Judicial Prejudice or Politics:
Which Explains Black-White Gaps in Incarceration Rates?

Kyung Park*

University of Chicago
Harris School of Public Policy

January 2011

(Preliminary Draft)

Abstract

In this paper, I explore the possibility that elected district court judges engage in race-based sentencing due to political incentives rather than individual prejudice alone. The key trade-off is that the incumbent may be willing to deviate from her preferred sentence in order to increase the probability of re-election. Using administrative data from Kansas, I find that black defendants have a 5.3% higher probability of incarceration in comparison with white offenders with identical sentencing criteria. Consistent with the political incentives narrative, the black-white incarceration gap is concentrated among judicial districts that use competitive, partisan elections to select their judges. I also exploit electoral timing and find that judges are differentially punitive against black offenders in the election year. These results suggest that racial disparities in sentencing outcomes may have roots in the politicization of the judiciary rather than individual-specific judicial discretion.

* Email: kpark1@uchicago.edu. I thank Scott Ashworth, Lisa Barrow, Chris Berry, Dan Black, Ethan Bueno De Mesquita, Kerwin Charles, Jeffrey Grogger, Jonathan Guryan, William Howell, Damon Jones, Robert Lalonde, Jens Ludwig, Brian Meltzer, Pablo Montagnes, and numerous seminar participants for helpful comments. I am also grateful for Helen Pedigo, Scott Scultz, Kunlun Chang, and Brenda Harmon at the Kansas Sentencing Commission for their help with the sentencing data.

1. Introduction

In the United States, blacks account for nearly 40% of all prison inmates even though they constitute only 12% of the total residential population. To date, the sentencing literature has primarily focused on partitioning this racial disparity into a component that can be explained by differences in observable characteristics and a piece that remains unexplained. Far less attention is devoted to understanding the mechanisms that generate unwarranted black-white gaps. Current policy presumes that unexplained racial differences are motivated by taste-based prejudice. Since the 1980's, the federal government and numerous states have imposed sentencing guidelines in order to constrain judicial discretion, even though judicial prejudice may not be the fundamental cause. In this paper, I find compelling evidence for an alternative explanation - elected district court judges engage in race-based sentencing due to political incentives rather than individual prejudice alone.

The political motive seems plausible given the extensive literature that suggests elected officials respond to political pressures. The canonical theory posits that an incumbent may deviate from her preferred policy towards the marginal (or median) voter's ideal policy, and thereby increase her probability of re-election (Ferejohn 1986). Numerous empirical studies support this model across varied political institutions. For instance, Snyder and Stromberg (2008) find that congressional representatives exert more effort when their constituents have better information on congressional performance. Gordon and Huber (2004, 2007) find evidence that lower-level district court judges become increasingly punitive as they approach re-election. While these studies demonstrate that political incentives can influence policy, this is the first paper to test whether electoral politics is a viable explanation for black-white gaps in incarceration.

For the analysis, I use administrative data from Kansas which has two key advantages. First, Kansas is 1 of only 2 U.S. states that employs two distinct electoral systems across its 31 judicial districts. In 14 districts, judges are selected via competitive, partisan elections, whereas in the other 17, judges face retention elections, which are typically far less competitive. This variation in electoral systems is a pre-requisite to answering this research question. Second, the Kansas data provides judge identifiers which allow me to adjust for unobserved judge-specific characteristics that relate to both race and sentencing. To my knowledge, a nationally representative sample of criminal cases with accompanying judge identifiers does not exist.

The first half of the paper explores whether black-white incarceration gaps exist and if so, whether they are plausibly due to race. Racial differences in sentencing outcomes are notoriously difficult to interpret since all of the information used in judicial sentencing is rarely observed in data. I address this omitted variables problem by exploiting the fact that Kansas is a guideline state. In Kansas, all felony crimes are categorized into distinct severity-by-criminal history "cells", which are accompanied by a presumed sentence along both extensive (prison or probation) and intensive (sentence length) margins.[1] Conditioning on the sentencing cells produces estimates that compare outcomes between black and white offenders who share *identical* criminal histories and criminal severity levels. In this framework, the outcomes of white offenders are more likely to represent valid counterfactuals for the outcomes of black defendants in the absence of race.

[1] An important feature of the guidelines is that crimes are pre-categorized into 1 of 10 distinct severity levels by state law. Judges do not place individual offenders into specific severity-criminal history cells. As for criminal history, field officers conduct pre-sentencing investigations (PSI's) in which they compile official criminal history reports that are then submitted to the judge.

I estimate an adjusted 1.3 percentage point black-white gap in incarceration rates, which represents roughly a 5.3% increase in the likelihood of incarceration for black defendants.[2] This point estimate is statistically significant and robust across numerous specifications including one that allows case-level observables (such as type of lawyer and crime) to vary with the sentencing cells. This specification provides important reassurance that unobserved racial differences *within* sentencing cells are not driving the results. These estimates suggest that even though Kansas imposes sentencing guidelines, 15% of the unadjusted black-white gap in incarceration remains unexplained.

In the second half of the paper, I examine political incentives as a potential mechanism using two distinct identification strategies. The first is a difference-in-difference approach that basically compares the covariate-adjusted racial differences in incarceration rates across electoral systems. All models include district fixed effects in order to adjust for the fact that electoral systems are not randomly assigned. I find that the black-white gap is 1.5 percentage points higher among competitive versus retention districts, which implies that electoral concerns explain the entire residual black-white gap.

As a second identification strategy, I exploit variation in political incentives associated with electoral timing. Numerous studies have documented that political incentives strengthen as the electoral cycle nears its end (Levitt (2002)). Electoral timing provides an intuitive test of political incentive effects; if race-based sentencing is politically motivated, then we should expect to see a concentrated increase in the black-white gap in competitive districts and in the election year. Indeed, the point estimates suggest that this is true, even after conditioning on judge fixed effects.

[2] The mean incarceration rate for white defendants is 0.243.

These findings highlight an interactive nature of prejudice that is sometimes overlooked by the literature. It suggests that prejudiced persons (in this case – the marginal voter) can change market incentives in ways that induce *unprejudiced* persons to behave as though they were in fact prejudiced. Traditional frameworks often presume that prejudice operates through bilateral exchanges between, say, an employer and her employee. Understanding how prejudice can influence market conditions for unprejudiced actors could shed important light in other applications beyond judicial politics as well.[3]

This paper also provides a concrete example in which increased accountability via partisan elections compromises minority welfare. This departs from standard political agency models in which voters can improve their well-being by inducing higher levels of effort, especially when they can access better information on the elected official's performance (Ferraz and Finan (2008), Snyder and Stromberg (2008)). In this case, electoral accountability has potentially perverse effects. To the extent that highly prejudiced voters demand more severe punishments against black offenders, judicial accountability creates incentives that actually promote race-based sentencing.

The remainder of the paper is structured as follows. In section 2, I provide background information on Kansas' sentencing guidelines and electoral system. Section 3 describes basic features of the data. In section 4, I show descriptive estimates of both the overall and cell-specific black-white gap in incarceration rates. In section 5, I specify the empirical models used to identify political incentive effects and in section 6, I provide the estimates. In section 7, I conclude.

[3] Another example of this is Black (1995). He shows that the presence of prejudiced employers raises job search costs for black workers, which in turn, generates monopsonistic power for unprejudiced employers. Thus, even *unprejudiced* employers will offer black workers lower wages in comparison with white workers with equal ability.

2. Background on Kansas' Sentencing Guidelines and Electoral Systems

Sentencing Guidelines

In 1989, the Kansas state senate passed a bill that founded the Kansas Sentencing Commission (KSC). The KSC's mandate was to develop state sentencing guidelines that promote fair practices, and thereby, reduce the biases that existed in the system at that time. Figure 1 presents the sentencing guidelines that are used to map all crimes into their respective sentencing cells. The three numbers listed in each cell represent the minimum, expected, and maximum sentence duration (in months) that the judge can choose to impose.[4] This affords the judge some discretion along the intensive margin. The figure also shows cells with different colored shadings. These represent different presumptive sentences along the extensive margin; presumptive prison or presumptive probation.[5] Thus, the function of the guidelines is to assign each felony to a severity level-criminal history cell, which in turn, is associated with a pre-determined sentence.

As an example, consider an offender with 1 prior non-person felony who is caught committing theft of $100,000. By law, under Kansas statute 21-3701, any theft of $100,000 or more is automatically categorized as a severity level 5 crime. Regardless of race, our hypothetical offender will be placed in the row 5/column G cell and should expect to face anywhere between 38 to 43 months in prison. The only way a black and white offender within

[4] In multiple count cases, the judge is allowed to exceed the maximum sentence by sentencing the multiple counts consecutively. In these cases, the final sentence length will be the sum of each individual count's duration. There are limitations, however, as the total sentence cannot exceed twice the maximum sentence length of defendant's primary count (i.e. the defendant's most severe transgression).

[5] The third shading represents border box crimes, in which the presumed sentence is prison. However, the judge may freely deviate to probation if she deems that a viable alternative treatment plan exists. Although the border box cells appear to allow for heightened judicial discretion, in practice, supply constraints at local correctional centers significantly limits judicial discretion among border box crimes.

the same sentencing cell can receive different sentences is through judicial discretion; otherwise, black-white incarceration gaps cannot exist.

While the guidelines constrain judicial discretion, it does not eliminate it completely. There are two key margins along which judges are able to deviate from the presumptive sentences. First, judges can formally file for "departures" from the guidelines along either extensive or intensive margins. Departures along the extensive margin are referred to as "dispositional" departures, whereas deviations on the intensive margin are known as "durational" departures.

Departures are not necessarily based on hard, factual evidence. For example, consider Kansas statute 21-4716, which states that a defendant who plays a passive role in a crime may receive a downward departure; in contrast, being a leader or organizer of a crime is considered to be a legitimate aggravating factor that could merit an upward departure. *A priori*, there are no clear legal criteria to guide what might constitute a passive versus a leading role in a crime. Rather, the judge is expected to lean on her expertise and the legal evidence when deciding whether or not to file for departure. Given the room for subjective interpretation, the system requires that all departures are reviewed for approval in formal hearings. However, only upward departures (both dispositional and durational) are subject to appeal. The threat of appeal may explain why only a small fraction (12%) of all departures in the data is upward directional.

The second lever of judicial discretion comes in the form of special rule violations. These rules were designed to flexibly incorporate special circumstances into sentencing outcomes that are not necessarily captured by the grid. Approximately 1/5th of all felonies in the data constitute special rules violations, among which, the most common violation (~78%) is committing a crime while on probation, parole, or on bond. Other violations, such as committing the offense with a

firearm (~8%), felony domestic battery (0.1%), assault or battery of a law enforcement officer (1%), and other violations (12%) represent a minority of all special rules violations. When these special rules apply, judges are able to freely deviate from the presumptive sentences. In comparison with departures, special rules provide a heightened level of judicial discretion because these deviations are neither reviewed nor subject to appeal. Thus, among all other case-level observables (besides the sentencing cells) that could plausibly affect sentencing (such as age, type of crime, total number of counts, type of attorney, and etc.), racial differences in special rules violations have the most potential to explain black-white gaps in incarceration rates.

Competitive versus Retention Elections

In competitive elections, judges run for office in either partisan or non-partisan elections. In partisan elections, a judicial candidate must first win her party's nomination in the primaries, before she can continue on and compete in the general election on Election Day. In non-partisan elections, the judge may also face a challenger in an election, but neither candidate is affiliated with a specific political party. In Kansas, all competitive elections are partisan elections.

In contrast, in retention districts, an incumbent judge will never run in a contested election against an actual challenger. Instead, in order to receive a judgeship, a nominating commission must first recommend the potential judge to the governor. If the nomination is accepted, the judge will then serve a probationary year on the bench. In the following year, the judge faces a retention vote in which voters simply vote "Yes" or "No" on whether the judge should be retained. A simple majority vote share will secure the judge's position, after which she will then face a retention vote every four years.

The theoretical appeal of retention votes is that they hold the judge accountable while simultaneously insulating her from fund-raising, campaigning, and other political pressures that

are often associated with competitive elections. In contested elections, the challenger is believed to play a key role in revealing information regarding the incumbent's performance that otherwise might go unknown.[6] However, there is some empirical evidence that suggests retention systems can also compromise judicial independence.[7] In light of these mixed results, the key question is whether or not retention elections are competitive *within* Kansas.

In Kansas, there is a general sense that they are not. From 1996 to 2004, the incumbent retained her position in all 218 retention votes, winning with an average "Yes" vote share of 76%.[8] To the extent that judicial turnover reflect electoral competition, these statistics support the notion that retention elections in Kansas are relatively low-key affairs. Kansas State Supreme Court Justice Carol Beier also provides some informal evidence on this question. In 2010, Justice Beier drew the ire of conservatives across Kansas for allegedly stalling the prosecution of illegal abortions. Rather than actively campaign against her, the Republican Party tacitly allowed her to retain her seat. University of Kansas Law Professor Stephen Ware explains, "I wouldn't expect opponents of Justice Beier — or any other Kansas justice — to try to defeat her at the ballot box, because the so-called [retention] 'elections' used for Kansas justices are designed to give incumbents an easy win."[9] That the outcomes of higher-stakes State Supreme Court retention

[6] The theme of this literature is that challengers provide either explicit or implicit signals to the voter that the incumbent's performance is not entirely satisfactory. For instance, McCubbins and Schwartz (1984) suggest that challengers play the role of a "fire-alarm", explicitly alerting constituents of instances in which the incumbent performs poorly.

[7] On the one hand, Case-Wrone et al. (forthcoming) find a correlation between state supreme court judicial decisions and public opinion in abortion-related cases within retention states. Gordon and Huber (2004) also find that in Pennsylvania, a retention state, judges become increasingly punitive towards the end of the election cycle in order to signal a firm stance against crime. On the opposite side, Saphire and Moke (2008) find evidence of state supreme court judges in competitive systems changing their decisions in tort cases as they approach re-election, whereas retention judges do not. Hollibaugh (2011) explicitly estimates judicial ideal points and finds election-year shifts exclusively among judges in contested, partisan systems.

[8] I collected the elections data from the Kansas Secretary of State website.

[9] From the Kansas Liberty website. August 21, 2009. http://www.kansasliberty.com/liberty-update-archive/2009/24aug/republicans-largely-staying-out-of-2010-retention-vote-of-justice-beier/ .

votes are *de facto* uncontested, reinforces the likelihood that lower-level district court retention elections are also non-competitive.[10]

3. Data

Sentencing Data

After implementing the guidelines in 1993, the KSC has since collected extensive data in order to track their progress in achieving the initial goal of equitable sentencing. Among these data are sentencing "journals" which require judges to document the sentencing outcomes for all convicted felons. These journal entries consist of important case-level observables, such as criminal severity, criminal history, type of attorney, sentence date, and most importantly, the identity of the presiding judge, which are often omitted in other sentencing data sets. In addition, the KSC reserves the right to order the judge to re-do her journal entry in the event of obvious inconsistencies. This bureaucratic feature may mitigate measurement error concerns to some degree. To introduce this data source, figure 1 shows the first page of a blank 2011 journal entry.

Another attractive feature of the Kansas sentencing data is that it includes the entire universe of convicted felons in Kansas spanning the years 1998 to 2003. From this sample of 55,320 cases, I exclude those that have missing values for criminal severity level or criminal history. These cases are predominantly felony DUI's which are "off-grid" crimes, or in other words, sentenced outside of the sentencing guidelines.[11] I also restrict the final sample to include only black and white defendants, who account for 90% of all cases. I exclude Hispanics (9%), Native Americans, Asians, and Others to focus exclusively on the black-white gap. Finally, I

[10] In the Kansas' history, only 1 state Supreme Court justice has ever lost a retention vote. R.E. Miller was ousted in 1980 after allegedly threatening to burn down the attorney general's house.

[11] Other off-grid crimes include capital murder, child rape, and treason. This restriction is motivated by the fact that the identification of the black-white gaps rely on being able to condition on valid sentencing cells. The potential cost of this restriction is that some off-grid crimes (such as capital murder) may be more likely to attract media attention and thus also hold more political influence.

drop cases in which the presiding judge has fewer than 25 total cases. This is motivated by the fact that estimates of the judge-specific black-white gaps are likely to be dominated by sampling error for judges with very little experience. After imposing these restrictions, the final sample used in estimation includes 143 judges and 42,800 total cases.

Table 1 shows descriptive means for various observables separately by race. The statistics suggest that black defendants account for roughly 1/4th of all felons even though black persons represent less than 5% of Kansas' total population. Multiple observables suggest that most felonies are actually low severity crimes. For instance, 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 defendants 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, to be in a competitive district, and less likely to settle.

4. Estimating the Black-White Gap in Incarceration Rates

Empirical Framework

The black-white incarceration gap can be estimated using the following basic regression model:

$$(1)\ y_i = \alpha + \beta B_i + \rho_c + X\gamma + \varepsilon_i,$$

where y_i is an indicator for whether the defendant is incarcerated, B is an indicator for whether the defendant is black, ρ_c represents indicator variables for each cell in the sentencing guidelines, X includes a vector of case-level characteristics, such as an indicator for special rule violations. The ε_i can broadly be thought of the factor that drives the unexplained variation in incarceration.

A fundamental challenge to identification is that the judge's full information set used in sentencing is usually only partially observed in data. This is problematic to the extent that there are unobserved differences across race that also influences judicial sentencing. In this case, estimates of β will over or under-state the true role of race in sentencing disparities. The guidelines help mitigate these concerns to some degree. By conditioning on the sentencing cells, ρ_c, black defendants are directly compared to white offenders with *identical* criminal histories and who have also committed similarly severe crimes. In this framework, the sentencing outcomes of white defendants are more likely to represent a valid counterfactual for black defendants if they were, in fact, white. Unless unobserved racial differences exist *within* cell, including ρ_c in the conditioning set provides additional credibility for the conditional mean zero assumption used in OLS.[12]

Column 1 of table 2 begins by showing the unadjusted black-white incarceration gap. Given that the mean incarceration rate for black defendants is 0.243, the 8.5 percentage point estimate suggests that black offenders have a 35% higher likelihood of incarceration in comparison with white offenders, on average.

Column 2 runs a specification that includes indicators for criminal severity but intentionally omits criminal history altogether. In column 3, I then add include both criminal severity and criminal history in the same model. The motivation behind these two specifications is to see how much racial differences in prior criminal history can explain the unadjusted black-white gap. Several studies emphasize that omitting criminal history introduces severe bias in

[12] Whether or not to use matching or OLS is an open question, and in practice, estimates from the two methods can diverge depending on 1) their respective weighting schemes and 2) the degree to which the cell-specific black-white gaps vary across the sentencing grid (Angrist and Krueger (1999)). For instance, if the black-white gaps are atypically low among sentencing cells that are highly populated by black defendants, then OLS will tend to overstate the effect of race relative to matching. As it turns out, the matching and OLS estimates in this analysis are nearly identical.

estimation, and indeed, my estimates suggest this is true. The estimated black-white gap falls only by 1.1 percentage points, from 8.4 to 7.4 percentage points, when I control for criminal severity exclusively. However, when I include both criminal severity and criminal history in the conditioning set, the point estimate falls by nearly 73% (relative to the unadjusted gap) to 2.3 percentage points. This corroborates that a failure to condition on criminal history will lead to substantial over-estimates of the black-white gap. This is also consistent with the view that judges view criminal history as a strong predictor of future recidivism.

In column 4, I replace the severity level and criminal history indicators with a set of indicator variables for each sentencing cell in the grid. This specification is more flexible in that it allows for interactions between severity and criminal history. This may be important to the extent that judges are especially intolerant of highly severe crimes when they are committed by repeat offenders (Mustard (2001)). In practice, allowing for non-linearities have a negligible impact on the results. The estimate is 2 percentage points which is just 0.3 percentage points below the specification that restricts these types of interactions to be zero.

The next specification adds all of the defendant's demographic characteristics available in the data (other than race) - the defendant's age and gender. The estimate in column 5 is essentially the same as the previous specification which is not surprising given that the age and gender composition is similar across race.

The next 5 specifications sequentially adds the following case-level covariates - an indicator for special rule violations, the total number of counts, and indicators for the type of counsel (public versus private attorney), type of crime (violent versus non-violent), and whether the case is resolved via a plea bargain. Given that black offenders are associated with more incriminating case-level characteristics, it seems plausible that conditioning on these observables

could help explain the black-white gap. The point estimates from these specifications range from 1.3 to 1.8 percentage points and are all statistically significant at the 5% level. Moreover, the 95% confidence interval on the lowest point estimate still covers the previous estimates that include a sparser set of controls. Overall, these estimates imply that even after conditioning on a rich set of conditioning variables, roughly 15% the black-white gap in incarceration remains unexplained. Using my preferred specification in column 10, this translates into a 5.3% higher probability of incarceration for black offenders in comparison with whites who have identical sentencing criteria. (See Appendix Table 1 for additional robustness checks.)

Cell-Specific Black-White Gaps

In this section, I plot cell-specific black-white gaps. The purpose of this exercise is twofold; first, to examine the degree of heterogeneity in the cell-specific black-white gaps across the sentencing grid, and second, to gauge to what extent cell-specific gaps are explained by observed racial differences *within* cell.

The latter has important implications for identification. The key OLS assumption,

$$(A1) \qquad E[\varepsilon_i | B, \rho_c, X] = 0,$$

is not valid to the extent that unobserved racial differences at the within cell level also influence judicial sentencing. While this is difficult to test explicitly, an informal check is to estimate cell-specific black-white gaps with and without case-level controls. This is equivalent to running equation (1), but allowing for interactions between the sentencing cells and various case-level observables. If observed racial differences within cell do not explain the cell-specific BW gaps, then this will provide some reassurance that unobserved racial differences do not as well.

I also plot the accompanying OLS weights associated with each individual cell. Angrist and Krueger (1999) show that the OLS estimator can be re-written as a weighted average of the cell-

specific black-white gaps, in which the weights are proportional to the cell-specific conditional variance of race

$$w_c^{OLS} = \frac{p(B=1|c)\big(1-p(B=1|c)\big)p(c)}{\sum_c p(B=1|c)\big(1-p(B=1|c)\big)p(c)}.$$ [13]

The purpose is to formally show which cells contribute the most to the overall OLS estimates of the black-white gaps. It turns out that OLS places more weight on cells associated with less severe crimes. This differential weighting is due to the fact that the majority of offenses are concentrated in low severity crimes (see appendix figure 1) and because the cell-specific conditional variance of race is more or less even across the sentencing grid (see appendix figure 2).

Figure 3a shows the unadjusted cell-specific black-white gaps and the associated weights for non-drug related crimes. There are two notable features of this plot. First, the positive black-white gaps tend to be concentrated towards the low to middle regions of the support. On some level, this is not too surprising. Incarceration rates are close to 1 among cells on the far right regions, which mechanically limits the magnitude of their black-white gaps. On the other hand, it is surprising to see positive and statistically significant black-white gaps among numerous cells on the far left, which typically reflect less severe crimes. For instance, the black-white gap in the 8th lowest severity level (out of 10) and 8th lowest criminal history (out of 9) is 7.8 percentage points with a p-value less than 0.01. This pattern is especially pertinent in light of numerous sentencing studies that focus exclusively on assault, robbery, sexual assault, burglary, and other high severity crimes. This plot suggests that neglecting low severity, non-drug related crimes

[13] Angrist and Kruger show this is true under the assumption of linearity, which in this case is trivially satisfied since black is binary variable.

could greatly understate the magnitudes of the black-white gap. The fact that OLS essentially places zero weight on presumptive prison cells strongly reinforces this view.

Second, it is worth noting that among some cells, the estimated black-white gaps are actually negative. While most of these negative estimates are nowhere near statistical significance, the sign still raises two interesting possibilities. One interpretation of a negative black-white gap is reverse judicial discrimination. This is consistent with Becker's model of discrimination in the sense that he does not restrict prejudice to be unidirectional. Alternatively, a negative black-white gap could be the result of downward adjustments in the sentencing of black offenders in response to police or prosecutorial prejudice. For example, a judge may prefer leniency against a black offender who she feels is wrongly charged with a worse crime than is deserved. The two models have different implications for interpretation. In the first model, my estimates would accurately reflect the effect of judicial discretion on the black-white gap. However, prejudice in earlier stages of the sentencing process could lead to estimates of the black-white gap that are understated.

Figure 3b plots both the unadjusted and covariate-adjusted cell-specific black-white gaps for non-drug related crimes. The covariates include all of the usual case-level observables; age, gender, special rules indicator, the total number of counts, type of counsel, whether the case is settled, and an indicator for violent crime. In some cells, the adjusted gaps are lower than the unadjusted ones. This is expected since black offenders are associated with observables that are positively correlated with incarceration. However, the magnitudes of the declines are fairly limited. For many of the cells, sizeable black-white gaps remain even after adjusting for within-cell differences in covariates. In fact, the weighted sum of the cell-specific gaps suggests an overall 1.7 percentage point black-white gap, which is similar with the estimates that restrict the

interactions between the sentencing cells and X's to be zero. (See appendix figure 3a and 3b for similar plots for drug related crimes.)

Heterogeneous Effects of Race

Table 3 presents interaction effects of race. The estimates show that most of the interactions are fairly small and not statistically significant. The black-white gap does not seem to substantially vary with gender, type of crime, type of counsel, or plea status. The absence of a person-black interaction suggests that judges are not especially punitive against black offenders who commit violent crimes. There is, however, a statistically significant inverse relationship with age, suggesting that younger black offenders tend to be punished more severely than their elder counterparts. Finally, the interaction with special rules violations suggests that the entire black-white gap is among special rules cases. This is not surprising since special rules cases represent an important margin along which judges are able to exercise discretion.

5. Empirical Models of Political Incentives

Differences-in-Differences

To assess the importance of political incentives, I specify a statistical model in which the expected sentence is a function of the defendant's race (*b*) and allows the race effect to vary across electoral systems (e_d is an indicator that equals 1 for competitive districts):

$$(2) \quad y_i = \alpha + \beta_1 b_i + \beta_2 b_i * e_d + X\gamma + \rho_c + \delta_d + \varepsilon_i.$$

The model also conditions on sentencing cells (ρ_c) and other case-level covariates which ensures that we are comparing defendants with identical observed sentencing criteria. The δ_d represent district-level fixed effects that allow the level of the conditional expectation to vary with each district. The district fixed effects are important since electoral systems are not randomly

assigned. If districts chose their electoral system based on time-invariant district-level characteristics, then the inclusion of δ_d should account for endogenous selection.

The primary parameter of interest is β_2 and can be interpreted as the differential black-white gap across competitive versus retention districts. An estimate of $\beta_2 > 0$ would suggest that black defendants have higher probabilities of incarceration in districts with competitive elections. The β_1 parameter estimate describes the black-white gap in incarceration among retention districts.

Electoral Cycles

As a second identification strategy, I exploit variation in political incentives that is associated with electoral timing. The value of this approach is that it can distinguish between political incentives and alternative explanations that are static in nature. For instance, one explanation for a correlation between voter prejudice and race-based sentencing is endogenous sorting on prejudice; that is, prejudiced judges may choose to live in prejudiced places. In this case, the black-white gap should not systematically vary across the electoral cycle. Rather, prejudiced judges should engage in race-based sentencing consistently across time. In contrast, the political incentives model has the sharp implication that the black-white gap should respond the most in competitive districts and in the election year. This means that any unobserved racial difference that correlates with electoral systems cannot be a valid explanation unless they also co-vary with the electoral cycle.

The specific model is similar to the generalized difference-in-differences model used by Jacobsen, Lalonde, and Sullivan (1993):

$$(3) \quad y_i = \alpha + \beta_1 b_i + \sum_{k \geq -1}^{0} D_i^k \, \delta_k + \sum_{k \geq -1}^{0} D_i^k * b_i \gamma_k + X\gamma + \rho_c + \delta_d + \mu_t + \varepsilon_i$$

Here, D_i^k is an indicator variable that equals 1 if the case is sentenced k years from the election year. Specifically, $D_i^{-1} = 1$ for cases that are sentenced in the year prior to the election year and $D_i^0 = 1$ refers to cases sentenced in the election year. I have restricted the effects of the first two years of a judge's term to be zero; that is, for $k \in \{-3, -2\}$, $D_i^k = 0$. This restriction allows the parameters of interest γ_0 and γ_{-1} to be interpreted as the change in the black-white gap in the election year and in the year prior to the election in comparison with the predicted black-white gap from the first two years in office. The δ_k parameters have similar interpretation but reflect electoral cycle effects for white defendants. In some specifications, I allow the D_i^k timing indicators to differ by electoral system. This unrestricted specification explicitly tests whether or not political incentives bind even in retention districts.

The X and ρ_c include the usual covariates and sentencing cell indicators. The δ_d's are district fixed effects which allow for district-specific intercepts. I also run specifications that replace the district indicators with judge fixed effects. This model is especially important since it uses only within-judge variation across the electoral cycle. Finally, the μ_t's are year indicators and adjust for the general time pattern in incarceration. The year effects can be identified because judicial elections are staggered across time. Elections for the attorney general, however, are not staggered, which implies that the year effects will help adjust for prosecutorial electoral cycle effects.

Despite the empirical advantages of the electoral timing specification, there are potential sources of bias that warrant discussion. Generally speaking, any mechanism that leads to black offenders having worse unobservable characteristics in the election period is problematic for identification. As a check, I will also run equation (3) but replace the dependent variable with a series of case-level observables. These estimates will provide evidence on whether black

offenders have differentially worse characteristics in the election period relative to earlier points in the cycle. While this does not guarantee identification, if observable characteristics to do not differentially vary across the electoral cycle by race, then this will provide some reassurance that the unobserved factors do not as well.

6. Empirical Estimates of Political Incentives

Table 4 presents the difference-in-difference estimates from equation (2). Estimates in the first row represent the estimated black-white incarceration gap for retention districts. The second row shows the difference in the black-white gap between competitive versus retention districts. The sum of the two rows provides estimates of the black-white gaps among competitive districts. Finally, the 3rd row shows the p-values from statistical tests of whether the sum is equal to zero.

Across columns, the different specifications represent various perturbations of the conditioning set. Column 1 includes district fixed effects, sentencing cell indicators, and an indicator for special rules violations. Column 2 adds defendant demographic characteristics and in column 3, the remaining case-level X's are included. The final specification replaces the district indicators with judge fixed effects.

The estimates are fairly consistent across the different specifications. The estimate of the black-white gap in retention districts ranges from 0 to -0.7 percentage points, but is only marginally significant in one specification. In contrast, the estimates of the black-white gap in competitive districts range from 0.8 to 1.5 percentage points and their associated p-values range from 0.001 to 0.059. Finally, the differential black-white gap in competitive versus retention districts range from 1.5 to 1.7 percentage points and all of the point estimates are highly statistically significant. These results provide suggestive evidence that black offenders are sentenced more severely in districts that rely on partisan elections to select their judges.

The next table shows estimates that exploit variation in political incentives associated with the electoral cycle. The first two specifications restrict the timing indicators for retention districts to be zero. In these models, the year effects are identified by the retention districts.

The first two rows of column 1 show that the incarceration rate among white defendants does not move with the electoral cycle. The point estimates on both the year prior to the election and the election year indicators are close to zero and are not statistically significant. In contrast, the incarcerate rates among black offenders rise as the judge closes in on re-election. The black-white gap increases by 1.5 and 2.5 percentage points in the year prior to the election and in the election year, respectively. While the black-year prior indicator is only marginally significant, the black-election year interaction is statistically significant at the 5% level.

Column 2 runs the same specification but replaces the district indicators with judge-fixed effects. Using within-judge variation makes little difference to the estimated effects. The most notable difference is that the black-year prior estimate falls slightly and is no longer statistically significant at the 10% level. However, the 2.6 percentage point estimate of the black-election year interaction is highly statistically significant and reinforces the finding that judges become increasingly punitive towards black offenders closer to the election year.

The next two specifications allow for differential electoral timing effects for competitive versus retention districts. The estimates for the competitive districts are nearly identical to those generated by the more restrictive model. Comparing across columns 3 and 4, it is also clear that using within-judge variation has a negligible influence on the results.

The interesting features of columns 3 and 4 are the timing indicators among retention districts. The point estimates for both the main electoral timing effects and the interactive timing effects with race are generally close to zero and are nowhere near statistical significance. This is

true regardless of whether or not I condition on judge-fixed effects. This suggests that there is no evidence for either a general rise or a race-specific increase in incarceration in the two years leading up to re-election. These results provide more formal evidence that retention elections are not competitive in Kansas. This also suggests that retention districts do provide a valid counterfactual for what the sentencing outcomes in competitive districts might be in the absence of electoral concerns.

Potential Bias

Table 6 shows estimates from a series of regressions in which I replace the dependent variable in equation 3 with case-level observables. This exercise will allow me to see if black offenders have differentially worse observable characteristics in the election year. The first two rows show the estimates of the black-electoral timing interactions. The 3rd row provides the sample means of the dependent variable among white offenders in order to assess the magnitudes of these coefficients. The case-level characteristics include person crime, severity levels (separately for non-drug and drug crimes), criminal history, special rules violation, total number of counts, type of attorney, and plea status. Going across the columns, the estimates show no evidence of black offenders having differentially worse case characteristics in the 2 years leading up to re-election. If anything, some of the point estimates suggest that black offenders committed slightly less severe crimes, had slightly better records, violated fewer special rules, and became more likely to obtain private counsel both in the election year and in the year prior. These estimates mitigate concerns that differential selection in the election year is driving the electoral timing results.

Election Year Effects by Proxies of District-Level Prejudice

Next, I test whether the political incentive effects (measured by the election year effects) are more pronounced in districts where the marginal voter is likely to be more prejudiced against blacks. Ideally, I would be able to measure the prejudice levels of each district's marginal voter explicitly. In practice, a sufficiently large data set that measures individual prejudice at the judicial district level does not exist. Instead, I use 3 different proxies of district-level prejudice. While none of them are perfect, I use them to see whether there is any evidence that could be consistent with political incentives being differentially stronger in more prejudiced districts.

The first is the district's share of black constituents over the age of 25. In areas with relatively fewer black constituents, the median voter may be more prejudiced against blacks, to the extent that a) each race prefers her own (i.e. same-race preferences) and b) the prejudice of white constituents is distributed uniformly across Kansas. Of course, neither of these assumptions needs to be true. If familiarity breeds contempt (i.e. cross-race preferences) and/or less prejudiced whites are disproportionately drawn to more rural areas of Kansas, then fraction black could be positively related to prejudice. The point estimates should provide some guidance on the true direction of these unknown correlations.

I construct a second proxy using data from the Implicit Associations Test (IAT), which is an interactive online test designed to measure racial prejudice.[14] The respondent's age, education, and race are included in the data. I run regressions of the prejudice measure on these demographic characteristics and capture the parameter estimates. I then take these estimates and project them onto district-level means of age, education, and race in order to compute predicted

[14] The IAT asks respondents associate "bad" or "good" attributes (such as terrible, failure, glorious, peaceful) with race. The key idea behind the IAT is that if individuals harbor strong, negative stereotypes towards black persons, then they may have a more difficult time associating good words to "African-American or Good" than they do with "European-American or Good". The IAT uses differential *response times* to capture the degree of cognitive dissonance in associating "good" or "bad" adjectives across race. This provides an *implicit* measure prejudice.

prejudice-levels for Kansas' 31 judicial districts. Provided that there is sufficient cross-district variation in demographic composition, this prejudice index may be able to distinguish low prejudiced districts from high ones. In the data, districts with less education, older persons, and a smaller share of black constituents are expected to have higher prejudice levels on average.

The third proxy uses the change in vote share for the Democratic Party in the 2008 presidential election in comparison with the previous 4 presidential elections to capture cross-district variation in racial prejudice. Kansas is a deeply red state. From 1992-2004, the Democratic presidential candidate received only 36% of the state's total votes on average. In 2008, Barack Obama received sizeable increases in support in some judicial districts, but in roughly 1/3rd of the districts, support for the Democratic Party declined or roughly stayed the same. To the extent that Obama's race and ethnicity influenced voting behavior, cross-district variation in the support for Obama could reflect a district's prejudicial views towards blacks.

Table 7 presents estimates of the black-election year interactions separately for districts whose share of black constituents, predicted prejudice index, or change in Democratic vote share is below or above the median district. Across all 3 proxy measures, the point estimates uniformly suggest that the election year effects on the black-white incarceration gap are differentially higher in districts with low share of black constituents, high predicted prejudice, and low support for Obama. For instance, among low fraction black districts, the black-white gap rises between 7.1 to 7.5 percentage points in the election year whereas among high fraction black districts, the election year effects are roughly 5 percentage points lower. These differences are marginally significant when comparing across districts by fraction black or support for Obama, but is highly statistically significant along the predicted prejudice index. On the whole, the magnitudes

suggest that political incentives effects on race-based sentencing may be stronger in districts where the marginal voter is expected to be more prejudiced against blacks.

7. Discussion and Conclusion

This paper contributes to the discourse on black-white inequality along several dimensions. In the crime literature, there is considerable uncertainty regarding the role of judicial prejudice in generating racial disparities across various sentencing outcomes. This ambiguity is rooted in the ever-present possibility that judges are differentially harsher against black offenders for legitimate legal factors that are unobserved in data. In this paper, I use Kansas' sentencing guidelines to construct estimates of the black-white incarceration gap that essentially compare black and white offenders with *identical* observed sentencing criteria. Numerous robustness checks mitigate endogeneity concerns, while bolstering the credibility of the estimates. On the whole, I find that as much as 15% of the total unadjusted black-white gap in incarceration rates cannot be explained by racial differences in the set of case-level characteristics available in the data.

I also show that the unwarranted racial disparity is driven by differential sentencing among low severity crimes. This result implies that black offenders are disproportionately imprisoned for crimes that were never intended for incarceration in the first place. Methodologically speaking, this is relevant as the sentencing literature routinely restricts attention to high severity crimes, which helps understate estimates of the black-white gap.

A limitation of this paper is that the net welfare effects of judicial politics are not clear. On the one hand, the standard rational choice theory of crime predicts that criminal activity should decrease when its expected costs rise. Levitt (1996) finds empirical evidence that increases in imprisonment cause a reduction in crime. If criminals are sufficiently forward

looking, black offenders might respond to electoral politics by either decreasing crime in election years or substituting their criminal activity to earlier years. If judicial politics reduces crime without inter-temporal displacement, then the costs associated with electing judges would be offset by the social gains associated with a fall in crime. Future research should examine whether criminals endogenously respond to judicial politics in order to develop a clearer characterization of its overall social welfare effects.

On the other hand, recent research has shed new light on negative externalities associated with incarceration. For instance, Charles and Luoh (2010) find that higher male incarceration changes marriage markets in ways that lower the probability of marriage and reduces the quality of matches that do occur. This compounds the detrimental effects that incarceration may have in other areas of life, such as economic activity. Freeman (1992) finds that arrests reduce prospects of future labor market success either through skills depreciation or by raising the costs of job search. If employers view prison records as negative signals of productivity, then incarceration could reduce the quality of wage offers upon release.[15] Given that roughly half of all U.S. states select their judges via partisan or non-partisan elections, my results suggest that judicial politics may have an indirect hand in fueling racial inequality across a variety of social outcomes.

While the welfare effects of judicial politics remain an open question, these results have clear implications on the merits of electoral accountability. Even among low-level district court judicial elections, I find that politics can threaten judicial independence in ways that compromise minority rights. Broadly speaking, this is problematic since an independent judiciary has long been viewed as a cornerstone of a well functioning democracy due to its ability to separate power across different branches of government (Locke (1690), Montesquieu (1748), Madison,

[15] The negative relationship between incarceration and labor market outcomes has been debated in the literature. Grogger (1995) finds that the negative impact of arrests on earnings is short-lived.

Hamilton, and Jay (1788)). In the context of racial inequality, the judiciary duty of subjecting laws to constitutional review has led to numerous reforms (such as Brown v. Board) that protect minority rights. This paper illustrates how political pressures can undermine fundamental principles of our democracy through the erosion of judicial independence.

Moreover, the judiciary has experienced a sharp rise in politicization over the past 20 years. From 1999-2008, state supreme court candidates raised nearly $200 million in campaign funds, which is more than double the amount from the previous decade. Two recent Supreme Court rulings, *Minnesota v. White* and *Citizens United* may have facilitated the escalation of judicial campaigns even further. Retired Supreme Court Justice Sandra Day O'Connor, who voted for the majority decision, has said, "This rise in judicial campaigning makes last week's opinion in *Citizens United* a problem for an independent judiciary. No state can possibly benefit from having that much money injected into a political campaign." The consequences of judicial politics may become even more pronounced in higher-stakes, State Supreme Court elections in coming years.

References

Abrams, David S., Bertrand, Marianne, and Mullainathan, Sendhil, "Do Judges Vary in Their Treatment of Race?" (2010). *Scholarship at Penn Law*. Paper 364.

Alesina, Alberto and Eliana La Ferrara. 2011. "A Test of Racial Bias in Capital Sentencing." Working Paper.

Anderson, James M., Kling, Jeffrey R., and Stith, Kate. "Measuring Interjudge Sentencing Disparity: Before and after the Federal Sentencing Guidelines." *Journal of Law and Economics* 42 (1999): 271–308.

Antonovics, Kate L. and Knight, Brian G. "A New Look at Racial Profiling: Evidence from the Boston Police Department." National Bureau of Economic Research, Inc., NBER Working Papers: No. 10634, 2004. (Fix this citation.)

Becker, Gary S. [1957] 1971. The Economics of Discrimination. 2nd ed. Chicago: Univ. Chicago Press.

Bertrand, Marianne, Dolly Chugh, and Sendhil Mullainathan. 2005. "Implicit Discrimination." *The American Economic Review*, Vol. 95 (2), pp. 94-98.

Black, Dan A. 1995. "Discrimination in an Equilibrium Search Model." *Journal of Labor Economics* 13(2): 309-334.

Black, Duncan. 1948. "On the Rationale of Group Decision-making." *Journal of Political Economy* 56(1):23–34.

Bushway, Shawn D. and Anne M. Piehl. "Judging Judicial Discretion: Legal Factors and Racial Discrimination in Sentencing." *Law and Society Review*, Vol. 35 (4), pp. 733-764, 2001.

Canes-Wrone, Brandice, Tom S. Clark, and Jee-Kwang Park. Forthcoming. "Judicial Independence and Retention Elections." *Journal of Law, Economics, and Organization*.

Card, David, Alexandre Mas, and Jesse Rothstein. 2008. "Tipping and the Dynamics of Segregation." *Quarterly Journal of Economics*. 123, no. 1 (February): 177-218.

Charles, Kerwin K., and Jonathan Guryan. 2007. "Prejudice and the Economics of Discrimination." *Journal of Political Economy*.

Charles, Kerwin K., and Jonathan Guryan. 2011. "Studying Discrimination: Fundamental Challenges and Recent Progress." NBER Working Paper.

Stephen Coate and Glenn C. Loury 1993. "Will Affirmative-Action Policies Eliminate Negative Stereotypes?" *The American Economic Review*, Vol. 83, No. 5 (Dec., 1993), pp. 1220-1240

Dharmapala, Dhammika and Ross, Stephen L. “Racial Bias in Motor Vehicle Searches: Additional Theory and Evidence.” *Contributions to Economic Analysis & Policy*, 2004, 3(1), pp. 1–21.

Downs, Anthony. 1957. An Economic Theory of Democracy. Addison-Wesley Publishing.

Ferraz, Claudia and Frederico Finan. “Exposing Corrupt Politicians: The Effects of Brazil’s Publicly Released Audits on Electoral Outcomes.” *Quarterly Journal of Economics*. 2008. Volume 123, Issue 2. Page 703-745.

Florencio López de Silanes, Rafael La Porta , Cristian Pop-Eleches, and Andrei Shleifer . "Judicial Checks and Balances" *Journal of Political Economy* 112 (2004): 445-470.

Freeman, Richard B. (1992) “Crime and the Employment of Disadvantaged Youth. In Urban Labor Markets and Job Opportunity, George Peterson and Wayne Vroman, eds. Washington, DC: Urban Institute Press, 201-237.

Grogger, Jeffrey. (1995) “The Effect of Arrests on the Employment and Earnings of Young Men,” *Quarterly Journal of Economics*,” February, 51-71.

Hofer, Paul J.; Blackwell, Kevin R.; and Ruback, R. Barry. “The Effect of the Federal Sentencing Guidelines on Inter-judge Sentencing Disparity.” *Journal of Criminal Law and Criminology* 90 (1999): 239–322.

Jacobson, Louis S., Robert J. LaLonde, and Daniel G. Sullivan. “Earnings Losses of Displaced Workers.” *The American Economic Review*. Vol 83, no 4, Sept 1993.

Kessler, Daniel P., and Piehl, Anne Morrison. “The Role of Discretion in the Criminal Justice System.” *Journal of Law, Economics, and Organization* 14 (1998): 256–76.

Kleck, Gary. “Racial Discrimination in Criminal Sentencing: A Critical Evaluation of the Evidence with Additional Evidence on the Death Penalty.” *American Sociological Review* 46 (1981): 783–805.

Levitt, Steven D. “Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime.” *The American Economic Review*. Vol. 87, no. 3, June 1997.
Locke, John ([1690], 1976), The Second Treatise of Government, ed. J. W. Gough. Oxford, UK: Basil Blackwell.

Madison, John, Alexander Hamilton, and John Jay ([1788], 1961), The Federalist Papers, New York, NY: New American Library.

Marsh, H.L. (1991) ‘A comparative analysis of crime coverage in newspapers in the United States and other countries from 1960–1989: a review of the literature’, Journal of Criminal Justice, 19(1): 67–80.

Montesquieu, Charles de Secondat (1748), The Spirit of the Laws, Paris.

Mustard, David B. "Racial, Ethnic, and Gender Disparities in Sentencing: Evidence from the U.S. Federal Courts." *Journal of Law and Economics*, Vol. 44, pp. 285-314, 2001.

Payne, A. Abigail. "Does Inter-judge Disparity Really Matter? An Analysis of the Effects of Sentencing Reforms in Three Federal District Courts." *International Review of Law and Economics* 17 (1997): 337-66.

Reiner, R., Livingstone, S. and Allen, J. (2000a) 'Casino culture: media and crime in a winner-loser society', in K. Stenson and D. Cowell (eds), Crime, Risk and Justice. Cullumpton: Willan.

Saphire, Richard B. & Paul Moke. 2008. "The Ideologies of Judicial Selection Empiricism and the Transformation of the Judicial Selection Debate." *University of Toledo Law Review* 39: 551-590.

Schanzenbach, Max. "Racial and Sex Disparities in Prison Sentences: The Effect of District-Level Judicial Demographics." *Journal of Legal Studies*, Vol. 34, pp. 57-92, 2005.

Sunstein, Cass R., David Schkade, Lisa M. Ellman, and Andres Sawicki. 2006. "Are Judges Political? An Empirical Analysis of the Federal Judiciary." Washington, DC: Brookings.

Data Appendix

Kansas Sentencing Data

The Kansas sentencing data (provided by the Kansas Sentencing Commission) includes the entire universe of convicted felons in Kansas spanning the years 1998 to 2003. From this sample of 55,320 cases, I exclude those that have missing values for criminal severity level or criminal history. These cases are predominantly felony DUI's which are "off-grid" crimes, or in other words, sentenced outside of the sentencing guidelines.[16] I also restrict the final sample to include only black and white defendants, who account for 90% of all cases. I exclude Hispanics (9%), Native Americans, Asians, and Others to focus exclusively on the black-white gap. Finally, I drop cases in which the presiding judge has fewer than 25 total cases. This is motivated by the fact that estimates of the judge-specific black-white gaps are likely to be dominated by sampling error for judges with very little experience. After imposing these restrictions, the final sample used in estimation includes 143 judges and 42,800 total cases.

Kansas Election Data

I collected Kansas 1996-2004 election data from the Kansas Secretary of State website. The data includes 349 judge-by-election observations. The website contains information on the judge's district, division, political party affiliation, votes received, total votes in the election, and the number of votes the opposition received when the election is contested.

NHGIS

The National Historical Geographic Information System (NHGIS) provides publicly available data on county-level demographic characteristics. I compiled information on the county-level age, race, and education distribution for all counties in Kansas. I then aggregate these data to the judicial district level.

Implicit Association Test

In the IAT, individuals are asked to associate "bad" attributes (such as terrible and failure) with pictures of black or white persons. Respondents are also asked to classify "good" attributes (such as wonderful and joy) with pictures of either black or white persons.[17] Respondents who exhibit relative ease in associating bad words to blacks in comparison with bad words to whites display more implicit prejudice against blacks. The differential response times provide an *implicit* measure as to whether the respondent harbors negative, positive, or neutral stereotypical associations towards blacks or whites.

The IAT is an online test. To adjust for potential endogenous selection, I employ Inverse Probability Weights. Selection probabilities into the IAT are estimated non-parametrically; specifically, within age-race-education-puma cells. This is possible because the number of all IAT respondents in each cell is known and the total number of persons in each cell can be estimated using Census data. This method resolves the selection problem so long as selection is

[16] Other off-grid crimes include capital murder, child rape, and treason. This restriction is motivated by the fact that the identification of the black-white gaps rely on being able to condition on valid sentencing cells. The potential cost of this restriction is that some off-grid crimes (such as capital murder) may be more likely to attract media attention and thus also hold more political influence.

[17] The full list of good words is "joy, love, peace, wonderful, pleasure, glorious, laughter, and happy" and the full list of bad words is "agony, terrible, horrible, nasty, evil, awful, failure, and hurt".

driven by these observable characteristics. Deutsch and Wood (2011) show that if selection is driven by variables other than age, race, education, and geography, then IPW can exacerbate bias. However, in this application, the results are stable with or without the IPW correction.

Appendix table 3 shows descriptive regressions of the prejudice measures on all of the demographic characteristics available in the data; education, age and race using the inverse probability weights. The estimating sample includes roughly 230,000 respondents from across the entire United States. The estimates suggest that more educated, younger, black persons harbor less prejudicial views in comparison to less educated, older, white individuals. I then take these estimates and project them onto district-level means to construct a district-level measure of implicit prejudice.

David Leip's Atlas of U.S. Presidential Elections

David Leip's atlas provides data on voting outcomes for presidential elections at the county-level. I have Kansas voting results from the past 5 presidential elections from 1992-2008. I aggregate this data to the district-level.

Table 1. Descriptive Means of Sentencing Data

	Overall	Black	White	BW Diff
Sentencing Outcomes				
Incarceration Rate	0.266	0.328	0.243	0.085***
Incarceration Rate (Non-Drug Crimes)	0.269	0.332	0.245	0.087***
Incarceration Rate (Drug Crimes)	0.257	0.315	0.238	0.077***
Duration (in Months)	27.9	29.7	27.2	2.5***
Duration (Non-Drug Crimes)	29.1	31.8	28.0	3.8***
Duration (Drug Crimes)	25.0	23.7	25.5	-1.8***
Defendant Demographics				
Black	0.273			
Age	30.401	30.047	30.534	-0.486***
Female	0.180	0.179	0.180	-0.001
Competitive District	0.479	0.573	0.444	0.129***
Type of Crime				
Non-Drug Crimes	0.711	0.742	0.699	0.042***
Person Crimes	0.278	0.322	0.262	0.061***
Case-Level Characteristics				
Severity Level (Non-Drug Crimes)	3.245	3.345	3.206	0.139***
Criminal History (Non-Drug Crimes)	3.955	4.364	3.793	0.572***
Severity Level (Drug Crimes)	7.532	7.488	7.546	-0.057***
Criminal History (Drug Crimes)	3.255	3.921	3.040	0.881***
Special Rule Violations	0.208	0.244	0.195	0.049***
Counts	1.577	1.513	1.601	-0.088***
Private Counsel	0.259	0.206	0.279	-0.073***
Plea Bargain	0.947	0.928	0.955	-0.026***
N = 42,800				

Table 2. Adjusted Mean Differences in Incarceration Rates by Race

Dependant Variable: Incarceration Indicator Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BW Gap	0.085***	0.074***	0.023***	0.020***	0.022***	0.017***	0.018***	0.015***	0.015***	0.013**
	(0.012)	(0.008)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Incarceration Rate (Whites)	0.243									
Severity Level		X	X							
Criminal History			X							
Severity x Criminal History Cells				X	X	X	X	X	X	X
Defendant's Gender, Age					X	X	X	X	X	X
Indicator for Special Rule Violation						X	X	X	X	X
Total # of Counts							X	X	X	X
Indicator for Type of Counsel								X	X	X
Indicator for Person Crime									X	X
Indicator for Plea Status										X
Observations	42753	42753	42753	42753	42718	42718	42718	42586	42555	42438
R-squared	0.007	0.190	0.386	0.413	0.415	0.466	0.466	0.468	0.468	0.472

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Standard errors are clustered at the district level.

Table 3. Interaction Effects of Race on Incarceration

Dependant Variable: Indicator for Incarceration	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Black	0.013**	0.055***	0.012**	0.008	0.009	0.011*	0.011	0.007	0.010	0.035*
	(0.005)	(0.009)	(0.005)	(0.005)	(0.008)	(0.006)	(0.008)	(0.011)	(0.012)	(0.020)
Black*Age		-0.001***								-0.001***
		(0.000)								(0.000)
Black*Female			0.005							0.009
			(0.008)							(0.008)
Black*Special Rule				0.021*						0.022*
				(0.011)						(0.011)
Black*Counts					0.002					0.002
					(0.003)					(0.003)
Black*Private Counsel						0.008				0.010
						(0.010)				(0.010)
Black*Person Crime							0.004			0.004
							(0.016)			(0.020)
Black*Plea								0.006		0.007
								(0.010)		(0.011)
Black*Non-Drug Crime									0.003	-0.001
									(0.012)	(0.016)
Observations	42438	42438	42438	42438	42438	42438	42438	42438	42438	42438
R-squared	0.472	0.473	0.472	0.472	0.472	0.472	0.472	0.472	0.472	0.473

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Standard errors clustered at the district level. All regressions include age, indicators for gender, a special rule violation, private counsel, plea status, the total # of counts, and sentencing cell indicators.

Table 4: Difference-in-Difference Estimates: Black-White Gaps by Electoral System

Dependant Variable: Indicator for Incarceration

	(1)	(2)	(3)	(4)
Black	-0.003	0.000	-0.007*	-0.007
	(0.005)	(0.004)	(0.004)	(0.005)
Black*Competitive District	0.016**	0.015**	0.017***	0.015**
	(0.006)	(0.006)	(0.006)	(0.006)
p-value of the sum	0.003	0.001	0.042	0.059
Covariates				
District Fixed Effects	X	X	X	
Severity x Criminal History Cells	X	X	X	X
Special Rule Indicator	X	X	X	X
Defendant's Age and Gender		X	X	X
Case-Level Covariates			X	X
Judge Fixed Effects				X
Observations	42753	42718	42438	42438
R-squared	0.470	0.472	0.479	0.487

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: Standard errors are clustered at the district level. The case-level covariates include the total number of counts, type of counsel, type of crime (person or non-person), and plea status.

Table 5. Electoral Cycle Effects on BW Incarceration Gap

Dependent Variable: Indicator of Incarceration				
	Restricted Model		Unrestricted Model	
Competitive Districts	(1)	(2)	(3)	(4)
Election Year$_{-1}$	-0.002	-0.001	-0.001	-0.000
	(0.004)	(0.004)	(0.005)	(0.005)
Election Year	0.003	-0.002	0.005	-0.001
	(0.011)	(0.013)	(0.010)	(0.013)
Black*Election Year$_{-1}$	0.015*	0.013	0.010	0.009
	(0.008)	(0.008)	(0.010)	(0.009)
Black*Election Year	0.025**	0.026***	0.021**	0.023**
	(0.010)	(0.010)	(0.010)	(0.010)
Retention Districts				
Election Year$_{-1}$			-0.008	-0.009
			(0.007)	(0.007)
Election Year			0.001	0.001
			(0.007)	(0.007)
Black*Election Year$_{-1}$			0.010	0.013
			(0.015)	(0.015)
Black*Election Year			-0.001	0.002
			(0.005)	(0.006)
District Fixed Effects	X		X	
Judge Fixed Effects		X		X
Observations	42400	42400	42001	42001
R-squared	0.481	0.489	0.482	0.489
Standard errors in parentheses				
*** p<0.01, ** p<0.05, * p<0.1				

Note: Standard errors are clustered at the district level. All regressions include indicators for black, gender, special rule violation, private counsel, person crime, plea status, sentencing cells, year, as well as the total number of counts and age. The restricted model constrains the timing indicators among retention districts to be zero. The unrestricted model relaxes this constraint. Judicial elections are staggered and occur every 4 years.

Table 6. Racial Differences in Case-Level Observables Across the Electoral Cycle

Dependent Variable:	Person Crime (1)	Severity (Non-Drug) (2)	Severity (Drug) (3)	Criminal History (4)	Special Rule (5)	Total Counts (6)	Private Counsel (7)	Plea Bargain (8)
Black*Election Year$_{-1}$	-0.007	0.026	0.045	-0.100	-0.028**	-0.016	0.051***	0.008
	(0.008)	(0.037)	(0.054)	(0.084)	(0.013)	(0.057)	(0.014)	(0.007)
Black*Election Year	-0.006	-0.059	0.053	-0.034	-0.015	0.017	0.022	-0.008
	(0.007)	(0.090)	(0.040)	(0.088)	(0.013)	(0.040)	(0.017)	(0.009)
Means (White Defendants)	0.262	3.206	7.546	3.566	0.195	1.601	0.279	0.955
Observations	42411	30139	12272	42411	42411	42411	42411	42411
R-squared	0.103	0.423	0.102	0.148	0.058	0.098	0.128	0.103

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Standard errors are clustered at the district level.

Table 7. Election Year Effects by District-Level Characteristics

Dependent Variable: Indicator for Incarceration	Share of Black Constituents (Ages 25+)		Predicted Prejudice Index		Δ in Democratic Vote Share In '08	
Black*Election Year Effects Among Districts With:	(1)	(2)	(3)	(4)	(5)	(6)
Low Fraction Black (γ_0^{low})	0.075**	0.071**				
	(0.030)	(0.029)				
High Fraction Black (γ_0^{high})	0.019**	0.021**				
	(0.009)	(0.008)				
Low Predicted Prejudice (γ_0^{low})			0.011*	0.016***		
			(0.006)	(0.006)		
High Predicted Prejudice (γ_0^{high})			0.068***	0.068***		
			(0.020)	(0.020)		
Low Obama Support (γ_0^{low})					0.050***	0.051***
					(0.016)	(0.016)
High Obama Support (γ_0^{high})					0.023***	0.027***
					(0.004)	(0.003)
P-value of the following test:						
$\gamma_0^{low} = \gamma_0^{high}$	0.086	0.109	0.013	0.018	0.110	0.149
District Fixed Effects	X		X		X	
Judge Fixed Effects		X		X		X
Observations	42400	42400	42400	42400	42400	42400
R-squared	0.482	0.489	0.482	0.489	0.479	0.485

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Standard errors are clustered at the district level. Regressions also include black indicator, age, gender, special rule indicator, total counts, private counsel, person crime, plea status, sentencing cells, and year indicators. Low and high districts are defined in relation to whether the district is below or above the district-level characteristic

Figure 1: Kansas Sentencing Guidelines

Non-Drug Offenses

	Criminal History								
Severity Level	A	B	C	D	E	F	G	H	I
	3+ Person Felonies	2 Person Felonies	1 Person & 1 Non-Person Felonies	1 Person Felony	3+ Non-Person Felonies	2 Non-Person Felonies	1 Non-Person Felony	2+ Misdemeanors	1 Misdemeanor or No Record
1 (Most Severe)	653/620/592	618/586/554	285/272/258	267/253/240	246/234/221	226/214/203	203/195/184	186/176/166	165/155/147
2	493/467/442	460/438/416	216/205/194	200/190/181	184/174/165	168/160/152	154/146/138	138/131/123	123/117/109
3	247/233/221	228/216/206	107/102/96	100/94/89	92/88/82	83/79/74	77/72/68	71/66/61	61/59/55
4	172/162/154	162/154/144	75/71/68	69/66/62	64/60/57	59/56/52	52/50/47	48/45/42	43/41/38
5	136/130/122	128/120/114	60/57/53	55/52/50	51/49/46	47/44/41	43/41/38	38/36/34	34/32/31
6	46/43/40	41/39/37	38/36/34	36/34/32	32/30/28	29/27/25	26/24/22	21/20/19	19/18/17
7	34/32/30	31/29/27	29/27/25	26/24/22	23/21/19	19/18/17	17/16/15	14/13/12	13/12/11
8	23/21/19	20/19/18	19/18/17	17/16/15	15/14/13	13/12/11	11/10/9	11/10/9	9/8/7
9	17/16/15	15/14/13	13/12/11	13/12/11	11/10/9	10/9/8	9/8/7	8/7/6	7/6/5
10 (Least Severe)	13/12/11	12/11/10	11/10/9	10/9/8	9/8/7	8/7/6	7/6/5	7/6/5	7/6/5

Drug Offenses	A	B	C	D	E	F	G	H	I
1 (Most Severe)	204/194/185	196/186/176	187/178/169	179/170/161	170/162/154	167/158/150	162/154/146	161/150/142	154/146/138
2	83/78/74	77/73/68	72/68/65	68/64/60	62/59/55	59/56/52	57/54/51	54/51/49	51/49/46
3	51/49/46	47/44/41	42/40/37	36/34/32	32/30/28	26/24/23	23/22/20	19/18/17	16/15/14
4 (Least Severe)	42/40/37	36/34/32	32/30/28	26/24/23	22/20/18	18/17/16	16/15/14	14/13/12	12/11/10

Notes: The rows represent different severity levels and the columns represent different criminal histories. The cells shaded in white, gray, and blue represent presumptive prison, presumptive probation, and border box cells. The 3 numbers in each cell represent the high/expected/low sentence lengths (in months) that the judge is allowed to impose. Judges cannot deviate from the presumed sentence unless they are willing to file a departure or the defendant has committed a special rules violation. Border box cells are presumptive prison, but the judge is allowed to deviate if an alternative treatment plan is available.

Figure 2. Journal Entry of Judgment (page 1)

For Court Use Only (Seal)

2011 KANSAS SENTENCING GUIDELINES JOURNAL ENTRY OF JUDGMENT
Please use Journal Entry based on OFFENSE DATE

SECTION I. CASE IDENTIFYING INFORMATION			1. Transaction No.
2. STATE v. ☐ Male ☐ Female		3. Court O.R.I. Number	4. K.B.I. Number
5. County	6. Court Case Number	7. Sentencing Judge	8. Sentencing Date

9. Defense Counsel: ☐ Appointed ☐ Retained ☐ Self ☐ Waived Orally ☐ Waived in Writing
Counsel Name (please print)_____

10. Type of Proceeding (Trial) ☐ Bench Trial (includes a plea on stipulated facts) ☐ Jury Trial ☐ Guilty Plea ☐ *Nolo contendere* Plea

11. Date of Conviction:

12. Pre-Trial Status of Offender ☐ In Custody ☐ Released on Bond ☐ Other Release

SECTION II. CRIMINAL HISTORY CLASSIFICATION

1. Offender's Overall Criminal History Classification as Found by the Court: ☐ A ☐ B ☐ C ☐ D ☐ E ☐ F ☐ G ☐ H ☐ I

2. Objection to Criminal History? ☐ Yes ☐ No If Yes, By: ☐ Defendant or ☐ State
Court's Ruling on Objection: ☐ Criminal history was amended ☐ Criminal history was not amended

SECTION III. CURRENT CONVICTION INFORMATION

1. Name of PRIMARY Offense of Conviction: _____
Count No.: ____ Date of Offense: _____ ☐ Designated by court as domestic violence case based upon special finding (see #14 this section)

2. K.S.A. Title, Section, Subsection(s): _____
☐ Attempt (formerly K.S.A. 21-3301) ☐ Conspiracy (formerly K.S.A. 21-3302) ☐ Solicitation (formerly K.S.A. 21-3303)

3. Grade of Offense: (Check one in each row.) ☐ Felony, Severity Level ____ ☐ Misdemeanor, Class ____
☐ Person ☐ Nonperson

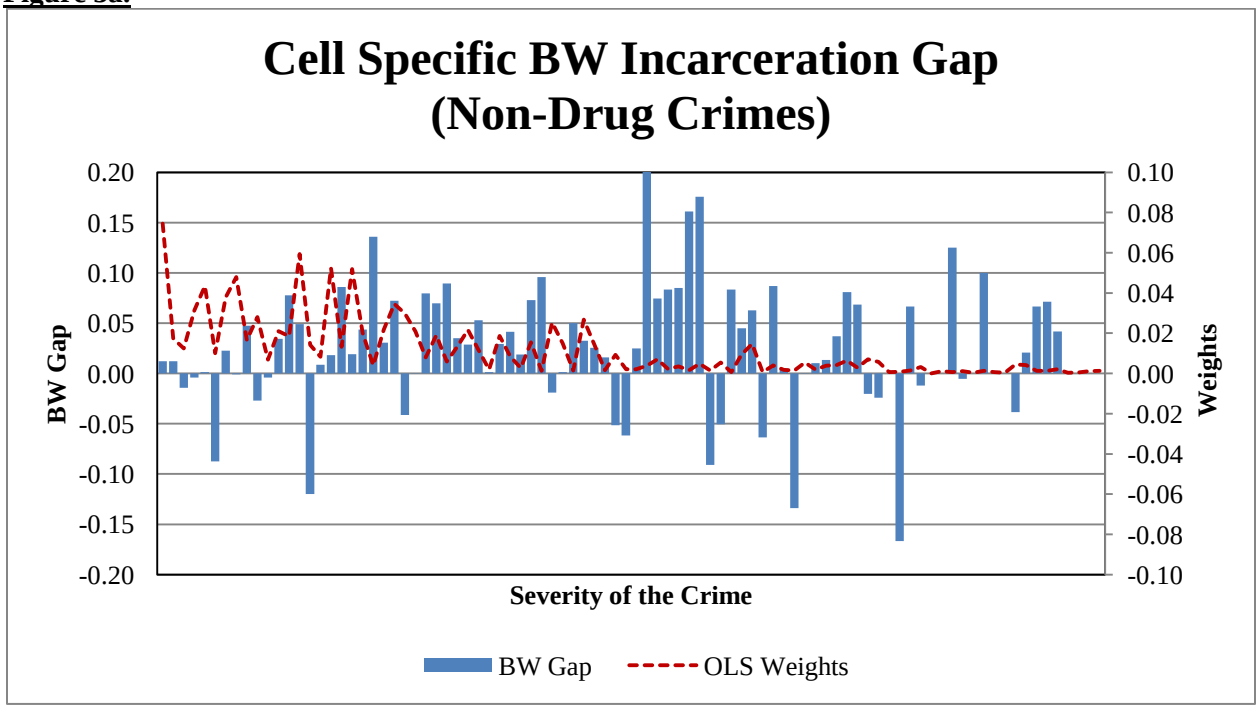
4. Offense Category: ☐ Nondrug ☐ Drug ☐ Off-grid ☐ Nongrid

5. Presumptive Sentencing Range: (Enter terms from appropriate grid.) Aggravated ____ Standard ____ Mitigated ____
Check applicable box(es) ☐ Presumptive Prison ☐ Presumptive Probation ☐ Border Box
☐ Drug Treatment for up to 18 months. (formerly K.S.A. 21-4729)
☐ Special Rule Applies (**Complete Special Rules Supplemental Page and Attach**)

THIS FORM MUST BE ACCOMPANIED BY A COPY OF THE PRESENTENCE INVESTIGATION FORM PURSUANT TO K.S.A. 22-3439 AND A DOCUMENT CONTAINING INFORMATION REQUIRED BY K.S.A. 22-3426.
PLEASE USE AN ADDITIONAL OFFENSES PAGE FOR ADDITIONAL OFFENSES OF CONVICTION.

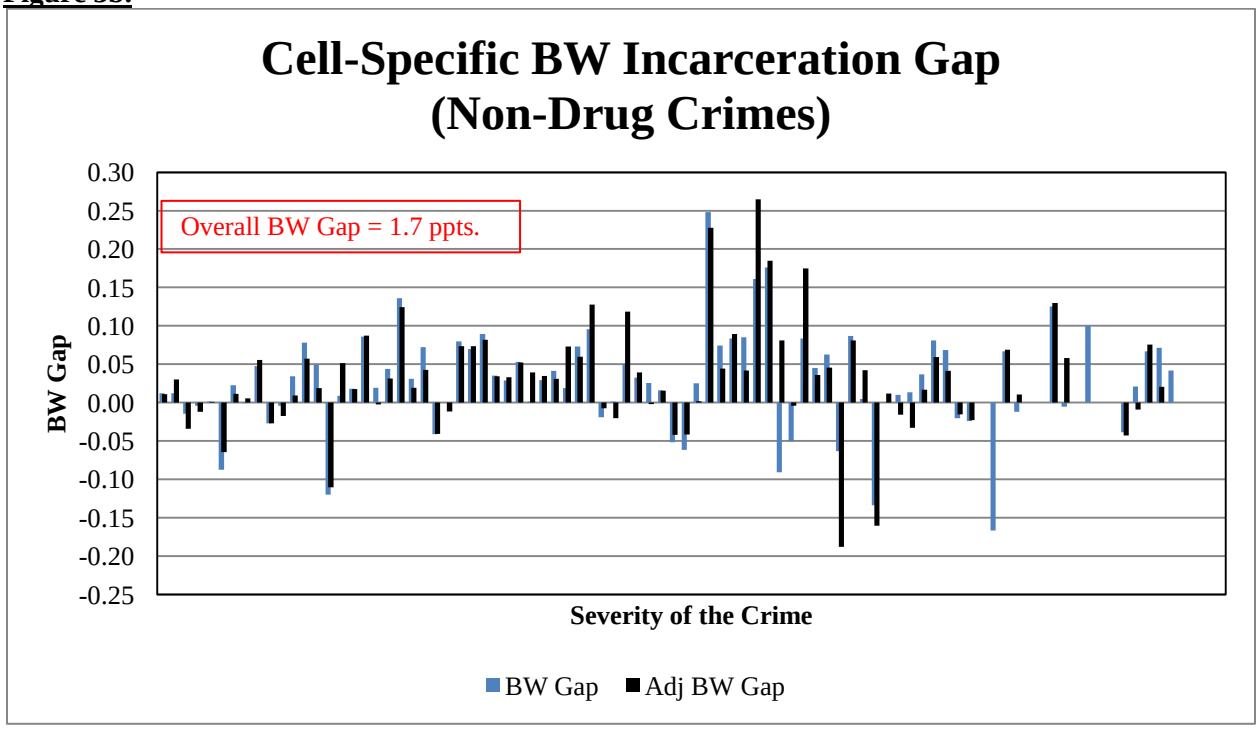
Rev. 7/2011

Figure 3a.



Note: The x-axis lines up the sentencing cells according to the expected sentence length (which is the middle number in each cell – see Figure1.) The cells on the right half of the plots are generally presumptive prison cells whereas the cells on the left are presumptive probation.

Figure 3b.



Note: The x-axis lines up the sentencing cells according to the expected sentence length (which is the middle number in each cell – see Figure1.) The cells on the right half of the plots are generally presumptive prison cells whereas the cells on the left are presumptive probation.

Appendix Tables and Figures

Table A1. Additional Specification Checks

Dependant Variable: Indicator for Incarceration	(1)	(2)	(3)	(4)	(5)	(6)
BW Gap	0.013**	0.012**	0.012**	0.012**	0.012***	0.012**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.005)
Baseline Specification	X	X	X	X	X	X
Replace Age w Age Cubic		X				
Replace Age w Age Indicators			X			
Include Year Effects				X		
Include Type of Crime Indicators					X	
Indicators for each Special Rule						X
Observations	42438	42438	42438	42438	42467	42438
R-squared	0.472	0.474	0.475	0.474	0.488	0.474
Standard errors in parentheses						
*** p<0.01, ** p<0.05, * p<0.1						

Notes: Standard errors clustered at the district level. The baseline specification refers to the specification in column 10 in table 2.

Table A2. Year After Election Effects

Dependent Variable: Indicator of Incarceration		
	(1)	(2)
Election Year	0.003	-0.003
	(0.011)	(0.014)
Year After Election	0.000	-0.003
	(0.009)	(0.008)
Black*Election Year	0.022*	0.023**
	(0.012)	(0.011)
Black*Year After Election	-0.004	-0.004
	(0.012)	(0.011)
District Fixed Effects	X	
Judge Fixed Effects		X
Observations	42400	42400
R-squared	0.481	0.489
Standard errors in parentheses		
*** p<0.01, ** p<0.05, * p<0.1		

Note: Standard errors are clustered at the district level. All regressions include indicators for black, gender, special rule violation, private counsel, person crime, plea status, sentencing cells, year, as well as the total number of counts and age.

Table A3. Demographic Predictors of Implicit Prejudice using the IAT

Dependant Variable: Implicit Prejudice

Educational Attainment	(1)	(2)	(3)	(4)
Less Than High School	-0.081			-0.039
	(0.017)**			(0.015)*
High School Grad				
Some College	-0.008			-0.006
	(0.009)			(0.009)
BA	-0.002			-0.018
	(0.010)			(0.011)
More Than BA	-0.024			-0.051
	(0.011)*			(0.012)**
Age Groups				
7-17 yos		-0.070		-0.040
		(0.014)**		(0.013)**
18-24 yos				
25-34 yos		-0.008		0.006
		(0.002)**		(0.003)*
35-44 yos		-0.018		-0.008
		(0.005)**		(0.005)
45-54 yos		-0.004		-0.001
		(0.006)		(0.005)
55-64 yos		0.032		0.029
		(0.009)**		(0.009)**
65+ yos		0.106		0.075
		(0.018)**		(0.018)**
Race Indicators				
White			0.101	0.093
			(0.008)**	(0.007)**
Black			-0.350	-0.356
			(0.009)**	(0.009)**
Hispanic			0.004	0.001
			(0.026)	(0.024)
Other				
Constant	0.366	0.354	0.308	0.341
	(0.009)**	(0.003)**	(0.008)**	(0.017)**
Observations	227914	231008	231008	227914
R-squared	0.01	0.01	0.09	0.1

Robust standard errors in parentheses
* significant at 5%; ** significant at 1%

Notes: All regressions include state fixed effects to control for regional variation in prejudice, the standard errors are clustered at the state level, and all regressions are weighted by the inverse probability weights. At the individual level, the mean IAT is 0.35 and the standard deviation is 0.43.

Table A4. Election Year Effects by Uncontested vs. Contested Elections

Dependent Variable: Indicator for Incarceration

	(1)		(2)	
Election Cycle Effects	Uncontested	Contested	Uncontested	Contested
Election Year$_{-1}$	-0.000	-0.010	-0.004	0.005
	(0.006)	(0.013)	(0.005)	(0.007)
Election Year	0.008	-0.016	-0.001	-0.006
	(0.009)	(0.020)	(0.013)	(0.016)
Black*Election Year$_{-1}$	0.008	0.047**	0.008	0.030
	(0.007)	(0.023)	(0.008)	(0.019)
Black*Election Year	0.023*	0.041***	0.025**	0.028***
	(0.014)	(0.011)	(0.011)	(0.006)
District Fixed Effects	X			
Judge Fixed Effects			X	
Observations	42403		42403	
R-squared	0.482		0.490	
Standard errors in parentheses				
*** p<0.01, ** p<0.05, * p<0.1				

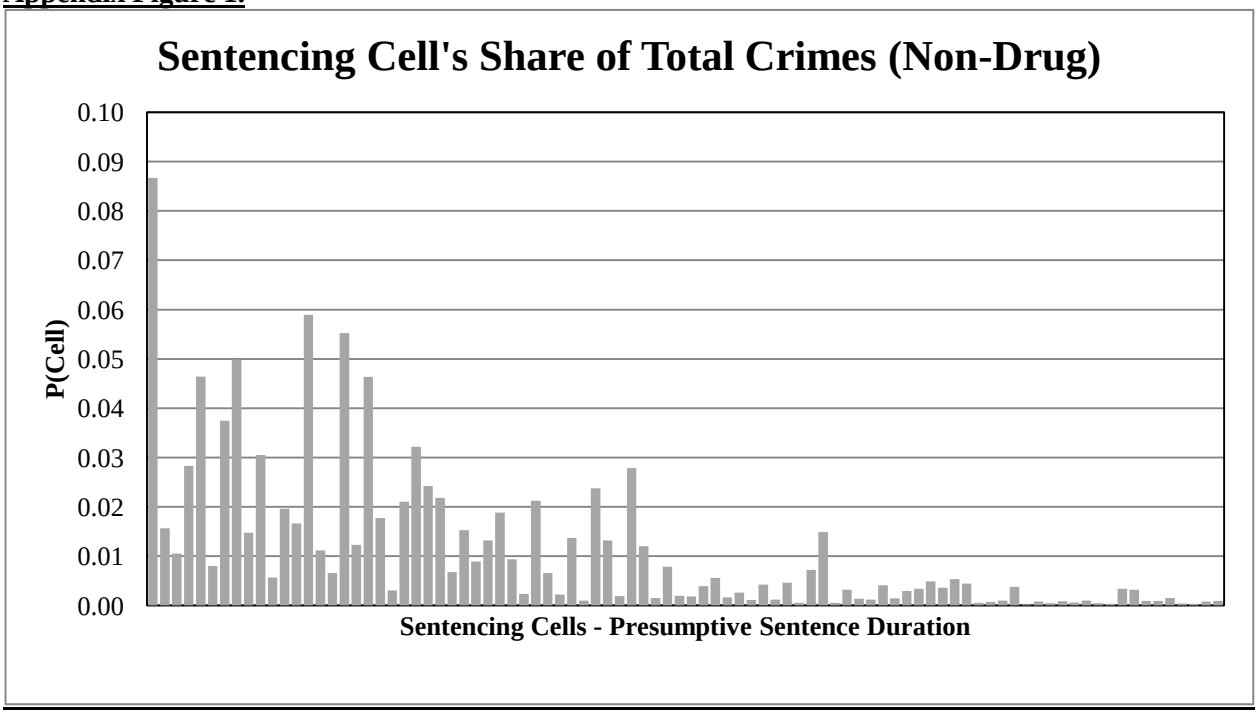
Note: Standard errors are clustered at the district level. All regressions include indicators for black, gender, special rule violation, private counsel, person crime, plea status, sentencing cells, year, as well as the total number of counts and age.

Table A5. Political Incentive Effects on Sentence Length

Dependant Variable: Duration (in Months)						
	Unadjusted	Adjusted	w/ District FE	w/ Judge FE	w/ District FE	w/ Judge FE
Panel A. Descriptive Estimates of the BW Gap	(1)	(2)	(3)	(4)	(5)	(6)
Black	2.452***	-0.545***				
	(0.875)	(0.145)				
Panel B. BW Gap Across Electoral Systems (Diff-in-Diff)						
Black			-0.309	-0.356		
			(0.217)	(0.224)		
Black*Competitive District			0.003	-0.080		
			(0.408)	(0.364)		
Panel C. Election Year Effects on BW Gap						
Black*Election Year					-0.155	-0.197
					(0.574)	(0.604)
Observations	42605	42297	42297	42297	42259	42259
R-squared	0.001	0.719	0.719	0.721	0.727	0.728
Standard errors in parentheses						
*** p<0.01, ** p<0.05, * p<0.1						

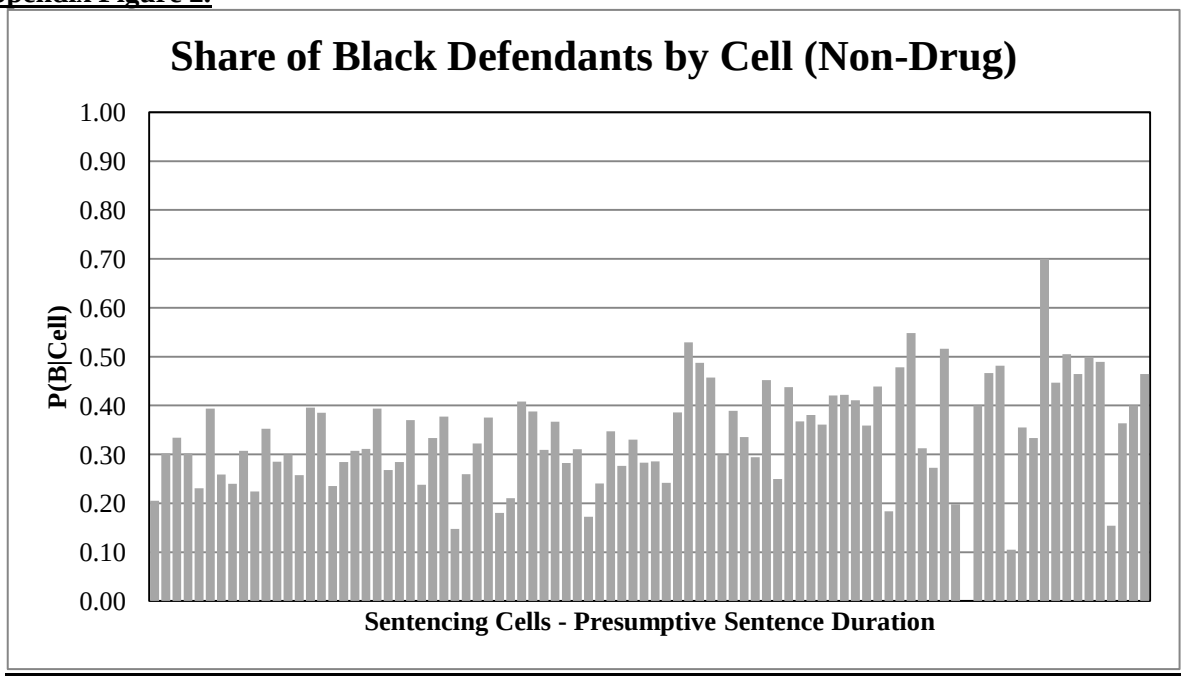
Note: Standard errors are clustered at the district level. Regressions also include black indicator, age, gender, special rule indicator, total counts, private counsel, person crime, plea status, sentencing cells, and year indicators.

Appendix Figure 1.



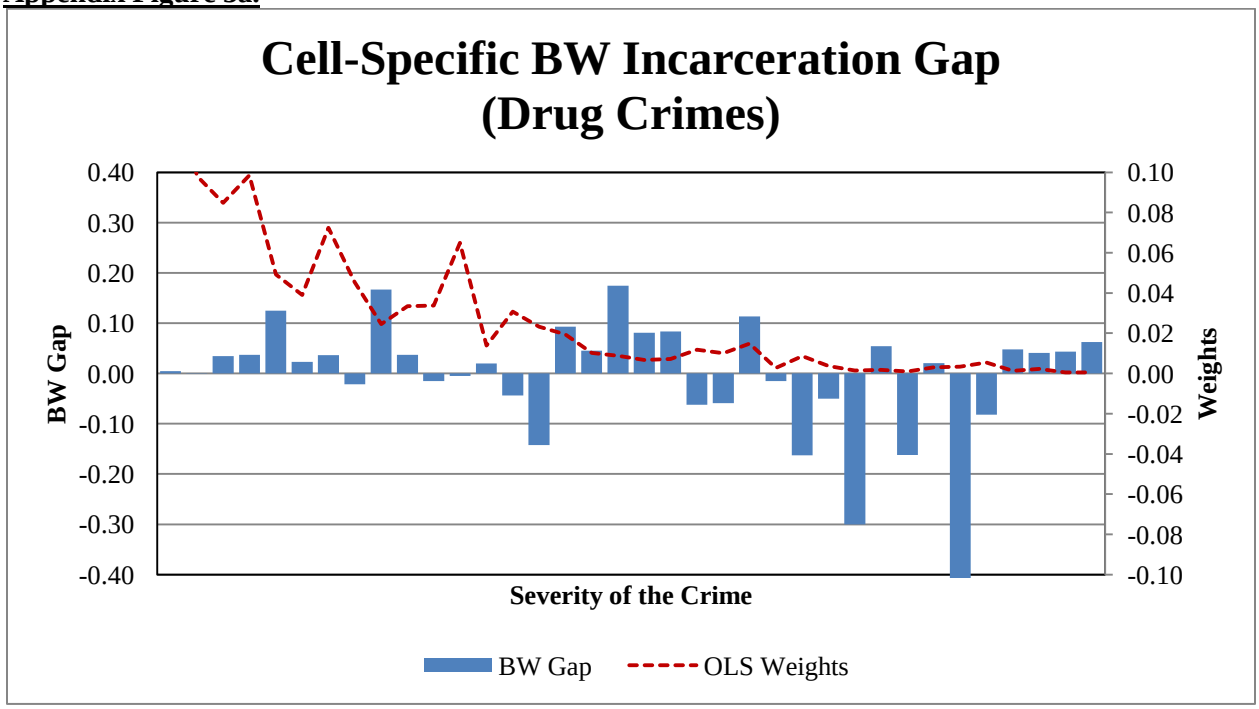
Notes: The x-axis lines up the sentencing cells according to the expected sentence length (which is the middle number in each cell – see Figure1.) The cells on the right half of the plots are generally presumptive prison cells whereas the cells on the left are presumptive probation. The plot indicates that the majority of crimes are low severity crimes.

Appendix Figure 2.



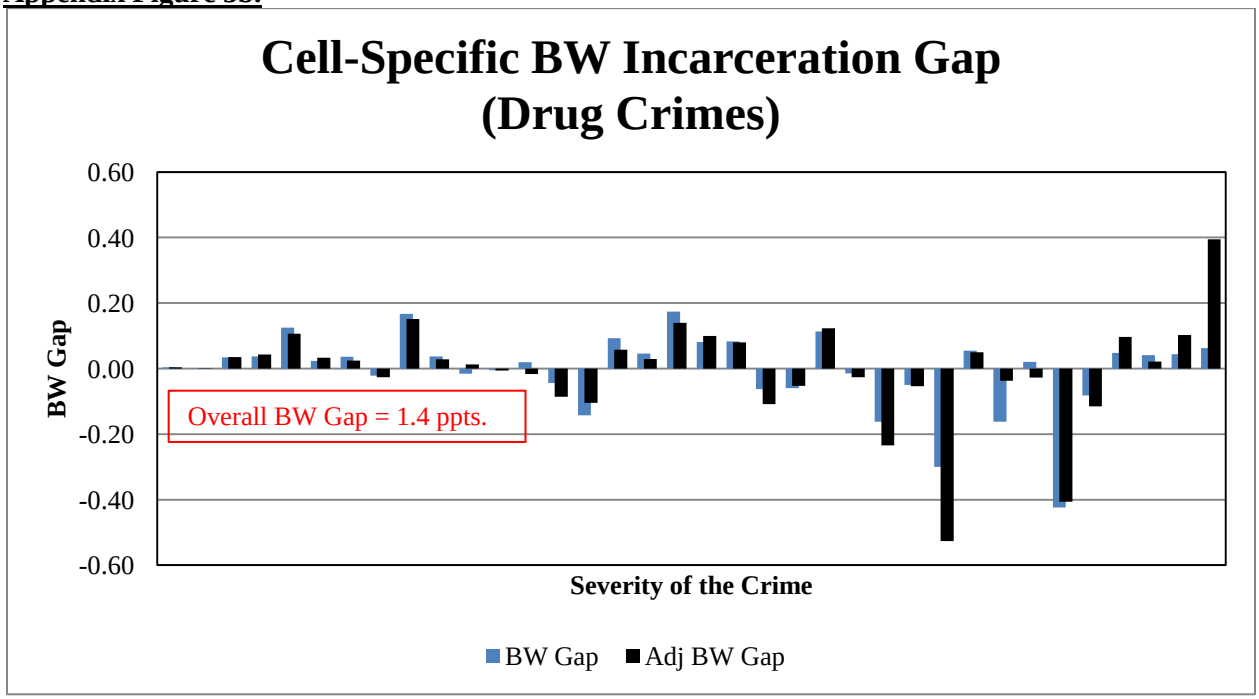
Notes: The x-axis lines up the sentencing cells according to the expected sentence length (which is the middle number in each cell – see Figure1.) The cells on the right half of the plots are generally presumptive prison cells whereas the cells on the left are presumptive probation. The plot shows that there is common support in every single cell in the sentencing grid. The only cell in which the fraction black is 0 actually contains zero observations of either race.

Appendix Figure 3a.



Note: The x-axis lines up the sentencing cells according to the expected sentence length (which is the middle number in each cell – see Figure1.) The cells on the right half of the plots are generally presumptive prison cells whereas the cells on the left are presumptive probation.

Appendix Figure 3b.



Note: The x-axis lines up the sentencing cells according to the expected sentence length (which is the middle number in each cell – see Figure1.) The cells on the right half of the plots are generally presumptive prison cells whereas the cells on the left are presumptive probation.