Supplementary Tables for Online Publication: Impact of Judicial Elections in the Sentencing of Black Crime

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A Kansas Background

A.1 Partisan versus Retention Districts

Table A1 shows partisan and retention districts are balanced across a wide range of variables. The means are shown with and without Kansas City because Kansas City's racial composition is notably different from the rest of the state (nearly 30% of Kansas City's residents are black - the next highest district is 9.5%). Excluding Kansas City, partisan and retention districts have similar shares of black, white, and Hispanic residents. Partisan districts are no more urban than retention districts. Gender, age, and population are more or less balanced across the two types of districts. There are, however, some differences in educational attainment. For example, residents in partisan districts are associated with less educational attainment than those in retention districts. Consonant with the differences in education, residents in partisan districts are more likely to be receiving food stamps and have lower home values. However, family income and political partisanship is balanced across the two types of districts.

The history of sorting into either selection method began in the 1950's. In 1956, political scandal erupted when the exiting Governor exploited a loophole to ensure that the Chief Justice of the Supreme Court would not be appointed by the incoming governor of the opposing political party. Two years after this "Triple Play of 1956" scandal, Kansas decided to give districts the option of either using retention elections or maintaining partisan elections. The issue can be placed on the general election ballot conditional on securing a petition signed by more than 5% of the district's voters from the previous election. District selection methods have been stable since 1984, although in 1986, two districts tried to switch from retention to partisan elections but failed.

Table A1: Descriptive Statistics of Partisan vs Retention Districts

	All Districts			Excluding Kansas City		
Demographic Composition (Shares)	Partisan	Retention	P vs. R	Partisan	Retention	P vs. R
Black	0.084	0.038	0.047*	0.052	0.038	0.015
	(0.018)	(0.015)	(0.023)	(0.010)	(0.008)	(0.013)
White	0.816	0.893	-0.076**	0.854	0.893	-0.039
	(0.025)	(0.021)	(0.032)	(0.019)	(0.015)	(0.024)
Hispanic	0.093	0.053	0.039	0.082	0.053	0.028
	(0.020)	(0.017)	(0.026)	(0.021)	(0.017)	(0.027)
Rural	0.274	0.295	-0.021	0.309	0.295	0.014
	(0.068)	(0.058)	(0.090)	(0.073)	(0.058)	(0.093)
High School Dropouts	0.111	0.071	0.040***	0.102	0.071	0.030***
	(0.008)	(0.007)	(0.011)	(0.007)	(0.006)	(0.009)
High School Graduates	0.314	0.286	0.028	0.309	0.286	0.024
	(0.019)	(0.016)	(0.025)	(0.021)	(0.017)	(0.027)
Some College but No Degree	0.25	0.242	0.008	0.255	0.242	0.013**
	(0.004)	(0.004)	(0.006)	(0.004)	(0.003)	(0.005)
Associates Degree	0.058	0.058	0.000	0.058	0.058	0.000
	(0.003)	(0.002)	(0.004)	(0.003)	(0.003)	(0.004)
College Graduates	0.137	0.195	-0.059**	0.146	0.195	-0.049*
	(0.020)	(0.017)	(0.026)	(0.021)	(0.017)	(0.027)
More than College	0.063	0.106	-0.043***	0.066	0.106	-0.040**
	(0.011)	(0.009)	(0.015)	(0.012)	(0.010)	(0.015)
Median Age	35.234	35.15	0.084	35.676	35.15	0.526
	(0.985)	(0.843)	(1.297)	(1.057)	(0.839)	(1.350)
Male	0.495	0.494	0.001	0.496	0.494	0.002
	(0.003)	(0.002)	(0.004)	(0.003)	(0.002)	(0.004)
Population	224,955.98	189,779.54	35,176.444	235,788.96	189,779.54	46,009.419
	(51,135.524)	(43,718.247)	(67,276.496)	(55,816.554)	(44,278.406)	(71,246.508)
Economic Characteristics						
Fraction Receiving Food Stamps	0.053	0.032	0.021***	0.050	0.032	0.018***
	(0.489)	(0.418)	(0.644)	(0.515)	(0.408)	(0.657)
Median Family Income ($)	49,809.00	49,552.80	256.199	48,534.54	49,552.80	-1,018.261
	(2,086.183)	(1,783.579)	(2,744.688)	(2,195.091)	(1,741.332)	(2,801.903)
Median Home Value ($)	68,730.99	97,472.54	-28,741.553**	71,061.70	97,472.54	-26,410.836**
	(8,602.541)	(7,354.731)	(11,317.941)	(9,361.117)	(7,426.029)	(11,948.909)
Political Affiliation						
Share of Registered Voters: Democrats	0.315	0.256	0.059*	0.278	0.256	0.022
	(0.022)	(0.019)	(0.029)	(0.015)	(0.012)	(0.019)
Share of Registered Voters: Republicans	0.431	0.461	-0.030	0.471	0.461	0.010
	(0.025)	(0.021)	(0.033)	(0.018)	(0.014)	(0.023)

Notes: N=31. All means are weighted by the district's total population. Data source is the *NHGIS*.

A.2 Descriptive Statistics on Election Outcomes

In partisan elections, winning the election is a high probability event but less so in comparison with retention elections. In nearly 9% of partisan elections, the incumbent will face a challenger and conditional on running in a contested election, the probability of winning is only 66%. In addition, the margin of victory is much more modest in partisan elections. For example, in the median election the difference in vote share between the winner and loser is 13% versus 25% in partisan and retention elections, respectively. These descriptive patterns are broadly consistent with the intuition that, in Kansas, retention elections insulate judges from the political process.

Table A2: Descriptive Statistics on Judicial Elections

	Partisan Election			Retention Election
Conditional on Incumbent Running:	Primary	General	All Elections	General
Probability of Contested Election	0.107	0.067	0.087	0.000
Conditional on Contested Election:				
Probability of Incumbent Win	0.650	0.667	0.656	
Average Margin of Victory	0.190	0.175	0.182	
Distribution of Margin of Victory:				
5th Percentile	0.005	0.003	0.004	0.135
10th	0.015	0.065	0.022	0.158
25th	0.069	0.075	0.075	0.207
50th	0.124	0.130	0.130	0.246
75th	0.288	0.190	0.261	0.279
90th	0.448	0.356	0.406	0.304
95th	0.493	0.406	0.475	0.325
Number of Elections	247	221	468	253
Probability of Incumbent Running	0.754	0.811	0.781	0.838

Notes: Kansas election results can be found online at the Kansas Secretary of State website. These statistics are computed using results from the 1996-2010 elections.

B Additional Descriptive Statistics

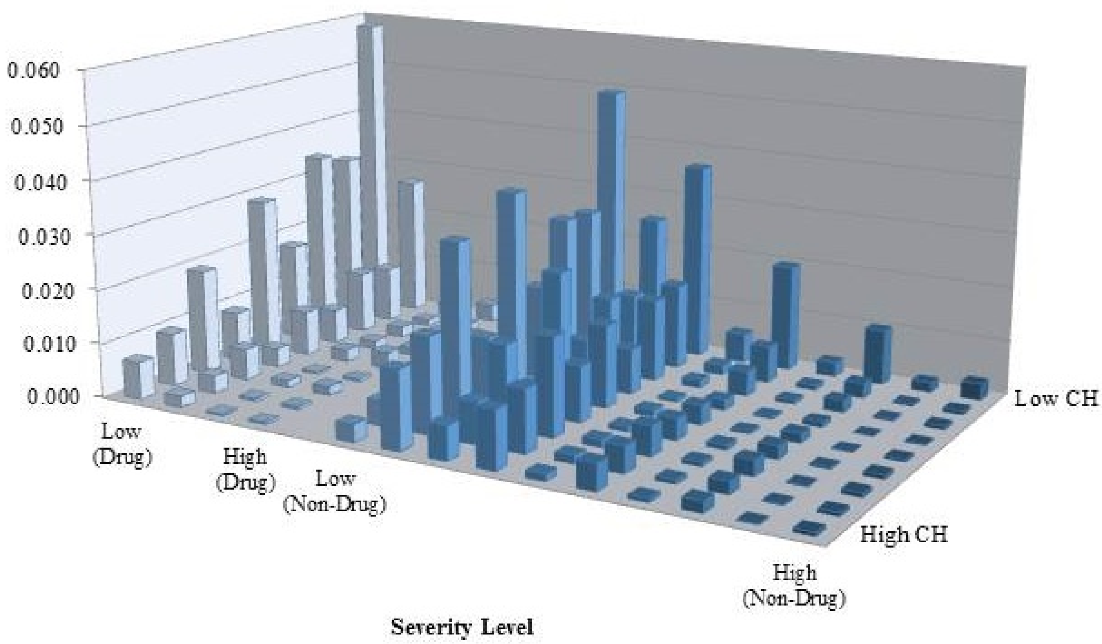
B.1 Distribution of Felons Across Sentencing Grid

Figure B1 shows the distribution of cases across the sentencing grid. High CH refers to the highest criminal history (felons with 3+ person felonies) and Low CH refers to the lowest (felons with either 1 misdemeanor or no prior record). The figure shows that the distribution is highly non-uniform across the sentencing grid. Crimes are disproportionately low-severity and this is true across both drug and non-drug related crimes.

B.2 Incarceration Rates Across Sentencing Grid

Table B1 shows overall incarceration rates across the sentencing grid. The table shows that the cross-partial derivative of incarceration with respect to severity and criminal history is negative. For example, going from no record to 3+ non-person felonies raises the incarceration rate by 0.27 for the lowest severity crime in comparison with 0.06 for the most severe crime. This reflects the fact that incarceration rates are already extremely high for the most severe crimes as well as for felons with extensive prior records. This is clear when comparing incarceration rates across different criminal history levels for the most severe crimes. Going across the top row, the incarceration rate falls but is still 0.90 for felons with no prior record. This implies that felons who commit severe crimes are almost certain to be incarcerated regardless of criminal history.

Figure B1: Distribution of Cases Across the Grid



Notes: High CH refers to the highest criminal history (felons with 3+ person felonies) and Low CH refers to the lowest (felons with either 1 misdemeanor or no prior record).

Table B1: Incarceration Rates Across the Sentencing Grid

Non-Drug Offenses	A	B	C	D	E	F	G	H	I
Severity Level	3+ Person Felonies	2 Person	1 Person+1 Non-Person	1 Person	3+ Non-Person	2 Non-Person	1 Non-Person	2+ Misde-meanors	1 Misde-meanor or No Record
1 (Most Severe)	1.00	1.00	0.99	0.97	0.96	0.95	0.96	0.94	0.90
2	1.00	1.00	1.00	1.00	0.96	1.00	0.98	1.00	0.90
3	0.96	0.88	0.92	0.87	0.88	0.88	0.81	0.77	0.76
4	0.95	0.91	0.78	0.76	0.81	0.76	0.71	0.66	0.77
5	0.89	0.84	0.76	0.67	0.69	0.65	0.54	0.32	0.31
6	0.91	0.78	0.76	0.68	0.74	0.64	0.31	0.11	0.08
7	0.82	0.68	0.29	0.13	0.26	0.16	0.10	0.05	0.04
8	0.79	0.65	0.32	0.17	0.25	0.14	0.09	0.05	0.03
9	0.76	0.64	0.25	0.09	0.24	0.12	0.07	0.01	0.02
10 (Least Severe)	0.78	0.67	0.33	0.15	0.32	0.19	0.13	0.07	0.05
Drug Offenses									
1 (Most Severe)	0.89	0.85	0.87	0.87	0.81	0.76	0.71	0.59	0.58
2	0.80	0.74	0.69	0.81	0.79	0.75	0.62	0.55	0.45
3	0.75	0.60	0.63	0.36	0.41	0.31	0.25	0.13	0.11
4 (Least Severe)	0.62	0.51	0.43	0.23	0.20	0.13	0.04	0.01	0.01

Notes: In the grey and clear boxes, the presumptive sentence is probation and prison, respectively. Blue boxes are “Border Box” cells in which the judge can issue a sentence subject to the availability of an appropriate rehabilitation program.

C Case Shifting

C.1 Case Shifting

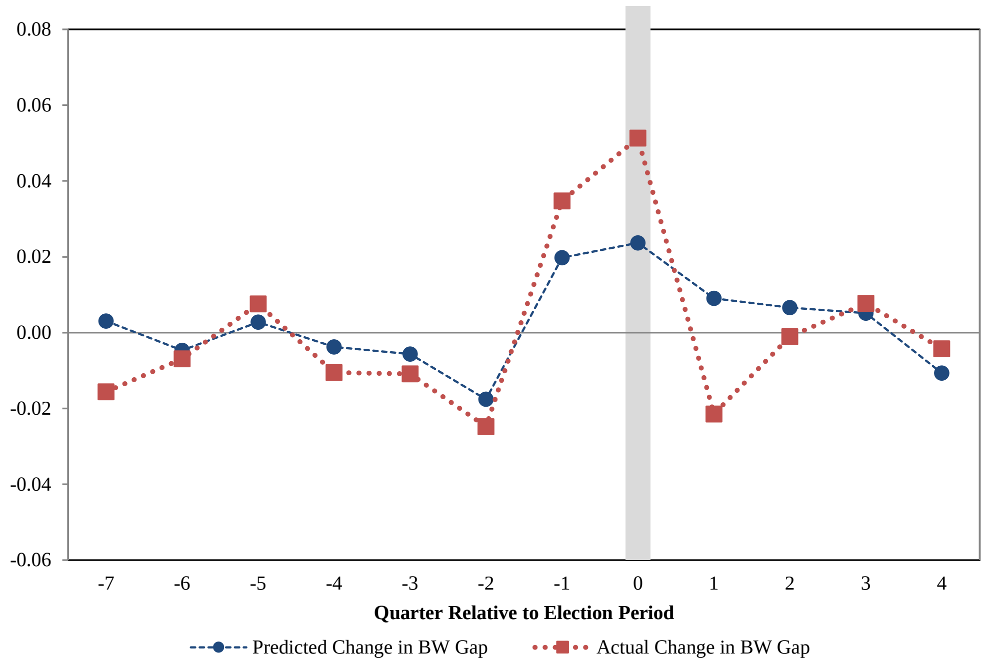
Figure D3 plots the actual and predicted change in the black-white incarceration gap across the election cycle. The actual change is based on a regression of incarceration on the timing indicators, race-by-timing interactions, judge and year fixed effects. The predicted change is based on the following two step procedure. First, I regress incarceration on all of the observable case facts and retrieve the predicted values. Second, I regress predicted incarceration on the timing indicators, race-by-timing interactions, judge and year fixed effects. The parameters of this regression describes what we would expect the change in the black-white incarceration gap to be based on the changes in observable characteristics. If the change in case facts fully explains the increase in the black-white gap, then the graphs of the actual and predicted change should lie atop one another. However, as shown in the plots, the actual and predicted changes deviate considerably in the quarters just before, during, and after the general election date. This is interesting because in all other periods, the actual and predicted changes track one another fairly closely. This figure highlights the fact that the change in observables can only explain roughly 50% of the increase in the black-white incarceration gap in the quarter prior to the general election date. It is interesting that in the quarter immediately following the general election date, the actual change is less than what we would predict based on observables. This is perhaps suggestive of judges compensating for their increased severity prior to the election with increased leniency ex-post. However, this deviation is not precisely estimated.

In Table C1, I show estimates of $Black_i * D^0_{it}$ from a series of models that regress a case fact on the timing indicators, race-by-timing interactions, judge and year fixed effects, and the set of case characteristics (excluding the dependent variable). This estimate conveys the change in the black-white gap along different margins in the final 6 months of the election cycle. For most of the case facts, the change in the black-white gap is fairly small and not statistically different from zero. However, there are two margins along which black offenders are worse than whites during the election period - total counts and special rule violations. On the one hand, the fact that two case facts show racial imbalance across the election cycle may not be too surprising given the sheer number of case facts that are tested. On the other hand, the fact that black offenders are associated with more counts and special rules violations in the final 6 months of the election cycle is worrisome. The next two sections will show results from decomposition methods that will quantify the extent to which the increase in the black-white incarceration gap can be explained by changes in case characteristics.

C.2 Oaxaca-Blinder Decomposition

In this section, we will conduct Oaxaca-Blinder style decompositions that parse the increase in the black incarceration rate during the election period into a portion attributable to changes in case facts (e.g. differences in X's or covariates) and a portion attributable to changes in behavior (e.g. differences in the β's or coefficients) (Oaxaca and Ransom (1994)). This exercise differs from the regression based approach shown in the paper because it allows the coefficients associated with the elements of the crime to vary across the electoral cycle.

Figure C1: Actual versus Predicted Change in Black-White Gap



Notes: The actual change is based on a regression of incarceration on the timing indicators, race-by-timing interactions, judge and year fixed effects. The predicted change is based on the following two step procedure. First, I regress incarceration on all of the observable case facts and retrieve the predicted values. Second, I regress predicted incarceration on the timing indicators, race-by-timing interactions, judge and year fixed effects. This figure plots the estimates associated with the race-by-timing interactions. The second year of the cycle serves as the baseline period. The election cycle is partitioned into 3-month intervals.

Thus, the Oaxaca-Blinder results may diverge from the previous regression based results depending on degree to which the coefficients vary in the election period.

To operationalize the decomposition, I first restrict the sample to black offenders in partisan districts. Thus, this analysis focuses on the change in the black incarceration rate rather than the change in the black-white incarceration gap. The election cycle for judges is four years long, and for this exercise, I partition the cycle into just two intervals; the first interval is the final 6 months of the cycle leading up to the election day (e.g. Election Period) and the second interval is all other months (e.g. Pre-Election Period). We can decompose the increase in the black incarceration rate in the final 6 months of the cycle in partisan districts as follows:

$$\bar{Y}_E - \bar{Y}_P = \hat{\beta}_E(\bar{X}_E - \bar{X}_P) + \bar{X}_P(\hat{\beta}_E - \hat{\beta}_P) \tag{1}$$

$$\bar{Y}_E - \bar{Y}_P = \hat{\beta}_P(\bar{X}_E - \bar{X}_P) + \bar{X}_E(\hat{\beta}_E - \hat{\beta}_P) \tag{2}$$

where $\bar{Y}_E$ denotes the black incarceration rate in the election period, which is defined as the final 6 months of the cycle, and $\bar{Y}_P$ denotes the black incarceration rate in the pre-election period, which is defined as all other months. The $\bar{X}_E$ and $\bar{X}_P$ terms denote the means of a

Table C1: Election Cycle Effects on BW Gap in Case Facts

Covariates:	Estimate of $Black_i * D^0_{it}$	Standard Error	Mean (White Felons)	Relative Change
Age	0.104	0.165	31.328	0.003
Female	-0.010	0.009	0.205	-0.049
Private Counsel	-0.003	0.011	0.245	-0.012
Person Crime	-0.006	0.006	0.271	-0.022
Plea Status	-0.009	0.010	0.962	-0.009
Severity (Non-Drug Crime)	0.050	0.116	3.282	0.015
Severity (Drug Crime)	0.001	0.053	1.463	0.001
Criminal History (Non-Drug Crime)	-0.012	0.091	3.976	-0.003
Criminal History (Drug Crime)	0.085	0.103	3.313	0.026
Drug Crime	-0.013	0.010	0.331	-0.039
Presumptive Sentence Length (in months)	2.283	1.591	24.016	0.095
Objection to Criminal History	0.005	0.005	0.043	0.117
Total Counts	0.042***	0.010	1.295	0.032
Special Rule Violation	0.032***	0.010	0.272	0.118
Type of Special Rule Violation:				
Person Felony Committed with a Firearm	0.014***	0.003	0.013	1.116
Aggravated Battery Law Enforcement Officer	0.001	0.001	0.001	1.611
Aggravated Assault (Law Enforcement Officer)	-0.001	0.001	0.002	-0.590
Crime Committed for Benefit of Criminal Gang	-0.001*	0.001	0.000	6.447
Felony DUI	-0.001	0.000	0.001	-1.842
Felony Domestic Battery	-0.000	0.000	0.000	0.000
Crime Committed While on Probation or Parole	0.011	0.009	0.154	0.071
Persistent Sex Offender	0.002	0.001	0.003	0.723
Crime Committed While on Felony Bond	-0.003	0.004	0.040	-0.075
Other	0.010**	0.005	0.059	0.169

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$. n=119,074. The election cycle is partitioned into eight 6-month intervals. These regressions control for the usual set of case facts excluding the case fact that is the dependent variable. Estimates of $Black_i * D^0_{it}$ are presented. Standard errors are clustered at the district level.

vector of case facts during the election period and the pre-election period, respectively. The case facts include indicators for age, gender, whether the offender attained private counsel, plea status, an indicator for drug versus non-drug crime, the presumptive sentence length, interactions between presumptive sentence length and drug crime, an indicator for person crime, whether a special rule has been violated, and total number of counts. The $\bar{X}_E - \bar{X}_P$ is the key term of interest since the hypothesis that we wish to explore is whether the increase in black incarceration is due to changes in case facts rather than changes in judicial behavior and the $\bar{X}_E - \bar{X}_P$ term represents the change attributable to case facts. Note that these decompositions are not unique. In equations (1) and (2), the change in case facts is weighted by the coefficients $\hat{\beta}_E$ and $\hat{\beta}_P$, respectively. The $\hat{\beta}_E$ and $\hat{\beta}_P$ terms denote the coefficients associated with the election and pre-election periods, respectively. Thus, the decompositions depend on which coefficients are used to weight the differences in the X's. For example, if judges sentence person crimes more severely in the final 6 months of the cycle, and the election period coefficients are used to evaluate the portion of the change due to covariates, then more of the increase in the black-white gap will be deemed "explained" than if the pre-election coefficients are used.

Table C2 shows the results. Prior to the election period, the incarceration rate for black offenders is 0.322 and increases by roughly 4.1 percentage points to 0.363 in the final 6 months of the election cycle. This actual increase is shown in the first column of the table. Columns (2) and (3) decompose the increase in the black incarceration rate into a portion explained by the difference in case facts and a portion explained by the difference in coefficients. The difference in case facts is weighted by the election period coefficients.

Table C2: Oaxaca-Blinder Decomposition

Dep Var: Incarceration				
	Increase Due to:		Increase Due to:	
Actual Increase	Covariates	Coefficients	Covariates	Coefficients
$\bar{Y}_E - \bar{Y}_P$	$\hat{\beta}_E(\bar{X}_E - \bar{X}_P)$	$\bar{X}_P(\hat{\beta}_E - \hat{\beta}_P)$	$\hat{\beta}_P(\bar{X}_E - \bar{X}_P)$	$\bar{X}_E(\hat{\beta}_E - \hat{\beta}_P)$
0.041	0.021	0.020	0.022	0.019
(0.012)	(0.006)	(0.010)	(0.007)	(0.010)
Share Explained:	0.512	0.488	0.536	0.464

Notes: This decomposition uses cases involving black offenders that are sentenced in partisan districts. The election cycle is divided into two periods: the last 6 months of the cycle and all other periods. The covariates include includes race, gender, age, indicator for whether crime violates special rule, is a property vs. person crime, whether the defendant obtained private counsel, plead, total counts, presumptive sentence length, presumptive sentence length-by-drug interaction, and drug offense.

The results in column (2) imply that roughly 2.1 of the 4.1 percentage point increase in the black incarceration rate can be attributed to the fact that black offenders are associated with different case characteristics in the election period. Column (3) shows that the remaining 2 percentage point increase in the black incarceration rate cannot be explained by changes in case facts. Columns (4) and (5) shows results from the decomposition that weights the difference in covariates using the pre-election period coefficients. The results are qualitatively similar. Roughly 54% of the increase in the black incarceration rate is explained by case facts whereas the remaining 46% remains unexplained. Overall, this analysis implies that roughly 50% of the increase in black incarceration rates cannot be explained by case shifting or observable changes in covariates across the election cycle. It is worth noting that these findings are consistent with the results from our regression based approach.

C.3 Semi-Parametric Decomposition

In this section, I assess how much of the increase in the black-white incarceration gap in the election period is due to changes in case characteristics using a semi-parametric approach as in DiNardo et al. (1996) (henceforth DFL). A potential criticism of both the OLS and Oaxaca-Blinder approach is that these techniques presume a linear relationship between the conditional expectation function and the covariates. There are studies that find the Oaxaca-Blinder decomposition can be sensitive to this restriction (Barsky et al. (2002)), and thus, using the DFL approach will be provide reassurance that the residual increase in the black-white incarceration gap in the election period is not driven solely by functional form assumptions.

The DFL procedure will yield estimates of counterfactual rates; specifically, we can estimate how the black-white incarceration gap would have changed if black offenders in the election period had the same characteristics as the black offenders in the pre-election period. We can also estimate the reverse - how the black-white incarceration gap would have changed if black offenders in the pre-election period had the same characteristics as black offenders in the election period. Thus, like the Oaxaca-Blinder decomposition, this approach is not unique and we will compute estimates of both counterfactual incarceration rates. To operationalize this technique, we need to construct re-weighting functions that will allow us to estimate the counterfactual distributions. While details can be found in DiNardo et al. (1996), we provide a brief overview of this procedure next.

Formally, consider an observation in our dataset represented by the vector (s, x, e, b), where s is the sentencing outcome (incarceration or not), x is a vector of case characteristics, e is an indicator for whether the felon is sentenced in the election period (e.g. the final 6 months of the election cycle), and b indicates whether the offender is black. The observed joint distribution of these data is given by $f(s, x, e, b)$. The actual black incarceration rate can be obtained by integrating the product of the conditional distribution and the period-specific covariate distribution over the support of x, Ω_x.

$$f(s|e, b) = \int_{\Omega_x} f(s|x, e, b) f(x|e, b) dx$$

As discussed above, we are interested in estimating counterfactual incarceration rates. Let s_0 and s_1 denote the potential sentencing outcome that an offender would receive if sentenced in the pre-election and election period, respectively. Then $f(s_0|e = 1, b)$ denotes the counterfactual incarceration rate that we would observe for black offenders in the election period (e.g. $e = 1$) if black offenders in the election period were sentenced in the pre-election period instead. Similarly, $f(s_1|e = 0, b)$ denotes the counterfactual incarceration rate for black offenders in the pre-election period (e.g. $e = 0$) that we would observe if black offenders in the pre-election period were sentenced in the election period instead. While these counterfactual incarceration rates are unobserved, they can be estimated by constructing the appropriate re-weighting function. For example, $f(s_0|e = 1, b)$ is given by:

$$\begin{aligned} f(s_0|e = 1, b) &= \int_{\Omega_x} f(s|x, e = 1, b) f(x|e = 0, b) dx \\ &= \int_{\Omega_x} f(s|x, e = 1, b) \psi(x) f(x|e = 1, b) dx \end{aligned}$$

where $\psi(x) \equiv \frac{f(x|e=0,b)}{f(x|e=1,b)}$. After applying Bayes Rule, we can re-write the weighting function as $\psi(x) = \frac{P(e=0|x,b)}{P(e=1|x,b)} \frac{P(e=1)}{P(e=0)}$. This shows that in order to obtain the counterfactual incarceration rate, $f(s_0|e = 1, b)$, we re-weight covariates such that the covariates associated with a higher relative likelihood of being observed in the pre-election period receive more weight. This process equalizes the distribution of covariates such that black offenders in the election period has the same distribution of covariates as those black offenders in the pre-election period. Note that if x is discrete, then the weighting function can be computed non-parametrically by directly computing the relative likelihood in each covariate cell. Otherwise the weighting function can be estimated using a logit or probit. In this exercise, I use a probit to construct the weighting functions. These weighting functions can then be used to assess the change in the black-white gap across the election cycle.

Table C3 shows the results. In Panel A, I show the increase in the black-white incar-

Table C3: Semi-Parametric Decomposition of Increase in BW Gap

Panel A: Re-weight Pre-Election Period Black Offenders

	(1)	(2)	(3)	(4)	(5)
$Black * D^0_{it}$	0.045***	0.032***	0.026***	0.025***	0.023***
	(0.007)	(0.008)	(0.008)	(0.007)	(0.007)
Share Explained		0.288	0.422	0.444	0.488

Panel B: Re-weight Election Period Black Offenders

	(1)	(2)	(3)	(4)	(5)
$Black * D^0_{it}$	0.045***	0.028***	0.021**	0.020***	0.017**
	(0.007)	(0.008)	(0.007)	(0.007)	(0.006)
Share Explained		0.378	0.533	0.556	0.622
Re-weighting Functions:					
Sentencing Cells	N	Y	Y	Y	Y
Special Rule	N	N	Y	Y	Y
Case Facts	N	N	N	Y	Y
Demographics	N	N	N	N	Y

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. n=119,074. The cycle is partitioned into 6-month intervals. The covariates included in the weighting functions are listed in the table. Case facts include an indicator for whether crime violates special rule, is a property vs. person crime, whether the defendant obtained private counsel, plead, total counts, and cell fixed effects. The demographic characteristics are age and gender. Standard errors are clustered at the district level.

ceration gap that re-weight pre-election period black offenders to have the same covariate distribution as those in the election period. Column (1) shows that the unadjusted increase in the black-white incarceration gap is 4.5 percentage points. In column (2), we compute the increase in the black-white incarceration gap but re-weight the black offenders in the pre-election period such that they have the same sentencing cell distribution as black offenders in the election period. This reduces the increase in the black-white incarceration gap to 3.2 percentage points or explains roughly 29% of the actual increase. Column (3) shows that the increase in the black-white gap would be 2.6 percentage points if pre-election period black offenders had the same distribution of sentencing cells and special rule violations as those in the election period. Thus, the differences in criminal severity, criminal history, and special rule violations explains roughly 42% of the observed election period effect. In columns (4) and (5), we control for additional case facts and demographic variables, but the estimates do not change substantially in comparison to those in column (3). On the whole, these results imply that differences in the covariate distribution among black offenders across the election cycle accounts for roughly half of the observed increase in the black-white gap in the election period.

Panel B shows similar results but re-weights black offenders in the election period to have the same covariate distribution as those in the pre-election period. These results are qualitatively similar to the results in Panel A. In column (2), the estimates show that when we re-weight black offenders in the election period to have the same sentencing cell distribution as black offenders in the pre-election period, the increase in the black-white gap falls to 2.8 percentage points. In column (3), when we re-weight black offenders to have the same distribution of sentencing cells and special rule violations, the increase in the black-white gap falls to 2.1 percentage points and remains stable thereafter. Overall, these results imply that the differences in the covariate distribution among black offenders across the election cycle accounts for roughly 60% of the increase in the black-white gap and roughly 40% remains unexplained.

All three approaches, the regression based approach, Oaxaca-Blinder Decomposition, and the semi-parametric decomposition yield results that are qualitatively similar. These results imply that changes in the observables across the election cycle (e.g. case shifting) explains between 40 and 60% of the increase in the black-white incarceration gap. This implies that a sizable fraction of the increase cannot be accounted for by the *observable* characteristics. Moreover, the magnitude of the unexplained increase is not trivial. Given that the incarceration rate for white offender is 0.237, the estimates imply that the likelihood of incarceration increases by roughly 7 to 10% in the election period.

D Additional Results and Robustness Checks

D.1 Selection versus Election Effects

It is possible that retention and partisan methods could encourage discriminatory sentencing through *selection* effects. In retention districts, this seems plausible because when filling a vacancy, a nominating committee consisting of local attorney and non-attorney constituents selects an initial pool of candidates from which the governor will appoint one. To the extent that the nominating committee selects on the candidate's racial preference, the appointment process could have substantial impact on the level of discriminatory sentencing.[1] In addition, Lim (2013) finds that partisan elections leads to higher rates of exit from the profession among judges with high levels of education. It is conceivable that partisan elections could have additional selection effects that affect racial disparity in sentencing as well. Because the empirical approach of this paper identifies changes rather than levels, the estimates may miss potentially salient selection effects in either retention or partisan districts.

I explore this possibility using regression models that focus on estimating the *level* of racial disparity in incarceration rates in retention and partisan districts prior to the final 6 months of the election cycle. If either retention or partisan elections yields a composition of

[1] This is consistent with Lim (2013) who finds that, in Kansas, retention districts select judges whose sentencing preferences are more congruent with voter preferences. However, an open question is whether the selection of judges is based on preferences for differential sentencing by race.

judges who are predisposed to sentencing black offenders more harshly, then we may expect to see racial sentencing disparities prior to the election period. The usual limitations with regression analysis apply. If we find evidence of a racial disparity in incarceration rates, it is possible that this result is explained by differences in unobservable characteristics. Otherwise, a zero black-white gap prior to the election period would imply that, on average, judges sentence black and white offenders evenly in the absence of reelection concerns.

Table D1 shows estimates from a regression of incarceration on an indicator for black, a black-by-partisan district interaction, district fixed effects, and the usual set of case facts using only cases that are sentenced prior to the final 6 months of the election cycle. The various columns reflect estimates from specifications that systematically add case facts to the conditioning set. The key paramters of interest are the coefficients on black and the black-by-partisan district interaction. These two parameters reflect the black-white incarceration gap in retention districts and the difference in the black-white gap between partisan and retention districts during the non-election period, respectively.

Table D1: Black-White Incarceration Gap Prior to the Election Period

Dep Var: Incarceration									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Black	0.080***	0.013**	0.007	0.008*	0.009**	0.009**	0.005	0.005	0.003
	(0.011)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Black*Partisan	-0.018	-0.011	-0.007	-0.005	-0.006	-0.006	-0.005	-0.005	-0.005
	(0.014)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Controls:									
District Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Sentencing Cells	N	Y	Y	Y	Y	Y	Y	Y	Y
Special Rule Violation	N	N	Y	Y	Y	Y	Y	Y	Y
Age Indicators	N	N	N	Y	Y	Y	Y	Y	Y
Gender	N	N	N	N	Y	Y	Y	Y	Y
Total Counts	N	N	N	N	N	Y	Y	Y	Y
Private Counsel	N	N	N	N	N	N	Y	Y	Y
Person Crime	N	N	N	N	N	N	N	Y	Y
Plea Status	N	N	N	N	N	N	N	N	Y

Notes: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$. n=104,767. This regression uses only cases sentenced prior to the final 6 months of the general election date. These regression estimates control for race, gender, age, special rule violations, person crime, private counsel, plea status, total counts, a set of indicator variables for each severity-by-criminal history cell, year effects, and district fixed effects. The incarceration rate for white offenders is 0.237. Standard errors are clustered at the district level.

Column (1) shows that there is a sizable black-white gap in both retention and partisan districts in the raw data. The likelihood of incarceration is 8 and 6.2 percentage points higher for black offenders in retention and partisan districts, respectively. However, column (2) shows that this racial disparity falls almost in its entirety when we control for the severity of the crime and the criminal history of the offender via the sentencing cell fixed effects. As we add even more case facts to the set of controls, the black-white gap eventually falls to 0.3 and -0.5 percentage points in retention and partisan districts, respectively. Neither of these estimates are statistically significant. It is rather interesting that case facts cannot explain the black-white gap during the last 6 months of the election cycle when reelection concerns are arguably high, but can fully explain the racial disparity in incarceration rates when reelection concerns are arguably low.

D.2 Graphical Results for Retention Districts

Recall that in partisan districts, there is no change in the black-white gap prior to final 6 months, a rise in the black-white gap in the final 6 months, and subsequent return to a zero black-white gap. These three striking features in partisan districts are not apparent in retention districts. As shown in Panel (a) of Figure D1, in retention districts, some of the estimates deviate notably from zero in period well before the final 6 months of the cycle. For example, the largest estimate is in period -3 (e.g. 3 quarters prior to the election period) when there is arguably diminutive reelection concerns. In addition, there is weak evidence of a rise in the black-white gap in the final 6 months as the estimated change at time 0 is negative. Panel (b) shows the analogous plot corresponding to a specification that partitions the election cycle into 1 month intervals. Overall, these patterns do not suggest that judges in retention districts increase sentencing severity in close proximity to the general election.

D.3 Graphical Results from Retiring Judges

Figure D2 shows estimated changes in the BW gap in partisan districts in Panel (a), and as a comparison, for retiring judges in Panel (b). The point estimates are estimated imprecisely, and thus, statistically speaking we cannot rule out an election cycle effect. However, the actual point estimates show no evidence of an increase in the BW gap that coincides with the general election during the last election cycle among retiring judges.

D.4 Robustness of Standard Errors

Table D2 examines how robust statistical significance is across different levels of the clustering variable. I try clustering at the judge, district-by-calendar year, judge-by-calendar year, district-by-year in election cycle, and judge-by-year in the election cycle levels. The highest p-value is 0.054 which is associated with clustering at the judge level. For all other levels, the p-values associated with the $Black_i * Dit^0$ estimate is equal to or below 0.05.

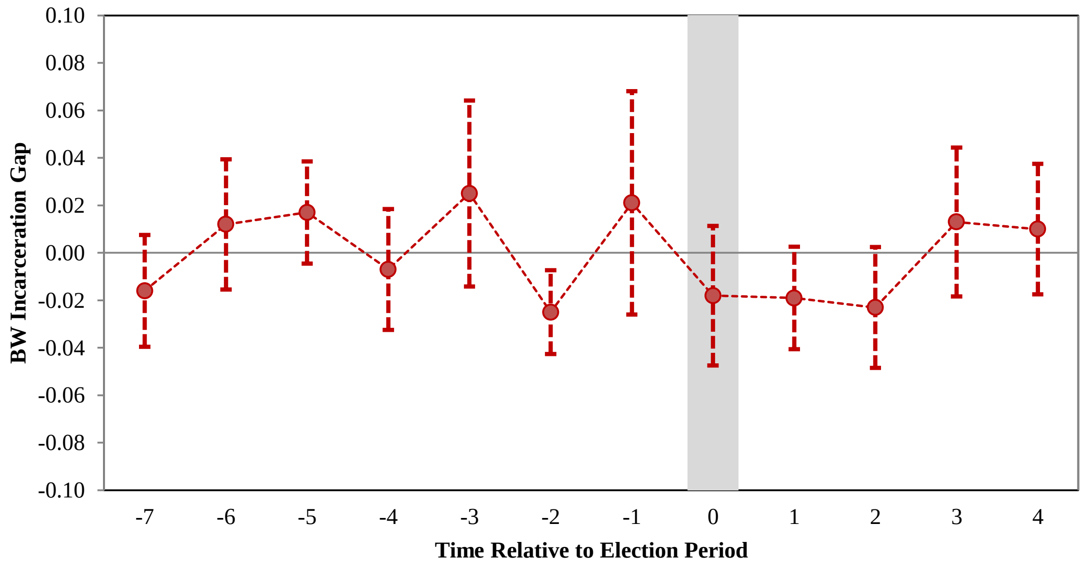
Table D2: Robustness to Different Cluster Variables

Dep Var: Indicator for Incarceration						
	(1)	(2)	(3)	(4)	(5)	(6)
$Black * D_{it}^0$	0.024***	0.024*	0.024**	0.024**	0.024**	0.024**
	(0.008)	(0.013)	(0.010)	(0.012)	(0.011)	(0.012)
	[0.006]	[0.054]	[0.012]	[0.036]	[0.022]	[0.050]
Cluster By:	District	Judge	District-by-Year	Judge-by-Year	District-by-Year in Election Cycle	Judge-by-Year in Election Cycle

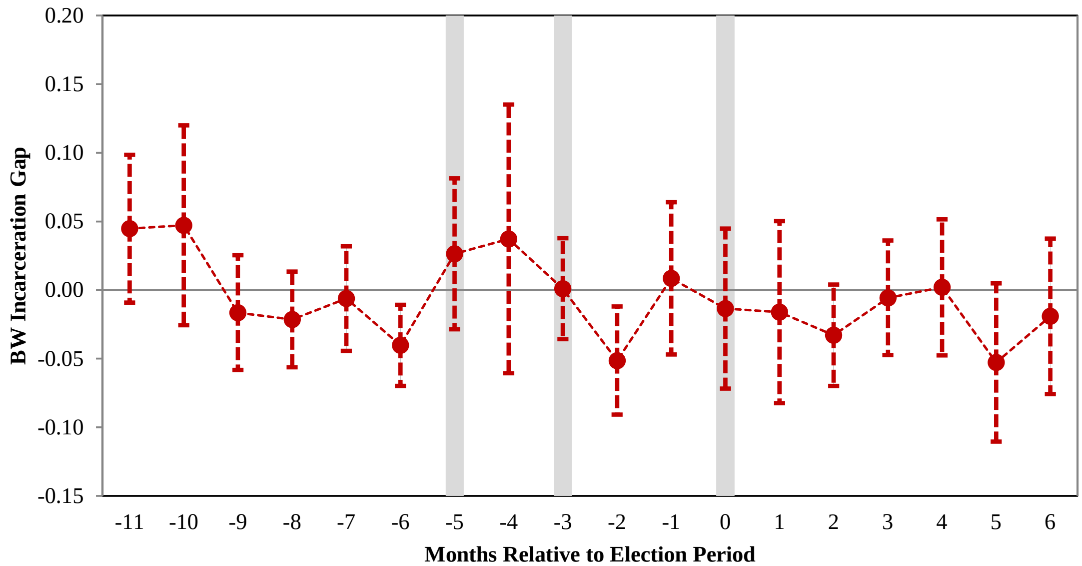
Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$. P-values are in brackets, standard errors are in parentheses. n=119,074. Column (1) shows the estimate from the main specification of the paper.

Figure D1: Election Cycle Effects for Retention Districts

(a) 3-Month Partition



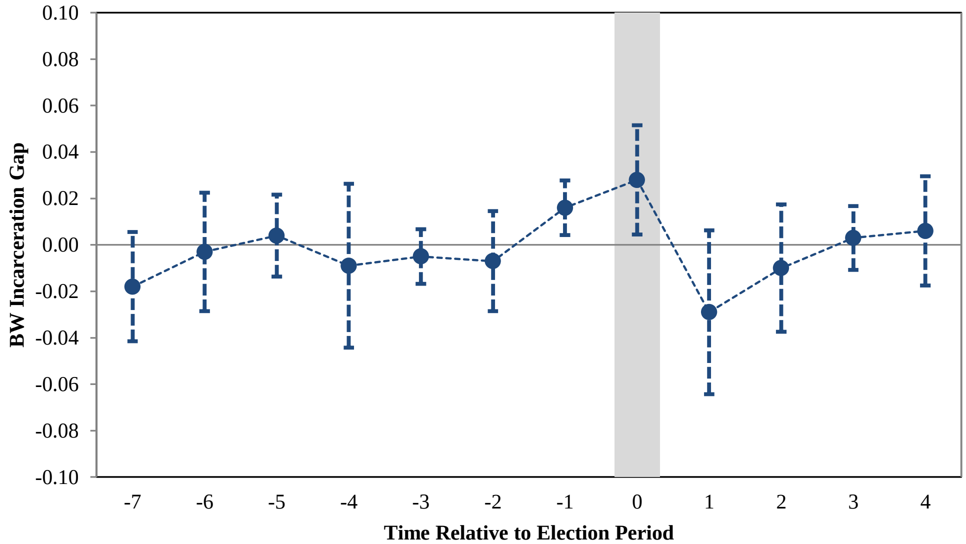
(b) 1-Month Partition



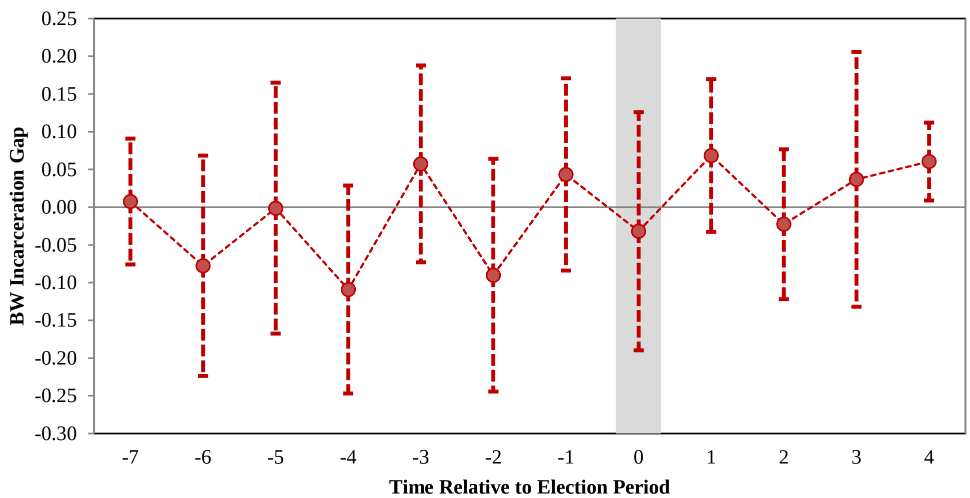
Notes: The election cycle is partitioned into 3-month intervals in Panel (a) and 1-month intervals in Panel (b). In Panel (b), the first, second, and third grey bars correspond to the month prior to the filing deadline, primary election date, and general election date, respectively. These regression estimates control for race, gender, age, special rule violations, person crime, private counsel, plea status, total counts, a set of indicator variables for each severity-by-criminal history cell, year effects, and judge fixed effects. Standard errors are clustered at the district level.

Figure D2: Election Cycle Effects Using a 3-Month Partition

(a) BW Gap in Partisan Districts



(b) BW Gap in Partisan Districts for Retiring Judges



Notes: The election cycle is partitioned into 3-month intervals. These regression estimates control for race, gender, age, special rule violations, person crime, private counsel, plea status, total counts, a set of indicator variables for each severity-by-criminal history cell, year effects, and judge fixed effects. Standard errors are clustered at the district level.

D.5 Change in Judicial Behavior Independent of Race

In this section, we examine the possibility that the increase in the black-white gap is due to a judicial response to reelection concerns that is independent of race. Instead, the main results may be explained by an increase in punitive sentencing towards crimes that happen to correlate with race. For example, suppose that voters have specially strong preference for punitive sentencing towards person crimes which include robbery, rape, aggravated arson, and battery. This implies that judges may have elevated incentive to issue harsh punishments towards person crimes in close proximity to the election. In Kansas, black offenders are 24% more likely to commit person crimes in comparison with whites. In this case, black incarceration rates may rise simply because black offenders disproportionately commit crimes that have high demand for incarceration rather than for reasons related to race.

Table D3: Robustness to Interactions Between Case Facts and Election Timing (Partisan Districts)

Dep Var: Incarceration										
Race-Specific Election Cycle Effects	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$Black * D_{it}^{-3}$	-0.014	-0.014	-0.015*	-0.014	-0.014*	-0.014	-0.014	-0.014*	-0.014	-0.014
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)	(0.008)	(0.009)	(0.009)
$Black * D_{it}^{-2}$	-0.000	-0.000	-0.000	-0.000	0.000	-0.000	-0.002	-0.000	-0.002	-0.002
	(0.011)	(0.012)	(0.012)	(0.011)	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)
$Black * D_{it}^{-1}$	-0.004	-0.004	-0.004	-0.004	-0.004	-0.004	-0.004	-0.004	-0.004	-0.004
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
$Black * D_{it}^{0}$	0.021**	0.021**	0.021**	0.021**	0.021**	0.021**	0.021*	0.021**	0.022**	0.020**
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.009)
Baseline Covariates	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Interactions between Timing Indicators and:										
Age		Y								Y
Gender			Y							Y
Counts				Y						Y
Special Rule Violation					Y					Y
Private Counsel						Y				Y
Person Crime							Y			Y
Plea Status								Y		Y
Presumptive Sentence Length-by-Drug Crime									Y	Y

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$. n=119,074. The election cycle is partitioned into eight 6-month periods. The baseline specification includes race, gender, age, special rule violation, person crime, private counsel, plea status, total counts, the presumptive sentence length, drug crime, interaction between presumptive sentence length and drug crime, year effects, and judge fixed effects. The incarceration rate for white offenders is 0.237. Standard errors are clustered at the district level.

I asses this hypothesis by systematically adding a set of interactions between the observable case facts and the timing indicators to our main specification. If the main results are driven by non-race related changes in sentencing behavior, then the increase in the black-white incarceration gap should be explained away as we include more and more of these interaction terms to the estimating equation. Table D3 presents the coefficients associated with the $Black_i * D^k_{it}$ terms. Column 1 shows the estimates from a regression that includes the usual set of covariates.[2] The remaining columns show estimates from models that systematically add interactions between the covariates and the set of timing indicators. The point estimate associated with $Black_i * D^0_{it}$ fall within a tight range between 2.0 and 2.2 percentage points and are all statistically significant at the 5% level. The stability across the specifications imply that the main results are not driven by a race-neutral change towards specific types of crime.

D.6 Outlier District & Additional Sensitivity Analysis

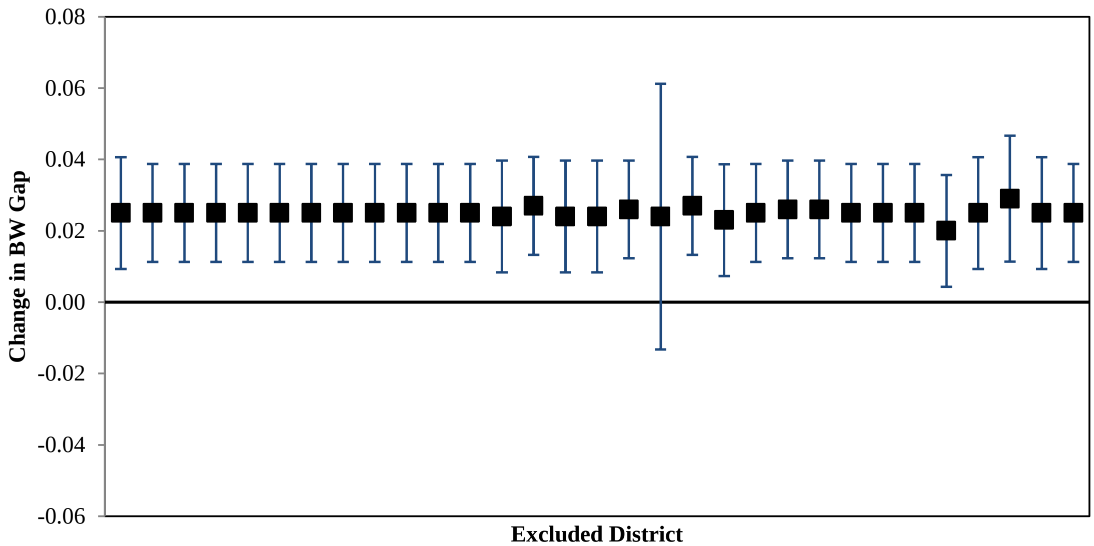
In this section, I conduct additional robustness checks to ensure that the main results are not driven by a peculiarity of the primary empirical model. To begin, I address the concern that the main results are explained by an outlier district. I re-run the main specification 31 times excluding 1 out of the 31 districts in each iteration. If the results are driven by a single judicial district, then the estimate should fall to zero when that district is excluded. Figure D3 plots the 31 estimates of $Black_i * D^0_{it}$ and their accompanying 95% confidence intervals. The point estimates are demonstrably stable ranging from 2 to 2.9 percentage points. The one estimate that is not statistically significant at the 5% level arises when I exclude Wichita. This is not surprising given that 20% of all felony cases are adjudicated in Wichita. Overall, these results suggest that a single district is unlikely to be driving the main results.

Table D4 shows results from additional specifications. Column (1) shows estimates from our main specification which controls for the severity of the crime and criminal history by including indicator variables for each cell of the sentencing grid. The remaining columns show estimates from specifications that allow for more or less flexibility in the relationship between incarceration with respect to severity and criminal history. For example, the specification in column (5) replaces the cell fixed effects with the presumptive sentence length and an interaction between presumptive sentence length and whether the offense is drug related.[3] This specification is more restrictive because it precludes non-linear effects of severity and criminal history on the likelihood of incarceration. In contrast, column (7) includes indicator variables for each type of offense in addition to controlling for cell fixed effects. This specification is the most flexible because it allows for offenses within a cell to have differential effect on the likelihood of incarceration. The estimates are reassuring because they demonstrate stability across these various modeling assumptions.

[2] This regression model controls for severity and criminal history by including the presumptive sentence length rather than cell fixed effects. I allow the presumptive sentence length to have a different effect for drug versus non-drug crimes since there are separate grids for drug and non-drug crimes.

[3] The interaction with whether the offense is drug related is motivated by the fact that there are separate grids for non-drug and drug related crimes.

Figure D3: Sensitivity of the BW Incarceration Gap to Outlier District



Notes: This graph plots the estimated increase in the BW Incarceration Gap in the last 6 months of the election cycle in partisan districts from 31 different regressions. Each regression excludes 1 of the 31 judicial districts.

Table D4: Additional Robustness Checks

Dep Var: Incarceration							
	Partisan Districts						
Election Cycle Effects	(1)	(2)	(3)	(4)	(5)	(6)	(7)
D_{it}^{-3}	0.003	-0.002	-0.000	0.002	-0.001	0.003	0.001
	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)
D_{it}^{-2}	0.005	0.003	0.002	0.004	0.004	0.004	0.006
	(0.003)	(0.005)	(0.003)	(0.004)	(0.005)	(0.004)	(0.004)
D_{it}^{-1}	0.006	0.002	0.007	0.006	0.002	0.005	0.005
	(0.007)	(0.009)	(0.006)	(0.007)	(0.007)	(0.007)	(0.008)
D_{it}^{0}	-0.006	-0.008	-0.005	-0.005	-0.005	-0.005	-0.007
	(0.009)	(0.011)	(0.007)	(0.009)	(0.009)	(0.010)	(0.010)
Race-Specific Election Cycle Effects							
$Black_i * D_{it}^{-3}$	-0.007	-0.010	-0.011	-0.010	-0.014	-0.007	-0.004
	(0.009)	(0.008)	(0.010)	(0.009)	(0.008)	(0.008)	(0.008)
$Black_i * D_{it}^{-2}$	0.002	0.001	-0.002	0.001	-0.000	0.004	0.000
	(0.007)	(0.010)	(0.011)	(0.007)	(0.011)	(0.006)	(0.006)
$Black_i * D_{it}^{-1}$	-0.003	-0.008	-0.007	-0.005	-0.004	-0.002	-0.002
	(0.007)	(0.007)	(0.008)	(0.007)	(0.007)	(0.006)	(0.008)
$Black_i * D_{it}^{0}$	0.024***	0.024**	0.023***	0.022**	0.021**	0.025***	0.025***
	(0.008)	(0.011)	(0.006)	(0.008)	(0.010)	(0.008)	(0.008)
Indicator for each Sentencing Cell	Y	N	N	N	N	Y	Y
Severity + Severity-by-Drug Effects	N	Y	N	Y	N	N	N
Criminal History + Criminal History-by-Drug Effects	N	N	Y	Y	N	N	N
Presumptive Sentence Length + Interaction with Drug Crime	N	N	N	N	Y	N	N
Indicators for Each Type of Special Rule Violation	N	N	N	N	N	Y	N
Indicators for Each Offense Type	N	N	N	N	N	N	Y

Notes: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$. n=119,074. The election cycle is partitioned into eight 6-month periods. The baseline specification includes race, gender, age, special rule violation, person crime, private counsel, plea status, total counts, the presumptive sentence length, drug crime, interaction between presumptive sentence length and drug crime, year effects, and judge fixed effects. The incarceration rate for white offenders is 0.237. Standard errors are clustered at the district level.

D.7 Heterogeneous Effects Across Districts

Table D5 shows more heterogeneous effects across different types of districts. Columns 1 and 2 show results separately for districts that have low and high fraction of black residents. Low and high districts are defined as to whether the district is below or above the median district. In low fraction black districts, the increase in black incarceration rates in the 6 months prior to election is roughly 3.4 times the increase in districts with high fraction black (7.9 vs. 2.3 percentage points). This difference is statistically significant at the 5% level. Columns 3 and 4 show results separately for rural versus urban districts. The NHGIS data provides the share of residents living in rural areas, which are defined as areas with less than 50,000 people. Low (high) districts are those whose fraction of residents in rural areas is below (above) the median district. The magnitudes of the point estimates suggest larger effects in more rural districts (5.6 vs. 2.7 percentage points). However, this difference is not statistically significant. Columns 5 and 6 show results separately by districts with a low versus high share of registered Republican voters. Again, low and high are defined as to whether or not the district is below or above the median district. The estimates show larger effects in districts that favor the Republican party, but the difference is not statistically significant.

Table D5: Additional Heterogeneous Effects

	Fraction Black		Fraction Rural		Fraction Repub	
Election Cycle Effects	Low	High	Low	High	Low	High
D_{it}^{-3}	0.003	0.002	0.000	0.005	0.005	-0.004
	(0.010)	(0.002)	(0.001)	(0.009)	(0.004)	(0.011)
D_{it}^{-2}	0.006	0.004	0.002	0.008	0.000	0.016**
	(0.006)	(0.003)	(0.002)	(0.005)	(0.002)	(0.006)
D_{it}^{-1}	0.000	0.010	0.008	0.005	0.005	0.009
	(0.007)	(0.013)	(0.013)	(0.007)	(0.009)	(0.010)
D_{it}^{0}	0.004	-0.013	-0.017	0.006	-0.014*	0.013
	(0.013)	(0.011)	(0.010)	(0.012)	(0.008)	(0.016)
Race-Specific Election Cycle Effects						
$Black * D_{it}^{-3}$	-0.014	-0.006	-0.001	-0.032	-0.009	-0.003
	(0.041)	(0.006)	(0.002)	(0.033)	(0.008)	(0.044)
$Black * D_{it}^{-2}$	-0.025	0.005	0.007	-0.014	0.005	0.003
	(0.020)	(0.008)	(0.009)	(0.015)	(0.008)	(0.017)
$Black * D_{it}^{-1}$	-0.010	-0.006	0.000	-0.029	-0.001	-0.011
	(0.037)	(0.010)	(0.007)	(0.028)	(0.006)	(0.036)
$Black * D_{it}^{0}$ (γ_0)	0.079***	0.023**	0.027***	0.056**	0.027***	0.063
	(0.026)	(0.010)	(0.008)	(0.027)	(0.008)	(0.043)
P-value of the following test:						
$\gamma_0^{low} = \gamma_0^{high}$	0.063		0.329		0.414	

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. n=119,074. The election cycle is partitioned into eight 6-month periods. The specification includes race, gender, age, special rule violation, person crime, private counsel, plea status, total counts, the presumptive sentence length, drug crime, interaction between presumptive sentence length and drug crime, year effects, and judge fixed effects. The incarceration rate for white offenders is 0.237. Standard errors are clustered at the district level.

Table D6: Election Cycle Effects from Logit and Probit Models

Dep Var: Indicator for Incarceration				
	Logit		Probit	
Race-Specific Election Cycle Effects	Retention	Partisan	Retention	Partisan
$Black_i * D_{it}^{-3}$	0.002	-0.005	0.003	-0.005
	(0.009)	(0.004)	(0.008)	(0.004)
$Black_i * D_{it}^{-2}$	0.009	0.002	0.009	0.001
	(0.009)	(0.004)	(0.009)	(0.004)
$Black_i * D_{it}^{-1}$	0.000	-0.002	0.004	-0.001
	(0.010)	(0.005)	(0.011)	(0.005)
$Black_i * D_{it}^{0}$	0.008	0.019***	0.009	0.019***
	(0.016)	(0.007)	(0.015)	(0.007)

Notes: n=119,074. The election cycle is partitioned into eight 6-month intervals. These regressions control for race, gender, age, special rule violations, person crime, private counsel, plea status, total counts, a set of indicator variables for each severity-by-criminal history cell, year effects, and judge fixed effects. Standard errors are clustered at the district level.

D.8 Probit and Logit

Table D6 shows the results from the main specification using logit and probit models instead of the linear probability models. The table shows that the marginal or incremental effects computed as the average derivative from Probit and Logit models yield qualitatively similar results. Both models estimate that the black-white gap increases by 1.9 percentage points in the last 6 months of the cycle. These estimates are statistically significant at 1% level. While the qualitative results are similar as the linear probability model, I present results from regression models because of the well-known incidental parameters problem associated with estimating non-linear models with a large number of fixed effects (Neyman and Scott (1948)).

D.9 Cases Sentenced Outside of the Grid

There are two types of crimes that are sentenced outside of the sentencing grid. First, *off-grid* crimes include capital murder, first degree murder, treason, terrorism, illegal use of weapons of mass destruction, and sex offenses involving victims less of 14 years of age. Second, *non-grid* crimes include felony DUI, 3rd or more conviction of felony domestic battery, and animal cruelty. Among all cases that are sentenced outside of the grid, 91% are 3rd or 4th convictions of felony DUI's. I exclude these cases because I thought it made more sense to focus the analysis on cases associated with a clear presumptive sentence. It seems possible that cases sentenced outside of the grid may be subject to a different decision calculus which would make it difficult to compare across crimes that are sentenced inside and outside of the grid.

In this section, I include crimes that are sentenced outside of the grid into the analysis. First, I examine whether there is evidence of an election cycle effect on the incidence of these types of crimes. Second, I reexamine whether the main results on incarceration change when we include these cases into the analysis. Table D7 shows these results. Columns (1) and (2) show the election cycle effects in retention and partisan districts, respectively. The usual set

Table D7: Robustness to Cases Sentenced Outside of the Grid

Dependent Variable:	Outside Grid		Incarceration	
	Retention	Partisan	Retention	Partisan
	(1)	(2)	(3)	(4)
D_{it}^{-3}	-0.003	0.003	-0.006	0.002
	(0.006)	(0.006)	(0.005)	(0.005)
D_{it}^{-2}	-0.003	-0.001	-0.010*	0.005
	(0.004)	(0.007)	(0.005)	(0.003)
D_{it}^{-1}	0.000	-0.012	0.004	0.005
	(0.004)	(0.009)	(0.005)	(0.006)
D_{it}^{0}	-0.000	-0.011	-0.005	-0.004
	(0.006)	(0.008)	(0.005)	(0.008)
$Black * D_{it}^{-3}$	0.012	0.006	0.001	-0.001
	(0.008)	(0.008)	(0.010)	(0.009)
$Black * D_{it}^{-2}$	0.011	0.005	0.009	0.001
	(0.008)	(0.006)	(0.009)	(0.008)
$Black * D_{it}^{-1}$	0.007*	0.002	0.004	-0.002
	(0.004)	(0.012)	(0.010)	(0.008)
$Black * D_{it}^{0}$	0.002	0.008	0.002	0.026***
	(0.006)	(0.007)	(0.012)	(0.007)

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$. n=119,074. The election cycle for district court judges is four years in Kansas. The cycle is partitioned into eight 6 month periods and D_{it}^{0} reflects the 6 months leading up to election day. The baseline specification includes race, gender, indicators for age, indicator for whether crime violates special rule, is a property vs. person crime, whether the defendant obtained private counsel, plead, total number of counts, indicators for severity-by-criminal history cells, year effects, and judge fixed effects. Standard errors are clustered at the district level.

of covariates are included in these regressions. Both columns show little evidence of a change in the likelihood of presiding over an off-grid or non-grid case in the election period for either white or black offenders. In columns (3) and (4), we show the main results from an analysis that includes cases that are sentenced off of the grid. Controls for whether or not the case is adjudicated off of the grid is added to the usual set of case facts. These estimates show the same qualitative pattern as the main results. In particular, in retention districts, we see no evidence of an increase in the likelihood of incarceration for either black or white offenders in the election period. However, in partisan districts, the black-white gap increases by 2.6 percentage points in the election period.

This analysis implies that excluding these types of cases does not affect the main results. This is not surprising because there is very little variation in incarceration among these types of cases. For example, the sentencing outcome for **all** cases involving an off-grid crime (e.g. capital murder, first degree murder, and etc.) is prison whereas nearly all cases involving non-grid crimes (e.g. felony DUI's, felony domestic battery) are sentenced to probation. Thus, these are not the marginal cases that are driving the main results.

D.10 Effects for Hispanic Felons

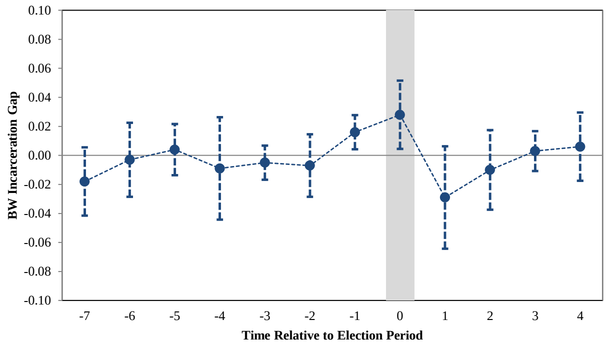
I would like to elaborate the main reasons for focusing on the black-white difference. First, this focus is grounded in numerous academic studies across a wide range of disciplines that find constituents have stronger preference for issuing severe punishments towards black offenders in comparison with whites. As noted in the paper, a particularly compelling piece of evidence comes from Anwar et al. (2012) who finds that all white juries are substantially more likely to convict black defendants in comparison with mixed race juries using quasi-random variation in the racial composition of jury pools. In general, the literature does not test whether there is similar preference towards Hispanic offenders. In some cases, this omission is due to the small sample size of Hispanic offenders or because Hispanics are not identifiable in the relevant data sets. However, even in studies in which the researchers design their own surveys or conduct mock trials, the preeminent question is whether constituents have stronger preferences for severe punishment towards black offenders.

One contributing factor may be the long and uniquely complicated history of interactions between African-Americans and criminal justice in the United States. Historians have detail how after the Civil War, slavery did not end but essentially took another form for newly emancipated blacks (Blackmon (2009)). Southern states passed new laws called the "Black Codes" that facilitated the detainment of African-Americans for petty crimes that did not warrant incarceration. Perhaps the most well known are vagrancy laws that allowed southerners to convict blacks for being unemployed. These convicts were then rented to plantations, mines, lumber camps, and other industrial firms for their labor. Weaver and Lerman (2010) argue that there was a similar reliance on the criminal justice to marginalize blacks following the passage of the Civil Rights Act of 1965. This literature highlights that the contentious relationship between African-Americans and the criminal justice system may be especially unique.

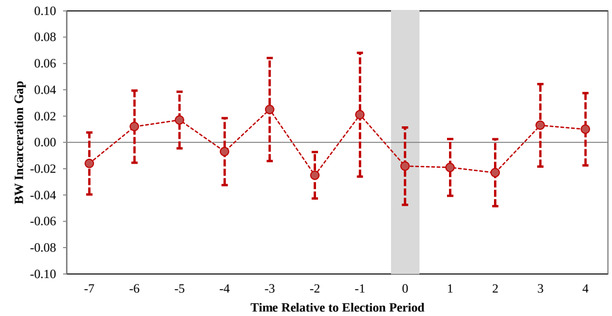
In present day, it remains exceedingly difficult to extricate issues of criminal justice from the black community. From both economists and academics in other fields, there is a coalescing sentiment that inequities in the criminal justice system are likely to play an influential role in driving black-white inequality more generally (Neal and Rick (2014)). Alexander (2012) argues explicitly that the system of mass incarceration is the reincarnation of the old Jim Crow. High profile examples of police brutality, from Freddie Gray, Michael Brown, and others, continue to fuel the popular narrative that the criminal justice system is skewed in a way that disproportionately affects black communities. Statistics that show gross racial disparities in incarceration rates further complicate the linkage between crime and race. For example, Bonczar and Beck (1997) finds that black males are twice as likely than Hispanics and six times as more likely than whites to ever be incarcerated in their lifetime. Kearney et al. (2014) finds that black males without a high school diploma have 70% chance of going to prison by the time they are 30 years of age. Muhammad (2010) argues that statistics such as these have contributed mightily towards the melding of black identity with notions of criminality.

Overall, the findings from academic studies, oft-cited statistics, historical origins, and

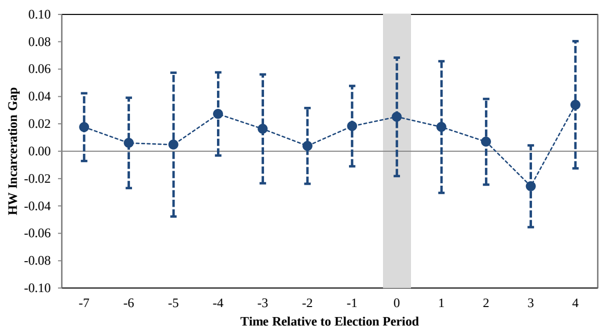
Figure D4: Election Cycle Effects in 3-Month Intervals



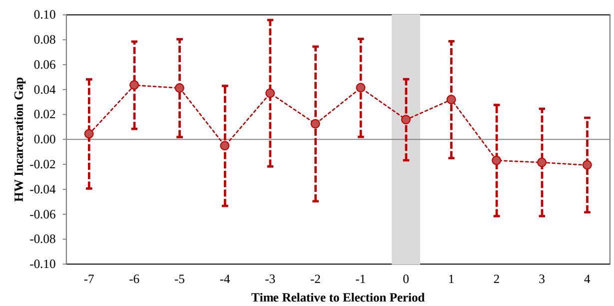
(a) BW Gap in Partisan Districts



(b) BW Gap in Retention Districts



(c) HW Gap in Partisan Districts



(d) HW Gap in Retention Districts

Notes: The election cycle is partitioned into 3-month intervals. These regression estimates control for race, gender, age, special rule violations, person crime, private counsel, plea status, total counts, a set of indicator variables for each severity-by-criminal history cell, year effects, and judge fixed effects. Standard errors are clustered at the district level.

media attention surrounding high profile events all provide motivation for why constituents might exhibit different preferences for sentencing severity towards black offenders. Of course, this does not guarantee that there are not different preferences for other minority groups too. Thus, the important question is whether or not there is any empirical evidence of reelection incentives on the sentencing of Hispanic crime. In these data, the evidence for this is not compelling. Perhaps the clearest illustration of the evidence is shown in Figure D4. This figure shows regression-adjusted estimates of election cycle effects on the black-white and Hispanic-white gap in incarceration rates separately for partisan and retention districts. In the specification that produces these estimates, the election period is partitioned into 3 month intervals and the second year of the election cycle serves as the baseline period. All plots are on the same scale.

Panel (a) shows that in partisan districts the black-white incarceration gap is fairly stable and close to zero until the final two quarters of the election cycle. At that point, the black-white incarceration gap increases by 1.5 and 2.8 percentage points in the last two quarters, respectively. Both estimates are statistically significant at the 5% level. In the quarter immediately following the election, the black-white gap declines by roughly 3 percentage points; however, this estimate is not statistically significant. In subsequent periods, the black-white incarceration gap returns to zero and is not statistically different. To summarize, there are three striking features of the pattern in partisan districts - there is no change in the black-white gap prior to final 6 months, a rise in the black-white gap in the final 6 months, and subsequent return to a zero black-white gap.

Panel (b) shows how the black-white incarceration gap changes across the election cycle in retention districts. The three striking features of the previous plot are not apparent in this one. In retention districts, prior to the final 6 months of the cycle, the estimates are not all close to zero as they were in partisan districts. In fact, the largest estimate is in period -3 (e.g. 3 quarters prior to the election period) when there is arguably diminutive reelection concerns. Importantly, there is weak evidence of a rise in the black-white gap in the final 6 months as the estimated change at time 0 is negative. Overall, this pattern does not suggest that judges in retention districts increase sentencing severity in close proximity to the election period.

Panels (c) and (d) show the hispanic-white incarceration gap in partisan and retention districts, respectively. The patterns are not what we would expect to see under the hypothesis that reelection concerns lead to more severe sentences for Hispanics. Panel (c) shows that in partisan districts, the two largest increases in the HW gap are in the periods 4 quarters pre and post the general election date when there is arguably diminutive reelection concerns. In addition, in partisan districts, in the quarter after the general election, there is a nontrivial increase in the HW gap when reelection concerns are likely to be relatively low. None of the estimates are statistically significant.

In retention districts, the estimates show fairly large increases in incarceration for Hispanics in comparison with whites long before either the filing deadline, primary, or general election dates and immediately after the general election date. These two plots show no evidence of an election cycle effect on incarceration for Hispanic offenders in either partisan or retention districts. Thus, the empirical evidence implies that elections have differential impact on black and Hispanic offenders. Given the unique relationship between African-Americans and the criminal justice system in the United States, I do not view these results as surprising. Instead, they plausibly reflect different preferences for severe sentencing across various minority groups.

There are two important caveats worth noting. First, this analysis is predicated on the presumption that the periods -1 and 0 are the time intervals associated with elevated political pressure. The referee raises a valid point that this may not be true given that the filing deadline and primary election may be critical junctures in the election cycle. We will explore this issue shortly. Second, I suspect that the null findings on Hispanics may not hold in studies that use more recent data or examine other states. This is because the policy discourse on crime has become increasingly linked to immigration which has disproportionate affect on Hispanics. Thus, the findings of this paper suggests a potential avenue for future research; that is, to see whether elections have disparate policy impact for Hispanics in states where issues of immigration are much more contentious.

D.11 Effects on Intensive Margin

In this section, I will present results on the length of the prison sentence. A common approach used in the literature is to assign a prison term of 0 for all offenders who are sentenced to probation (Gordon and Huber (2007)). I follow this procedure. In order to examine relative changes, I transform the sentence length, s, into $ln(1+s)$. Table D8 shows results from specifications in which the log of sentence length replaces incarceration as the dependent variable. Columns (1) and (2) show the percent change in the length of the prison sentence using the full sample in retention and partisan districts, respectively. Consistent with the analysis on incarceration, column (1) shows no evidence of an increase in sentence length in retention districts for either white or black offenders. However, in partisan districts, we observe an 8% increase in the black-white gap in the length of the prison sentence in the last 6 months of the election cycle.

Table D8: Effects on the Intensive Margin

Dep Var: Log(Sentence Length)				
	Full Sample		Conditional on Prison	
	Retention	Partisan	Retention	Partisan
Election Cycle Effects	(1)	(2)	(3)	(4)
D_{it}^{-3}	-0.011	0.006	0.005	0.028***
	(0.018)	(0.016)	(0.011)	(0.009)
D_{it}^{-2}	-0.019	0.006	-0.015	-0.003
	(0.021)	(0.014)	(0.017)	(0.011)
D_{it}^{-1}	0.009	0.019	-0.011	0.007
	(0.020)	(0.025)	(0.008)	(0.019)
D_{it}^{0}	-0.003	-0.027	0.016	-0.012
	(0.015)	(0.032)	(0.010)	(0.009)
Race-Specific Election Cycle Effects				
$Black_i * D_{it}^{-3}$	-0.020	-0.021	-0.009	-0.039**
	(0.030)	(0.028)	(0.013)	(0.015)
$Black_i * D_{it}^{-2}$	0.014	0.017	0.036	0.023
	(0.033)	(0.016)	(0.025)	(0.018)
$Black_i * D_{it}^{-1}$	0.005	-0.017	0.010	-0.007
	(0.026)	(0.024)	(0.013)	(0.017)
$Black_i * D_{it}^{0}$	-0.006	0.081***	-0.021	-0.001
	(0.038)	(0.023)	(0.022)	(0.015)
n	119,074		30,924	

Notes: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$. These regressions include the usual set of case facts. Standard errors are clustered at the district level.

Columns (3) and (4) show results using only offenders who receive a positive prison term. Focusing on the estimate for $Black_i * D_{it}^{0}$, we see that there is no evidence of an increase in the length of the prison sentence in the election period in either retention or partisan districts among those offenders who are sentenced to prison. If anything, the signs of the estimates suggest that in partisan districts, white and black offenders, are sentenced to shorter prison terms during the election period.

This analysis highlights the complications that arise in interpreting results on the length of the prison sentence. On the one hand, if we adopt the approach of including offenders who are sentenced to probation into the analysis, then we find results that show an increase in the length of the prison sentence. Of course, this finding is, in part, mechanically driven by changes along the extensive margin. Because judges incarcerate black offenders at higher rates in the election period, there has to be fewer black offenders with values of 0 in the election period in comparison with other periods. On the other hand, the results that use the restricted sample of offenders who are assigned a positive prison sentence conflate two effects: 1) selection into prison may change during the election period and 2) judges may have a higher willingness to increase the length of the prison sentence due to reelection concerns.

It is worth noting that there are two pieces of empirical evidence that imply selection effects may exert downward pressure on the length of the average prison term conditional on being sentenced to prison. First, recall that in the paper, I present heterogeneous effects by criminal severity that show the increase in the black-white incarceration gap is highest among cases involving the four lowest levels of criminal severity. Second, in our analysis of departures, there is evidence that black offenders are more likely to have characteristics that warrant *lenient* sentence lengths even though black offenders are more likely to be incarcerated during the election period.

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