Judicial Elections and Discrimination in Criminal Sentencing

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Abstract

This paper explores the possibility that trial judges engage in discriminatory sentencing in order to gain electoral support, rather than to indulge their tastes for discrimination. This seems plausible given the extensive literature that finds electoral preferences for punitive sentencing depend on race. I use a research design that 1) distinguishes between the election effects versus judicial preferences and 2) separates the effects of judicial elections versus the electoral pressures of other public officials. Using this design, I find that incarceration rates rise by 2.2 to 2.6 percentage points in the election year, but only for black not white defendants. These effects are more pronounced in districts where the median is expected to have higher levels of racial prejudice.

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1 Introduction

There are now multiple papers across different disciplines that show trial court judges adjust their sentencing behavior in response to judicial elections (Gordon and Huber (2007), Huber and Gordon (2004), Berdejo and Yuchtman (2009)). In particular, trial judges issue increasingly punitive sentences as the election date draws near, especially those judges whose re-election prospects are less secure. Less clear is which felons bear the burden of this rise in sentencing severity. Substantial heterogeneity across criminal cases make it highly unlikely that the electoral returns to sentencing are uniform across all felons. Incarcerating a beloved public figure, for example, could damage rather than enhance a judge’s reputation. This suggests important strategic choices not just on average, but also in how a judge allocates punitive sentences across the distribution of felons. This paper asks the narrow question of whether or not racial minorities disproportionately bear the burden of judicial politics.

My focus on race is fundamental. The Supreme Court’s legal doctrine subjects any racial classification to the highest levels of judicial scrutiny. This is in recognition of the fact that racial minorities, and in particular blacks, have historically been most vulnerable to political repression. The possibility that judges engage in discriminatory sentencing for their own electoral benefit is also well-grounded in the literature. There is extensive evidence that a judge’s reputation suffers when she appears “soft on crime” (Swisher (2010)), that crime is a highly racialized policy issue (Hurwitz and Peffley (1997), Hurwitz and Peffley (1997)), and that political candidates, including judges, actively engage in racialized campaigns (Mendelberg (2001)). This analysis on the racial effects of judicial politics is a new but natural iteration in this line of existing research.

There are two key empirical challenges to identifying the effects of judicial elections on minority sentencing. First, discriminatory judicial behavior could appear to be induced by elections but in fact be driven by the judge’s own racial preferences. Identification of election

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1 This is not true of classifications based on gender or sexual orientation. See footnote 4 in United States v. Carolene Products Co.
effects requires varying electoral pressure while simultaneously holding racial preferences fixed. I address this issue by using administrative sentencing data from the state of Kansas which includes judge identifiers. The identifiers are critical; by conditioning on judge fixed effects, they allow me to observe within judge changes in behavior towards racial minorities over the course of the electoral cycle. This difference-in-difference approach exploits the well-established idea that electoral pressures vary over time, and in particular, peak in the election year (Levitt (2002)). To the extent that racial preferences are time-invariant, changes in minority sentencing over the electoral cycle will capture the causal effects of electoral pressure on judicial sentencing towards minorities.

The second challenge is the fact that trial judges are rarely, if ever, on a ballot alone. Elections for district court judges often coincide with elections for the governor, state senators, U.S. congressional representatives, and district attorneys among others. This is problematic because fighting crime is a responsibility that is shared by diverse elected officials. Both mayors and governors increase police patrols in election years in order to reduce crime and gain electoral favor (Levitt (2002)). Prosecutors could also face elevated pressures to raise their conviction rates in election years. In this case, judges could adjust their sentencing towards minorities not because of their own elections, but because others respond to their elections by changing the composition of criminal cases in ways that are not race-neutral.

I address this concern by exploiting a unique institutional feature of Kansas. Kansas is only 1 of 4 U.S. states that employs different judicial selection methods within state. Among these 4, Kansas is the only one in which covariates are relatively balanced across selection methods.\(^2\) The two selection methods, partisan and retention elections, reflect the current ambivalence on whether we should subject state judges to electoral accountability or maintain an independent judiciary.\(^3\) Retention elections are specially designed to insulate judges from the political process. All other public officials are elected under the convention

\(^{2}\)In the other 3 states, a small number of districts use one selection method while the rest of the state uses another.

\(^{3}\)Federal judges are not subject to electoral accountability.
of partisan elections. Thus, if others are driving the empirical results, then there should be no differential effects across judicial selection methods. A triple-difference approach that compares racial sentencing disparities across the electoral cycle and across retention versus partisan districts will separate the election effects of judges versus others.

Using this research design, I find evidence that the rise in sentencing severity is not evenly distributed across racial groups. I find that incarceration rates rise by 2.2 to 2.6 percentage points in the election year, but only for criminal defendants who are black. This constitutes a 10% increase in the likelihood of incarceration for black defendants in election years. This pattern is only characteristic of partisan districts; I find no analogous rise in incarceration in retention districts. Moreover, I explore but find no evidence that prosecutors differentially charge blacks defendants on either extensive or intensive margins in the election year (i.e. charge blacks with more crimes or with more severe crimes). I also consider the possibility that an electorate prefers punitive sentencing for certain types of crime; specifically, violent crimes or drug crimes. If these crimes are disproportionally committed by blacks, then racial sentencing disparities can rise even though the electorate is race-neutral. I find no evidence that either of these mechanisms explain the main results.

What drives these results? I write down a simple agency model of judicial politics. The model is useful because it motivates why judges might internalize electoral preferences on minority sentencing even when the electorate has little information on judicial performance. In the model, judicial sentencing reflects the electorate’s preferences because the representative voter votes based on her beliefs about judicial performance, and in equilibrium, judicial behavior must be congruent with the voter’s beliefs. The model predicts that if the voter has strong preferences for discriminatory sentencing, then judges are more likely to engage in racially disparate sentencing.

I assess this prediction by testing whether the election effects vary with two constructed proxies of district-level prejudice. The first proxy uses data from the Implicit Association

\footnote{The model extends the career concerns model described in Persson and Tabellini (2002).}
Test, which is an online test designed to measure a respondent’s level of implicit prejudice. The second proxy computes the district level change in the Democratic vote share in the 2008 Presidential election from the previous 4 Presidential elections. These measures are motivated by separate literatures that 1) validates the IAT as indicator of racial prejudice (Bertrand et al. (2005)) and 2) finds that racial resentment played an influential role in voting behavior during Obama’s Presidential election in 2008 (Tesler and Sears (2010), Stephens-Davidowitz (2012)). Consistent with the model, the estimates show that the increase in black incarceration rates are 2 to 4.5 times larger in districts associated with higher levels of predicted racial prejudice.

The rest of this paper is organized as follows. In section 2, I discuss two literatures, one that documents that judges do respond to electoral incentives and another that shows that the demand for sentencing severity depends on the felon’s race. In section 3, I provide background on partisan and retention elections in Kansas. In section 4, I introduce the data. In section 5, I write down the statistical models. In section 6, I show the empirical results and finally, I conclude.

2 Previous Research

There is an extensive cross-disciplinary literature that shares a common finding despite using very diverse methodologies; that is, the demand for punitive sentencing depends on the color of the criminal defendant’s skin. Exploiting quasi-random variation in the racial composition of seated juries, economists find that all-white juries are substantially more likely to convict black versus white felons (Anwar et al. (2012)). Using public opinion survey experiments, political scientists find that white respondents with negative stereotypes of blacks have a stronger willingness to issue more punitive sentences towards blacks and are less optimistic that black criminals can rehabilitate (Hurwitz and Peffley (1997), Peffley et al. (1997)). In “mock-jury” trials, there is socio-psychological evidence of own-race bias
as both white and black student jurors are less likely to convict defendants of the same race (Wuensch et al. (2002)).\(^5\) In lab experiments, also performed by social psychologists, white subjects are more likely to label actions as violent when the protagonist is black versus white (Duncan (1976)).

The origins of this phenomenon are complicated. An apparent explanation is taste-based discrimination (Becker (2010)). A more subtle explanation is the idea that stereotyping is rational because race provides an informative signal regarding a criminal’s latent criminality (Arrow (1973)). Oft-cited statistics that show blacks are significantly overrepresented in our prison population are likely to facilitate linkages between race and criminality.\(^6\) These statistics are impactful to the extent that the average voter fails to control for socio-economic, institutional, or political factors that underlie the correlations between race and crime.\(^7\) Another factor is television news; the evidence shows that news media heavily influences viewer perceptions of race and crime. Stories that feature blacks predominantly involve violent crime, which reinforces viewer support for punitive crime policy and negative stereotypes of blacks (Entman (1992), Gilliam Jr. and Iyengar (2000)).

The conflation of race and crime has potential implications for judicial behavior because crime is a central issue in judicial campaigns. In an analysis of 4 U.S. states, Champagne (2001) finds that crime is a major theme in television ads aired during State Supreme Court elections. Both Republican and Democratic judicial candidates actively campaign on being “tough on crime” (Linde (1987)). Candidates often boast that they support police officers, are prosecution-oriented, believe in victim’s rights, and are unafraid to issue punitive sentences (Weiss (2006), Swisher (2010)). Attack ads will often skew the facts of a single case in order to portray judicial leniency as too “soft” regardless of the ruling’s legal merits. In some instances, the attacks will strategically employ racial appeals in order to capitalize on

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\(^6\) For example, in the U.S. roughly 40% of the prison population is black, even though blacks comprise of only 12% of the residential population.

\(^7\) See Weaver (2007), Muhammad (2010), and Blackmon (2009) for historic-institutional origins of black criminality.
voter beliefs about race and criminality (Mendelberg (2001)). In the 2008 Wisconsin State Supreme Court election, challenger Michael Gableman uses Willie-Horton style television ads that feature an image of a black criminal accompanied by a voice-over that suggests the incumbent “worked to put criminals back on the street” (Brown (2009)).

“Tough on crime” campaigns translate into tough sentencing. Numerous studies show that liberal State Supreme Court Justices are more likely to vote conservatively in death penalty cases in order to avoid political fallout, especially when the election date draws near (Hall (1992), Hall (1995), Brace and Hall (1997)). At lower court levels, the empirical evidence also shows that trial judges respond to elections by increasing sentencing severity, even though voters tend to have less information on judicial performance (Gordon and Huber (2007), Huber and Gordon (2004), Berdejo and Yuchtman (2009)) Such judicial behavior is rational. Capital punishment cases are highly visible and so contentious that a single ruling can quickly cripple an incumbent’s prospects for re-election (Weiss (2006)). At lower levels, there are also examples of motivated interest groups mobilizing opposition in response to unfavorable judicial decisions.\(^8\) The open question is to what extent “tough on crime” is campaign code for “tough on black crime” and whether this language actually translates into discriminatory sentencing.

3 Kansas Judicial Elections

Kansas is only 1 of 4 U.S. states that uses mixed judicial selection methods across its judicial districts. Of its 31 judicial districts, 14 use partisan elections, which are synonymous with elections used to select governors, senators, congressional representatives, and other public officials. The filing deadline for official candidacy is usually in the first week of June. In partisan districts, each judicial candidate is affiliated with a political party. Candidates of the same party compete in primary elections which are held on the first Tuesday of August.

\(^8\)In a 1986 judicial election, the Chicago police successfully mounted a campaign against a local incumbent judge for acquitting a defendant who brutally attacked a police officer.
The winner of each party’s primary then compete in the general election, which is held on the first Tuesday of November. The other 17 judicial districts use retention elections. There are two key distinctions between retention and partisan elections. First, in retention districts, voter ballots reveal no explicit information regarding a retention judge’s political ideology. Second, retention judges never compete against an actual challenger. The voter’s decision is to vote either “Yes” or “No” on whether the judge should retain office. If a simple majority of the votes are “Yes”, then the judge is re-elected. Retention elections are also held on the first Tuesday of November.

In Kansas, retention judges have extraordinary success, both in terms of the odds of retention and the margin of victory, conditional on winning. Figure 1 uses election results from 1996-2010 to illustrate these two points. The figure plots the share of “Yes” votes against different percentile points of the “Yes” vote distribution. First, notice that all of the points lie above the 0.50 threshold required for retention. The minimum share of “Yes” votes in this time period is 0.513, which implies that every single judge won retention throughout these 14 years. Second, notice that even in the 1st percentile election, judges win retention with a margin of victory close to 10%. Thus, in 99% of retention elections, the incumbent judge receives at least a 60% approval rating from the electorate. In 75% of retention elections, the incumbent judge receives at least 71% approval. In contrast, for partisan judges, job security is far less imminent. In 2000 alone, nearly 16% of incumbent judges in partisan districts lost their bid for re-election (Lim (2013)). These patterns underscore the intuition that in Kansas, retention elections insulate judges from the political process.

Figure 2 shows a map of judicial districts by selection method, where the black lines overlay congressional district boundaries. The map illustrates that judges are elected in highly localized districts, as there is nearly an 8-to-1 ratio of judicial to congressional districts.

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9In retention districts, if there is a vacancy, a pool of candidates are nominated by a 9 member commission, which consists of 5 members of the Kansas Bar and 4 non-attorney citizens. The governor chooses an interim judge from the pool, who subsequently runs in retention elections. In partisan districts, the governor directly appoints an interim judge, who then runs in the next general election.

10Election returns are available on the Kansas Secretary of State website.
in Kansas. The decentralization of judicial elections is likely to exert two pressures on the interactions between judges and their electorate in the context of race. First, there should be much more between district variation in the median voter’s racial preferences when judicial districts are more disaggregated. Second, within district, racial preferences should be more homogeneous in more localized districts. This is implied by empirical findings that show race and racial prejudice are key determinants of residential sorting (Card et al. (2008)). The potential for heterogeneous racial preferences across districts and homogeneous racial preferences within district is relevant because Gerber and Lewis (2004) find that elected officials are much more constrained by the median when the electorate is more homogeneous.

Although districts self-select into their judicial selection method, partisan and retention districts are balanced along numerous observable dimensions, albeit not perfectly so. Figure 3 is emblematic of the medial balance between partisan and retention districts. It plots each judicial district’s share of the state’s black residential population separately for districts below and above the median district. The figure shows substantial overlap in the share of black residents across retention and partisan districts. There is roughly an even number of partisan and retention districts conditional on being either below or above the median
district. The 4 most populated districts (Topeka, Kansas City, Wichita, and Overland Park) also have the highest shares of black residents, and of these, 2 are partisan and 2 are retention districts. Kansas City is the clear outlier. Nearly 30% of Kansas’ black population reside in Kansas City, which is nearly 20 percentage points higher than the next highest district. The empirical results that follow will not be driven by any one district in particular.

4 Kansas Sentencing Data

4.1 State Sentencing Guidelines

Kansas is a guideline state. Each felony is associated with a guideline sentence that recommends either prison or probation as well as the sentence length. Figure 4 shows that the guideline sentence is a function of the severity of the crime and the felon’s criminal history. In each cell, the three numbers are the minimum, expected, and maximum sentence length (in months). The grey boxes indicate that the guideline sentence is probation, whereas in the
clear boxes, the presumptive sentence is prison. For example, a defendant who is charged with theft of $100,000 (a severity level 5 crime) and who has 1 prior non-person felony should expect between 38 to 43 months in prison, regardless of her race. All regressions include indicator variables for each cell in the sentencing grid. Thus, the regression estimates of racial sentencing disparities can be thought of as weighted averages of cell-specific black-white gaps (Angrist and Krueger (1999)).

There are three primary ways in which racial disparities can arise under the sentencing guidelines. First, the guidelines provide judges with the discretion to issue longer sentences within cell. Second, judges can formally depart from the guideline sentence, on either extensive or intensive margins, based on mitigating or aggravating factors. However, formal departures are subject to appeal and can be reversed. Roughly 9% of cases are sentenced

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11 The blue boxes are “Border Box” cells. The presumptive sentence for these crimes is prison, but the judge can choose probation without the departure being subject to review.

12 Some examples of departing factors are whether the offender played a passive role in the crime, the crime is excessively brutal, the crime is a reaction to prolonged abuse, the crime is in self-defense, and etc.

13 There are limits to upward durational departures. The departure cannot exceed twice the base sentence length. In cases involving multiple convictions, the upward departure cannot exceed four times the base sentence length.
with a formal departure. Third, judges have more freedom to depart from the guideline sentence when the crime violates a special rule. Approximately 20% of all cases involve a special rule violation, which include committing a person felony with a firearm, aggravated battery against a law enforcement officer, committing crimes for the benefit of a street gang, persistent sex offenses, and more. The modal violation is committing a crime while on probation, parole, conditional release, or post-release supervision (78%). In this case, even if the criminal commits a low severity crime, the judge can sentence her to prison without being subject to formal review.

### 4.2 Data Description

I use administrative sentencing data from Kansas which contains the universe of convicted felons from mid-1997 to 2003. I exclude cases that are sentenced outside of the sentencing guidelines. Off-grid cases comprise of 8% of all crimes. These include felony DUI’s, 1st degree murder, treason, and sex offenses against victims less than 14 years of age. The
punishment for these crimes is life imprisonment. I also restrict attention to black and white felons, who comprise 90% of the sample. The data includes a rich set of legal covariates. I have information on the felon’s age, race, and gender, criminal history, plea status, attorney quality (i.e. private versus public attorney), total number of counts, the actual crime committed, and characteristics of the crime (e.g. whether the crime violates a special rule, is a person or non-person crime, drug or non-drug offense). Person crimes inflict harm on persons (e.g. robbery, rape, aggravated arson, and battery) whereas non-person crimes are analogous to property crimes.

Table 1 shows descriptive means for all of the covariates separately black and white felons. The statistics suggest that black offenders account for roughly 1/4th of all felons even though blacks represent less than 5% of Kansas total population. Most felonies are actually low severity crimes. The average severity level for non-drug crimes is 3.245 out of scale of 1 to 10 and 7.532 for drug-crimes on a scale of 7 to 10. Only 28% of all crimes are violent and 29% are drug related. In terms of racial differences, black offenders have worse case-level characteristics along several dimensions; they are more likely to commit non-drug and person crimes, to be represented by a public defender, to violate a special rule, to have worse criminal histories, and are less likely to settle.

5 Statistical Model

There are three ways in which elections could change judicial sentencing behavior towards minorities. First, there is a potential selection effect in which constituents elect judges who share similar tastes for discriminatory sentencing. Second, holding judicial preferences fixed, elections could induce a persistent change in judicial behavior towards minorities. Third, it is possible that judges exhibit changes in how they sentence minorities over the course of the electoral cycle. This paper uses a statistical approach that estimates the effects of elections via the latter channel; that is, whether the electoral cycle effects are race-specific. This

14 I have run analysis using Hispanics but find no election cycle effects on Hispanic defendants.
Table 1: Descriptive Means

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<thead>
<tr>
<th></th>
<th>Overall</th>
<th>Black</th>
<th>White</th>
<th>BW Diff</th>
</tr>
</thead>
<tbody>
<tr>
<td>Incarceration Rate (Unconditional)</td>
<td>0.267</td>
<td>0.329</td>
<td>0.244</td>
<td>0.085***</td>
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<tr>
<td>Incarceration Rate (Non-Drug Crimes)</td>
<td>0.272</td>
<td>0.333</td>
<td>0.246</td>
<td>0.087***</td>
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<tr>
<td>Incarceration Rate (Drug Crimes)</td>
<td>0.255</td>
<td>0.317</td>
<td>0.239</td>
<td>0.078***</td>
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<tr>
<td>Sentence Length (in Months)</td>
<td>27.871</td>
<td>29.762</td>
<td>27.31</td>
<td>2.452***</td>
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<tr>
<td>Sentence Length (Non-Drug Crimes)</td>
<td>29.223</td>
<td>31.866</td>
<td>28.086</td>
<td>3.780***</td>
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<tr>
<td>Sentence Length (Drug Crimes)</td>
<td>24.649</td>
<td>23.723</td>
<td>25.514</td>
<td>-1.791***</td>
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**Defendant Demographics**

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<tr>
<td>Black</td>
<td>0.247</td>
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<td></td>
</tr>
<tr>
<td>Age</td>
<td>30.19</td>
<td>30.025</td>
<td>30.532</td>
<td>-0.507***</td>
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<tr>
<td>Female</td>
<td>0.171</td>
<td>0.178</td>
<td>0.180</td>
<td>-0.002</td>
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<tr>
<td>Partisan District</td>
<td>0.489</td>
<td>0.574</td>
<td>0.448</td>
<td>0.127***</td>
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**Type of Crime**

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<tr>
<td>Non-Drug Crimes</td>
<td>0.705</td>
<td>0.742</td>
<td>0.699</td>
<td>0.043***</td>
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<tr>
<td>Person Crimes</td>
<td>0.282</td>
<td>0.323</td>
<td>0.262</td>
<td>0.061***</td>
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**Case-Level Characteristics**

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<th>White</th>
<th>BW Diff</th>
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</thead>
<tbody>
<tr>
<td>Severity Level (Non-Drug Crimes)</td>
<td>3.268</td>
<td>3.350</td>
<td>3.211</td>
<td>0.139***</td>
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<tr>
<td>Criminal History (Non-Drug Crimes)</td>
<td>3.910</td>
<td>4.361</td>
<td>3.800</td>
<td>0.560***</td>
</tr>
<tr>
<td>Severity Level (Drug Crimes)</td>
<td>1.528</td>
<td>1.491</td>
<td>1.548</td>
<td>-0.057***</td>
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<tr>
<td>Criminal History (Drug Crimes)</td>
<td>3.149</td>
<td>3.915</td>
<td>3.059</td>
<td>0.875***</td>
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<tr>
<td>Special Rule Violations</td>
<td>0.205</td>
<td>0.244</td>
<td>0.195</td>
<td>0.049***</td>
</tr>
<tr>
<td>Counts</td>
<td>1.266</td>
<td>1.210</td>
<td>1.295</td>
<td>-0.086***</td>
</tr>
<tr>
<td>Private Counsel</td>
<td>0.26</td>
<td>0.205</td>
<td>0.277</td>
<td>-0.072***</td>
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<tr>
<td>Plea Bargain</td>
<td>0.948</td>
<td>0.928</td>
<td>0.955</td>
<td>-0.027***</td>
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</tbody>
</table>

N = 46,466
is because the main comparative advantage of this dataset is the availability of judge fixed
effects, which lends itself to within-judge analysis. These estimates, though, could understate
the total effect of judicial politics on minority sentencing to the extent that elections induce
selection effects or affect judicial behavior in steady state.

I estimate how judicial elections effect changes in minority sentencing using the following
statistical model:

\[ y_{ijt} = \sum_{k=3}^{4} D_{it}^k \delta_k + \sum_{k=3}^{4} D_{it}^k b_i \gamma_k + X \eta + \tau_j + \mu_t + \varepsilon_{ijt} \]  \hspace{1cm} (1)

Where \( i \) denotes each case, \( j \) denotes the presiding judge, and \( t \) reflects the calendar year.
The outcome variable, \( y_{ijt} \), is an indicator of whether or not the felon is sentenced to prison,
\( b_i \) is an indicator of whether or not the felon is black, and \( X \) represents a vector of case
facts including indicator variables for each severity level-by-criminal history cell, whether
the crime is a person crime, violates a special rule, whether the defendant uses a public
versus private attorney, plea status, total number of counts, and the defendant’s age, race,
and gender. The \( \tau_j \) and \( \mu_t \) are judge and calendar year fixed effects, respectively and finally,
the \( \varepsilon_{ijt} \) is the error term.

This specification assumes that the incarceration decision varies over the course of the
electoral cycle. This is captured by the pair of indicator variables \( D_{it}^3 \) and \( D_{it}^4 \), which repre-
sents whether or not the criminal case is sentenced during either the 3rd or 4th year of the
electoral cycle, respectively. The 4th year is the election year as election cycles are 4 years
long. The \( \delta_3 \) and \( \delta_4 \) parameters should be interpreted as the change in white incarceration
rates in the 3rd and 4th years in office relative to the expected white incarceration rates
based on the first two years in office. The key parameters of the model are \( \gamma_3 \) and \( \gamma_4 \), which
are attached to the interactions between \( D_{it}^k \) and \( b_i \). These should be interpreted as the
change in the black-white incarceration gap in the 3rd and 4th years in office relative to the
expected black-white gap based on the first two years in office.

Estimates of \( \delta_k \) and \( \gamma_k \) will provide information as to whether 1) judges respond to
elections by increasing sentencing severity and 2) the response is race-neutral. If elections
induce judges to become increasingly “tough on crime”, then we should expect $\hat{\delta}_4 > \hat{\delta}_3 \geq 0$.
If the judicial response is to increase sentencing severity but in a race-neutral way, then
$\hat{\delta}_4 > \hat{\delta}_3 \geq 0$ and $\hat{\gamma}_4 = \hat{\gamma}_3 = 0$. If elections motivate judges to both increase sentencing
severity and to be differentially punitive towards blacks, then $\hat{\delta}_4 > \hat{\delta}_3 \geq 0$ and $\hat{\gamma}_4 > \hat{\gamma}_3 \geq 0$.
The relative magnitudes of $\hat{\delta}_4$ versus $\hat{\delta}_3$ and $\hat{\gamma}_4$ versus $\hat{\gamma}_3$ provides an informal test of the
model. Evidence of either $\hat{\delta}_3 > \hat{\delta}_4$ or $\hat{\gamma}_3 > \hat{\gamma}_4$ would cast doubt on whether these parameters
represent election cycle effects since electoral pressures should peak rather than diminish in
the election year.

A primary concern of the model is that other elected officials share the same electoral
cycle. This implies that the sentencing outcomes may be influenced by the electoral responses
of myriad public officials and not just judges. I address this issue by allowing $D^k_{it}$ and $D^k_{it}\hat{b}_i$
to vary across judicial selection systems by adding a set of interactions of these terms with
an indicator variable for whether the case is sentenced in a partisan district. The parameter
estimates of the interactions (denoted as $\hat{\delta}_p^k$ and $\hat{\gamma}_p^k$) compare the difference in the election
cycle effects across partisan and retention districts. For example, $\hat{\delta}_4^p$ is the difference in the
change in white incarceration rates in the election year from the first two years in office
between partisan versus retention districts. Similarly, $\hat{\gamma}_4^p$ is the difference in the change in
the black-white gap in the election year from the first two years in office between partisan
versus retention districts.

This triple-difference approach can separate the election effects of judges versus other
public officials. If the election cycle effects are driven entirely by other public officials, then
neither the change in sentencing severity nor the race-specific effects should differ across
judicial selection methods; that is, $\hat{\delta}_4^p = \hat{\delta}_3^p = 0$ and $\hat{\gamma}_4^p = \hat{\gamma}_3^p = 0$. The fact that retention
and partisan districts are balanced across a wide range of covariates makes it less likely that
state or local politicians would behave substantially differently across partisan and retention
districts unless they are judges. In contrast, if the election cycle effects are driven by judicial
behavior, then there should be evidence of differential effects. In particular, we should expect stronger effects in partisan districts because in Kansas, retention elections essentially insulate judges from the political process; that is, \( \hat{\delta}_4^p > 0, \hat{\delta}_3^p \geq 0 \) and \( \hat{\gamma}_4^p > 0, \hat{\gamma}_3^p \geq 0 \). The sign of \( \hat{\delta}_k^p \) and \( \hat{\gamma}_k^p \) provide an informal test of the statistical model because neither should be less than zero. The relative magnitudes are less informative because it is possible for partisan elections to have a uniform impact across the electoral cycle, i.e. \( \hat{\delta}_4^p = \hat{\delta}_3^p > 0 \) and \( \hat{\gamma}_4^p = \hat{\gamma}_3^p > 0 \).

### 6 Empirical Findings

#### 6.1 Main Results

Table 2 presents the estimates of the statistical model. Column 1 shows estimates from the difference-in-difference specification which compares the black-white incarceration gap across the electoral cycle. Estimates of \( \hat{\delta}_3 \) and \( \hat{\delta}_4 \) are both approximately zero and not statistically significant. This implies that judges are no more likely to incarcerate white felons in either the 3rd or 4th years of the cycle relative to the 1st two years in office. The point estimate of \( \hat{\gamma}_3 \) suggests that the black-white incarceration gap increases by 1.1 percentage points in the 3rd year; however, this estimate is not statistically significant. In the election year, the black-white incarceration gap increases by 2.6 percentage points and this estimate is statistically significant at the 5% level. This pattern is the exact opposite of what we would have expected under the null hypothesis that the judicial response to elections is race-neutral. Under the null, we would expect \( \hat{\delta}_4 > \hat{\delta}_3 \geq 0 \) and \( \hat{\gamma}_4 = \hat{\gamma}_3 = 0 \). While these estimates are consistent with a rise in sentencing severity, they show that black not white felons are the ones who bear this burden.

Columns 2 and 3 show the election cycle effects separately for retention and partisan districts. Going down the rows of column 2, the estimates show that \( \hat{\delta}_4 \approx \hat{\delta}_3 \approx 0 \) and \( \hat{\gamma}_4 \approx \hat{\gamma}_3 \approx 0 \), which implies that in retention districts, there is no rise in incarceration rates for either black or white felons during the second half of the electoral cycle. Column 3 shows a
Table 2: Election Cycle Effects on BW Incarceration Gap

<table>
<thead>
<tr>
<th>Electoral Cycle Effects</th>
<th>Diff-in-Diff</th>
<th>Triple Differences</th>
</tr>
</thead>
<tbody>
<tr>
<td>Election Year_{-1} (δ_3)</td>
<td>0.001</td>
<td>-0.009</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.007)</td>
</tr>
<tr>
<td>Election Year (δ_4)</td>
<td>0.002</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
<td>(0.007)</td>
</tr>
<tr>
<td>Black*Election Year_{-1} (γ_3)</td>
<td>0.011</td>
<td>0.007</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.014)</td>
</tr>
<tr>
<td>Black*Election Year (γ_4)</td>
<td>0.026**</td>
<td>-0.000</td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
<td>(0.007)</td>
</tr>
</tbody>
</table>

Note: ***p<0.01, ** p<0.05, *p<0.10. N = 46,466. R-squared is 0.487. Cluster-robust standard errors are shown in parentheses. Clusters are defined at judicial district level. All regressions include indicators for black, gender, special rule violation, private counsel, person crime, plea status, criminal severity-by-criminal history cells, year effects, judge fixed effects, the total number of counts and age.
contrasting pattern for partisan districts. While the estimates show that white incarceration rates are stable over the election cycle ($\hat{\delta}_4 \approx \hat{\delta}_3 \approx 0$), black incarceration rates rise by 2.2 percentage points in the election year. The point estimate is statistically significant at the 5% level. There is no evidence that black incarceration rates increase in the year prior to the election year, $\hat{\gamma}_3 \approx 0$. The fact that we only see a rise in black incarceration rates in partisan and not retention districts is consistent with 1) retention elections insulating judges from the political process and 2) that these patterns reflect judicial behavior. Otherwise, there should also be a response in retention districts.

6.2 Robustness

I conduct a number of robustness checks to gauge the sensitivity of these results. First, I re-run equation 1 31 times, but in each iteration I exclude 1 of the 31 judicial districts. This is motivated by a concern that the results are driven entirely by a single judicial district. If this is true and I exclude that district, then the estimate of the election year effect on black incarceration rates should fall close to zero. Figure 5 plots each of the 31 estimates of $\hat{\gamma}_4$ from the triple-difference specification. While some of the estimates are no longer statistically significant at the 5% level, none of the magnitudes fall to zero. The estimates range from 1.5 to 3.8 percentage points. The minimum estimate is 30% lower that the baseline estimate of 2.2 percentage points and is a result of excluding the 29th district (i.e. Kansas City). On the whole, this figure shows it is unlikely that a single district is driving the main results.

Next, I estimate equation 1 by varying the specification in some of the conventional ways. In one specification, I replace age with an age cubic to account for a possible non-linear relationship between age and sentencing. In another, I experiment by including separate indicator variables for each type of crime, as the baseline specification only controls for criminal severity. This could be relevant to the extent that sentencing varies across crimes within severity levels. I also run a more flexible version that disaggregates special rule violations into multiple indicator variables for each type of violation. I also examine whether
the election effects are heterogeneous across different types of crime, specifically non-drug versus drug related crimes. Finally, I run the model using a logit and a probit. These estimates are shown in table 3. The point estimate is slightly higher for drug versus non-drug related crimes, but this difference is not statistically significant. Overall, the point estimates demonstrate consistency across all of these different specifications.

6.3 Potential Explanations

In this section, I consider two explanations of the main results. First, it is possible that the racial composition of criminal cases changes in ways that warrant increases in black incarceration across the electoral cycle. Imagine discriminatory police officers or prosecutors who anticipate election effects on partisan judges, and in turn, adjust their respective behavior in non-race neutral ways. For example, police officers could exert more effort to arrest the most

---

\[\text{Figure 5: Election Cycle Effects Excluding Each Judicial District}\]

\[\text{Excluded District}\]

\[\text{Triple Difference}\]

\[\text{0.09}\]

\[\text{0.07}\]

\[\text{0.05}\]

\[\text{0.03}\]

\[\text{0.01}\]

\[\text{-0.01}\]

\[\text{-0.03}\]

\[\text{-0.05}\]

---

I show estimates from a linear probability model in light of the incidental parameters problem associated with the estimation of non-linear models that include a large number of fixed effects (Neyman and Scott (1948)). The logit and probit estimates are larger, which suggests that the LPM estimates understate the election effects relative to the non-linear models.
Table 3: Robustness of Race-Specific Election Cycle Effects

<table>
<thead>
<tr>
<th>Different Specifications</th>
<th>Black*Election Year</th>
</tr>
</thead>
<tbody>
<tr>
<td>Baseline</td>
<td>0.026**</td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
</tr>
<tr>
<td>Replace Age with Age Cubic</td>
<td>0.026**</td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
</tr>
<tr>
<td>Add Indicators for Type of Crime</td>
<td>0.028**</td>
</tr>
<tr>
<td></td>
<td>(0.008)</td>
</tr>
<tr>
<td>Separate Indicators for each Special Rule Violation</td>
<td>0.026**</td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
</tr>
<tr>
<td>Restrict Sample to Non-Drug Crimes</td>
<td>0.023**</td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
</tr>
<tr>
<td>Restrict Sample to Drug-Related Crimes</td>
<td>0.041**</td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
</tr>
<tr>
<td>Marginal Effects from Probit</td>
<td>0.034**</td>
</tr>
<tr>
<td></td>
<td>(0.015)</td>
</tr>
<tr>
<td>Marginal Effects from Logit</td>
<td>0.033*</td>
</tr>
<tr>
<td></td>
<td>(0.018)</td>
</tr>
</tbody>
</table>

Note: ***p<0.01, ** p<0.05, *p<0.10. N = 46,466. R-squared is 0.487. Cluster-robust standard errors are in parentheses. Clusters are at the district level.
violent black criminals, if they anticipate that the returns to policing are higher in election years. This could lead to a self-fulfilling equilibrium in which judges increase incarceration towards blacks not to gain electoral favor, but because compositional changes demand such a response.

To assess this possibility, I run equation 1 but replace the dependant variable with various case facts. Table 4 shows the point estimates of $\hat{\gamma}_4$ which now convey the race-specific election year effect on the particular case fact. On some dimensions, black felons actually exhibit less criminality in the election year. For example, column 1 shows that the black-white gap in person crimes falls in the election year, albeit only by 2%. Columns 2, 3, and 4 show inconsequential changes in the severity of the crime and the total number of counts, which further corroborates that the main results are not driven by prosecutors charging blacks with either more crimes or more severe crimes. Columns 5-8 show little change in the racial disparities across criminal history, special rule violations, private counsel and plea status. On the whole, there is no evidence of a meaningful change in the racial composition of case facts that would merit more punitive sentencing towards blacks in the election year.

Second, holding the composition of cases fixed, it is possible that judges increase black incarceration for reasons orthogonal to racial prejudice. Consider a race-neutral electorate who has heterogeneous sentencing preferences across different types of crime. In particular, the electorate prefers punitive sentencing towards violent crimes or drug crimes. If elections
because congruence between judicial behavior and electoral preferences, then judges will issue harsher punishments for violent or drug crimes in order to gain electoral favor. In this case, black incarceration rates may rise not because the electorate discriminates, but because race correlates with the types of crimes that the electorate happens to dislike.

To examine this alternative explanation, I add interactions between the election cycle timing indicators, $D_{it}^k$ and $D_{it}^k b_i$, and indicator variables for person or drug related crimes to equation 1. If this story is correct, then adding these interactions should explain away the main results. Column 1 of table 5 shows the estimates from the baseline difference-in-difference specification and columns 2, 3, and 4 show estimates that add drug-timing interactions, person-timing interactions, and then both sets of interactions, respectively. Across all the four columns, the point estimates are very stable, as the estimate of $\hat{\gamma}_4$ ranges from 2.2 to 2.6 percentage points. This suggests that the main results are not driven by
7 Model of Judicial Politics

What explains the main results? In this section, I introduce a highly stylized model of judicial elections in which a race-neutral judge is elected by a representative voter whose sentencing preferences may depend on the felon’s race. Even though the voter may have low information on judicial performance, judicial behavior will reflect voter preferences because the voter’s choice depends on her beliefs about judicial behavior. In equilibrium, the black-white incarceration gap is determined by the voter’s racial preferences.

7.1 Voter’s Problem

Consider a representative voter whose utility is described in equation 2, where $w$ and $b$ are the incarceration rates for whites and blacks, $\beta$ and $1 - \beta$ reflect the weight attached to incarcerating black versus white felons, respectively, and $\theta$ denotes the judge’s ability, which may reflect, for example, her knowledge of the law:

$$u = (1 - \beta)w + \beta b + \theta$$

(2)

I assume that $\theta \sim f(\theta)$ and that the distribution of ability is independent of whether or not the judge is a challenger or incumbent. I also assume that $E(\theta) = 0$.

The voter knows her own utility, but observes noisy signals of judge-specific incarceration rates, where $\varepsilon \sim N(0, \sigma^2_\varepsilon)$:

$$\tilde{w} = w + \varepsilon$$

$$\tilde{b} = b + \varepsilon$$

(3)

Conditional on her utility and signals, the voter will form beliefs regarding the judge’s ability,
which after plugging in 2 can be expressed as:

$$\tilde{\theta} \equiv E[\theta|u, \tilde{w}, \tilde{b}] = \theta + (1 - \beta)(w - \tilde{w}) + \beta(b - \tilde{b})$$  \hspace{1cm} (4)

The voter will elect the incumbent if $E[\theta|u, \tilde{w}, \tilde{b}] > E[\theta]$. In other words, if the voter believes that the incumbent has above average ability, then the voter favors the incumbent rather than drawing another $\theta$ from the candidate distribution.

### 7.2 Judge’s Problem

Judges receives a benefit $B$ from being in office and her outside option is normalized to 0. I assume that incarcerating felons is associated with convex costs, which can be motivated by the fact that the likelihood of appeal and reversal is differentially higher when incarceration rates are already high:

$$c(w, b) = \frac{w^2}{2} + \frac{b^2}{2}$$  \hspace{1cm} (5)

The problem for the incumbent judge is to choose $w$ and $b$ that maximizes her expected utility, where the probability of winning election is $P(\tilde{\theta} > 0)$:

$$\max_{w,b} P(\theta > -(1 - \beta)(w - \tilde{w}) - \beta(b - \tilde{b})B - \frac{w^2}{2} - \frac{b^2}{2})$$  \hspace{1cm} (6)

### 7.3 Equilibrium

If we impose that voter beliefs are correct in equilibrium, $w^* = \tilde{w}$ and $b^* = \tilde{b}$, then the first-order conditions will reduce to the following:

$$[w] : w^* = f(0)(1 - \beta)B$$

$$[b] : b^* = f(0)\beta B$$  \hspace{1cm} (7)
In equilibrium, the black-white incarceration gap is:

\[
\frac{b^*}{w^*} = \frac{\beta}{1-\beta}
\]  \( (8) \)

Equation 8 contains the empirical prediction of the model. The black-white incarceration gap reflects the voter’s marginal rate of substitution across black and white felons. Larger values of \( \frac{\beta}{1-\beta} \) imply that the voter values incarcerating blacks more than whites, and in turn, the more racially disparate judicial sentencing will be. If the voter is race-neutral, then there is no racial disparity as \( \frac{b^*}{w^*} = 1 \). This result is independent of the quality of information that voters receive since \( \frac{b^*}{w^*} \) does not depend on \( \sigma^2 \). Note that the model does not shed light on what motivates \( \frac{\beta}{1-\beta} \). Voters could favor incarcerating black versus white felons because of either taste-based or statistical discrimination. My goal is to empirically assess whether the election effects vary with proxies of district-level prejudice, whatever form of prejudice that might be.

## 8 Effects by District-Level Racial Prejudice

I construct two proxies of district-level prejudice. The first proxy uses data from the Implicit Associations Test (IAT), which is an interactive online test designed to measure implicit racial prejudice. Each respondent’s age, education, and race are observed in the data. Because the IAT is an online test, it is likely that the sample is not representative. To adjust for selection, I re-weight the distribution of covariates in the IAT with inverse probability weights that I construct using population counts from Census data.\(^{16}\) I then run regressions of implicit prejudice on these demographic characteristics, take the parameter estimates, and project them onto district averages to construct a district-level measure of prejudice.

\(^{16}\)Suppose we want to estimate \( E[y] \), but only observe \( sy \) where \( s \) is an indicator if \( y \) is observed. With sample selection, we compute \( E[y|s=1]P(s=1) \) and not \( E[y] \). Assuming selection on observables, \( E[y|s = 1, X] = E[y|X]P(s = 1|X) \), this reduces to \( E[y|X]P(s = 1|X) \). If we re-weight the data with \( \frac{P(X)}{P(s=1|X)} \), then we can recover \( E[y] \).
racial prejudice. Districts with less educated, older cohorts, and a small shares of black constituents are predicted to have higher prejudice levels on average.

The second proxy computes the change in the Democratic vote share in the 2008 presidential election from the previous four presidential elections. Even though Kansas is a predominantly red state, roughly 2/3rds of the judicial districts showed increased support for the Democratic Party in 2008, while the remaining 1/3rd either decreased support or stayed the same. While voters select their politicians based on numerous criteria, several studies find that racial prejudice was an important determinate of voting behavior during President Obama’s 2008 election (Tesler and Sears (2010), Stephens-Davidowitz (2012), Kam and Kinder (2012)). Thus, it seems plausible that district-level changes in support for Obama will reflect prejudice, albeit with some error. If the error is additive and classical, then this should attenuate the results. The correlation between the two proxies is 0.42.

Table 6 presents estimates of the election cycle effects separately for districts above or below the median prejudiced district as measured by each proxy. Using the IAT proxy, the estimates show that in the election year, black incarceration rates increase by 6.8 percentage points in high prejudiced districts versus a 1.5 percentage point increase in low prejudiced districts. This is nearly a 4.5 fold difference and it is statistically significant at the 5% level. Using the change in the Democratic vote share proxy, the election year effect on black incarceration rates in high prejudiced districts is nearly double the effect in low prejudiced districts, 5.3 versus 2.4 percentage points, respectively. This difference is statistically significant at the 10% level. One departure from the main results is that in low prejudiced districts, black incarceration rates also increase in the 3rd year of the election cycle. This suggests that high prejudiced districts are associated with more elongated and consistent electoral pressure over the last two years of the election cycle. On the whole, these estimates lend support to the model’s prediction that judges respond to the voter’s racial preferences.

\[\text{From 1992 to 2004, the Democratic presidential candidate received 36\% of all votes in Kansas.}\]
Table 6: Election Cycle Effects by District-Level Prejudice

<table>
<thead>
<tr>
<th>Dependent Variable: Indicator for Incarceration</th>
<th>Predicted Prejudice Using:</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Implicit Associations Test</td>
<td>Low</td>
<td>High</td>
</tr>
<tr>
<td>Electoral Cycle Effects</td>
<td>Δ in Democratic Vote Share In ’08</td>
<td>Low</td>
<td>High</td>
</tr>
<tr>
<td>Election Year(_{-1}) ((\delta_2))</td>
<td></td>
<td>0.000</td>
<td>0.002</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.003)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>Election Year ((\delta_4))</td>
<td></td>
<td>0.010</td>
<td>-0.008</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.021)</td>
<td>(0.015)</td>
</tr>
<tr>
<td>Race-Specific Electoral Cycle Effects</td>
<td></td>
<td>0.011**</td>
<td>0.005</td>
</tr>
<tr>
<td>Black(*)Election Year(_{-1}) ((\gamma_3))</td>
<td></td>
<td>(0.005)</td>
<td>(0.030)</td>
</tr>
<tr>
<td>Black(*)Election Year ((\gamma_4))</td>
<td></td>
<td>0.015**</td>
<td>0.068***</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.005)</td>
<td>(0.020)</td>
</tr>
<tr>
<td></td>
<td>P-value of the following test:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(\gamma_{4low}^{\text{Low}} = \gamma_{4high}^{\text{High}})</td>
<td></td>
<td>0.019</td>
<td>0.083</td>
</tr>
</tbody>
</table>

Note: ***p<0.01, **p<0.05, *p<0.10. N = 46,466. R-squared is 0.487. Cluster-robust standard errors are in parentheses. Clusters are at the district level. All regressions include indicators for black, gender, special rule violation, private counsel, person crime, plea status, criminal severity-by-criminal history cells, year effects, judge fixed effects, the total number of counts and age.
9 Conclusion

Previous research on the effects of judicial elections disproportionately focuses on the question of whether or not judges engage in strategic behavior. The evidence overwhelmingly shows that they do. Less clear are the distributive consequences of judicial politics. The fact that criminal cases are extremely heterogeneous suggests that the returns to punitive sentencing are unlikely to be uniform across all cases. Differential returns raises the possibility that judges will not only increase their level of sentencing severity, but also be strategic in how they allocate punitiveness across cases. My focus on race is a natural starting point because it is well-known that judges are held accountable for being “tough on crime”, but that the demand for punitive sentencing also depends on the criminal’s race. This creates a tension for the judge - whether or not to uphold equal protection at the expense of potentially losing electoral favor.

The evidence shows that the burden of judicial politics is not evenly borne across racial groups. Incarceration rates rise by 2.2 to 2.6 percentage points in the election year, but only for black not white felons. These patterns are neither explained by changes in the composition of cases in election years, nor race-neutral election year responses towards specific types of crime. Instead, the evidence is consistent with a model that predicts judges engage in racially disparate sentencing because the electorate has preferences for discriminatory sentencing. Using proxies of district-level prejudice, I find that the increase in black incarceration rates are 2 to 4.5 times larger in districts associated with higher levels of predicted racial prejudice. These results speak directly to the existing debate on the merits of judicial accountability versus independence. While judicial elections increase policy congruence, policy congruence seems less palatable when the electorate’s ideal policy undermine fundamental democratic principles.
References


