgia, Kentucky, Minnesota, and North Carolina).[26] This 'electoral rank' measure correlates positively with the sentencing cycle estimate, as expected. The top-right panel shows weak evidence that election-cycle length may correlate positively with sentencing cycles, possibly because longer cycles imply judgeships are more secure and thus more valuable. The bottom-left shows that measures of donor activity correlates positively and strongly with the sentencing cycle estimate, indicating that differences in electoral competition may explain the differences in sentencing cycles. The bottom-right suggests the same when donor activity is replaced with the probability of being challenged in the race. The correlations in Figure 3 only use the point estimate from Table 3.

Table 7: Pooled Panel with Interactions

	I	II	III	IV	V	VI	VII	VIII	IX	X
	Dependent variable: Sentence (months)									
Proximity to election	3.742	1.025	-0.194	-5.074	-3.639*	-4.740	-2.923	-3.283	-6.441	-9.711
	(2.4180)	(2.1194)	(9.8918)	(6.2894)	(1.8597)	(4.8511)	(1.9769)	(4.7055)	(5.4406)	(7.1870)
Proximity to election										
x Partisan Election		7.838	9.057				6.042	4.687		
		(5.2230)	(10.2324)				(5.6452)	(6.2315)		
x Nonpartisan Election			1.369							
			(9.2299)							
x Cycle-Length				1.312					0.589	0.837
				(0.8672)					(0.9580)	(0.8234)
x Prob. electoral challenge					0.279***		0.173*		0.236**	
					(0.0906)		(0.0890)		(0.1152)	
x # donors per judge-race						0.303		0.196		0.281
						(0.1942)		(0.1604)		(0.1939)
R-squared	0.440	0.440	0.440	0.440	0.440	0.261	0.440	0.261	0.440	0.261
Observations	133,756	133,756	133,756	133,756	133,756	104,311	133,756	104,311	133,756	104,311

Notes: *(a)* This table pools all states and interacts the main regressor of interest (proximity to election) with measures of the competitiveness of judicial elections. All baseline controls are included. *(b)* Standard errors are two-way clustered by state-year and three state-specific quarter-of-year indicators, thus essentially just stacking the number of clusters of the individual regressions in Section 3. *** p<0.01, ** p<0.05, * p<0.1

The correlations in Figure 3 are only suggestive. To better gauge the relative importance of these factors (still without making causal claims), we therefore estimate

$$\text{SentenceLength}_{it} = \beta \cdot PtE_{jt} + \beta_C \cdot PtE_{jt} \times competitiveness_s \tag{4}$$

$$+\beta_X^s \cdot X_i + \mu_j + \mu_{ts} + \mu_c + \epsilon_{ijt},$$

[26] We recognize that both the ordinality and the cardinality of this ranking can be contested.

where β_C estimates whether sentencing cycles are a function of the competitiveness of judicial elections;[27] μ_{ts} are state-specific year fixed effects, β_X^s are state-specific coefficients on defendant controls, and judge and district fixed effects (which are always state-specific). We two-way cluster standard errors by state-year and state-specific quarter-year, essentially stacking the number of clusters of the individual regressions in Section 3.

Table 7 reports on the results. The first column reports on a simple pooled version of the sentencing cycle specification (which corresponds to specification (4) without the term $\beta_C \cdot PtE_{jt} \times competitiveness_s$). In the pooled sample, the estimated sentencing cycle is marginally insignificant overall. In columns II–III of Table 7, we test whether primary and non-primary elections are associated with stronger sentencing cycles than retention elections or appointments. Partisan elections do appear to perhaps give rise to stronger electoral cycles, but the effect is very imprecise. The results continue to show a strong effect for partisan elections, and no significant effect for non-partisan elections. Next, Column IV provides some evidence that longer cycles are in fact associated with stronger cycles, albeit again imprecisely so.[28] The two determinants for $competitiveness_s$ considered in Columns II–IV are rules-based or institutional measures.

In contrast, challenger activity and donor activity are more direct measures of the revealed competitiveness of elections. We introduce these in Columns V–VI. The average number of donors per race is 26, and the average share of contested races is 28%. At these averages, sentencing cycles are positive: $-3.639 + 0.279 \times 26 = 3.62$; and $-4.740 + 0.303 \times 28 = 3.74$. We introduce these in columns V–VI. Challenger activity is highly significant (columns V), whilst donor activity is marginally insignificant (column VI). In Columns VII–X, we "horse-race" these observed measures of the competitiveness against each of the two rules-based measures. In all cases, challenger activity is the dominant predictor of the strength of electoral cycles.[29]

[27] Of the four measures of $competitiveness_s$ that we consider, the first two are state-rules which naturally cannot vary by judge. The third and fourth measures are state-aggregates of individual races. In principle, the third and fourth measure are measurable separately for each judge. However, at the judge-level, these measures are likely to be endogenous to characteristics of the race. Because of this, and to maintain a consistent granularity between the four measures, we consider only state averages for the third and fourth measure.

[28] Mechanically, column IV reflects the fact that North Carolina and Pennsylvania both have long electoral cycles. **??** re-estimates the results without Virginia to provide a comparison only within states with elected judges.

[29] The number of observations is reduced for the **?** measure because it is unavailable for Colorado and Tennessee. **??** shows that the other results look similar when we exclude Colorado and Tennessee everywhere. In **??**, we show that the principal component of the two measures of revealed competition also interacts significantly in shaping the strength of electoral cycles.

In summary, electoral rules appear to play some role in explaining the heterogeneity in the presence of sentencing cycles, but the stronger correlate with the presence of sentencing cycles is residual variation in the competitiveness of judicial elections (i.e. challenger and donor activity) that is unexplained by formal electoral rules. As an additional check, Table 8 interacts each of the two measures of revealed electoral competition with the dummy for partisan elections. The reported results suggest that revealed electoral competition has a larger effect in states with partisan elections. This is consistent with the conjecture that the stakes in partisan judicial elections are higher, as suggested for instance by the contention associated with the electoral challenges that worked their way through the courts in several battleground states in the aftermath of the 2020 presidential election.

Table 8: Competitiveness Interactions

	I	II
	Dependent variable: Sentence (months)	
Proximity to election	-2.451	-2.673
	(1.5613)	(3.0905)
Proximity to election		
x Partisan Election	-31.506***	-80.506***
	(4.7490)	(2.4692)
x Prob. electoral challenge	0.153***	
	(0.0376)	
x Partisan Election x Prob. electoral challenge	1.136***	
	(0.1747)	
x # donors per judge-race		0.171
		(0.1086)
x Partisan Election x # donors per judge-race		2.574***
		(0.0547)
R-squared	0.440	0.261
Observations	133,756	104,311

Notes: Standard errors are two-way clustered by state-year and three state-specific quarter-of-year indicators. *** p<0.01, ** p<0.05, * p<0.1

5 Discussion

In this section, we first discuss what may explain differences across states in the likelihood of sitting judges being challenged during election time, and we secondly discuss the virtues of the American practice of electing public officials who in most other countries are appointed bureaucrats.

To explore some avenues for future research on the first question, we conducted several informal interviews with judges and legal scholars. Based on these conversations, it appears that the answer might lie with cross-state differences in norms within the judicial profession. In one state, a sitting judge told us that incumbents are almost never challenged because judicial electoral competition is "frowned upon" and judgeships are viewed as something to be bestowed by gubernatorial appointment as a hallmark of one's professional standing. In another state, a sitting judge told us of unfettered competition for judgeships and a complete lack of checks and constraints on electoral competition. Demonstrating the existence of such differences in professional norms in a quantitative and statistically well-identified way, and ultimately understanding their origins, appears to us a fruitful avenue for future research.

Finally, we briefly discuss how our research relates to a more general discussion of the American practice of electing public officials who in most other countries would be appointed bureaucrats. In addition to the tens thousands of elected judges to be found in each states' judicial districts, the U.S. has today around 90,000 unique governments, including one federal, 50 state, roughly 3,000 county, 35,000 municipal and township governments, as well as roughly 50,0000 school and special districts (**?**, Table 11). All of these governments give rise to thousands of elected positions that may be appointed in less federal systems. Examples include school board members in education, city council members and county supervisors in general administration, and sheriffs in law enforcement.

It is clear that there are trade-offs in having such a federal system. In the opening paragraph of our paper, we cite SCOTUS Chief Justice Roberts' concern that "judges are not politicians, even when they come to the bench by way of the ballot," as well as **?**'s two-century old prediction that judicial elections "will sooner or later lead to disastrous results, and that some day it will become clear that to reduce the independence of magistrates in this way is to attack not only the judicial power but the democratic republic itself." The presence of electoral cycles is one manifestation of these concerns: from a defendant's perspective, it is clearly not just to receive a higher sentence merely because one's presiding judge was closer to their re-election. On the other hand, electoral cycles also reflect the fact that judicial elections create an underlying baseline "policy congruence" between the preferences of public officials and their electors (**?**). This policy congruence in itself is arguably a positive thing: if a local population prefers stricter "law and order" relative to the (state or federal) center, it can obtain this, at least to degree, through local elections of its public officials. Locally elected public officials can also be a moderating force when the central government's policy bliss point diverges substantially from the bliss point of some of its sub-localities. In theory, such sub-localities may then break off into forming new polities (**?**). In practice, however, such break-offs are rarely thought to be practical or efficient. The federalism provided by locally elected public officials can therefore be an important moderating force. A good recent example for this comes from California in 2020 during the COVID-19 epidemic. California was the first U.S. state to impose stay-at-home order on March 19 after the National Emergency was announced on March 13, 2020. When the governor's stay-at-home orders diverged too far from many Californians' policy bliss points, a sizable share of California's locally elected sheriffs decided not to enforce them.[30]

6 Conclusion

This paper makes two core contributions. The first is to empirically document that the strength and shape of electoral sentencing cycles across U.S. states is much more heterogeneous than previously appreciated. The second is to provide evidence on the cause for this heterogeneity: sentencing cycles appear to be a function of the observed competitiveness of judicial elections, as measured by challenger activity. States where incumbents are least likely to be challenged display the least evidence for sentencing cycles. This second finding corroborates the mechanism, previously postulated in the existing literature, that electoral cycles exist because judges internalize their voters' views more when they seek re-election. This finding also resonates with a broader literature showing that electoral accountability impacts the choices of elected officials. We end the paper with a discussion of avenues for future research on why incumbent judges rarely challenged at all in some states but frequently challenged in others.

[30] https://reason.com/2020/12/08/southern-california-sheriffs-rebel-over-gavin-newsoms-new-stay-at-home-order/

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Online Appendix to

"Rules Versus Norms: How Formal and Informal Institutions Shape Judicial Sentencing Cycles"

by Christian Dippel and Michael Poyker

Online Appendix A Data Description

Online Appendix A.1 Sentencing Data

Sentencing data was collected separately from each state. 15 states were willing to share their data with us for free or at reasonable cost: Alabama, Arkansas, Georgia, Kentucky, Maryland, Minnesota, Mississippi, Nevada, North Carolina, Oregon, Pennsylvania, Tennessee, Texas, Virginia, and Washington.

We contacted each state with the following initial data request:
The data we are looking for has a court case (or ’sentencing event’) as the unit of observation. In some states the data is organized by charge (with several charges making up the case or sentencing event) and that is equally fine. The key data that we need are:
1. date, month and year of sentencing,
2. type of crime,
3. length of sentencing,
4. type of sentencing (low-security, high security, etc),
5. defendant’s sex,
6. defendant’s race,
7. court identifier
8. name of judge or judge identifier number,
9. type of court that convicted (trial, appeal, etc),
10. in what prison the person was sent
We do not seek any information that identifies defendants.
Sincerely, XXX

There were 10 states that (*i*) shared their sentencing data in digitized form and (*ii*) their data included the judge identifiers needed to estimate judge political cycles.[31] The following reports for each state the office responsible for storing the data, as well as relevant contacts at the time we requested the data between late 2016 and late 2018. Some states had considerably longer processing times than others. These were typically do either to backlogs of data-technicians or to having to go get our request vetted and signed off on by other individuals.

1. Alabama
   - Initial contact with the Sentencing Commission at http://sentencingcommission.alacourt.gov/
   - After emailing sentencing.commission@alacourt.gov, Bennet Wright processed our request.
   - Time between data application and delivery: 16 months.
2. Colorado
   - Initial contact with the Colorado Court Services Division, at https://www.courts.state.co.us/Administration/Division
   - Jessica Zender, the Court Programs Analyst at the Court Services Division processed our request.

[31] We also obtained sentencing data from Arkansas, Maryland, Mississippi, Nevada, Oregon, and Texas, but these states’ data does not include judge identifiers

- Time between data application and delivery: 1 month.

3. Georgia

    - Initial contact with Department of Corrections at http://www.dcor.state.ga.us/Divisions/ExecutiveOperations/OPS/OpenRecords.
    - After emailing open.records@gdc.ga.gov it was recommended we go through their 'Media Inquiries' under +1-478-992-5247, where Jamila Coleman coordinated our request with their data technicians.
    - Time between data application and delivery: 3 months.

4. Kentucky

    - We spoke on the phone to Cathy Schiflett at the Kentucky Courts Research and Statistics Department.
    - She guided us to https://courts.ky.gov/Pages/default.aspx, where we had to select 'Statistical Reports' and then submit our data request.
    - Daniel Sturtevant handled our request.
    - Time between data application and delivery: 9 months.

5. Minnesota

    - Initial contact with the Minnesota Sentencing Guidelines Commission at http://mn.gov/sentencing-guidelines/contact/contact-us.jsp
      Email address: sentencing.guidelines@state.mn.us
    - Kathleen Madland was the Research Analyst who processed our request
    - Time between data application and delivery: 2 months

6. North Carolina

    - Initial contact though http://www.ncdoj.gov/Top-Issues/Public-Integrity/Open-Government/Understanding-Public-Records.aspx
    - Then we were put in touch with the North Carolina Administrative Office of the Courts, where our data request was processed by the 'Remote Public Access' data technicians
    - Time between data application and delivery: 3 months

7. Pennsylvania

    - In Pennsylvania, sentencing data can be requested from the Sentencing Commission at http://pcs.la.psu.edu/data/request-and-obtain-data-reports-and-data-sets/sentencing/data-sets
    - Leigh Tinik processed our request
    - Time between data application and delivery: 1 month

8. Tennessee

    - Initial contact with Tennessee's Department of Corrections at https://www.tn.gov/correction/article/tdoc-prison-directory

- Tanya Washington, the DOC's Director of Decision Support: Research & Planning, processed our request
- Time between data application and delivery: 6 months

9. Virginia

- Initial contact was through a web-form of the Virginia Criminal Sentencing Commission at http://www.vcsc.virginia.gov/
- After being initially denied on the grounds that FOIA requests could only be processed for Virginia residents, we called +1-804-225-4398, and were eventually approved after speaking to the director Meredith Farrar-Owens.
- Time between data application and delivery: 3 months

10. Washington

- Initial contact with the Department of Corrections at http://www.doc.wa.gov/aboutdoc/publicdisclosure.asp, where Duc Luu processed our request
- We use essentially the same data as **?**
- Time between data application and delivery: 2 weeks

Online Appendix A.2 Judicial Biography Data

All data about judge electoral cycles was taken from the ballotpedia.org. The site contain information about the judges of each circuit court for each state.[32] The individual page of each judge contain data for age and gender of a judge, the dates when she was appointed/elected, date of retirement (if already retired), name of a governor by whom she was appointed (if appointed), and whom the judge replaced.

To collect the data research assistants started with the contemporary judges, collected their data and proceeded with their predecessor judges. This procedure resulted in collecting information for approximately 80% of the judges mentioned in the sentencing data. For the states where the name of a judge was known we searched those judges individually on the sites of their courts and added them to the dataset.

Ten of the states in this paper include judge names or identifiers in the sentencing data: Alabama, Colorado, Georgia, Kentucky, Minnesota, North Carolina, Pennsylvania, Tennessee, Virginia, and Washington. We then code up judge biographies, including when they are up for reelection from Where judges are identified by name, merging the judge biographies is straightforward. Where only judge identifiers are given, these identifiers still almost always include a variant of the judges' initials. As well we observe entry and exit dates and which circuit a judge id is identified with.

[32]Or courts of the similar level.

Online Appendix B Robustness Checks

Online Appendix Table 1 reports on a version of Table 3 where we alternatively cluster by calendar-quarter, replacing $4 + t$ fixed effects with $4 \times t$ fixed effects (for t years). We find similar results.

Table Online Appendix Table 1: Baseline with S.E. Clustered by Calendar-Quarter

	Dependent variable: Sentence (months)									
	WA	GA	KY	MN	NC	AL	TN	PA	CO	VA
Panel A: ~ year x quarter s.e.										
Proximity to election	4.318	1.013	0.346	2.595	2.989***	4.375	3.822	15.214*	-0.500	-7.739
	(7.8991)	(3.2327)	(9.0335)	(2.8742)	(0.9593)	(9.5358)	(5.9321)	(7.7291)	(11.6138)	(8.8728)
R-squared	0.527	0.737	0.420	0.386	0.423	0.119	0.656	0.389	0.465	0.323
Observations	13,124	2,433	4,150	11,888	34,906	10,701	20,515	12,866	6,395	2,412

Notes: *(a)* Each panel reports on results of a one specification, run for each state separately across columns. *(b)* All regressions include defendant's race, gender, age, age squared, and an indicator for recidivism. All regressions also include the case's severity, and the number of charges in each case. Finally, all regressions include judge fixed effects and year as well as three quarter-of-year fixed effects. *(c)* This table replicates the baseline specification from Panel A of Table 3 but clusters standard errors by calendar quarter (i.e., quarter-year). *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table Online Appendix Table 2: Baseline with Days of the Cycle

	Dependent variable: Sentence (months)									
	WA	GA	KY	MN	NC	AL	TN	PA	CO	VA
Panel A: ~ x days of cycle										
Proximity to election	0.0030**	0.0007	0.0001	0.0012	0.0010**	0.0020	0.0013	0.0042	-0.0002	-0.0027
	(0.0008)	(0.0035)	(0.0031)	(0.0009)	(0.0003)	(0.0061)	(0.0021)	(0.0025)	(0.0070)	(0.0019)
R-squared	0.527	0.737	0.420	0.386	0.423	0.119	0.656	0.389	0.465	0.323
Observations	13,124	2,433	4,150	11,888	34,906	10,701	20,515	12,866	6,395	2,412

Notes: *(a)* Each panel reports on results of a one specification, run for each state separately across columns. *(b)* All regressions include defendant's race, gender, age, age squared, and an indicator for recidivism. All regressions also include the case's severity, and the number of charges in each case. Finally, all regressions include judge fixed effects and year as well as three quarter-of-year fixed effects. *(c)* This Table replaces the linear regressor from expression (1) with a "number of days since the election"; for example, Washington's electoral cycle is 4 years, i.e., 1461 days, so that the coefficient goes from 0 to 1461 instead of from 0 to 1. *(d)* Standard errors are two-way clustered by calendar-year and quarter-of-year in all panels. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table Online Appendix Table 3: Robustness to State-Specific Data Issues

	Dependent variable: Sentence (months)									
	WA	GA	KY	MN	NC	AL	TN	PA	CO	VA
Panel A: redefine recidivism as dummy										
Proximity to election	4.318**	0.899	-	2.485	3.174**	-	4.653	14.880	-	-
	(1.3219)	(5.0656)	-	(2.0686)	(0.7896)	-	(5.9734)	(9.1835)	-	-
R-squared	0.527	0.736		0.385	0.418		0.654	0.386		
Observations	13,124	2,434		11,888	34,915		20,517	12,866		
Panel B: w/o recidivism variable										
Proximity to election	4.550**	0.906	-	3.252	3.039**	-	3.964	15.189	-	-
	(1.1617)	(4.9945)	-	(1.9956)	(0.9334)	-	(5.7643)	(9.7623)	-	-
R-squared	0.509	0.736		0.340	0.403		0.645	0.370		
Observations	13,124	2,434		11,888	34,915		20,517	12,866		
Panel C: add sent. guidelines										
	4.062**	-	-	1.410	-	1.132	-	32.454*	-	-
	(0.9546)	-	-	(1.7554)	-	(12.9622)	-	(12.9622)	-	-
R-squared	0.810			0.713		0.803		0.510		
Observations	13,124			11,888		10,701		12,866		

Notes: *(a)* Each panel reports on results of a one specification, run for each state separately across columns. *(b)* All regressions include defendant's race, gender, age, age squared, and an indicator for recidivism. All regressions also include the case's severity, and the number of charges in each case. Finally, all regressions include judge fixed effects and year as well as three quarter-of-year fixed effects. *(c)* This Table's specifications are based on Panel A of Table 3. Panels A–B re-code recidivism as a dummy, or omit it. (The number of observations goes up in some states because fewer observations are absorbed by recidivism-category fixed effects.) Panel C add sentencing guidlines as a control variable. *(d)* Standard errors are two-way clustered by calendar-year and quarter-of-year. *** p<0.01, ** p<0.05, * p<0.1

To confirm that the broad patterns in the sentencing data are consistent in all states, **??** reports the coefficients on defendant characteristics (race, gender and recidivism) that went unreported Table 3. All of these patterns have the expected signs, match previous research, and are sign consistent with each other: judges in all states pass shorter sentences for women, judges in all but one state pass longer sentences for black defendants, and judges in all states pass longer sentences for recidivists. The coefficient on recidivism is the most variable across the states, which reflects the fact that —unlike the race and gender dummies— the recidivism dummy can cloud substantial variation in the degree of recidivism. This is discussed in footnote 20.

Table Online Appendix Table 4: Effect of Defendant Characteristics on Sentence Length

	Dependent variable: Sentence (months)									
	WA	GA	KY	MN	NC	AL	TN	PA	CO	VA
Female	-4.982***	-7.566***	-3.277***	-5.513***	-3.748***	-15.466***	-13.180***	-1.750***	-2.846	-
	[0.0007]	[0.0000]	[0.0063]	[0.0001]	[0.0001]	[0.0001]	[0.0001]	[0.0004]	[0.2406]	-
Black	2.291***	2.250**	2.251*	3.101***	1.552***	6.390***	5.322***	1.961***	-0.058	-
	[0.0030]	[0.0138]	[0.0582]	[0.0005]	[0.0018]	[0.0014]	[0.0035]	[0.0019]	[0.9709]	-
Recidivist, (0 or 1)	11.946***	69.223***	-	29.499***	10.267***	-	140.035***	10.779***	-	-
	[0.0000]	[0.0005]	-	[0.0000]	[0.0000]	-	[0.0000]	[0.0001]	-	-
R-squared	0.564	0.254	0.802	0.542	0.420	0.125	0.477	0.360	0.492	
Observations	139,900	100,413	81,442	122,616	251,907	94,071	215,539	463,236	53,683	

Notes: *(a)* Each panel reports on results of one specification, run for each state separately. All cases included. *(b)* We use dummy for recidivism instead of the scaled variable for the sake of data representation; however, estimates for the proximity to election do not change if we use scaled recidivism. *(c)* We report p-values in square brackets. *** p<0.01, ** p<0.05, * p<0.1

Table Online Appendix Table 5: Pooled Panel with Interactions - Data Quality

	I	II	III	IV
	Dependent variable: Sentence (months)			
Proximity to election	2.624	7.379*	3.717	3.632
	(2.9788)	(3.5866)	(3.0241)	(2.0652)
Proximity to election				
x D(Observed recidivism)	1.556			
	(2.4891)			
x D(Concurently sentence)		-5.496		
		(3.1115)		
x D(Ordinal severity categories)			0.048	
			(3.6533)	
x D(# of crime categories)				0.0003
				(0.0031)
R-squared	0.445	0.445	0.445	0.445
Observations	116,082	116,082	116,082	116,082

Notes: *(a)* This table re-runs Column I of Table 7 with interactions related to state-level data quality. *(b)* Standard errors are multi-way-clustered by quarter-year and state level. *** p<0.01, ** p<0.05, * p<0.1

Table Online Appendix Table 6: Baseline with Alternative Fixed Effects

	Dependent variable: Sentence (months)									
	WA	GA	KY	MN	NC	AL	TN	PA	CO	VA
Panel A: ~ county x quarter FEs										
Proximity to election	4.8208***	0.6746	1.4409	2.5621	3.0358**	7.1997	3.7154	15.3821	-1.5959	-5.3227
	(0.6155)	(3.6948)	(11.4120)	(2.1447)	(0.7857)	(12.9804)	(6.1508)	(9.4199)	(14.5865)	(5.8507)
R-squared	0.532	0.761	0.451	0.390	0.429	0.127	0.661	0.395	0.470	0.369
Observations	13,124	2,433	4,150	11,888	34,906	10,701	20,515	12,866	6,395	2,412
Panel B: ~ county x year x FEs										
Proximity to election	7.2910**	-1.0076	11.0339	2.9153	3.3364**	0.8342	2.5250	15.7413	-1.2419	9.1324
	(1.3671)	(3.4039)	(21.8589)	(3.0298)	(0.8374)	(12.6809)	(8.1264)	(8.9478)	(13.8308)	(9.6950)
R-squared	0.541	0.758	0.536	0.404	0.437	0.148	0.676	0.425	0.476	0.401
Observations	13,124	2,433	4,150	11,888	34,906	10,701	20,515	12,866	6,395	2,412

Notes: *(a)* Each panel reports on results of a one specification, run for each state separately across columns. *(b)* All regressions include defendant's race, gender, age, age squared, and an indicator for recidivism. All regressions also include the case's severity, and the number of charges in each case. Finally, all regressions include judge fixed effects and year as well as three quarter-of-year fixed effects. *(c)* This Table replicates the baseline specification from Panel A of Table 3 but uses alternative fixed effects. Panel A adds county-quarter fixed effects. Panel B adds county-year fixed effects. *(d)* Standard errors are two-way clustered by calendar-year and quarter-of-year in all panels. *** p<0.01, ** p<0.05, * p<0.1

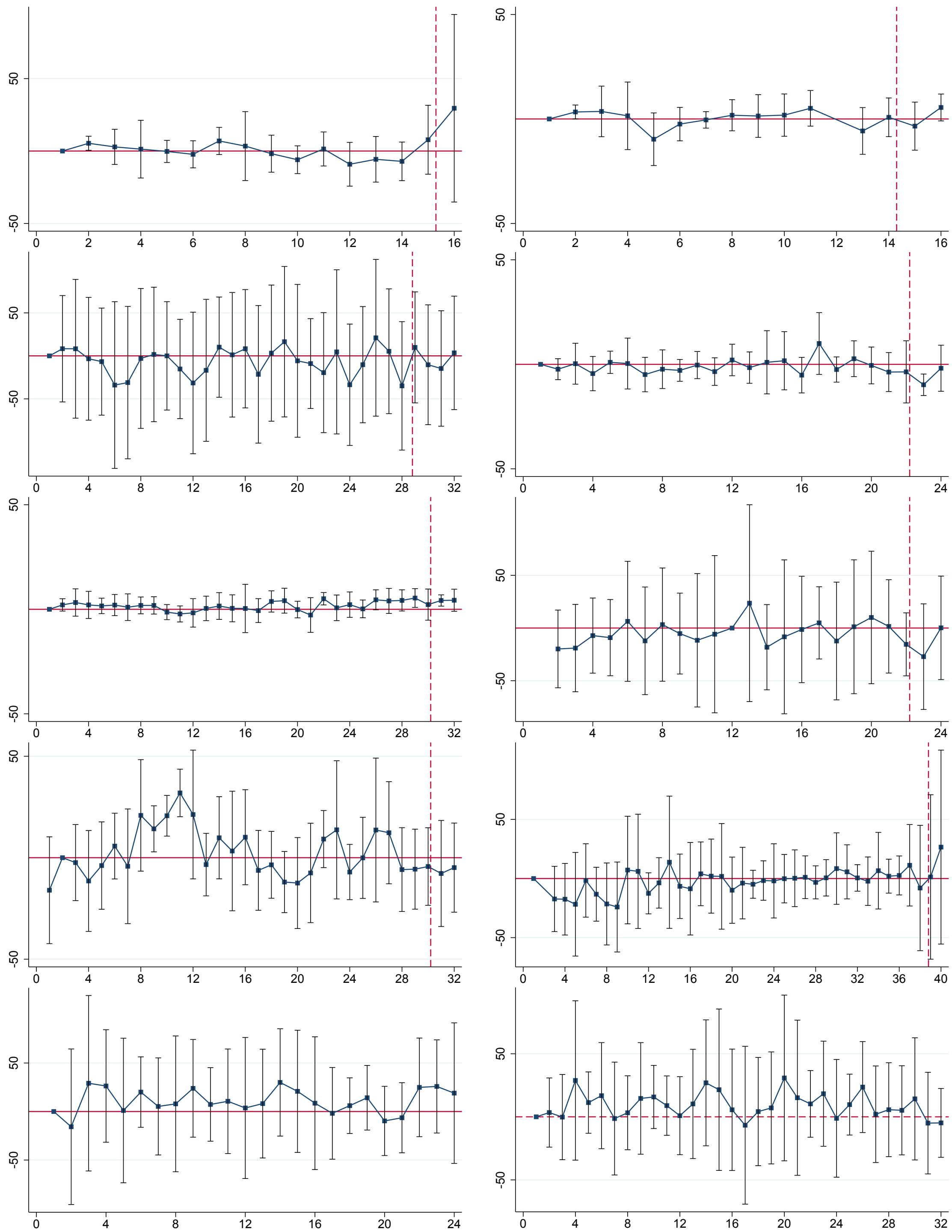
Table Online Appendix Table 7: Quarters and Filing Date

	Dependent variable: Sentence (months)									
	WA	GA	KY	MN	NC	AL	TN	PA	CO	VA
Panel A: Include Cases after Filing-Date										
Proximity to election	8.822**	1.629	-2.250	-0.574	2.844**	3.270	6.166	13.988	-	-
	(2.7494)	(1.9602)	(7.5145)	(1.5761)	(0.7428)	(11.3719)	(3.2564)	(7.6693)	-	-
R-squared	0.517	0.571	0.420	0.386	0.421	0.105	0.641	0.427		
Observations	13,774	7,664	4,250	15,532	35,290	14,045	24,866	17,781		
Panel B: Quarters (Ordinal-Scale)										
Proximity to election	0.283*	0.119	0.006	0.056	0.098**	0.165	0.187	0.320	0.101	-0.261
	(0.0962)	(0.1769)	(0.2941)	(0.0599)	(0.0262)	(0.5103)	(0.2136)	(0.2148)	(0.6328)	(0.1823)
R-squared	0.527	0.737	0.420	0.386	0.423	0.119	0.656	0.389	0.465	0.323
Observations	13,124	2,433	4,150	11,888	34,906	10,701	20,515	12,866	6,395	2,412

Notes: *(a)* Each panel reports on results of a one specification, run for each state separately across columns. *(b)* All regressions include defendant's race, gender, age, age squared, and an indicator for recidivism. All regressions also include the case's severity, and the number of charges in each case. Finally, all regressions include judge fixed effects and year as well as three quarter-of-year fixed effects. *(c)* Panel A adds cases after the filing date. Panel B replaces the linear regressor from expression (1) with a "count of quarters"; for example, Washington's electoral cycle is 4 years, i.e., 16 quarters, so that the coefficient in Panel B is roughly $1/16$ the baseline coefficient $(0.28 \approx 4.32/16)$. *(d)* Standard errors are two-way clustered by calendar-year and quarter-of-year in all panels. *** p<0.01, ** p<0.05, * p<0.1

As discussed in footnote 21, **??** adds confidence bands to Figure 2.

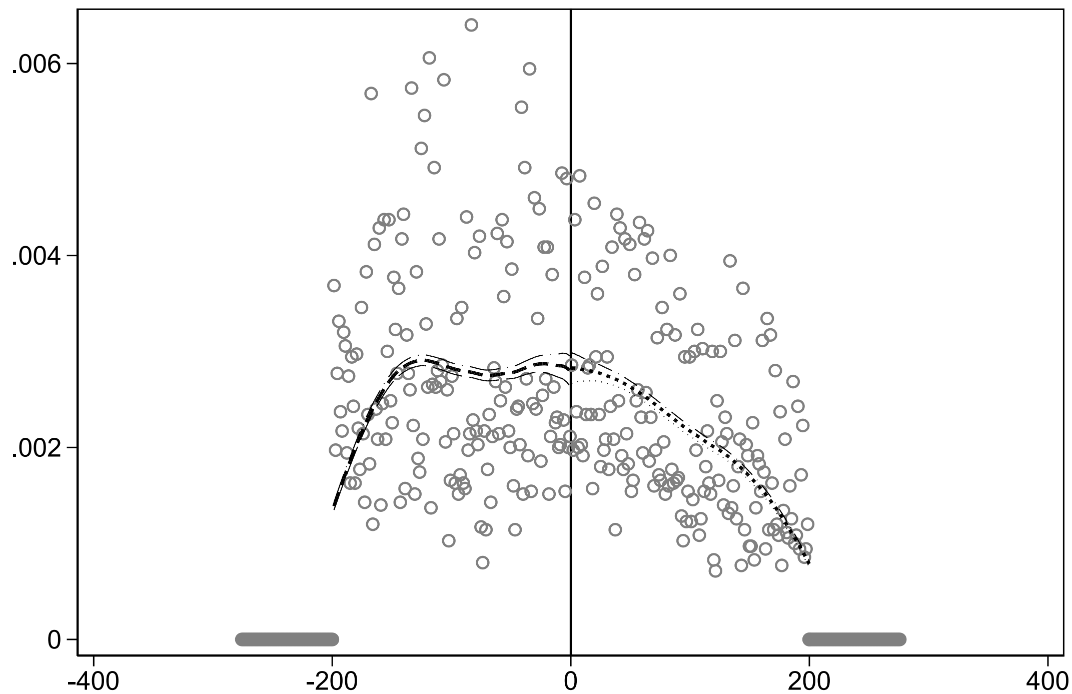
Figure Online Appendix Figure 1: Quarterly Indicators with Confidence Bands

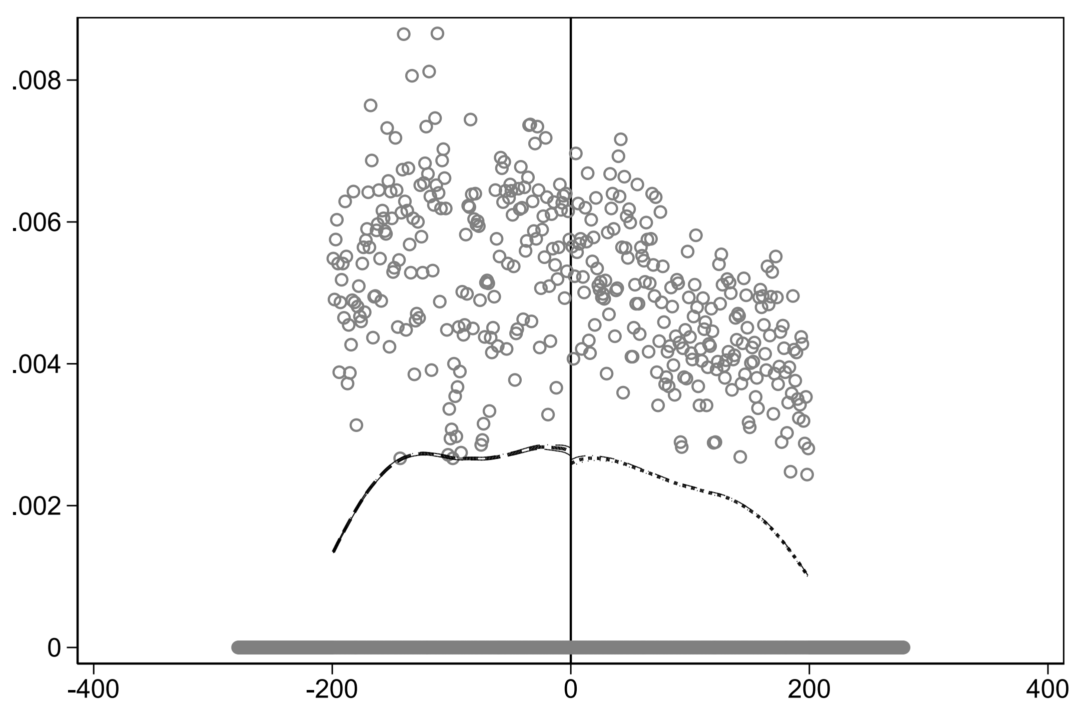


Notes: The 0 vertical line is the date of the general election. The second vertical line left of 0 marks the filing date. (In Georgia, there were 3 different election dates within our data.) This figure reports on the estimated quarterly dummies when cases after the filing date are omitted.

One concern with our omission of cases between the filing date and the general election date is that judges may , instead of levying harsher sentences in the lead-up to the filing date, postpone contentious or visible cases until after the filing date to avoid a challenger running against them on the basis of a contentious ruling. If this was the case we would expect some bunching of cases after the filing date, and we would expect this to be concentrated in the severe cases. **??** presents the results of a **?** test to test for this. There is no evidence of bunching either side of the filing date for severe-crime cases (top-panel). The associated test shows a log difference in height of 0.015, with a standard error of 0.042, giving rise to a t-statistic of 0.359, i.e., the hypothesis of no bunching is not rejected. But there is some evidence for bunching of non-severe cases before the filing date (bottom-panel). The associated test shows a log difference in height of -0.061, with a standard error of 0.015, giving rise to a t-statistic of -4.11. If anything, this suggests that judges may try to get smaller cases dealt with before the filing date in case they need to devote some of their time after the filing date to the campaign trail.

Figure Online Appendix Figure 2: McCrary Tests





Notes: (*a*) This figure shows the McCrary Test for bunching of a running variable (**?**). In our case, that running variable is days within an election cycle, centered around the filing date. The sample is cases that fall within six month either side of a filing date and inside the same electoral cycle. (*b*) The top-panel displays the test for 25,000 severe-crime cases. The bottom-panel displays the test for 202,000 non-severe cases. (Because the number of observations in the bottom panel is very large, the scatter has to use coarser bins than the smoothing function so that it lies everywhere above the smoothed function.) (*c*) The associated test in the top-panel shows a log difference in height of 0.015, with a standard error of 0.042, giving rise to a t-statistic of 0.359, i.e. the hypothesis of no bunching is not rejected. The associated test in the bottom-panel shows a log difference in height of $-.061$, with a standard error of 0.015, giving rise to a t-statistic of -4.11.

In Table 7, we set the measures of electoral competition to zero in Virginia, where judges are always appointed. To test that the inclusion of Virginia in the pooled sample does not drive our results, we re-run all specification without Virginia in **??**.

Table Online Appendix Table 8: Pooled Panel with Interactions - Drop VA

	I	II	III	IV	V	VI	VII	VIII	IX	X
Panel: no VA	Dependent variable: Sentence (months)									
Proximity to election	4.048*	1.340	3.034	-5.074	-3.639**	-4.740*	-2.923**	-3.283**	-6.441	-9.711
	(2.2169)	(1.8064)	(4.8508)	(4.0759)	(1.3879)	(2.2640)	(1.2674)	(1.2882)	(3.8229)	(5.8370)
Proximity to election										
x Partisan Election		7.568*	5.874				6.042	4.687		
		(3.4288)	(3.4034)				(3.3923)	(3.1340)		
x Nonpartisan Election			-1.811							
			(4.0586)							
x Cycle-Length				1.312*					0.589	0.837
				(0.6157)					(0.6594)	(0.5930)
x Prob. electoral challenge					0.279***		0.173***		0.236***	
					(0.0731)		(0.0373)		(0.0711)	
x # donors per judge-race						0.303**		0.196***		0.281**
						(0.1166)		(0.0368)		(0.1055)
R-squared	0.440	0.440	0.440	0.440	0.440	0.260	0.440	0.260	0.440	0.260
Observations	131,028	131,028	131,028	131,028	131,028	101,583	131,028	101,583	131,028	101,583

Notes: *(a)* This table re-runs Table 7, omitting Virginia because judges are always appointed there. *(b)* Standard errors are multi-way-clustered by quarter-year and state level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

?? shows that the results look similar when we exclude Colorado and Tennessee, where the **?** measure is unavailable.

For **??**, we constructed the principal component of the two measures of revealed competition. Alternatively, we also took the average (for columns 2 and 4). In all specifications, the average of these measures interacts significantly in shaping the strength of electoral cycles.

Table Online Appendix Table 9: Pooled Panel with Interactions - Drop CO and TN

	I	II	III	IV	V	VI	VII	VIII	IX	X
Panel: no CO and TN	Dependent variable: Sentence (months)									
Proximity to election	3.405**	1.457	-4.946*	-3.294	-1.640	-4.740*	-1.323	-3.283**	-4.331	-9.711
	(1.3784)	(1.0282)	(2.1973)	(4.6413)	(1.7459)	(2.2640)	(1.5925)	(1.2882)	(4.8613)	(5.8370)
Proximity to election										
x Partisan Election		6.448	12.852**				5.276	4.687		
		(3.6063)	(4.5709)				(3.5375)	(3.1340)		
x Nonpartisan Election			6.750**							
			(2.6038)							
x Cycle-Length				1.010					0.557	0.837
				(0.7912)					(0.7056)	(0.5930)
x Prob. electoral challenge					0.199*		0.123		0.159*	
					(0.0968)		(0.0746)		(0.0712)	
x # donors per judge-race						0.303**		0.196***		0.281**
						(0.1166)		(0.0368)		(0.1055)
R-squared	0.261	0.261	0.261	0.261	0.261	0.261	0.261	0.261	0.261	0.261
Observations	104,311	104,311	104,311	104,311	104,311	104,311	104,311	104,311	104,311	104,311

Notes: *(a)* This table re-runs Table 7, omitting Virginia, Colorado and Tennessee because we do not observe **?**'s average number of donors per race in those three states. *(b)* We report p-values in square brackets. Standard errors are multi-way-clustered by quarter-year and state level. *** p<0.01, ** p<0.05, * p<0.1

Table Online Appendix Table 10: Principle Component and/or Average of Revealed Competition

	I	II	III	IV
	Dependent variable: Sentence (months)			
Sample	All		No CO and TN	
Proximity to election	3.787*	3.922*	3.582**	3.782**
	(1.9911)	(1.9926)	(1.2992)	(1.3742)
Proximity to election				
x PCA	3.076**		2.371*	
	(0.9775)		(1.0166)	
x avg of normalized		4.362***		3.280*
		(1.2367)		(1.4089)
R-squared	0.440	0.440	0.261	0.261
Observations	133,756	133,756	104,311	104,311

Notes: Standard errors are two-way clustered by state-year and state-specific quarter-year, thus essentially just stacking the number of clusters of the individual regressions in Section 3. *** p<0.01, ** p<0.05, * p<0.1