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HOW COMMON ARE ELECTORAL CYCLES IN CRIMINAL SENTENCING?

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ABSTRACT

Existing empirical evidence suggests a pervasive pattern of electoral cycles in criminal sentencing in the U.S.: judges appear to pass more punitive sentences when they are up for re-election, consistent with models of signaling where voters have more punitive preferences than judges. However, this pervasive evidence comes from only three states. Combining the existing evidence with data we collected from eight additional states, we are able to reproduce previous results, but find electoral cycles in only one of the eight additional states. Sentencing cycles appear to be the exception rather than the norm. We find that their existence hinges on the level of competition in judicial elections, which varies considerably across states.

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1 Introduction

Elected judges are a distinctly American phenomenon among Western democracies (Shugerman, 2012). From a political economy perspective, there are compelling arguments against having them. In particular, while they may promote policy congruence between judge and voter preferences, such congruence may not be desirable if judicial decisions are meant to be based only on the facts and the law (Kessler and Piehl, 1998). Furthermore, there is a worry that judge elections create inconsistent sentencing behavior if judges give more weight to voter preferences closer to elections, which may also give special interest groups more influence over judicial sentencing. Elections may even reduce the quality of judges if re-election pressures deter highly qualified judges from entering (Lim, 2013).

These considerations have always made judicial elections controversial. As early as 1835 de Tocqueville predicted that they "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). Judicial elections have remained controversial since. In 2015, the Supreme Court ruled that states could prohibit judges from soliciting funds for their election campaigns,[1] and Chief Justice Roberts wrote in the majority opinion 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."

The potential pitfalls of electing judges have motivated a body of empirical research into the effects of this practice. One line of research has focused on voters. Lim and Snyder (2015) for instance show that in partisan elections, partisan affiliation trumps judge quality (measured by third party evaluations) in determining re-election probabilities, and conclude that judge elections should at least be non-partisan. Another line of research has focused on judges, and in particular on whether judges pass more punitive sentences when they are up for re-election: Early work by Hall (1992, 1995) shows evidence of such electoral cycles among state supreme court justices. More recent empirical work has tended to focus on states' lower trial courts because these handle a vastly larger number of cases than states' appellate or supreme courts.[2] Huber and Gordon (2004),

[1] In Williams-Yulee vs. Florida Bar, 575 U.S.

[2] Trial courts are the states' lower criminal courts. They hear the majority of all criminal cases in the U.S. and can

Gordon and Huber (2007), Berdejó and Yuchtman (2013), and Park (2017) all show evidence of electoral cycles. In fact, we could not find any study that shows the absence of such an effect in trial court data. As a result, the current body of literature can be summarized as showing a pervasive pattern of judicial electoral cycles in criminal sentencing. The problem with such a conclusion is that the combined evidence above comes from only three U.S. states—Kansas, Pennsylvania, and Washington—and each study used data from only one state.[3]

In this paper, we add to this existing evidence the results from eight additional states, which we collected as part of a broader research agenda on judicial sentencing. These states are Alabama, Colorado, Georgia, Kentucky, North Carolina, Minnesota, Tennessee, and Virginia.[4] We test for judicial electoral cycles in these eight states, as well as in Pennsylvania and Washington, which were the focus of previous research.[5] We reproduce existing findings of judicial electoral cycles in Pennsylvania and Washington. However, we reproduce such electoral cycles in criminal sentencing in only one of the other eight states, namely North Carolina.

The absence of such cycles in most of the other states is not explained by institutional differences.[6] Our data include the full range of institutional arrangements: states with partisan elections, states with non-partisan elections, and states with retention elections.[7] Their absence is also not driven by systematic differences in data quality or coding: The level of detail provided in the data varies by state, but states with electoral cycles do not appear systematically different on any dimension. Other patterns found in previous research, e.g., gender and race biases, and a strong effect of recidivism show up consistently across states. Lastly, their absence is not driven by broad trends in sentencing because the data cover similar years in all states.

We hypothesize that the observed variation in judicial sentencing cycles is explained by cross-

sentence defendants to long prison sentences and in some states even to death.

[3] It is clear why this is so: because trial court data is managed by each state's sentencing commission individually, it is costly to collect and to make internally consistent the data from a combination of states.

[4] We requested court sentencing data from all U.S. states, but many states either have not consistently digitized their sentencing data yet, or they do not share it, or they do not track judge identifiers in their data.

[5] Kansas, the third state considered in previous research, would have charged a data processing fee that was an order of magnitude larger than the next-most expensive, so that we decided against collecting it. We have no doubt the results would replicate in the Kansas data since there are a number of papers from different researchers that use it (Lim, 2013; Gordon and Huber, 2007; Park, 2017).

[6] The exception to this statement is Virginia, which actually re-appoints its judges in the state legislature rather than re-electing them. (Our approach was to consider all states where we had judge identifiers and we therefore included Virginia as a benchmark of sorts. In fact, Virginia is the only state that displays evidence, albeit marginally insignificant, for a negative sentencing cycle, i.e., sentencing becoming more lenient in the lead-up to re-appointment dates.

[7] This list represents all possible institutional arrangements, and we discuss these in Section 2.2. In retention elections, incumbents face only a yes/no vote and no challenger.

state differences in the competitiveness of judicial elections. To test this, we pool all the data and interact the timing of electoral cycles with state-level measures of the competitiveness of judicial elections, perusing (*i*) the average probability that a judicial race is contested, and alternatively (*ii*) the average number of donors per judicial race. Both measures' interaction with electoral cycles is highly significant. We also test whether differences in cycles might be explained by differences in judges' seniority (which could shield them from electoral competition), and find this not to be the case. We find some evidence that states with more judicial discretion are more likely to exhibit sentencing cycles, but the main interaction with the competitiveness of judicial elections is robust to controlling for this.

To better understand the observed cross-state differences in the competitiveness of judicial elections, we conducted interviews with lawyers, legal scholars, and judges. A clear narrative pointing to differences in professional norms emerged from these interviews: In some states, we were told that incumbents are almost never challenged because judicial electoral competition is "frowned upon" and judgeships are viewed as something to be bestowed by the governor as a hallmark of one's professional standing. In other states, we were told of unfettered competition for judgeships and a complete lack of any norms preventing electoral competition.

This paper contributes to the literature on court sentencing and judge behavior (Posner, 2008; Epstein, Landes, and Posner, 2013; Ash, Chen, and Naidu, 2017; Cohen and Yang, 2019). Within this literature, we speak in particular to the previously cited works showing that judge elections lead to electoral cycles in sentencing. Where our data overlap with data used in that research, we reproduce the existing results. However, while electoral cycles appear to be real and statistically robust in some states, they are absent in the majority of states. Electoral cycles in sentencing appear to be a function of the degree of electoral competition across states. The key finding of this paper is that most states do not display near the level of competition in judicial elections that would be required to generate electoral cycles in judges sentencing decision.

More generally, this paper contributes to the active debate on external validity, site selection and out-of-sample prediction in applied social science research, see e.g. Allcott (2015). Conceptually, perusing the taxonomy of replication failures in Clemens (2017), our case is a failure of 'reproduction' or a lack of 'robustness to extension'. This failure of reproduction does not appear to be driven by publication bias in the usual sense that non-findings from other states have gone unpublished:[8] we are unaware of *any* research that has used sentencing data from any of the eight additional states whose data we collected. It also does not appear to be driven by specification searching, another classic source of publication bias (Leamer, 1983): while the specification in Huber and Gordon (2004) slightly favors results in Pennsylvania, and that in Berdejó and Yuchtman (2013) slightly favors results in Washington, overall these two states are consistently among the only three that display evidence for judicial electoral cycles across a range of specifications. Regarding 'site selection' in the sense discussed in the RCT literature (Allcott, 2015): previous research seems to have used data from Kansas, Pennsylvania, and Washington simply because these states digitized rich sentencing data and made it accessible earlier than other states did. It does, however, seem plausible that this fact is endogenous to higher levels of competition in judicial elections in those states. Our findings confirm the old adage that shoe leather remains the best way of affirming the external validity of any research finding (paraphrasing Freedman 1991).

2 Data

2.1 Sentencing Data

We have contacted the majority of U.S. states' sentencing commissions with requests for access to their circuit court sentencing data (alternatively referred to as trial 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, which is a requirement for estimating judicial 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 s data had the longest time coverage (1980–2017), Colorado the shortest (2010–2016). See Table 1. (The ordering of states follows their electoral institutions, which we discuss in Section 2.2.)

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

[8] As discussed e.g., in Christensen and Miguel (2018).

Table 1: Sentencing Data

State	WA	GA	KY	MN	NC	AL	TN	PA	CO	VA
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	226,802	358,991	263,165	252,607	214,682	204,565	435,265	886,879	138,067	254,594
Mean Sentence (All, in months)	13.8	81.0	50.6	29.2	14.5	60.2	64.5	10.1	44.1	22.4
Severe Cases	21,658	61,983	16,299	34,649	6,748	17,763	76,630	29,045	6,678	20,920
Mean Sentence (Severe, in months)	53.0	86.7	134.4	62.7	70.5	103.3	91.3	47.8	83.4	56.3
Defendant Race	✓	✓	✓	✓	✓	✓	✓	✓	✓	-
Defendant Gender	✓	✓	✓	✓	✓	✓	✓	✓	✓	-
Defendant Recidivism	✓	-	✓	✓	✓	-	✓	✓	-	-

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 and Pennsylvania are set off visually as the states whose data was used in previous research on electoral cycles.

average (relative to sentencing guidelines). Huber and Gordon (2004) therefore censor their study of Pennsylvania sentencing to court cases of "aggravated assault, rape, and robbery convictions." Similarly, Berdejó and Yuchtman (2013) restrict their study of Washington to severe crimes "as defined by the FBI ... assault, murder, rape, and robbery." Crime characteristics information differs in its nature from state to state. Crimes always have indicators for their characteristics but occasionally lack full characterizations of what these mean. Our approach is therefore to consider as severe the set of criminal cases involving assaults, murders, rapes, and robberies in state s, to define the smallest observed sentence within that set as our threshold, and to then label as 'severe' all criminal cases in state s with a sentence at least as high as this threshold. Aside from capturing un-identified cases involving assaults, murders, rapes, and robberies in the data, this procedure may potentially add less severe crimes committed by repeat offenders, particularly violent repeat offenders. This coding is slightly broader than Huber and Gordon (2004) and Berdejó and Yuchtman (2013), but still gets at the same idea, since court cases involving repeat offenders are also likely to be more visible to voters.[9]

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

[9]The share of recidivists for the full sample is 45% and it rises to 53% for the subsample of severe crimes (for states where we can identify recidivism). Outliers, e.g., prison terms that sum up to multiple life sentences, are a concern in studying criminal sentencing data. We therefore drop them.

women and 30% are black. For the sample of severe crimes 11% of defendants are women and 38% are black. We also almost always observe the other major non-white race groups (Asians, Native Americans, and Hispanics),[10] but neither previous research nor our own estimations display a consistent relation between these and sentence lengths (relative to the omitted white category). For brevity, we therefore do not report these, although we include them as unreported regressors.

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 three states do not report recidivism at all, although one of those reports minimum sentencing guidelines which are a function of recidivism. We verify that transforming the recidivism measure or omitting it altogether has no bearing on the judicial electoral results that are our focus.

2.2 Electoral Cycles

There is considerable variation in electoral systems across the states we study. As Table 2 shows, 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 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.[11] Our data represent all variants of electoral systems that exist in the U.S. 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.[12]

Beyond the electoral rules, we observe considerable variation in the empirical patterns of entry and exit. For the most part, it turns out judges exit the profession at times that do not coincide

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

[11] Vidal and Leaver (2011) suggest that even appointed judges are not perfectly insulated from the electorate.

[12] The numbers sum up to over 50 because 4 states have within-state variation in these rules. See Lim, Snyder, and Strömberg (2015, Table.1) and Park (2017) for excellent discussions of cross-state variation in judicial electoral/appointment rules.

with the electoral cycles.[13] As a result, with seats needing to be filled, a large majority of all judges initially enters via gubernatorial appointment, the rules about initial selection notwithstanding. Washington is the only state where just over fifty percent of judges enter the profession through elections. 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 re-elections.[14]

Table 2: Judicial Elections and Electoral Cycles

<table>
<tr><th>State</th><th>WA</th><th>GA</th><th>KY</th><th>MN</th><th>NC</th><th>AL</th><th>TN</th><th>PA</th><th>CO</th><th>VA</th></tr>
<tr><td>Initial Selection</td><td colspan="5">Nonpartisan</td><td colspan="3">Partisan</td><td colspan="2">Appointment</td></tr>
<tr><td>Re-Election</td><td colspan="5">Nonpartisan</td><td colspan="2">Partisan</td><td colspan="2">Retent-Reel.</td><td>Reappt.</td></tr>
<tr><td>Most Common Entry Method</td><td>Nonpart.</td><td colspan="9">Appointment</td></tr>
<tr><td>Cycles</td><td>4y.</td><td>4y.</td><td>8y.</td><td>6y.</td><td>8y.</td><td>6y.</td><td>8y.</td><td>10y.</td><td>6y.</td><td>8y.</td></tr>
<tr><td># Merged Judges</td><td>165</td><td>246</td><td>572</td><td>359</td><td>1348</td><td>126</td><td>102</td><td>262</td><td>178</td><td>137</td></tr>
</table>

Notes: This table reports on judicial electoral institutions in each state. Washington 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' is to our knowledge a novel fact, and we have not seen it discussed anywhere else in the literature.

As Table 2 further shows, there is also considerable variation in judicial electoral cycle lengths: Two states have four-year cycles, three have six-year cycles, four have eight-year cycles, and Pennsylvania has ten-year cycles.[15]

2.3 Filing Dates, Primaries, and General Elections

With the exception of Georgia and Pennsylvania, judicial elections are held on the general election cycle, i.e., in early November of every even-numbered year.[16] There is, however, 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

[13] Most exiting judges retire, relatively few die, 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'.

[14] 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 the previous judge retired.

[15] Interestingly, the combination of retention elections and unusually long election cycles would suggest it is ex ante less likely to find electoral cycles in Pennsylvania, a fact already noted by the authors of the Pennsylvania study (Huber and Gordon, 2004, 250-251).

[16] 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.

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. In some states and years in our data, the official filing data was as early as March, i.e., there was de facto no electoral pressure at all during the eight months from March to November of the election year.

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 followed by an uncontested general election, because all candidates are from the same party. In such cases, electoral pressure peaks between the filing date and the primary date and then goes to zero. Even states with non-partisan elections may 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.

In summary, the details of each individual election are complicated, and the evolution of electoral pressure on an incumbent can be highly non-monotonic through the election year. The next section explains how we deal with this.

2.4 Judge Biographies

Judicial elections in U.S. states are staggered, i.e., only a portion of judges is up for re-election in any given election year.

One fact that our data has revealed is that the majority of judges enter and exit the profession outside of electoral cycles, i.e., by appointment and retirement rather than by winning and losing elections. This is noted as the 'most common entry method' in Table 2. The main consequence of this observation is that entry and exit in the sentencing data are not sufficient information for establishing in which year a judge is running for re-election. We therefore coded up individual judge biographies (entry/appointment dates, re-election dates, and retirement dates) from `www.ballotpedia.org`, and then linked these to the sentencing data to construct judges' electoral cycles. See details in Online Appendix A.2. Unfortunately, the available information is very incomplete when it comes to information about challengers at either the primary or general election level. This is true even for the most recent electoral cycles, and for earlier cycles the data is mostly simply missing. As discussed in Section 2.3, nothing can be assumed about how electoral pressure builds in any given case between the filing date and the general election date. Attempting to fill in this fragmentary auxiliary information is likely to result in a very non-representative sample, where the availability of the information is endogenous to the characteristics of the election or the judge in question. Our approach is therefore to measure electoral cycles only relative to the filing date and omit all cases that fall between the filing and general election date for those judges who are running.[17] 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.

We follow Berdejó and Yuchtman (2013) and Huber and Gordon (2004) in defining our main regressor of electoral competition that judge i is exposed to at time t as a linear running variable that is scaled from 0 to 1. It starts at 0 on the day after a general election, and equals 1 on the day of the next general election. It increases by $1/T_s$ each day, where T_s is the length of state s's electoral cycle, i.e., $T_{WA} = 4 \times 365 + 1$ and $T_{NC} = 8 \times 365 + 2$. Because we omit all cases between the filing and election date, our measure of electoral competition usually peaks somewhere around 0.9.

Figure 1 illustrates our coding using for illustration two randomly drawn judges, one in Washington, one in Minnesota.

3 Results

This section presents results from a variety of tests for the presence of judicial electoral cycles across states. Our regression specification is

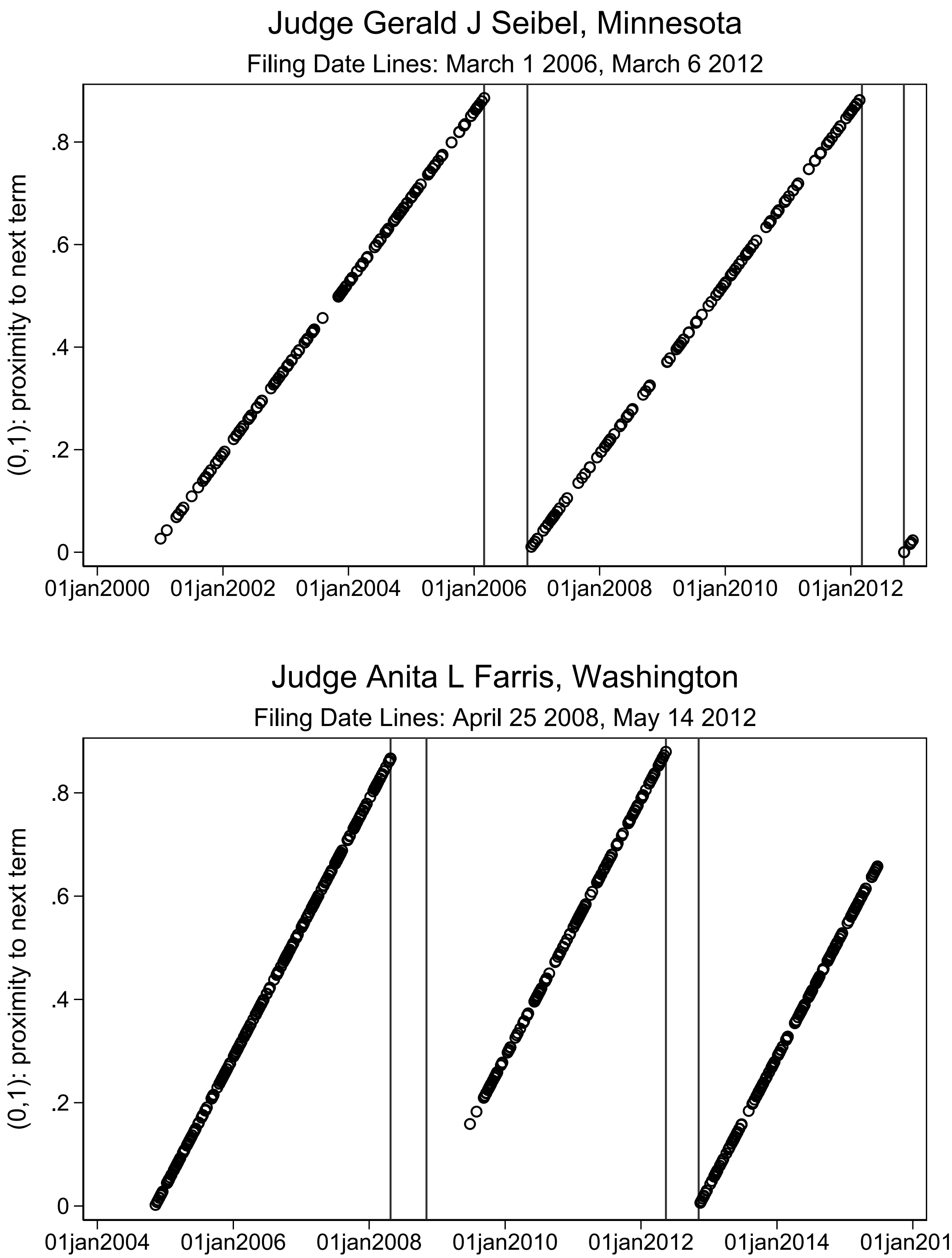
$$\text{Sentence}_{ijt} = \beta \cdot \textit{proximity-to-election}_{jt} + \beta^{\text{X}} \cdot X_i + \mu_t + \mu_j + \mu_c + \epsilon_{ijcs}, \tag{1}$$

where i identifies the criminal case, $\textit{proximity-to-election}_{jt}$ is a time-varying characteristic of judge j, X_i are defendant characteristics (race, gender, age, and recidivism), μ_t are time controls, μ_j are fixed effects that control for unobserved judge characteristics, and μ_c are fixed effects that control for unobserved local characteristics of the judicial district.[18]

[17] The exception is Pennsylvania, Colorado, and Virginia where the electoral institutions (retention elections and re-appointments) mean that there are no filing dates or primaries.

[18] Judges often spend their entire tenure inside the same judicial district, but it is also not uncommon for them to switch over the course of their tenure. In particular, we use county fixed effects that are more restrictive that judicial district as in our sample of states counties are nested within judicial districts.

Figure 1: Judicial Electoral Cycles Examples



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, 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.

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: Base-Sample										
Proximity to election	4.546**	-0.215	-2.250	1.157	5.351***	5.384	5.295	9.514	-0.882	-8.575
	[0.0359]	[0.9303]	[0.7573]	[0.4363]	[0.0040]	[0.7374]	[0.2422]	[0.1664]	[0.8917]	[0.2315]
R-squared	0.529	0.494	0.420	0.369	0.484	0.104	0.626	0.549	0.488	0.327
Observations	13,630	15,499	4,250	13,181	6,833	8,426	35,821	10,423	17,348	2,728
Panel B: ~Panel A, robust s.e.										
Proximity to election	4.546	-0.215	-2.250	1.157	5.351*	5.384	5.295	9.514*	-0.882	-8.575
	[0.3948]	[0.8728]	[0.8026]	[0.6172]	[0.0850]	[0.6225]	[0.2824]	[0.0737]	[0.9113]	[0.1974]
R-squared	0.529	0.494	0.420	0.369	0.484	0.104	0.626	0.549	0.488	0.327
Observations	13,630	15,499	4,250	13,181	6,833	8,426	35,821	10,423	17,348	2,728
Panel C: All Crimes										
Proximity to election	0.421	1.226	-0.980	-0.085	0.776**	1.520	-0.137	0.342	2.231	-1.889
	[0.2519]	[0.3014]	[0.3860]	[0.6547]	[0.0377]	[0.5579]	[0.9029]	[0.4148]	[0.6037]	[0.2132]
R-squared	0.564	0.254	0.802	0.568	0.438	0.125	0.481	0.370	0.492	0.505
Observations	139,900	100,413	82,151	122,616	253,732	94,071	215,539	463,236	53,683	33,007

Notes: *(a)* Each panel reports on results of one specification, run for each state separately. *(b)* All regressions include a defendant's race, gender, age, age squared, and an indicator for recidivists. All regressions also include the case's severity, and the number of charges in each case. Finally, all regressions include judge fixed effects (nested inside county fixed effects), and year fixed effects. *(c)* Panel A and B include only severe crimes, the benchmark sample selection in this literature. Panel C includes all cases. *(d)* We report p-values in square brackets. In Panels A and C, standard errors are clustered at the quarter-year level. We use robust standard errors in Panel B. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3 reports the results of estimating equation (1), separately across columns for each state. Each panel of Table 3 reports on results of one specification. Panel A is the baseline specification where we focus on severe crimes, always including defendant's race, gender, age and recidivism in all regressions. The choice of clustering in Panel A is the one in Berdejó and Yuchtman's study of Washington. Panel B is identical except that we use heteroscedasticity-robust standard errors, as in Huber and Gordon's study of Pennsylvania. Starting with the two states covered by previous research, the reported coefficients for Washington and Pennsylvania across Panels A and B imply that a judge at the beginning of their electoral cycle levies sentences that are roughly 4.5 to 9.5 months shorter than a judge right before filing for re-election. The preferred clustering in Berdejó and Yuchtman (2013) strengthens the results for Washington in Panel A, while the preferred clustering in Huber and Gordon (2004) strengthens the results for Pennsylvania in Panel B. (It is worth noting in this context that heteroscedasticity-robust standard errors were the norm in applied research in 2004, while clustering standard errors was the norm in applied research in 2013.) The key observation is that across the three panels of Table 3, Washington and Pennsylvania are consistently among the three that come closest to displaying sentencing cycles. The third state that appears to have a statistically significant sentencing cycle is North Carolina. The magnitude of the estimated sentencing cycle in North Carolina is between those in Washington and Pennsylvania. In Panel C, we re-estimate equation (1) in a sample of all crimes. Previous studies of sentencing cycles have been clear about the fact that these are only discernible when focusing on the more visible severe crimes. It is therefore worth noting that the same set of three states continues to display the most evidence of electoral cycles, although these are only significant in North Carolina. Point estimates are reduced by around an order of magnitude in all three states.

No other state displays evidence of electoral cycles. The closest any other state comes to a significant effect is Tennessee, with a *p-value* of 0.25. Virginia, the only state with appointments instead of elections, we see weak evidence for negative electoral cycles. While this is speculative, we interpret this fact as consistent with the observation in other studies that judges who internalize the governor's preferences instead of the voters' may pass shorter sentences to reduce imprisonment-related government expenditures (Ouss, 2015; Poyker and Dippel, 2019).

The core observation in Table 3 is that electoral cycles in criminal sentencing are not as common as previously thought. In fact, they appear to be the exception rather than the norm. Before we attempt to shed light on what can explain this finding, we ensure it is not driven by differences in data quality across states. For this purpose, we perform a number of validation exercises and robustness checks: To confirm that the broad patterns in the sentencing data are consistent in all states, Online Appendix Table 1 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. One concern related about variable data quality is the very low *R-squared* in Alabama. This turns out to be due to coarse severity measures and a lack of information on recidivism. Fortunately, Alabama's data includes minimum sentencing guidelines in the data, which subsume all relevant considerations about the severity of a case. We do not include this in the baseline results because we only observe it in two states. Panel A of Online Appendix Table 2 reports on results when we include this variable. The *R-squared* in Alabama goes from 0.1 to 0.84. Importantly, this dramatic increase in explanatory power leads to less, not more, evidence for a sentencing cycle in Alabama. Another measurement concern, discussed in Section 2.1, is that recidivism is not very consistently measured across states. Panels B and C of Online Appendix Table 2 report on results when we include recidivism as a scaled ordinal variable instead of an indicator, or when we omit the variable altogether. Again, the baseline results are unaffected by this. In sum, these exercises suggest that differential data quality does not drive the differential pattern of sentencing cycles that we find.

One other concern is with our omission of cases between the filing date and the general election date. It is conceivable 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. In fact, what we find is the opposite: Online Appendix Figure 1 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. 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.

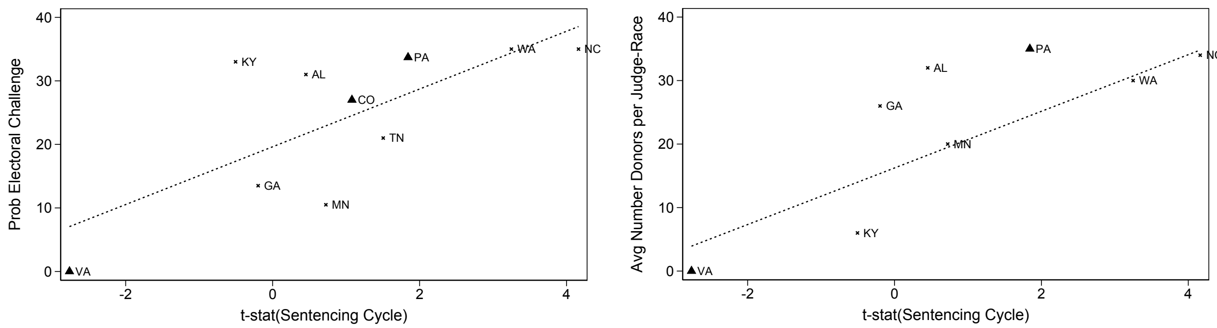
What then can explain the striking variation in the strength of judicial sentencing cycles? Any model that would predict electoral cycles in sentencing would also predict that these are a function of the level of electoral competition judges expect to face. We therefore hypothesize that the observed variation in judicial sentencing cycles can be explained by cross-state differences in the competitiveness of judicial elections.

We were able to construct two measures of competitiveness of judicial elections. First, we used `ballotpedia.org` to construct a state-level measure of the share of judicial elections that faced any challenger in each state from 2012–2016.[19] We find considerable variation in the overall degree of electoral competition that judges face. In Washington, 35% of elections we observe were contested, while in Minnesota, this share was 10.5%. Second, we perused the number of donors who contributed to the average judge's electoral campaign as another common measure of electoral competition (Bonica, 2016). The left and right panel of Figure 2 show that these two

[19] We treat this as a time-invariant state-specific feature of the level of judicial competition, for two reasons: from an econometric point of view, information collected at the level of the race or even a single year would be endogenous to the unobserved characteristics of the judicial races being investigated; from a practical point of view, information about challenges becomes very incomplete outside the 2012–2016 window.

measures correlate strongly with the strength of states' judicial sentencing cycles, measured by the t-statistic on our main regressor in each state. The two measures of judicial competitiveness have a surprisingly similar distribution: The average share of contested races is 28% and the average number of donors per race is 26.

Figure 2: Strength of Sentencing Cycles vs Strength of Electoral Competition



Notes: In both panels, the horizontal axis of the figure has the t-statistics on the electoral proximity coefficient reported in Table 3. (*a*) In the left panel, the vertical axis measures the degree of electoral competition that judges face, using data from `ballotpedia.org`. For the seven states with regular judicial elections, this is the share of seats that are contested (at either the primary or general election level in the 2014 and 2016 elections), excluding seats where this information is missing. For Virginia, we set this value to zero since judges are almost always re-appointed. For Colorado and Pennsylvania, it is not obvious how to measure the degree of electoral pressure in retention elections. We did the most obvious thing which was to compute the average share of votes that were a "No." This was 27% in Colorado. 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. Virginia, we set this value to zero since judges are almost always re-appointed. (*b*) In the right panel, the vertical axis is the average number of donors per judge per election in the data, using data from Bonica (2016). These data do not cover Colorado or Tennessee. For Virginia, we set the number to zero.

To test our hypothesis more formally, we pool all the state-level data and interact our main proximity-to-election regressor with the state-level measures of judicial competitiveness. Table 4 Column I report results for the baseline specification from Panel A of Table 3 but with all states pooled together. The point estimate is a weighted average of estimates for each states and is insignificant. To investigate the extent to which judicial competition matters, Column II adds the interaction with state-specific probability of electoral challenge. The estimate for proximity to election become negative and significant, while the interaction is positive and significant. The sum of the two coefficients turns positive at a 19-percent probability of an electoral challenge. Column III reports results when interacting the main regressor with with the state-level average number of donors per judge-race. The results are slightly less precise but very similar to those in Column II. The sum of the two coefficients turns positive at 20 donors per-judge race, with the mean number of donors being 26. While Columns II and III use two very difference sources of data for the interaction, the resulting estimates are remarkably close and support the theory that electoral cycles are more pronounced in states with stiffer electoral competition.

Table 4: Pooled Panel with Interactions

	I	II	III	IV	V	VI
	Dependent variable: Sentence (months)					
	Baseline	Prob. electoral challenge	# donors per judge-race	Tenure	F-stat	Prob. electoral challenge & F-stat
Proximity to election	1.162	-4.832***	-5.723***	0.114	-1.468	-5.036***
	[0.5530]	[0.0066]	[0.0089]	[0.9528]	[0.5052]	[0.0041]
Proximity to election						
x Prob. electoral challenge		0.263***				0.191*
		[0.0031]				[0.0756]
x # donors per judge-race			0.301*			
			[0.0889]			
x Tenure				0.152		
				[0.5248]		
x F-stat					0.709	0.494
					[0.1214]	[0.2645]
R-squared	0.375	0.375	0.274	0.375	0.375	0.375
Observations	128,187	128,187	74,998	128,187	128,187	128,187

Notes: *(a)* This table pools all states and the main regressor of interest (proximity to election) with a number of mediating variables that could possibly explain the extent of sentencing cycles. All the same controls as in the baseline are included. *(b)* We report p-values in square brackets, standard errors are multi-way-clustered by quarter-year and state. *** p<0.01, ** p<0.05, * p<0.1

The next columns investigate whether the differential baseline results could be driven by alternative explanations. One possibility we consider is that some states have more senior judges who are more shielded from electoral pressures. Since tenure is a judge-year specific measure, we can actually include this interaction state-by-state. In Online Appendix Table 3, we find this interaction to be significant only in Washington, where it has the expected negative sign, suggesting that more senior judges may be less concerned about re-election. However, when we include the same interaction in the pooled data in Column IV, this finding does not survive: the interaction is completely insignificant.[20]

Another possibility is that states vary in the discretionary room left to judges, e.g., in how tight sentencing guidelines are, and that only judges with enough discretion can generate sentencing

[20] The tenure of judge is measured in years and as a separate control it is absorbed by judge and year fixed effects.

cycles. This is captured in Column V, which interacts the main coefficient with the state-specific joint *F-statistic* of the judge fixed effects.[21] This interaction is sign-consistent and almost significant, with a *p-value* of 0.12. In Column VI, we therefore test this explanation directly against the electoral competition explanation. While the interaction with electoral challenge remains significant, the *p-value* for the *F-statistic* interaction drops to 0.26. Online Appendix Table 4 shows that the key results in Table 4 are robust to dropping Virginia, which has only appointed judges.

4 Discussion and Conclusion

A natural follow-up question that arises from our findings is what gives rise to cross-state differences in the competitiveness of judicial elections? We cannot provide a statistical answer to this question with the existing data, so instead we conducted interviews with a number of lawyers, legal scholars and judges to provide a qualitative answer. The overarching theme that emerged from these interviews points at striking cross-state differences in the professional norms governing judicial competition. In states with low levels of electoral competition, judgeships appear to be viewed as positions that should be obtained by appointment, never mind the electoral rules. What might sustain such a norm in equilibrium is the fact that trial court judgeships are often a stepping stones to later appointments into positions on appellate courts or state supreme courts. As well, many judges retire into private practice as out-of-court 'private judges', and having a tainted standing in the profession may hurt their prospects of referrals. These considerations stand in marked contrast to stories in other states, where we were told of a "blatant disregard for the sanctity of the office and the bench". This included anecdotes of lobbyists running protection rackets by contacting judges to offer their services, and making thinly veiled threats of otherwise mobilizing challengers for the next electoral cycle. This narrative leads us to hypothesize that the root of cross-state variation in sentencing cycles is variation in professional norms of entry into the judicial occupation. We view statistical testing of this hypothesis, and a deeper explanation of the historical or cultural factors that might give rise to such cross-state differences in professional norms, as a fruitful avenue for future research.

[21] We compute F-statistics by regressing (state-by-state) sentence length on the full set of controls (without proximity to election) on the full sample and taking the joint F-statistics of the judge fixed effects. Results hold if we restrict the sample to severe crimes only.

In conclusion, this paper uses rich and novel data that confirms that electoral cycles in criminal sentencing are real in some states, but also shows that they are nowhere near as ubiquitous as previously thought. Our findings suggest they only become an empirical regularity when there is stiff electoral competition for judgeships. In most states, this appears simply not to be the case.

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Online Appendix to

"How Common are Political Cycles in Criminal Sentencing?"

Online Appendix A Data Description

Online Appendix A.1 Sentencing Data

Sentencing data was collected separately from each state. 14 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, 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 for
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.[22] 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.

[22] 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 requesed 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 Berdejó and Yuchtman (2013)
- 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.[23] 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.

Six of the states in this paper include judge names or identifers in the sentencing data: Alabama, Georgia, Kentucky, North Carolina, Tennessee, and Washington. We the code up judge biographies, including when they are up for re-election 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.

[23] Or courts of the similar level.

Online Appendix B Robustness Checks

The core insights that comes out of Table 3 is that electoral cycles in criminal sentencing are not as common as previously thought. They appear to be the exception rather than the norm. Before we move on to shedding light on what can explain the differences across states that we observe, we want to make sure that this is not about differences in data quality or functional form assumptions. For this purpose, we perform a number of validation exercises here.

To confirm that the broad patterns in the sentencing data are consistent in all states, Online Appendix Table 1 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.

One concern related about variable data quality is the very low R-squared in Alabama. This turns out to be due to very coarse severity measures and missing information on recidivism. Fortunately, Alabama's data includes minimum sentencing guidelines in the data, which subsume all relevant considerations about the severity of a case. We do not include this in the baseline results because we only observe it in two states. Panel A of Online Appendix Table 2 reports on results when we include this variable. The R-squared in Alabama goes from 0.104 to 0.841. Importantly, this dramatic increase in explanatory power leaves the core coefficient of interest unaffected in magnitude and significance. Another measurement concern, discussed in Section 2.1, is that recidivism is not very consistently measured across states. In fact, as Table Online Appendix Table 1 indicates, Kentucky, Alabama, and Colorado do not include information on recidivism at all. To rule out the possibility that this differential data quality gives rise to differential electoral cycles, Panels B and C of Online Appendix Table 2 report on results when we include recidivism as a scaled ordinal variable instead of an indicator, or when we omit the variable altogether. Again, the baseline results are unaffected by this. In sum, these exercises suggest that differential data quality does not drive the differential pattern of sentencing cycles that we find.

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. Online Appendix Figure 1 presents the results of a McCrary (2008) 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.

Table 4 Column IV tests whether judges with longer tenure are less prone to exhibit sentencing cycles. In the pooled sample, we find no interaction. But this is the only interaction that is judge (and year specific) so that we can also test it separately state-by-state. Online Appendix Table 3 shows that the interaction with tenure appears to be insignificant in all states but Washington. As expected the interaction is negative and significant there. It suggests that in Washington, on top of positive effect of electoral cycle, more senior judges may be less concerned about their re-election.

In Table 4, 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 Online Appendix Table 4.

Table Online Appendix Table 1: Effect of Defendant Characteristics on Sentence Length

	WA	GA	KY	MN	NC	AL	TN	PA	CO	VA
Proximity to election	0.421	1.201	-0.980	-0.038	0.808**	1.643	-0.184	0.144	2.231	-1.889
	[0.2521]	[0.3097]	[0.3860]	[0.8287]	[0.0181]	[0.5141]	[0.8750]	[0.3889]	[0.6037]	[0.2132]
Female	-2.853***	-6.784***	-2.677***	-3.002***	-3.579***	-15.183***	-6.677***	-1.144***	-2.698	-
	[0.0029]	[0.0001]	[0.0013]	[0.0006]	[0.0002]	[0.0002]	[0.0004]	[0.0010]	[0.2864]	-
Black	1.162**	2.486***	-0.191	1.602***	1.744***	6.101***	1.865*	1.390***	0.315	-
	[0.0479]	[0.0007]	[0.7427]	[0.0025]	[0.0018]	[0.0032]	[0.0751]	[0.0033]	[0.9079]	-
Recidivist, (0 or 1)	11.884***	10.791***	-	14.558***	3.349***	-	41.186***	3.684***	-	-
	[0.0000]	[0.0000]	-	[0.0000]	[0.0004]	-	[0.0000]	[0.0002]	-	-
R-squared	0.564	0.254	0.802	0.542	0.420	0.125	0.477	0.360	0.492	0.505
Observations	139,900	100,415	82,151	122,616	253,745	94,071	215,539	463,236	53,683	33,007

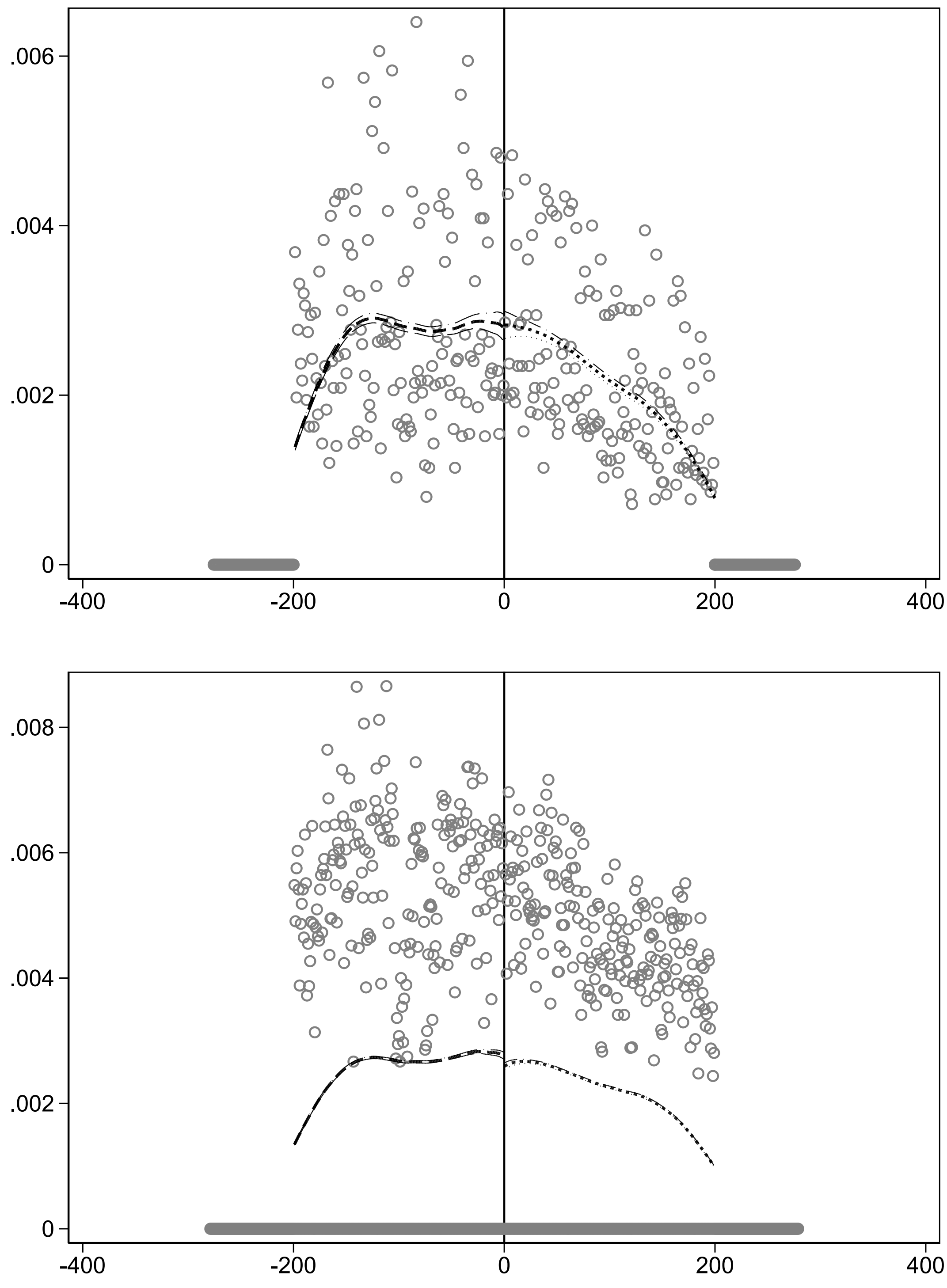
Notes: *(a)* Each panel reports on results of one specification, run for each state separately. All cases included. *(b)* We use dummy for recidivism instead of the scaled variable for the sake of data representation; however, estimates for the proximity to election do not change if we use scaled recidivism.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

Table Online Appendix Table 2: Robustness Checks

	WA	GA	KY	MN	NC	AL	TN	PA	CO	VA
Panel A~add guidelines	3.729**	-	-	-	-	1.848	-	-	-	-
	[0.0185]	-	-	-	-	[0.6692]	-	-	-	-
R-squared	0.807					0.841				
Observations	13,402					8,310				
Panel B~omit recidvism										
Proximity to election	4.910**	-0.114	-	2.043	4.413**	-	4.684	8.721	-	-8.575
	[0.0266]	[0.9631]	-	[0.2481]	[0.0369]	-	[0.2826]	[0.2486]	-	[0.2315]
R-squared	0.511	0.490		0.314	0.450		0.614	0.538		0.327
Observations	13,630	15,499		13,181	8,144		35,823	10,423		2,728
Panel C~redefine recidvism as dummy										
Proximity to election	4.546**	-0.198	-	1.157	6.034***	-	5.533	8.781	-	-8.575
	[0.0357]	[0.9359]	-	[0.4458]	[0.0026]	-	[0.2235]	[0.2459]	-	[0.2315]
R-squared	0.529	0.493		0.369	0.475		0.624	0.546		0.327
Observations	13,630	15,499		13,181	6,838		35,823	10,423		2,728

Notes: *(a)* This table re-estimates results in Table 3, with some modifications to the data. In Panel A, we add sentencing guidelines for the two states that include these in the data. This dramatically increases the *R-squared*. In Panel B, *(b)* We report p-values in square brackets. *** p<0.01, ** p<0.05, * p<0.1

Figure Online Appendix Figure 1: McCrary Tests



Notes: (*a*) This figure shows the McCrary Test for bunching of a running variable (McCrary, 2008). 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.

Table Online Appendix Table 3: Tenure Interaction

	WA	GA	KY	MN	NC	AL	TN	PA	CO	VA
Proximity to election	29.866*	1.762	-40.422	13.269	4.517	-17.514	1.865	7.399	-3.705	1.018
	[0.0719]	[0.6832]	[0.1853]	[0.1673]	[0.7250]	[0.5929]	[0.7417]	[0.4119]	[0.6276]	[0.9653]
Proximity to election	-5.816*	-0.546	4.804	-1.276	0.091	3.496	0.280	-0.333	0.591	-1.920
x tenure	[0.0639]	[0.7129]	[0.2958]	[0.2205]	[0.9931]	[0.1320]	[0.5617]	[0.9977]	[0.7134]	[0.6361]
R-squared	0.531	0.494	0.420	0.369	0.484	0.104	0.626	0.550	0.488	0.327
Observations	13,630	15,499	4,250	13,181	6,833	8,426	35,821	10,423	17,348	2,728

Notes: *(a)* This table re-estimates Table 3. Each panel reports on results of one specification, run for each state separately, with all the same controls as in Table 3. In addition, we include an interaction of the main regressor with judges' seniority. *(b)* We report p-values in square brackets, standard errors are clustered at the quarter-year level. *** p<0.01, ** p<0.05, * p<0.1

Table Online Appendix Table 4: Pooled Panel with Interactions - Drop VA

	I	II	III	IV	V	VI
	Dependent variable: Sentence (months)					
	Baseline	Prob. electoral challenge	# donors per judge-race	Tenure	F-stat	Prob. electoral challenge & F-stat
Sample	w/o VA					
Proximity to election	1.358	-5.045***	-6.213**	0.267	-1.268	-5.195***
	[0.4943]	[0.0009]	[0.0124]	[0.8933]	[0.5623]	[0.0002]
Proximity to election						
x Prob. electoral challenge		0.272***				0.200
		[0.0030]				[0.1025]
x # donors per judge-race			0.318*			
			[0.0866]			
x Tenure				0.157		
				[0.5053]		
x F-stat					0.692	0.489
					[0.1117]	[0.2552]
R-squared	0.375	0.375	0.273	0.375	0.375	0.375
Observations	125,437	125,437	72,248	125,437	125,437	125,437

Notes: *(a)* This table re-runs Table 4, omitting only Virginia where judges are always appointed. *(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