CRIME, PUNISHMENT, AND POLITICS: AN ANALYSIS OF POLITICAL CYCLES IN CRIMINAL SENTENCING

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Abstract—We present evidence that Washington State judges respond to political pressure by sentencing serious crimes more severely. Sentences are around 10% longer at the end of a judge's political cycle than at the beginning; judges' discretionary departures above the sentencing guidelines range increase by 50% across the electoral cycle, accounting for much of the greater severity. Robustness specifications, nonlinear models, and falsification exercises allow us to distinguish among explanations for increased sentencing severity at the end of judges' political cycles. Our findings inform debates over judicial elections, and highlight the interaction between judicial discretion and the influence of judicial elections.

I. Introduction

WHETHER judges should be subject to electoral review has long been debated in designing constitutions and judicial systems and has received recent attention in both the legal and economic literature, as well as in the popular press.[1] Editorialists, jurists (notably retired Supreme Court justice Sandra Day O'Connor), and private organizations have expressed concern that judicial decision making could be influenced by political pressure, to the detriment of the public.[2] Researchers have endeavored to determine whether judicial behavior responds to differences in political environments, part of a large literature examining judges' responses to the incentives and constraints they face.[3] Such work not only informs our understanding of the judicial branch but is also relevant to the broader political economy question of the effect of political pressure and electoral accountability on public servants' behavior.[4]

Much of the literature on the impact of judicial elections on judges' behavior has used cross-state variation in retention methods to identify elections' effects (for example, Besley & Payne, 2005). A weakness of this methodology is that differences in retention methods across states could be correlated with unobservable factors that affect the outcome of interest.[5] Prior work has also generally ignored criminal case outcomes in courts of general jurisdiction. Work on criminal case outcomes has focused on higher courts (for example, Hall, 1992, 1995). Most studies analyzing lower courts have examined civil cases (for example, Hanssen, 1999; Tabarrok & Helland, 1999; and Besley & Payne, 2005).

One might prefer to focus on criminal case outcomes in lower courts for several reasons. First, the stakes are high: depending on the outcome of a criminal case, the state may deprive a citizen of his rights, property, and even his life. Because courts of appeals give considerable deference to the findings of facts by trial courts and, in states with determinate sentencing, convicted felons serve their full sentences, trial court outcomes are paramount. Second, crime is an issue about which the citizenry has well-defined preferences. In addition, since convicted felons lose their right to vote in most states (including Washington; see Revised Code of Washington [RCW] 9.94A.637), they could be an attractive target for politically opportunistic judges.

Among the work that has focused on the impact of politics on criminal case sentencing is Lim (2008), who estimates a structural model of felony sentencing by Kansas judges and finds that judges in counties using partisan judicial elections exhibit different sentencing patterns from judges in counties using referendum judicial elections. These different patterns are attributed to both different preferences and the effects of elections on judges' behavior. Gordon and Huber (2004, 2007) estimate the effect of the proximity of a judge's retention election on that judge's sentencing decisions using data from Pennsylvania and Kansas. In Pennsylvania, where judges are retained in referendum elections, they find that sentencing becomes more severe as elections approach (that is, sentencing severity exhibits cycles corresponding to the political calendar); however, they do not find sentencing cycles in

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[1] The Federalist Papers 78 and 79 address this issue in justifying lifetime appointment for U.S. federal judges in the U.S. Constitution. While federal judges are appointed for life, well over half of U.S. states use judicial elections for some judges (39 states, according to the Brennan Center at NYU School of Law, as of September 2010). See, for example, Besley and Payne (2005), Lim (2008), Pozen (2008), and Liptak (2008).

[2] O'Connor (2010) argues in a *New York Times* op-ed against the election of judges. Examples of organizations critical of judicial elections include the Justice at Stake Campaign (http://www.justiceatstake.org), the Elmo B. Hunter Citizens Center for Judicial Selection (http://www.ajs.org/selection/index.asp), and the Illinois Campaign for Political Reform (http://www.ilcampaign.org). DeBow et al. (2002) and Bonneau and Hall (2009) argue in favor of judicial elections.

[3] Lim (2008) and Gordon and Huber (2004, 2007) examine judicial elections. Schanzenbach and Tiller (2007, 2008) and Cross and Tiller (1998) study judges' sentencing under courts of appeals with differing political compositions; Freeborn and Hartmann (2010) study judges constrained by sentencing policy, and Posner (2005) writes more generally on judicial behavior.

[4] Theoretical studies include Barro (1973), Nordhaus (1975), Besley and Coate (2003), Maskin and Tirole (2004), and Alesina and Tabellini (2007, 2008). Empirical work outside the realm of the judicial branch includes Besley and Case (1995) and Dal Bó and Rossi (2011).

[5] Besley and Payne (2005) acknowledge this and attempt to resolve it by using the method states use to select public utility regulators as an instrument for the methods used to select judges. Of course, the method used to select regulators could be correlated with institutional, economic, or political factors that affect judicial behavior.

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Kansas counties where judges are so retained. Gordon and Huber do find sentencing cycles in Kansas counties where judges are retained in partisan elections.

We also examine the impact of elections on judicial behavior by testing for the presence of political sentencing cycles. Under a broad range of specifications, we find that sentencing of serious offenses becomes more severe as elections approach: sentence lengths increase by around 10% between the beginning and the end of a judge's political cycle. In contrast to much of the existing literature, we are able to test—and rule out—alternatives to the hypothesis that longer sentences are the effect of political pressure on judges. First, we directly examine case characteristics (including variables that reflect the result of any bargaining between the defense and district attorneys) across judges' political cycles and do not find evidence that these systematically vary across the judge's political cycle. A range of specifications suggests that changes in the behavior of defense and district attorneys across judges' political cycles do not explain the sentencing cycles we observe. We distinguish between judicial political cycles and the political cycles of other officials by exploiting differences in the timing of electoral pressure across offices and exploring nonlinear models of the effect of electoral proximity. We find that sentence lengths exhibit a break precisely at the end of judges' political cycles but not at the end of the political cycles of other officials. We can rule out cyclical patterns in sentencing due to factors other than politics (for example, variation in unobservable case characteristics) by examining sentencing by retiring judges, who do not face electoral pressure; the sentencing of less serious crimes, about which the public (and potential competitors for a judge's seat) is likely less concerned; and sentencing in nearby Oregon, where judges are elected on a different cycle. We do not find sentencing cycles for retiring judges in their final term, or cycles for less serious crimes; sentencing in nearby Oregon does not follow the Washington pattern. All of this analysis provides evidence of large and statistically significant sentencing differences in judges' sentencing behavior in response to political pressure. To better understand the nature of these sentencing cycles, we consider multiple outcome variables, shedding light on the role played by deviations outside the sentencing guidelines range. We find that deviations above the range increase by 50% across a judge's political cycle and account for a large part of the cycles in sentence length, suggesting that the effect of political pressure on sentencing is mediated by the availability of judicial discretion.

In this paper, we discuss judicial elections and criminal sentencing in the state of Washington in section II. In section III, we describe and provide empirical support for a theoretical framework that predicts sentencing cycles as a result of political pressure on judges. In section IV, we discuss our data and the construction of the variables used in our analysis. We present our empirical model and results in section V, and discuss the implications of our findings and conclude in section VI.

II. Judicial Elections and Criminal Sentencing in Washington

The structure of judicial elections and the laws governing sentencing provide the context for, and inform, our empirical analysis of judges' behavior.

A. *Judicial Elections*

Washington Superior Courts are currently organized into 32 judicial districts, either a county or a group of adjacent counties, and superior court judges are elected and retained by nonpartisan elections every four years (coinciding with presidential election years).[6] Judicial candidates are required to file for public office by the filing deadline—in our sample period, the last Friday of July—and if more than one candidate files for a given seat, the candidates face each other in the primary elections held in September. If no candidate receives more than 50% of the vote in the primary election, the two candidates with the most votes face each other in the general election in November.

For our purposes, judges' political cycles end either at the filing deadline, after which the threat of a challenger in the upcoming election no longer exists, or at the primary or general election, depending on the entry and success of challengers.

B. *Criminal Sentencing*

Criminal sentencing in Washington for felony crimes is governed by the 1981 Washington Sentencing Reform Act (WSRA), which established presumptive sentencing ranges based on the conviction offense and the defendant's criminal history.[7] The Washington guidelines are relatively simple and transparent, especially in cases that are adjudicated by plea agreements (the vast majority of Washington cases), as the plea agreement itself includes the prescribed "cell" within the sentencing guidelines including any enhancements (the relevant page from the Washington plea agreement form is in the online appendix, figure A2).

For each case, then, the applicable range of the sentencing guidelines cell can be thought of as the basic constraints on judicial discretion, which the judge takes as given.[8] In our baseline empirical analysis, we consider the use of judicial discretion controlling for the guidelines range that applies to

[6] In our sample period, there were thirty judicial districts until 1999, when Chelan and Douglas counties split into two judicial districts. Interim vacancies are filled by gubernatorial appointment, and in general, special elections are held for such seats in the yearly election following the appointment (RCW 2.08.120). Judges run again in the next presidential election year.

[7] RCW 9.94A.310(1). For a discussion of the Sentencing Reform Act and the effects of the *Blakely* decision, see Nussbaum (2005). See the online appendix, figure A1, for the sentencing guidelines grid from the year 2000; the online appendix, figure A2, for a form indicating the mapping from a conviction offense and criminal history into a guidelines cell; and the online appendix, figure A3, for the form determining the deadly weapon enhancement.

[8] Schanzenbach and Tiller (2007, 2008) find manipulation of the guidelines cells under the far more intricate federal sentencing guidelines.

a given case, as well as a dummy variable indicating the type of adjudication (plea or trial). This specification allows us to isolate the variation in sentence severity due to changes in judges' behavior across their political cycles. By conditioning on the outcomes of attorneys' bargains, we hope to remove any effect of changed outcomes of attorneys' bargains from the relationship we estimate between political pressure on the judge and sentence severity.

Of course, plea agreements and guidelines cells are negotiated by attorneys in the shadow of the trial judge (see, for example, Reinganum, 1988; Lacasse & Payne, 1999; Bibas, 2004), and thus might endogenously respond to changes in political pressure on the judge. To address this concern with our baseline empirical model, in our empirical analysis, we also examine the shifting of cases across adjudication categories, across sentencing guidelines cells, and across time.

Prior to the *Blakely v. Washington* decision of 2004, Washington Superior Court judges had full discretion to select a sentence within the applicable range and could sentence outside the standard range on making certain findings (see RCW 9.94A.390).[9] In practice, deviations outside the range were quite unusual. In our sample of felony convictions, judges imposed sentences above the range in fewer than 3% of cases (and in 6% of the serious crimes on which we focus). If the judge found that an exceptional sentence was warranted, the sentence length was left to his or her discretion but was subject to appellate review. Since *Blakely*, a judge may still sentence anywhere within the applicable range in the sentencing grid, but cannot impose a sentence above this range unless the jury finds, or the defendant pleads to, special circumstances prescribed by statute.

Under the WSRA, an individual convicted of a felony offense occurring on or after July 1, 1984, receives a determinate sentence and is expected to serve the sentence in full. This is important since the existence of a parole board that conditions the release dates of convicted felons on recidivism risk could mitigate any social welfare consequences of excessive sentences.[10]

III. Theoretical Framework

There exists a large literature, both theoretical and empirical, on political cycles among executive and legislative government officials (for example, Nordhaus, 1975; Rogoff & Sibert, 1988; Alesina & Roubini, 1992; Akhmedov & Zhuravskaya, 2004; for a recent overview of empirical evidence, see Franzese, 2002). In these models, there is a principal-agent problem with moral hazard: officials are voters' agents, and voters reward performance in office with their votes because they attribute good performance to either the incumbent's ability or willingness to further the electorate's interest rather than his or her own. If voters or potential challengers (who can inform voters) monitor and evaluate officials around the time of an election, there will exist incentives for the incumbent to perform well (that is, perform as voters prefer) precisely at this time. Cyclical behavior across an official's term can also arise from time discounting: early in their terms, officials will behave according to their own preferences, while late in their terms, they will place more weight on maintaining their official positions and thus behave according to voters' preferences.[11]

Elected judges in Washington State face political pressure similar to that faced by other elected officials. They are voters' agents, and there exists a divergence between voters' preferences and judges': voters prefer more severe penalties than judges hand down. We review the General Social Surveys (GSS) from 1972 to 2006 and find that when asked whether "the courts in [the respondent's] area deal too harshly or not harshly enough with criminals," 82.8% of the respondents answered that courts are "not harsh enough," while only 12.2% and 5.1% believed that courts were "about right" or "too harsh," respectively.[12] These differences in sentencing preferences may arise from various factors. First, judges do not like being reversed, and they can be reversed in a criminal case if their judgment results in a conviction or (in Washington State) if the sentence they impose is higher than the high end of the applicable guidelines range. Second, the public may have a biased perception of the average criminal (or crime), for example, as a result of the portrayal of crime in the media. Third, the judge has to personally confront the person who is being sentenced, which may make extremely punitive sentencing more costly. Finally, judges are, on average, better educated than voters and may be systematically different in other ways (culturally, socially, politically), which could lead them to prefer more lenient sentences. Regardless of the reason for the divergence in preferences, there is good reason to believe that judges might sentence more severely in order to please voters.

Of course, judges will change their behavior in response to political pressure only if there is some possibility that voters will punish them for sentencing too leniently. For voters to do so, they must vote in judicial elections, and they must have access to information on judges' sentencing. To explore the issue of turnout, we collected voting data for two counties (King and Yakima) in the 2004 judicial election cycle. In Yakima County, voter turnout for the superior court race during the 2004 general elections was 9% higher than the turnout for the three supreme court races and 90% of the

[9] Blakely v. Washington, 542 U.S. 296 (2004).

[10] For instance, Kuziemko (2007) finds that parole boards in Georgia assign longer terms to inmates with higher ex ante recidivism risk.

[11] Linear discounting will generate a linear relationship between an election's proximity and an official's behavior, while nonlinear (for example, $\beta\delta$) discounting will produce greater behavioral adjustment closer to the election.

[12] General Social Surveys, 1972–2006 [Cumulative File]: Courts Dealing with Criminals—(90, available at http://www.norc.org/GSS+Website/Data+Analysis. Although the GSS is a national survey (and we are not aware of a similar survey conducted solely in Washington), survey responses from the Pacific region are nearly identical to those for the nation as a whole. Survey results for the period we study (1995–2006) are also similar to those cited in the text.

turnout for the gubernatorial election. During the 2004 primaries, voter turnout for the superior court race was 11% *higher* than the turnout for the three supreme court races and 93% of the turnout for the gubernatorial primaries.[13] In King County, voter turnout for the two superior court races during the 2004 general elections was 95% of the turnout for the three supreme court races and 73% of the turnout for the gubernatorial election. During the 2004 primaries, there were three contested seats, and voter turnout for these races was 98% of the turnout for four supreme court races and 70% of the turnout for the gubernatorial primaries.[14] Although these numbers are anecdotal, they suggest that the public does vote in superior court elections.

The next question is whether voters have information on judges' sentencing behavior. A natural monitor of judicial behavior, and potential source of such information to the electorate, is the media. Using Lexis-Nexis, we searched major Washington newspapers to assess the frequency of stories involving Superior Court judges.[15] To provide some perspective, we compare it to the frequency of news stories involving other elected city officials.[16] For the period July 1995 to December 2006, there were 13,404 stories involving superior court judges compared to 14,434 involving city council members. Of the 13,404 stories involving superior court judges, 4,603 also involve sentencing.[17]

Importantly, newspapers focus on the serious crimes about which the public is likely most concerned: those classified by the FBI Uniform Crime Reporting Program (UCR) as violent crimes, namely, murder and nonnegligent manslaughter, forcible rape, robbery, and aggravated assault. Of the 4,603 stories involving sentencing by superior court judges, 3,671 (79.75%) involve at least one of the four crimes labeled as violent by the UCR.[18] These results support our use of the UCR-inspired set of visible crimes as the subset of crimes on which we focus our empirical work.

Potential challengers have an incentive both to monitor incumbents' current behavior and research their past behavior, and when they officially challenge an incumbent, challengers have the incentive to provide this information to the public. In their study of print media and judicial elections in Wisconsin, Kearney and Eisenberg (2002) find that in state circuit court (analogous to the Washington Superior Court) races, advertising by judicial candidates dominates newspaper articles as a means of disseminating information about judicial candidates. In addition, the authors find that advertisements often touch on criminal matters. We reviewed Washington judicial candidates' websites and voter pamphlets (from the 2008 election cycle) and found that crime and sentencing are among the issues challengers discuss.[19]

One might wonder whether the threat of competition can keep judges' behavior consistently in line with voters' preferences (resulting in no cyclicality in sentence severity). Incumbent judges may tend to sentence leniently early in their terms and more severely toward the end, despite the threat of competition, for several reason. First, if monitoring a judge's sentencing is costly, potential challengers might do so only when they are considering entering a race, which occurs late in a judge's term. Because researching past sentencing is costly, challengers will likely invest more effort in observing current judge sentencing, making it less costly for a judge to behave as he or she likes early in his or her term. Second, even if a potential challenger informs voters of a lenient sentence from a judge's past, this information may influence voters less than information on more recent sentences. Again, this would lead a judge to sentence relatively leniently early in his or her term. Finally, to the extent that the judge discounts the future, this would only make more pronounced his or her tendency to sentence severely only at the end of his or her term. Thus, we expect incumbent judges will sentence most severely close to the deadline for competitors to file to enter a race: this is when incumbents are most likely to be monitored and when they place the greatest weight on voters' preferences.

Thus, just as executive or legislative officials are incentivized to lower taxes or increase public spending before their elections to avoid being punished in the polls, judges face analogous pressure to impose longer sentences as they near the end of their political cycles. We next test whether they respond to this pressure by sentencing more severely.

IV. Data and Construction of Variables

A. *Description of the Data Set*

We obtained case-level data from the Washington State Sentencing Guidelines Commission (SGC) on criminal sentencing in felony cases. The data set includes 294,349 observations from the period July 1995 through December 2006. The data include information on case-specific variables such as defendant characteristics (for example, race, criminal history), date of sentence, name of judge, conviction

[13] For the gubernatorial primaries, 32,227 votes were cast in Yakima County compared to 72,188 in the general election.

[14] For the gubernatorial primaries, 374,784 votes were cast in King County compared to 874,928 in the general election.

[15] The Washington newspapers included in the searches discussed in this section are the *Seattle Times*, the *Seattle Post Intelligencer*, the *Columbian* (Vancouver), and the *News Tribune* (Tacoma).

[16] For a discussion of some of the potential methodological issues that may arise with the use of electronic searches on newspapers, see Glaeser and Goldin (2006) and Gentzkow, Glaeser, and Goldin (2006).

[17] To search for stories relating to superior court judges and city council members, we use the following search strings: ["Superior Court" w/5 Judge] and [(Councilman or councilwoman or councilperson or "council member") and "City Council"], respectively. To search for stories relating to superior court judges and sentencing, we use ["Superior Court" w/5 judge and sentenc!] as our search string.

[18] To search for such stories, we use the following search string: [("Superior Court" w/5 judge) and sentenc! and (murder or homicide or manslaughter or robbery or assault or rape)]. Roberts and Doob (1990) also find that violent crimes on the person (rape, robbery, assault) are most often reported in the media.

[19] The *Washington Judicial Voter* pamphlet was first published in 1996. In it, a candidate can provide a brief biography and state why voters should select the candidate.

TABLE 1.—CASE-LEVEL SUMMARY STATISTICS, VISIBLE CRIMES

Variable	Observations	Mean	SD
Defendant Gender (female=1)	18,447	0.079	0.27
Black Defendant	18,447	0.242	0.428
Hispanic Defendant	18,447	0.068	0.251
Asian Defendant	18,447	0.039	0.193
Native American Defendant	18,447	0.03	0.169
Age of Defendant (years)	18,447	29.392	10.145
Any Prior Convictions	18,447	0.527	0.499
3+ Priors	18,447	0.275	0.447
Adjudicated via Plea	18,447	0.886	0.317
Visible Crime, Excluding Murder	18,447	0.904	0.295
Sentence Length (in months, capped at 720)	18,447	67.166	120.118
Upward Deviation	18,447	0.063	0.243
Linear Distance to Election	18,447	0.506	0.293
Low end of Guidelines Range	18,447	50.943	79.873
High end of Guidelines Range	18,447	67.626	101.628

See the online appendix, table A3, for information on the construction of the variables.

offense, low and high end of the applicable sentencing guidelines range (including enhancements), type of adjudication (plea or trial), and sentence length for the most serious conviction offense (among other variables). We augment the data received from the SGC with information on judges, judicial districts, and judicial elections.

We restrict the sample of cases used in our analysis in several ways. We include only cases heard by judges serving in the superior court, the court of general jurisdiction for criminal cases. Cases heard by judges who were serving on the superior court as commissioners, in a pro tem capacity, or who were serving as district court judges at the date of sentence are excluded from our sample. Each superior court judge is matched to one of Washington's judicial districts, and we exclude cases heard by a judge outside his or her home district (judges may appear in multiple districts because of measurement error or because they are acting as visiting judges in a neighboring district). We also exclude cases heard by judges with fewer than 25 cases in the sample, cases heard by judges appointed to fill a midterm vacancy prior to their first election, and cases in which the judge has no sentencing discretion. We classify crimes according to classes based on two-digit offense codes (provided by the SGC) and restrict our analysis to felony classes for which there were at least 100 cases.

After applying these restrictions, there remain 276,119 cases heard by 265 full-time superior court judges for the period between July 1995 and December 2006 (the online appendix, table A1, contains case-level summary statistics for this sample). Our empirical analysis focuses on the most serious, visible crimes (as defined by the FBI and described in section III)—assault, murder, rape, and robbery—which make up 6.7% of the entire sample (18,447 cases). Among these visible crimes, 8% of defendants are women and 24% are black; around 53% have at least one prior conviction; the vast majority of cases are resolved by plea agreement (over 88%). The average case is associated with a 51-month low-range sentence from the sentencing guidelines grid and a 67.6-month high range; the average sentence is 67.2 months. Around 6.3% of cases result in sentences greater than the high end of the guidelines range. On average, cases are heard almost exactly halfway into a judge's election cycle (see table 1 for case-level summary statistics).

Of the 265 judges in our data set, 29% are women and 5% are black; judges on average were admitted to the Washington Bar in 1974 and took their seats on the superior court in 1992. Of those judges, 36% had some prior experience as prosecuting attorneys, and 46% had previous judicial experience. In the time period on which we focus (1995–2006), we observed 456 seated judges filing for reelection. Of these judges, 39 (8.4%) faced competition in a primary election, and 4 (.87%) faced competition through a general election (the online appendix, table A2, contains summary statistics for the 265 judges).

B. *Construction of Variables*

We examine several different sentencing outcomes in our analysis. Our primary measure of sentence severity is the length of a prison or jail sentence in months, top-coded at 720 months (following Abrams, Bertrand, 2008, & Mullainathan, 2008). We also consider sentence lengths with a top code of 1,200 months; in some specifications, we censor sentence length at the high and low ends of the applicable sentencing range. Finally, we consider a binary outcome variable equal to 1 if a judge imposed a sentence above the high end of the guidelines' range for a case.

The explanatory variable of interest is the electoral pressure on a case's judge, in particular, the proximity of the judge's next election or filing deadline (information was compiled from the Washington secretary of state's website, various county auditor websites, and county election websites). We construct both linear and nonlinear measures of electoral proximity. The linear measure of electoral proximity is in general for case i, heard by judge j, sentenced at time t, the number of days between time t and the next election's filing deadline for judge j, divided by 1,461 (the number of days in four years, a full election cycle).[20] This measure, which we call *linear distance*, ranges from 0 to 1, with 0 implying maximal electoral pressure. When a judge faces a competitor for his or her seat, we set our proximity measure equal to 0 for any cases sentenced between the filing deadline and the date that an election determines the winner of the seat. Based on this linear measure of electoral proximity, we construct a set of dummy variables that indicate the number of quarters remaining until a judge's upcoming filing deadline, ranging from one to sixteen (with any cases between the filing deadline and the end of that judge's election cycle included in the one-quarter-to-election period).

Case-specific controls include defendant's age, gender, race, and prior criminal history; an indicator of whether the

[20] For sentences dated before July 1, 1999, our data set lacks the specific day of the sentence. We have dated these sentences as having occurred on the fifteenth of the sentence month.

sentence resulted from a plea agreement; a set of offense-specific indicator variables; and the applicable sentencing guidelines range for each case. Additionally, we construct a set of fixed effects for the cell in the Washington Sentencing Guidelines Grid in which a case is located. The cells are constructed based on the high and low end of the sentencing guidelines range, including all enhancements. In order to avoid estimates based on cells with very few cases, we consider a variety of methods of grouping cases that are in the most unusual cells, grouping them 100, 150, or 200 at a time.

To control for changes in sentencing behavior resulting from the *Blakely* decision, we generate an indicator variable equal to 1 if the case's sentence date was after June 24, 2004. We also generate a set of year-specific fixed effects to capture shocks affecting criminal sentencing that are common to all judges in a given year and a set of fixed effects for each quarter of the year (January through March, April through June, and so on), which control for any seasonality in judges' sentencing behavior.

A set of judge fixed effects controls for differences in sentencing across judges, and a set of judicial district fixed effects controls for time-invariant features of a district (for example, stable differences in the various district attorneys' offices). We also collected information on time-variant judicial district characteristics: unemployment rates and crime rates (see the online appendix, table A3, for a detailed description of the names, definitions, and sources of all variables used in our empirical analysis).

V. Empirical Model and Results

A. Empirical Model and Identifying Assumptions

We constructed a data set by merging case-level information provided by the SGC to information on districts and judges compiled from a variety of sources. The unit of observation in our data set is the case, i, heard by judge j, sentenced at time t. Each case is associated with a specific offense, defendant, judge, sentence date, and sentence.

Our empirical model is the following:

$$severity_{ijt} = F(t) + \beta_1 Prox_{ijt} + \beta_2 Z_{ijt} + \epsilon_{ijt}, \qquad (1)$$

where $severity_{ijt}$ is a sentencing outcome associated with case i; $F(t)$ includes a set of year and quarter fixed effects, as well as a dummy variable indicating whether case i was decided after the *Blakely* decision; $Prox_{ijt}$, our explanatory variable of interest, is a measure of electoral proximity (linear or nonlinear); Z_{ijt} contains a set of defendant, crime, and sentencing guidelines controls, as well as a full set of judge and judicial district fixed effects; and ϵ_{ijt} is a mean-zero stochastic error term.

This baseline empirical specification goes beyond just examining the reduced-form relationship between judicial elections' proximity and sentencing (though this reduced-form relationship is in itself interesting). It incorporates various aspects of the structure and timing of judicial procedure in Washington State in order to try to identify changes in judges' behavior per se. In particular, two crucial outcomes of attorneys' actions—whether a case is adjudicated by plea or trial and the sentencing guidelines cell in which a case falls—are determined prior to a judge's sentencing decision and are observed in our data set. In the baseline analysis, we control for variation in the type of adjudication and the sentencing guidelines cell in which each case fell, thus purging from the estimated effect of electoral proximity that part which works through attorneys' bargaining. Under the identifying assumptions that cases are randomly assigned across the judge's political cycle (conditional on controls) and that the attorneys' negotiation process does not change across the judge's political cycle (assumptions that we examine in detail), this specification allows us to estimate the effect of judicial elections' proximity working through changes in a judge's sentencing, conditional on the outcomes of attorneys' negotiations.

Before presenting our baseline empirical results, it is important first to consider the fundamental identifying assumptions that need to be satisfied for our estimate of β_1 to be an unbiased estimate of the impact of judicial elections' proximity on the sentences that judges impose. We begin by examining whether cases are randomly assigned across the political cycle, conditional on the control variables included in the model. Next, we take up the issue of endogenous outcomes of attorney negotiations (we return to both of these issues in section V.C).

In practice, an important concern is that aspects of a case that we cannot observe (for example some characteristics of the crime, of the criminal) and that affect sentencing are changing over time in a way that is correlated with our measure of electoral proximity. This might arise from the strategic behavior of attorneys or the judge, or might be due to other sources of variation from outside the judicial system, such as changes in policing. Although we cannot directly test for systematic variation in unobservable characteristics across the political cycle, we attempt to address this issue as rigorously as possible.

We begin by simply examining observable case characteristics just before (and during) the judicial election period and compare them to cases just after the judicial elections. If observables are balanced across the political cycle, one might believe that unobservables are balanced, too. In table 2, columns 1 and 2, we present summary statistics for serious, visible crime cases sentenced in the two quarters before a judge's filing deadline (and up to his election, when applicable), as well as the analogous statistics in the two quarters after a judge's filing deadline or election (see the online appendix, table A4, for a comparison of case characteristics around the filing deadline for all crimes). Importantly, visible crimes make up a similar proportion of all cases before and after elections— around 6.9%. For the serious, visible crimes, both defendant and case characteristics look very similar just before and just after judicial elections (p-values testing for

TABLE 2.—CASE CHARACTERISTICS JUST BEFORE AND JUST AFTER ELECTIONS, VISIBLE CRIMES

	All Visible, Serious Cases			Pleas Only			Trials Only		
	(1) *Before Election*	(2) *After Election*	(3) *Before = After*	(4) *Before Election*	(5) *After Election*	(6) *Before = After*	(7) *Before Election*	(8) *After Election*	(9) *Before = After*
Variable	Mean	Mean	p-value	Mean	Mean	p-value	Mean	Mean	p-value
Gender (female=1)	0.087	0.084	0.68	0.091	0.085	0.50	0.058	0.071	0.53
Black Defendant	0.254	0.238	0.20	0.248	0.227	0.10	0.296	0.331	0.37
Hispanic Defendant	0.065	0.068	0.65	0.069	0.071	0.74	0.039	0.043	0.78
Age of Defendant (years)	28.608	29.208	0.04	28.311	28.900	0.05	30.715	31.759	0.22
Any Prior Convictions	0.518	0.512	0.65	0.510	0.504	0.71	0.579	0.575	0.92
3+ Priors	0.276	0.258	0.18	0.265	0.247	0.19	0.354	0.350	0.94
Adjudicated via Plea	0.877	0.892	0.09	1.000	1.000		0.000	0.000	
Linear Distance to Election	0.062	0.938	0.00	0.062	0.938	0.00	0.063	0.937	0.00
Low end of Guidelines Range	50.316	52.543	0.34	39.120	41.706	0.19	129.879	142.226	0.27
High end of Guidelines Range	66.584	69.549	0.32	52.420	55.875	0.17	167.236	182.704	0.28
Observations	2,521	2,356		2,210	2,102		311	254	

The period just before the election is defined as the period with two or one quarter to the next election (or filing date, depending on whether the judge faced competition); the period just after the election is defined as sixteen or fifteen quarters to the next election. *P*-values come from a two-tailed test that the mean just before the election equals the mean just after the election. See the online appendix, table A3, for information on the construction of the variables.

equality of means are reported in column 3). Women make up around 8.5% of the sample in both periods and black defendants around 25%; around 51% of defendants have at least one prior conviction. Most of the data in table 2, columns 1 to 3, are consistent with random assignment of cases across judges' political cycles.

Some case characteristics do differ across periods. The most important of these is the fraction of cases adjudicated by plea agreement: 87.7% just before elections and 89.2% just after elections, a marginally significant difference. This difference immediately raises the question of endogenous attorney negotiation across judges' political cycles. It is reassuring to see that the high and low ends of the sentencing guidelines range reveal no significant differences in table 2 (this suggests that neither judges nor attorneys successfully manipulate the sentencing guidelines range), but we must examine attorneys' bargains in greater depth.

Pleas might differ across the political cycle simply because of natural variation in case characteristics; in that case, we would simply want to control for whether a case resulted in a plea or trial. But the changing rate of plea agreements might systematically alter the types of cases ending in plea agreement across judges' political cycles. To see whether the difference in plea agreements seems to shift the types of cases in a given category, we check for balanced characteristics within adjudication categories across the election cycle. In table 2, columns 4 to 9, we present summary statistics just before and just after elections, along with tests for equality of means, for pleas alone and for trials alone. In both cases, observable case characteristics are quite balanced across the election cycle; for example, it is not the case that pleas just before and after the cycle end up with significantly different sentencing guidelines ranges.

To examine the potential endogeneity of pleas and sentencing guidelines range outcomes more systematically, we regress these outcomes on *linear distance* (our explanatory variable of interest in the baseline model), as well as year and quarter-of-the-year fixed effects (which control for changes in case characteristics across time). In all three regressions, the outcomes of attorney negotiations are not significantly associated with our measure of political pressure on the judge: *p*-values testing for a significant relationship range from 0.28 to 0.76. We also examined whether these outcomes were systematically different in the last six months of the judge's political cycle (relative to the rest of the cycle), regressing the three negotiation outcomes on a dummy indicating that a case was sentenced in the last six months of a judge's political cycle, year fixed effects, and quarter-of-the-year fixed effects. Again, *p*-values testing for a significant relationship between the political cycle and attorneys' negotiations are quite high: between 0.24 and 0.65. Finally, we ran 115 regressions with an indicator for each sentencing guidelines cell as the outcome and an indicator that a case was sentenced in the last six months of the political cycle as the explanatory variable of interest (using our baseline specification). In only five of these is the coefficient on the "last six months" indicator significant: cells are distributed essentially randomly across the political cycle.[21]

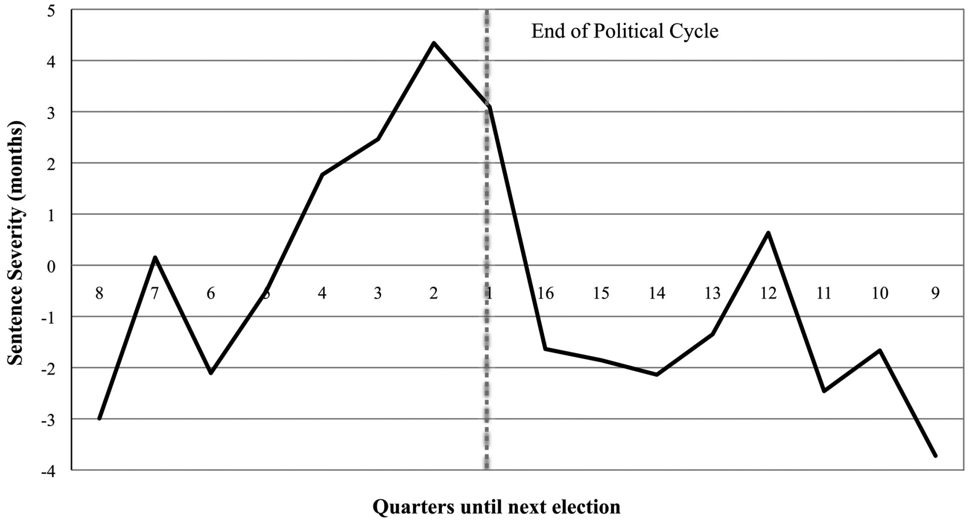
We will return to concerns about endogenous attorney bargains in section V.C, and take up more general concerns about unobservables correlated with judges' political cycles in section V.C as well. For now, given the reassuring evidence on the distribution of cases and attorneys' negotiations across judges' political cycles, we will estimate our baseline specification, controlling for the type of adjudication and the guidelines cells.

B. *Baseline Results and Sensitivity Analysis*

We begin our examination of sentence severity across judges' political cycles by presenting the sentencing patterns

[21] All of these regressions are available on request. They are omitted for brevity.

FIGURE 1.—SENTENCE SEVERITY ACROSS THE POLITICAL CYCLE



The average difference (for serious, visible crimes) between sentence length in months (capped at 720) and the high end of the sentencing guidelines range, by the number of quarters remaining until the end of a judge's political cycle (either the filing deadline for competitors or the election date).

for serious, visible crimes in the raw data. We plot the average difference between the sentence length and the high end of the applicable guidelines range by the number of quarters remaining until the next election or filing deadline (see figure 1). This graph shows an increase in sentence length from the beginning of the political cycle to the end and a sharp decline in the severity of sentences just after the cycle ends. In addition, sentences in the final year of the political cycle are, on average, above the high end of the guidelines range; this is almost never the case during the first three years of the cycle.

To examine this relationship more rigorously, we estimate equation (1) for visible crimes using a linear measure of electoral proximity (*linear distance*) and a full set of control variables and using the sentence length in months, capped at 720 months, as our outcome variable. This specification covers three judicial elections: 1996, 2000, and 2004. If judges sentence more harshly as their elections approach, one would expect distance to the election to be negatively correlated with sentence length; as can be seen in table 3, column 1, this is exactly what we find. We estimate that moving from the beginning of a judge's election cycle to the end adds a statistically significant 6.8 months to a defendant's sentence.[22] This represents over 10% of the average sentence for visible crimes in our sample.[23]

We next check the robustness of our results to the cases included in our empirical analysis. One may be concerned that the *Blakely* decision, which significantly reduced judicial discretion, may confound our estimates. To address this, we estimate equation (1) as above using only cases decided pre-*Blakely* (see table 3, column 2). One might worry that murder cases are driving our results: because murder convictions often produce very long sentences, including murder cases might allow outlying sentences to have undue influence.[24] We thus estimate equation (1) only for visible crimes other than murder (see table 3, column 3). Finally, we estimate equation (1) only on cases adjudicated by plea agreement (see table 3, column 4). In all three of these specifications, our estimated coefficient is negative, statistically significant, and large. Even for cases adjudicated by plea agreements, where one might expect judicial discretion to be more limited, the estimated coefficient is over 6% of the mean sentence for visible crimes.[25]

It is also important to evaluate the robustness of our results to the specification choices that we made. One concern is that our measure of electoral proximity is endogenous: if severe sentences make it less likely that an incumbent judge will face a challenger, then our proximity measure will be endogenous. Thus, we estimate our baseline specification using a purely exogenous measure of linear distance, which uses only the time until the next filing deadline to measure

[22] In all calculations of statistical significance, standard errors are clustered at the quarter-of-the-year level (for example, January 1, 2004, to March 31, 2004). Clustering at the judge or county level does not change our inferences, nor does clustering at the level of the election cycle.

[23] The coefficient estimates on variables other than *linear distance* are presented in the online appendix, table A5. These estimates are consistent with results from the literature: black defendants receive longer sentences, women shorter sentences, older defendants longer sentences, and defendants with prior convictions longer sentences, ceteris paribus.

[24] To some extent this problem is alleviated by our decision to top code sentences at 720 months (below we consider raising the top code to 1,200 months). Including murder cases involves other potential complications; for example, unobservables may play an unusually important role in murder sentences.

[25] If we estimate our baseline specification on trials alone, our point estimate is qualitatively similar to that on the entire sample: sentences are around 10% longer at the end of the cycle than at the beginning. Because there are relatively few trials in our sample (one-eighth the number of pleas), the estimate is not very precise and thus not quite statistically significant: the *p*-value of the coefficient estimate on *linear distance* is 0.177. See the online appendix, table A6, column 1.

TABLE 3.—TESTING FOR CYCLES IN SENTENCE LENGTH

	(1) Baseline	(2) Pre-*Blakely*	(3) Excluding Murder	(4) Only Pleas	(5) Exogenous Linear Distance	(6) No Unusual Cells	(7) High and Low End Linear Controls	(8) 1985–2006	(9) Log(1 + months) Outcome
linear distance	−6.802	−7.411	−5.766	−3.291	−6.614	−4.526	−6.831	−3.302	−0.027
	[2.209]***	[2.526]***	[2.726]**	[1.134]***	[2.079]**	[2.302]*	[2.556]**	[1.285]**	[0.012]**
Observations	18,447	14,459	16,668	16,353	18,447	15,619	18,447	29,463	18,447
R^2	0.73	0.73	0.59	0.78	0.73	0.54	0.73	0.62	0.92
Mean sentence length in sample	67.17	67.32	47.39	50.51	67.17	39.68	67.17	42.19	67.17

Robust standard errors, clustered at the quarter-year level, in brackets: Significant at *10%, **5%, and ***1%. Columns 1–7: The outcome variable is the sentence length in months, capped at 720. The explanatory variable of interest, *linear distance*, is a linear measure of distance to the next election (in column 5, this is based strictly on filing deadlines). All regressions include year fixed effects, quarter fixed effects, a post-*Blakely* dummy variable (except column 2), time-variant district controls (unemployment rate, crime rate, and violent crime rate), defendant controls (age, a gender dummy, race dummies, a dummy for prior convictions, and a dummy for three or more priors), offense fixed effects, a dummy indicating whether the case was resolved by plea agreement (except column 4), a set of judicial district fixed effects, a set of judge fixed effects, and a set of guidelines cell fixed effects (except column 7). Column 8: The outcome variable is the sentence length in months, capped at 720. The explanatory variable of interest, *linear distance*, is a linear measure of distance to the next filing deadline. Regression includes year fixed effects, quarter fixed effects, dummy variables for defendant's race, a dummy indicating whether the case was resolved by plea agreement, county fixed effects, offense fixed effects, and a set of guidelines cell fixed effects. Column 9 uses the same specification as column 1, but uses as an outcome variable the log(1+sentence length in months). Refer to the online appendix, table A3, for information on variable construction.

electoral proximity (see table 3, column 5).[26] Another issue is our method of controlling for the guidelines range relevant to each case. In our baseline estimates (table 3, column 1), we used cells constructed based on the low and high end of the range for each crime, and for crimes in cells with fewer than 150 cases, we grouped crimes, 150 at a time, based on similarity of the low end of the range for each crime.[27] One might be concerned about grouping crimes with different low- and high-end ranges. To see whether this grouping of crimes affects our results, we use only cases in cells that contain 50 or more observations, dropping cases in the most unusual cells instead of grouping them (see table 3, column 6). To determine whether our results depend on the use of any sort of cells as controls, we consider estimates that simply control linearly for the low end and the high end of the sentencing range applicable to the case (see table 3, column 7). Under all of these alternative specifications, we find significant and large sentencing cycles of around 10% of the average sentence length in the relevant sample.

Next, we estimate equation (1) on an extended data set covering the period 1985 to 2006, which includes five judicial elections.[28] This data set, also provided to us by the Washington SGC, does not have as much information as our 1995–2006 data set; in particular, it lacks information on each case's judge. Thus, we simply assign each case a linear electoral proximity measure based on the upcoming election's filing deadline. The estimates using these data will be imperfect, but they should reassure readers that our estimates in table 3, columns 1 to 7 were not the product of too few election cycles. These results are also based on an entirely exogenous measure of electoral pressure, as they do not incorporate information on competition. We thus estimate equation (1) using the entire 1985–2006 time period and again find a statistically significant, negative, and large coefficient on *linear distance* (see table 3, column 8).[29]

Finally, we use the log of the sentence length (plus 1) as the outcome in our regression, as this has been standard in the empirical literature on sentencing. We prefer the levels specification since adding 1 to the sentence length to avoid dropping sentences of length 0 is arbitrary; in addition, the log transformation of the outcome variable reduces the variation from which we identify the effect of electoral proximity, especially for longer sentences. Nonetheless, we observe statistically significant sentencing cycles in this specification as well (see table 3, column 9).[30]

We report several other robustness checks in the online appendix. First, because we include many fixed effects in our baseline specification, we estimate equation (1) but use judge characteristics instead of judge fixed effects. To test the sensitivity of our results to the choice of 720 months as a top code for our outcome variable, we estimate equation (1) but use the sentence length in months capped at 1,200 months as our outcome variable. Our baseline results are robust to all of these specification choices (see the online appendix, table A6, columns 3–5).

The results presented in table 3 are striking: essentially the same defendant (based on observable characteristics), having committed the same crime, facing the same judge, with his case ending in the same sentencing guidelines cell, receives a significantly longer prison sentence if he is sentenced at the

[26] While we focus on results using our standard *linear distance* measure throughout the paper, all of our estimates are robust to using the purely exogenous *linear distance* measure.

[27] To check whether our results are sensitive to this method of grouping, we instead group the crimes in cells with fewer than 100 cases, 100 at a time, and 200 cases, 200 at a time. We also construct cells based solely on the seriousness of the conviction offense and the defendant's criminal background. These constructed cells do not perfectly map onto the actual cells for all crimes because weapons enhancements, attempts, and other factors determine the low and high end for a given crime, along with the conviction offense and the criminal history. Finally, we estimate the model using all cells without grouping. The results are unaffected using these cell constructs.

[28] See the online appendix, table A3, for a brief description of the dataset.

[29] See the online appendix, table A6, column 2, for results from the 1995–2006 period using the rougher, 1985–2006 data set's data for this period. These estimates confirm the results from table 3, column 1, using the alternative data set. Note also that our results are robust to dropping the four years around each of the individual elections in our sample.

[30] We have estimated the "plus 1" specification, including as a control an indicator that the sentence was of length 0. This does not affect our results.

TABLE 4.—EVALUATING ALTERNATIVE HYPOTHESES

	(1) Time-Varying Effect of Adjudication Type	(2) No Adjudication or Sentencing Guidelines Controls	(3) Retirement Term	(4) Election Terms	(5) Less Visible Crimes	(6) Visible Crimes (Baseline)	(7) Oregon Data	(8) Washington Data
linear distance	−3.944	−7.854	−1.242	−16.416	−0.113	−6.802	0.716	−7.864
	[1.711]**	[3.227]**	[6.979]	[7.496]**	[.164]	[2.209]***	[2.943]	[2.631]***
Observations	18,447	18,447	1,694	1,499	257,672	18,447	16,990	18,447
R^2	0.74	0.58	0.71	0.77	0.66	0.73	0.76	0.54
Mean sentence length in sample	67.17	67.17	68.24	72.41	11.32	67.17	83.5	67.17

Robust standard errors, clustered at the quarter-year level, in brackets: Significant at *10%, **5%, and ***1%. Columns 1 and 2: The outcome variable is the sentence length in months, capped at 720. The explanatory variable of interest, *linear distance*, is a linear measure of distance to the next election. Column 1 adds to the baseline model of table 3, column 1, an interaction term between a trial dummy variable and *linear distance* (and includes a trial dummy rather than a plea dummy). Column 2 excludes from the baseline specification the plea dummy and all controls for sentencing guidelines cells. Columns 3 and 4: The outcome variable is the sentence length in months, capped at 720. The explanatory variable of interest, *linear distance,* is a linear measure of distance to the next election. Both regressions are estimated using only cases heard by judges who both retire and face electoral pressure in the sample. Column 3 is estimated using only cases heard by these judges in their final terms before they retire from office. Column 4 is estimated using only cases heard by these judges during terms when they faced electoral pressure. Both regressions include year fixed effects, quarter fixed effects, a post-*Blakely* dummy variable, time-variant district controls (unemployment rate, crime rate, and violent crime rate), defendant controls (age, a gender dummy, race dummies, a dummy for prior convictions, and a dummy for three or more priors), offense fixed effects, a dummy indicating whether the case was resolved via plea agreement, a set of judicial district fixed effects, a set of judge fixed effects, and a set of guidelines cell fixed effects.The outcome variable is the sentence length in months, capped at 720. Columns 5 and 6: The specification is as in columns 3 and 4, but all judges are included. Column 5 is estimated using only crimes that are not classified as "visible," and column 6 reproduces the baseline estimates using only visible crimes. Columns 7 and 8: The outcome and the explanatory variable of interest, *linear distance,* are as in columns 1–6. The visible-crime definition in Oregon was constructed by offense to match that used for Washington. Oregon sentences of life without parole and death were not included, as these sentences are generally not included in the Washington estimates. Both regressions include year fixed effects, quarter fixed effects, county fixed effects, defendant controls (age, a gender dummy, and a set of race dummies), and offense fixed effects. Refer to the online appendix, table A3, for information on variable construction.

end of the judge's political cycle rather than the beginning. This result is robust to a wide range of different specification choices, the exclusion of murder, and the construction of guidelines cells. It is also robust to examining the five judicial elections between 1985 and 2006.

C. *Ruling Out Alternative Hypotheses*

The results presented thus far strongly suggest that greater electoral proximity for a case's judge is associated with a longer sentence for that case. However, one could conceive of explanations for the relationship observed other than judges responding to political pressure. Here we evaluate several alternative explanations for our results: changes in the behavior of the defense attorney and the prosecutor, the effects of the political cycles of officials other than judges, and changes in unobservable case characteristics across judges' political cycles more generally.

Does attorneys' behavior change across judges' political cycles? In our baseline estimates, we attempted to identify judges' (as opposed to attorneys') response to political pressure by controlling for the outcomes of attorneys' negotiations. We now explore changes in attorney behavior across judges' political cycles in more depth. We consider in turn the changing rate of plea agreements across judges' political cycles, the roles of changing plea agreement practices and sentencing guidelines cells in generating sentencing cycles, and the shifting of cases across time by attorneys (or judges).

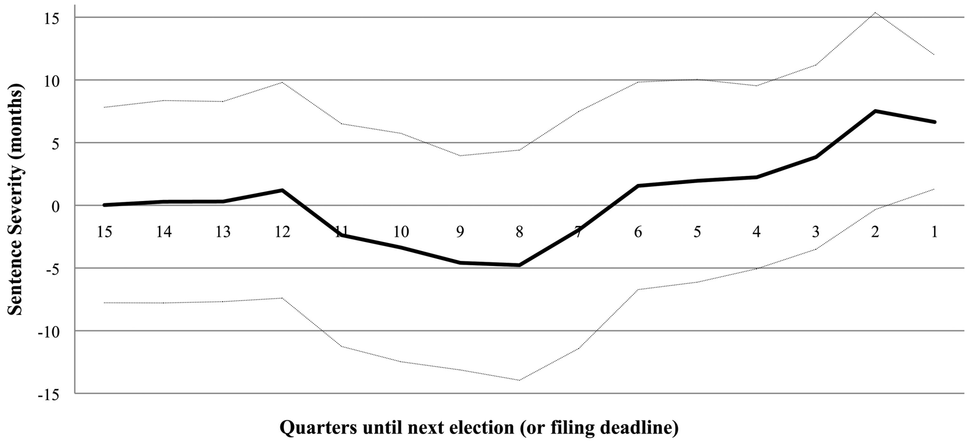
Our results in table 2 raised some concerns about differences in the types of plea agreements reached across judges' political cycles. Moreover, Piehl and Bushway (2007) find that charge bargaining and prosecutorial discretion in negotiating pleas are important in Washington. If the bargains struck varied across judges' political cycles—perhaps because attorneys understood that judges' incentives differed—this could affect both the rate of pleas across the cycle and the types of cases in each adjudication category. For example, if cases with "worse" unobservable characteristics were adjudicated by plea toward the end of judges' political cycles, simply controlling for the adjudication type would not be enough. One might worry that the association between judicial elections' proximity and sentence severity observed in our baseline specification resulted from misspecifying the effect of the type of adjudication on sentence length. To address this concern, we estimate our baseline model but now include an interaction between the type of adjudication (a trial dummy variable) and *linear distance*. The coefficient on *linear distance* here captures the impact of elections' proximity on sentencing for pleas, which will allow a comparison with the pleas-only specification presented in table 3, column 4.[31] As can be seen in table 4, column 1, we find a statistically significant coefficient on *linear distance* of approximately the same magnitude as in our only-pleas specification.[32] This suggests that controlling for a fixed effect of the type of adjudication did not drive our results.

We next estimate the effect of judicial elections' proximity on sentencing as in our baseline model, but excluding the potentially endogenous type of adjudication and sentencing guidelines controls. If attorneys' bargaining played a large role in generating longer sentences at the end of judges' political cycles one would expect a large change in the coefficient on *linear distance* when we do not control for these bargains.

[31] To be precise, we include the interaction between a trial dummy variable and *linear distance*, as well as the trial dummy alone (and we omit the plea dummy). The coefficient on the interaction of linear distance with trial plus the coefficient on *linear distance* captures the impact of elections on sentencing for trials. We do not report the latter in the paper, but it is statistically significant and large.

[32] We have also estimated a model allowing pleas to differentially affect sentencing in the last six months of a judge's political cycle and again find an estimated coefficient on *linear distance* similar to the only-pleas specification (results available upon request).

FIGURE 2.—SENTENCE SEVERITY BY QUARTER TO ELECTION



Estimated coefficients (relative to the omitted sixteen-quarters-to-election category) and 95% confidence intervals from a regression of sentence length in months on dummies indicating the number of quarters until the upcoming election and a rich set of control variables.

Similarly, if the unobservable characteristics of cases in particular cells differed across judges' political cycles and our baseline estimates were driven by mistaken comparisons of different types of cases in the same cell, then omitting these controls should change our results. In fact, the estimated effect of elections' proximity is very similar in these specifications to our baseline (compare table 4, column 2, to table 3, column 1). This result suggests that neither changed attorney bargains nor varying case characteristics within a guidelines cell or adjudication category across the judge's political cycle plays a large role in the cyclical sentencing pattern we observe.

Finally, case shifting across time—by attorneys or by the judge—is certainly a concern. However, shifting cases would likely generate unbalanced observable case characteristics across the political cycle, which we do not generally observe (see table 2). Case shifting would also likely generate significant differences across the political cycle in the time between charge and sentence. We have data on the time between charge and sentence for two-thirds of our cases. In results we omit for brevity, we run this time-to-sentence variable as the outcome in our main specification, and the point estimate suggests that it takes five days longer for a case to be sentenced at the end of the cycle than at the beginning: testing the significance of *linear distance* yields a p-value of 0.81. Furthermore, case shifting based on case characteristics unobservable to us should generate systematic differences not just at the end of the cycle (when we observe longer sentences), but also just after the political cycle ends. If attorneys or judges delayed cases with unobservable characteristics associated with lenient sentences until after the end of the political cycle, one would expect the first quarter after the election (or filing deadline) to have significantly more lenient sentences than quarters thereafter. We do not observe any such significant differences. If we estimate equation (1) using a set of quarter-to-election dummies as our measure of judicial elections' proximity, we observe severe sentences at the end of judges' political cycles, but there are no significant differences among dummies for sixteen, fifteen, and fourteen quarters to election (see figure 2).[33]

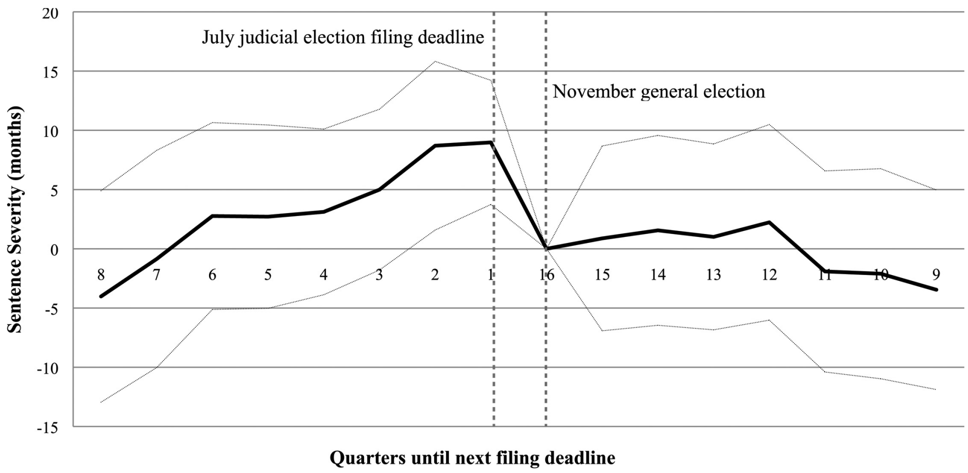
Other political cycles. Another possible alternative to the hypothesis that judges respond to electoral pressure is that judges' electoral cycles coincide with some other political cycle, which is in fact driving the sentencing differences we observe. For example, Levitt (1997) finds that there are cycles in the hiring of police officers associated with the elections of mayors and governors. This might produce spurious sentencing cycles by changing the composition of cases or affecting judges' preferences. One might also be concerned if district attorneys' or the attorney general's political cycles corresponded with those of judges, because changes in these officials' behavior might affect plea bargains and sentencing.[34]

Fortunately, we can make a strong case that our results are not being driven by nonjudicial political cycles. First, the elections of mayors in Washington take place on odd-numbered years, so mayors' political cycles do not correspond with judges' (RCW 29A.04.330 and RCW 2.08.060). Second, district attorneys in Washington run on the off-year election cycle (that is, they run in even-numbered but not presidential election years; RCW 36.16.010). Thus, our results are not driven by mayors, or district attorneys, responding to their own political cycles.

In Washington, the governor and the attorney general do run on the same electoral cycle as judges. However, we can exploit differences in the timing of political pressure across offices to isolate the impact of the judicial political cycle. Specifically, many judges in our sample face only a threat of competition; their political cycle effectively ends in late

[33] In the online appendix, table A10, we present these regression estimates.
[34] Dyke (2007) finds that district attorneys prosecute more cases during their election years.

FIGURE 3.—SENTENCE SEVERITY OF JUDGES WITHOUT COMPETITION



Estimated coefficients (relative to the omitted sixteen-quarters-to-filing date category) and 95% confidence intervals from a regression of sentence length in months on dummies indicating the number of quarters until the upcoming filing deadline and a rich set of control variables. Only judges whose electoral cycles end at the filing deadline are used in this regression.

July, the deadline for a competitor to file to run in an upcoming election. Gubernatorial and attorney general candidates always (in the years considered) face actual competition through the November general election.

To distinguish between the political pressure associated with judges and that associated with the governor or attorney general, we estimate equation (1) using quarters to the next filing deadline as our measure of electoral proximity. Importantly, we estimate this model using only judges who are not challenged. For these judges, we expect much harsher sentencing, and more upward deviations, just before (and through) July of an election year and much more lenient sentencing just afterward. If our findings were driven by the governor's or the attorney general's political cycle, we would expect a sharp decline in the severity of sentencing in the beginning of November rather than in the end of July. In terms of the quarters to the next filing deadline dummy variables we use, judges' cycles imply large, positive coefficients when there are few quarters remaining before the filing date (relative to the omitted sixteen quarters dummy). If the other political cycles matter, sixteen quarters to the filing date should still be associated with severe sentences. This quarter covers August, September, and October, just before the attorney general's and gubernatorial general election, so one or two quarters to the filing date should be insignificantly different from zero (when 16-quarters-to-filing date is the omitted category). Indeed, we find a large, statistically significant break in sentence length at the end of July that cannot be attributed to any election cycle other than the judges' (see figure 3).[35]

Falsification exercises. If the sentencing cycles we observed above were the result of some factor other than political pressure on judges—for instance, unobservable case characteristics changing in a manner correlated with judges' political cycles—one might expect to see sentencing cycles even for judges who are not running for reelection. To test for cycles among judges not facing political pressure, we next estimate equation (1) using only cases sentenced by judges who are retiring at the end of their term.[36] We present results in table 4, column 3. In fact, we do not find evidence of sentencing cycles among judges who are in their final terms. The estimated effect of greater electoral proximity is small and is not statistically significant.

It is important to verify that the judges who retired in our sample did cycle when they faced political pressure; otherwise one might think that the retiring judges in our sample were simply different and perhaps never "cycled." We thus estimate equation (1) for the judges who retire in our sample but during terms when they faced elections, and find that these judges did exhibit sentencing cycles when they faced political pressure (see table 4, column 4).[37]

Another check of the theory that political pressure affects sentencing involves consideration of crimes, the sentencing of which might be less salient or important to voters. Finding large cycles even for these less visible crimes might suggest that something other than political pressure is driving our results. Thus, we estimate equation (1) only using less visible crimes. Consistent with our hypothesis, we do not find significant sentencing cycles for the crimes that are not visible

[35] Regression coefficients and standard errors are reported in the online appendix, table A7, column 1.

[36] To be precise, we estimate equation (1) for judges who both face electoral pressure and retire in our sample, using cases sentenced in their final terms.

[37] One might also be concerned that retiring judges heard very different cases in their final terms. In the online appendix, table A8, we present summary statistics for serious crime cases heard by these judges during terms when facing elections and terms when not facing elections. While there are some differences across terms, the fraction of defendants who are black, the fraction with prior convictions, and the average low end and high end of the sentencing guidelines range are all quite similar.

TABLE 5.—TESTING FOR SENTENCING CYCLES: UPWARD DEVIATIONS

	(1) Baseline	(2) Pre-*Blakely*	(3) Excluding Murder	(4) Only Pleas	(5) Exogenous Lindist	(6) No Unusual Cells	(7) High and Low End Linear Controls	(8) 1985–2006	(9) Probit
linear distance	−0.033	−0.04	−0.027	−0.027	−0.037	−0.027	−0.033	−0.024	−0.029
	[.012]***	[.012]***	[0.009]***	[0.013]**	[0.011]***	[0.011]**	[0.013]**	[0.006]***	[.011]***
Observations	18,465	14,472	16,673	16,359	18,465	15,619	18,465	29,463	17,081
R^2	0.09	0.1	0.08	0.07	0.092	0.09	0.08	0.07	0.16
Mean probability of upward deviation	0.063	0.069	0.055	0.050	0.063	0.056	0.063	0.055	0.068

Robust standard errors, clustered at the quarter-year level, in brackets: Significant at *10%, **5%, and ***1%. Columns 1 through 7 and 9: The outcome variable is a dummy variable equal to 1 if the sentence imposed in case *i* exceeds the high end of the guidelines range for case i. The explanatory variable of interest, *linear distance,* is a linear measure of distance to the next election. Regressions estimated using OLS, except column 9, which reports marginal effects from a probit model. All regressions include year fixed effects, quarter fixed effects, a post-*Blakely* dummy variable (except column 2), time-variant district controls (unemployment rate, crime rate, and violent crime rate), defendant controls (age, a gender dummy, race dummies, a dummy for prior convictions, and a dummy for three or more priors), offense fixed effects, a dummy indicating whether the case was resolved by plea agreement (except column 4), a set of judicial district fixed effects, a set of judge fixed effects, and a set of guidelines cell fixed effects (except column 7). Column 9: The outcome variable and the explanatory variable of interest, *linear distance,* are as in the other columns. Regression includes year fixed effects, quarter fixed effects, dummy variables for defendant's race, a dummy indicating whether the case was resolved by plea agreement, county fixed effects, offense fixed effects, and a set of guidelines cell fixed effects. Column 8: The outcome variable is as in the other columns. The explanatory variable of interest, *linear distance,* is a linear measure of distance to the next filing deadline. Regression includes year fixed effects, quarter fixed effects, dummy variables for defendant's race, a dummy indicating whether the case was resolved by plea agreement, county fixed effects, offense fixed effects, and a set of guidelines cell fixed effects. Refer to the online appendix, table A3, for information on variable construction.

(see table 4, column 5, and compare with the baseline results for visible crimes reproduced in table 4, column 6).[38]

As a final falsification exercise, we examine felony sentencing in Oregon and test whether Oregon sentencing exhibits cycles that coincide with Washington judicial elections, even though Oregon's judicial election cycles do not generally overlap with Washington's (Oregon judges are elected every six years; see Oregon Constitution Art. VII). We obtained data from the Oregon Criminal Justice Commission (described in the online appendix, table A3) and estimated a model similar to equation (1) using sentence length in months as the outcome variable and *linear distance* as the explanatory variable of interest. We find no evidence of sentencing cycles in Oregon corresponding to the Washington electoral cycle (see table 4, column 7). This is not a result of the slightly different specification used; running the same specification on our Washington data yields large and significant cycles (see table 4, column 8).

These results strongly support the hypothesis that judges' behavior changes as they face greater political pressure near the end of their political cycles. To better understand this shift in judges' behavior, we now examine sentencing in more detail, focusing in particular on the role of deviations outside of Washington's sentencing guidelines in generating the sentencing cycles we have identified.

D. *Upward Deviations outside the Guidelines Range*

Prior to the *Blakely* decision, judges in Washington State could exercise their discretion to impose more severe sentences along two dimensions: they could impose longer sentences within the cell of the guidelines range that applied to a given case and could find aggravating factors that would allow them to deviate outside the cell of the sentencing guidelines grid. Since Blakely, judges have still been able to deviate above the high end of the guidelines cell, but the special factors must be found by the jury or pled to by the defendant.[39] Here we examine whether judges exercise their discretion to sentence above the guidelines range more often as their elections approach.

We estimate equation (1) for visible crimes, using *linear distance* as a measure of electoral proximity and using as our outcome variable a dummy variable equal to 1 if the imposed sentence exceeds the high end of the guidelines range.[40] Using our baseline specification, we find that, indeed, judges deviate above the guidelines range more often closer to their elections (see table 5, column 1). The effect of electoral proximity is both statistically and economically significant: moving from the beginning of a judge's political cycle to the end is estimated to increase the probability of an upward deviation by 3 percentage points—over half of the average probability of an upward deviation for visible crimes.

As we did above, we present a variety of robustness checks of our baseline estimates. In table 5, columns 2 to 8, we present robustness checks analogous to those presented in table 3, columns 2 to 8. Again we find that our results are not sensitive to the exclusion post-*Blakely* cases, murder cases, or trials; the use of a linear distance measure based only on filing deadlines; the construction and inclusion of guideline cells; or the use of the 1985–2006 data set discussed above.[41] In addition to these specification checks, we estimate equation (1) as a probit rather than as a linear probability model (see

[38] In results omitted for brevity, we estimate equation (1) using all crimes and include an interaction between a "visible crime indicator" and our linear electoral proximity measure. We find that the coefficient on electoral proximity alone (which applies to less visible crimes) is not significant, while the interaction between the visible crime indicator and electoral proximity is significantly different from 0. These results are available in the online appendix, table A9.

[39] Judges can also deviate below the low end of the guidelines cell that applied to a given case (see Engen et al., 2003). The circumstances under which judges can do so are relatively complicated, and deviations below the range therefore require careful study, which we leave to other work.

[40] We use only those crimes for which the high end of the guidelines range is fewer than 1,200 months in order to consider only those cases for which upward deviations are possible given our highest top-code for sentence length of 1,200 months. Using the 720 months top-code does not change the results.

[41] Our test for cyclicality of upward deviations is robust to all of the other specifications considered in our test for cyclicality of sentence length.

table 5, column 9). In the online appendix, we show, using a nonlinear electoral proximity measure, that the upward deviations are not driven by other elected officials' political cycles (see the online appendix, table A7, column 2); we also show that upward deviations among trials follow the same pattern as for other cases (see the online appendix, table A11). We consistently find that upward deviations are more likely at the end of the political cycle than at the beginning, and the magnitude of our estimates suggests that deviation outside the guidelines range is an important aspect of judges' use of their discretion in response to political pressure.[42]

Determining whether deviations above the range explain a large fraction of the difference in sentence length across the political cycle is important for at least two reasons. First, the ability of a judge to deviate above the range prescribed by the sentencing guidelines is granted by legislatures to allow judges to tailor sentences to fit unusual offenses that deserve unusually severe punishment.[43] One's views (and especially those of legislators) on the desirable range of judicial discretion might be affected if one thought that it was used in response to political pressure rather than to the circumstances of an offense. Note that we cannot say whether judges' greater use of discretion toward the end of the political cycle is harmful or beneficial to social welfare. That is, judges might be sentencing optimally toward the end of political cycles (when sentences are longer and upward deviations more common) or toward the beginning. Regardless, one would likely prefer judicial discretion to be used consistently in response to the facts of a case rather than to the timing of a case's sentencing.

Second, the *Blakely* decision affected judges' abilities to deviate above the sentencing guidelines range differently in different states. In Washington, judges can no longer find aggravating circumstances to deviate above the guidelines range. Thus, upward deviations have become much less a matter of judicial discretion, constraining the ability of a judge to impose different sentences across the political cycle. If upward deviations were responsible for a large fraction of the sentencing differences found above, one might expect more muted politically driven sentencing cycles in the post-*Blakely* period.

To determine the importance of upward deviations to the findings above, we estimate equation (1) using our baseline specification (as in table 3, column 1). Now, however, we use as our outcome variable the sentence length in months censored at the high end (and at the low-end) of the guidelines range. As above, sentence lengths are top-coded at 720 months. This can loosely be thought of as a counterfactual in which judges were forced to impose the high end of the guidelines range whenever they wanted to deviate from it.[44] Using this censored outcome variable, we find that the estimated coefficient on *linear distance* is very small for all cases, and for pleas alone (see table 6).[45] This suggests that a large fraction of the sentencing cycles found above was the result of out-of-range sentences rather than more severe sentences within the range. It will be interesting to examine data from the post-*Blakely* period, as they become available, to determine whether the pattern of upward deviations has changed and whether sentencing cycles have diminished in magnitude or are now more pronounced along the within-range margin.

Table 6.—Using Censored Sentence Lengths

	(1) All Cases	(2) Pleas Only
linear distance	−0.485	0.010
	[0.255]*	[0.323]
Observations	18,447	16,353
R^2	0.98	0.98
Mean censored sentence length	59.68	47.26

Robust standard errors, clustered at the quarter-year level, in brackets: Significant at *10%, **5%, and ***1%. The outcome variable is the sentence length in months, capped at 720, and censored at the high and low end of the guidelines range. The explanatory variable of interest, *linear distance,* is a linear measure of distance to the next election. Both regressions include year fixed effects, quarter fixed effects, a post-*Blakely* dummy variable, time-variant district controls (unemployment rate, crime rate, and violent crime rate), defendant controls (age, a gender dummy, race dummies, a dummy for prior convictions, and a dummy for three or more priors), offense fixed effects, a set of judicial district fixed effects, a set of judge fixed effects, and a set of guidelines cell fixed effects. Column 1 includes a dummy indicating whether the case was resolved by plea agreement. Refer to the online appendix, table A3, for information on variable construction.

VI. Conclusion

In this paper, we have presented a multilayered analysis of the impact of judicial elections on sentencing in Washington Superior Courts. We estimate that the difference in sentence length between the beginning and the end of a judge's political cycle is around 10% of the average sentence for serious crimes on the person. Importantly, we were able to provide suggestive evidence that judges' sentencing, rather than changes in attorneys' bargains, accounts for this pattern; we distinguish between judicial political cycles and the political cycles of other officials; and we rule out competing hypotheses, such as changing case characteristics, by conducting several falsification exercises. We show that judges' deviations outside the sentencing guidelines range account for a large part of the sentencing cycles we find, suggesting that constraining judicial discretion could affect judges' response to political pressure. We contribute to the existing literature on the consequences of judicial elections a detailed examination of the channels through which judicial elections affect sentencing. The evidence we present points to the importance of judges' behavioral responses to political pressure—and

[42] We also examine whether the magnitude of upward deviation outside the guidelines range differs over the political cycle, conditional on a sentence being above the guidelines range. Specifically, we estimate equation (1) using the difference between a case's sentence and the high end of the guidelines range as the outcome variable. We find suggestive evidence that deviations actually get larger toward the end of the political cycle and certainly do not offset the increased probability of upward deviations.

[43] See, for example, Shavell (2007) for a theoretical discussion of the optimal degree of discretion that should be granted to judges. Legislatures may also grant judges discretion simply because judges prefer to have discretion (see Boylan, 2004, and Posner, 1993).

[44] This constraint could affect sentencing decisions for cases other than those that result in upward deviations, but we believe that the exercise is still informative regarding the importance of upward deviations to our findings.

[45] Results for trials alone are very similar. See the online appendix, table A12.

specifically their use of discretion to go beyond sentencing guidelines ranges.

These results inform the debate on whether judges should be elected or appointed. We present evidence that the most commonly used method to retain judges, nonpartisan elections, generates different sentences for very similar crimes across a judge's political cycle. The results also highlight a potentially important interaction between the degree of discretion allowed to judges and the influence of politics on their behavior. We cannot say whether social welfare would increase or decrease if judges were appointed or if judicial discretion were more constrained (though our results imply a major violation of horizontal equity), but we can quite definitively say that sentencing patterns would differ and that the variation in sentencing solely due to political pressure would be diminished. More generally, these results contribute to the large but unsettled literature on the effects of elections on public servants' behavior.

Our results suggest several avenues for future work. Most basically, examining sentencing in other states and across longer time periods would test the generality of our findings. Further work should also consider the interaction between constraints on judicial discretion and the effects of political pressure: variation exists both across states and over time in judges' ability to deviate above guidelines ranges, and whether political cycles are muted when judges face tighter constraints is an open question. It is also important to study whether sentencing cycles disproportionately affect specific classes of individuals or are associated with specific classes of judges. Examination of the latter might shed light on how effectively judges' sentencing cycles deter the entry of competitors.

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