

Does Prosecutor Partisanship Exacerbate the Racial Charging Gap? Evidence from District Attorneys in Three States

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Abstract

In the United States, nearly all state prosecutors are elected. The policy discretion afforded to these officials has raised concerns that they may exercise their coercive authority in ways that exacerbate racial disparities. To what extent do local prosecutors' political preferences and electoral incentives affect the charges they bring against defendants from different racial groups? Using linked criminal records from three large states, I find that marginally elected Republican prosecutors seek significantly tougher charges than Democrats but only in cases involving Black defendants. Additional tests demonstrate that observable defendant characteristics (including sex, prior criminal history, crime type, and arrest offense) and sample selection bias (stemming from police expectations of prosecutor punitiveness) cannot explain this result. Next, this article examines the extent to which electoral selection or electoral incentives drive these overall effects. Consistent with differences in reelection concerns across parties, the overall partisan difference increases as a function of proximity to reelection. However, the racial gap is largely eliminated in election years as Republican prosecutors get tough all around. Instead, my findings point toward differences in local electoral incentives across parties due to residential segregation. I find that Black defendants tend to reside in, and racial differences are most substantial in, precincts where Republican candidates perform poorly. The final part of this article demonstrates that prosecutor partisanship does not merely affect charging outcomes but also shapes disparities in incarceration lengths.

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1 Introduction

Chief prosecutors are subject to periodic reelection in 47 U.S. states. Because prosecutors do not face formal oversight by higher offices or regulatory bodies (Barkow, 2019), prosecutorial elections are a critical feature of the criminal justice system. In 2018 alone, local prosecutors in 2,344 offices across the U.S. pursued 17 million criminal cases, including approximately 3.91 million felony cases (Court Statistics Project, 2020). Given evidentiary and resource constraints, these offices are free to determine which arrestees to seek charges against, which charges to select, which plea bargains to offer, and often which trial sentence to recommend. The broad and unchecked policy discretion afforded to prosecutors has raised several concerns (Stuntz, 2001), including that prosecutorial decisions may exacerbate racial disparities in criminal justice (Rehavi and Starr, 2014).

Do political preferences influence how local prosecutors treat defendants from different racial or ethnic groups? While leading empirical scholarship has demonstrated that political preferences are a strong predictor of legislator (Ansolabehere, Snyder Jr and Stewart III, 2001; Bafumi and Herron, 2010) and judicial behavior (Segal, Spaeth et al., 2002; Epstein, Landes and Posner, 2013; Cohen and Yang, 2019), the unparalleled coercive authority of prosecutors warrants special attention. Prosecutorial discretion has been linked to the unprecedented incarceration trend in the U.S. (Pfaff, 2017), home to less than 5% of the world’s population but over 20% of the world’s imprisoned population.¹ Communities of color have been the most affected by this trend - the incarceration rate for Black males is 5.7 times higher than for non-Hispanic White males (Carson, 2020). Widespread contact with carceral institutions has pernicious consequences on political participation (Gottschalk, 2008; Weaver and Lerman, 2010; White, 2019), economic mobility (Schwartz-Soicher, Geller and Garfinkel, 2011), and public health (Wildeman and Wang, 2017) in these communities.

However, to date, there has been insufficient variation at the level of existing studies (single county or state jurisdictions) to assess this question empirically. The non-random assignment of prosecutors to districts poses additional methodological challenges as there may be many differences between jurisdictions, including constituency preferences, that elect Democratic and Republican district attorneys. Furthermore, unobserved selection into conviction, indictment, and arrest limits

¹Source: <https://www.prisonpolicy.org/blog/2020/01/16/percent-incarcerated/>

one's ability to infer causal effects from widely available prison or court records.

To circumvent these challenges, I collect 39 million criminal charges linked from indictment (and often arrest) to incarceration in three large states (Texas, Virginia, and Washington). Descriptively, Black defendants face charges that would carry 11-14% longer imprisonment terms on average. The racial charging gap is robust to the inclusion of district dummies, defendant characteristics (such as sex and prior criminal history), and arrest charge fixed effects. Next, this article leverages a Regression Discontinuity Design (RDD) to estimate the causal impact of prosecutor partisanship on charge severity. The electoral RDD is a design-based, nonparametric estimation strategy that compares outcomes in areas that marginally elect Democratic or Republican officials (Eggers et al., 2015; De la Cuesta and Imai, 2016). I find that narrowly elected Republican prosecutors seek charges that would carry longer sentences, but this effect is concentrated on cases involving Black defendants. Furthermore, detailed arrest records from Texas indicate that selection into arrest (stemming from police expectations of prosecutor punitiveness) tends to downwardly bias both the overall results and the racial heterogeneity in treatment effects.

If voters are aware of their prosecutor's preferences but uncertain about prosecutorial quality, this RDD estimate is consistent with either primitive ideological differences, differences in electoral concerns, or both. Even in the absence of electoral incentives, ideological concerns will drive punitive district attorneys to pursue severe charges for which the risk of wrongful conviction might deter lenient prosecutors. Electoral incentives may induce prosecutors to pursue even more severe offenses (Bandyopadhyay and McCannon, 2014). In turn, these incentives may vary by party due to differences in outside options or competition from primary challengers. Allowing the Local Average Treatment Effect (LATE) to vary as a function of proximity to reelection, I find that the partisan effect increases during election years, implying that reelection pressures are more considerable for Republicans. However, the increase is more substantial for White defendants - indicating that partisan differences in reelection incentives cannot fully account for the racial charging gap.

Next, I consider an incentive-based account for the racial charging gap based upon geographic variation in electoral incentives within districts. If minority defendants tend to reside in communities where Republican candidates perform poorly, those defendants may bear the brunt of the

overall incentive effect. Consistent with this mechanism, I find that racial minorities tend to reside in, and the influence of partisanship on charging is most substantial in, precincts that Republican candidates do not rely on electorally.

The final component of my analysis turns to the effect of partisanship on incarceration. I find that partisan differences in indictment propagate through to significant partisan differences in the probability of conviction and sentence lengths.

2 Institutional Background

2.1 Local Criminal Prosecutions in the United States

Prosecutorial Elections. Voters in 47 U.S. states elect criminal prosecutors. Of these states, all but five hold partisan elections,² all but three mandate four-year terms³ and all but two elect prosecutors to the local level.⁴ While local prosecutors are appointed in Connecticut and New Jersey, an appointed Attorney General selects local prosecutors in Alaska. According to a nationwide study of prosecutor elections between 2012-2017, 70% of races did not feature more than a single candidate (Hessick, 2018). For comparison, nationwide studies indicate that 75-80% of local trial judge elections (Nelson, 2010), 65-70% of appellate judge elections and 33% of state supreme court elections (Streb, Frederick and LaFrance, 2007) are uncontested. Consequently, local law enforcement elections are challenged less often than federal (e.g., 5% of U.S. House general elections are uncontested)⁵ and state legislative races (35%).⁶

Prosecutorial Discretion. Local prosecutors are granted broad policy discretion in the U.S. (Stuntz, 2001). First, prosecutors are free to decline to bring charges against arrestees. While half of U.S. States have some form of grand jury requirement, legal observers have questioned the impartiality of this institution in practice. Next, prosecutors are free to select severe or minor charges. Over time, the proliferation of criminal codes has increased this form of prosecutorial discretion

²Arkansas, California, Minnesota, North Dakota, and Oregon (as well as certain counties in Hawaii and Montana)

³Prosecutors in Alabama, Kentucky, and Louisiana serve six-year terms.

⁴Voters in Delaware and Rhode Island elect an Attorney General with jurisdiction over the entire state.

⁵Source: https://ballotpedia.org/Annual_Congressional_Competitiveness_Report,_2020#cite_ref-mpo_2-3

⁶Source: https://ballotpedia.org/State_legislative_elections,_2020

by criminalizing more behavior and expanding the range of potential punishments attached to a single criminal act. Unlike federal prosecutors, state prosecutors are not encouraged or required to commit to initial charges. Charge bargaining in this manner reduces accountability by undercutting outside observers' ability to use initial charging information to draw inferences about the prosecutor's evidence (Wright and Miller, 2002). Finally, prosecutors may dismiss charges after filing or seek a plea bargain (whether favorable or not). Although judges have the statutory authority to approve or dismiss charges or reject guilty pleas in many jurisdictions, the volume of caseloads outstrips judicial monitoring capacity. In addition, separation of powers concerns further shield prosecutors from judicial scrutiny (Wright, 2008).

Notably, the Supreme Court has held the equal protection guarantee under the Fourteenth Amendment requires that the decision to prosecute cannot be based on "an unjustifiable standard such as race, religion, or other arbitrary classification" (*Oyler v. Boles*, 368 U.S. 448, 456). Accordingly, evidence of selective prosecution on the basis of race, the primary focus of this study, is one of the only legally permissible grounds for constraining prosecutorial discretion (*the United States v. Armstrong*, 517 U.S. 456).

2.2 Role of Race in Criminal Prosecutions

Mass imprisonment is experienced differentially in the U.S. Blacks and Hispanics, in particular, account for 32 and 22% of the nation's imprisoned population (Carson, 2020). These estimates, which refer only to incarceration in prisons and exclude jail imprisonment, may underestimate the total number of affected individuals. Moreover, mass imprisonment affects even larger sections of these communities indirectly. For example, 25% of all Black children born in 1990 have had a father sent to prison; this probability doubles for children of fathers who did not complete high school (Wildeman, 2009).

The differential impact of race-neutral policies is a critical driver of racial disparities. For example, on average, African American defendants may have lower access to the financial resources or support necessary to fight off a criminal prosecution. Additionally, penalties may be steeper for violations that tend to be committed by Black defendants (Bushway and Piehl, 2011).

Racial disparities are compounded by the actions taken by law enforcement at every step in the criminal justice system, from policing (Grogger and Ridgeway, 2006; Knox, Lowe and Mummolo, 2020), charging (Rehavi and Starr, 2014), pretrial detention (Arnold, Dobbie and Yang, 2018) to sentencing (Abrams, Bertrand and Mullainathan, 2012). In an important study, Rehavi and Starr (2014) estimate that half of the black-white sentence disparities in federal courts can be explained by differential use of mandatory minimum charges by prosecutors.

2.3 Related Research

Electoral Incentives of Prosecutors. Political scientists have long noted how the voter’s uncertainty about both the defendant’s guilt and the prosecutor’s quality implies a rich principal-agency problem (Gordon and Huber, 2002; Shotts and Wiseman, 2010). Gordon and Huber (2002) argue that although seemingly easily manipulated, voters can be better of conditioning reelection on a strong track record of conviction rates. Shotts and Wiseman (2010) argue that ideological differences between the principal and the prosecutor can exacerbate agency problems. Empirical studies demonstrate that electoral pressure is associated with prosecutorial punitiveness, higher reversal rates by appellate courts, and reduced caseloads/backlogs (Dyke, 2007; McCannon, 2013; Bandyopadhyay and McCannon, 2014, 2017).

Partisan Divergence. A rich literature has documented important differences between Republican and Democratic representatives (Ansolabehere, Snyder Jr and Stewart III, 2001; Bafumi and Herron, 2010; Fowler, Hall et al., 2016; Thompson, 2020). The extent to which non-convergence emerges in local government is more contested (Ferreira and Gyourko, 2009; Gerber and Hopkins, 2011; de Benedictis-Kessner and Warshaw, 2016). Recent work suggests that an important constraint on the local executive’s ability to drive partisan policy may be the executive’s discretion (de Benedictis-Kessner and Warshaw, 2016). Additionally, local partisans may share similar policy preferences. Thompson (2020) finds limited evidence of partisan effects in a setting with a great deal of discretion and clear partisan divides (local sheriff’s compliance with federal requests to detain unauthorized immigrants)

However, this evidence raises questions about what policy the electoral process produces in the

first place. Unless the electoral incentives for a mayor’s municipal budget or a sheriff’s compliance with detainer requests are known, the extent to which candidate partisanship would or would not interact with electoral incentives is unclear. The electoral environment raises related concerns about our ability to infer partisan effects from electoral RDDs, as these designs can inadvertently condition on characteristics that systematically differ across treatment (Marshall, 2019). For example, if voters have a bias against female candidates, female candidates may be more capable than male candidates conditional on winning a close election. Consequently, an electoral RDD estimate would capture both gender differentials and competence differentials. The mechanism highlighted in this paper suggests this bias may be more widespread in studies of partisan effects than previously thought.

Relative to Democrat-appointed judges, Cohen and Yang (2019) find that Republican-appointed federal judges issue sentences that are 5% longer sentences on average and 9% longer sentences for Black defendants. Closest to the current study are working papers by Krumholz (2019) and Arora (2018). Like this paper, those studies examine the effect of prosecutor partisanship on criminal justice outcomes. However, both studies restrict attention to the set of cases in which the defendant was incarcerated. By contrast, this paper examines outcomes associated with all arrests, which enable me to disaggregate the effects of partisanship into those driven by prosecutorial discretion and those driven by other (e.g., judicial) factors.

3 Partisanship and the Racial Charging Gap

I consider two channels through which the partisan affiliation of local prosecutors could induce racial charging disparities. First, there may be a direct effect of policy preferences either due to candidates sorting across jurisdictions or candidates’ independent incentives to prosecute (a selection effect). Second, there may be an indirect effect of partisanship through differential responsiveness to electoral concerns (an incentives effect).

Electoral Selection. A growing body of evidence suggests that public opinion strongly correlates with the partisanship of elected politicians and local policy outcomes (Tausanovitch and Warshaw,

2014; Warshaw, 2019). Previous work has conceptualized criminal justice preferences as the trade-off between Type I errors (prosecuting the innocent) and Type II errors (failing to prosecute the guilty; see Grossman and Katz (1983); Reinganum (1988); Baker and Mezzetti (2001)) and survey evidence suggests that these preferences strongly correlate with partisanship.⁷ A strong relationship between prosecutor partisanship and charging severity is therefore consistent with Republican prosecutors sorting into districts where voters prefer punitive outcomes. However, as constituency preferences are unlikely to vary when a Republican candidate is narrowly elected, this explanation cannot account for significant partisan differences at the electoral discontinuity.

To explain a discontinuous difference between marginally elected partisans, we might consider the influence of the prosecutor’s preferences. In such an account, the risk of wrongful conviction, in conjunction with trial costs, would deter prosecutions unless the strength of the evidence indicates that the probability of defendant guilt, and hence conviction at trial, is high. To the extent that they down-weight the risk of wrongful conviction, narrowly elected Republican prosecutors would seek more punitive outcomes.

Significantly, these preferences may induce differential treatment by prosecutors on the basis of defendant race. Most obviously, prosecutors may have in-group biases. Several studies conclude that elected officials are more responsive to constituents who share their racial or ethnic background (Broockman, 2013; Butler and Broockman, 2011; Gell-Redman et al., 2018). The overrepresentation of minority candidates in the Democratic party is an additional consideration, even though these candidates are too rare to drive a significant partisan difference overall.⁸ Finally, policy preferences may induce differential treatment if prosecutors anticipate racial disparities elsewhere in the criminal justice system. In this case, minority defendants may tend to have a higher probability of conviction (irrespective of their guilt), leading Republican prosecutors to seek more severe charges against them.⁹

⁷For example, a 2016 survey found that 75% of Bernie Sanders’ followers believed it was worse to imprison 20,000 innocent people than to free 20,000 guilty defendants. Only 48% of Trump’s supporters agreed with that statement. Source: <https://www.cato.org/policing-in-america/chapter-4/blackstones-ratio>

⁸According to data collected by the non-profit organization Color of Change, 85.7% of Black prosecutors and just 25% of White prosecutors affiliated with the Democratic party. However, only 3.8% of prosecutors in this data are Black. Source: <http://www.prosecutordb.org/>

⁹In theory, prosecutors could respond to racial disparities by adopting evidentiary thresholds that vary race. However, a study of criminal court judges found that while aware of racial inequities, 75% of interviewed judges did

Electoral Incentives. Prosecutors are elected officials and hence may be subject to electoral pressures that affect their behavior. As voters often lack information about prosecutorial choices (plea bargaining, sentence bargaining, etc), prosecutors may seek to signal their quality by securing tough outcomes. In turn, electoral incentives may differ for narrowly elected Democrats and Republicans for two reasons.

First, reelection incentives – office-holding benefits, outside options, and competition from primary challengers, etc – or the electoral consequences of charge leniency (e.g. media coverage) may vary across parties. To see how these considerations might matter, suppose electoral incentives were identically distributed across parties. In this case, policy preferences and reelection incentives would push punitive candidates to prefer more severe charges on marginal cases; the same factors would move lenient prosecutors in opposite directions. In contrast, if electoral concerns vary across parties, one would need to partition out differences in electoral incentives to pin down the impact of policy preferences on prosecutors’ behavior. Partisan differences in electoral concern may be more pronounced in cases involving certain groups over others. For example, a higher perceived probability of conviction may cause racial minorities to experience the brunt of incentive effects.

Second, because of the strong tendency of African American voters to cast their ballots for Democrats, Republican candidates may not be electorally reliant on the minority communities they represent. As residential segregation and racial diversity are strong predictors of incarceration at the local level (Burch, 2014; Feigenberg and Miller, 2018), electoral coalitions can vary considerably within a district.¹⁰ For example, just ten of the 1,632 zip codes in Texas account for 5% of the state’s total prison population.¹¹ 80.78% of the residents in the top ten zip codes identify as a racial minority. In addition, the public’s concern about crime correlates poorly with reported crime, indicating that the media and elected officials play a significant role in shaping public opinion (Beckett, 1999). To the extent that Republican candidates are less electorally competitive in

not adjust their behavior to correct for discrimination in earlier stages in the system (Clair and Winter, 2016).

¹⁰Using block-level imprisonment records in North Carolina, Burch (2014) estimates that neighborhoods in highly segregated counties imprison 0.3% more residents than neighborhoods in less segregated counties. Using charge-level records from several Southern states, Feigenberg and Miller (2018) find that defendants are 27%-54% more likely to be sentenced in highly racially heterogeneous jurisdictions relative to homogeneous jurisdictions.

¹¹Source:<https://commitpartnership.org/dashboard/visualizations/texas-prison-population-per-inmates-last-zip-code>

minority communities, these candidates may shore up broader political support by over-prosecuting those communities.

4 Data and Empirical Approach

4.1 Data

Court Data. I collected 39.85 million criminal records, including 16.46 million arrest charges in Texas, 21 million indictment charges in Virginia, and 2.39 million indictment charges in Washington. The Texas Department of Public Safety provided access to the Computerized Criminal History (CCH) database. At the Supreme Court of Virginia, the Office of the Executive Secretary provided access to charges and dispositions from District, Circuit, and Juvenile court, excluding Circuit courts in Alexandria and Fairfax.¹² Finally, the Washington State Administrative Office of the Courts provided access to the entire Superior Court caseload, excluding King County.¹³

Two of these states, Texas and Virginia, rank among the states with the highest incarceration rates and largest prison populations in the U.S. New felony filings in Texas have increased steadily over the past thirty years to an all-time high of 225,497 per year in 2019.¹⁴ In Virginia, prosecutors filed 193,658 felony cases in Circuit courts in the same year.¹⁵ Prosecutors in Washington, one of bottom 15 states with respect to incarceration rates, filed 34,170 felony cases that year.¹⁶

The primary analysis pools observations across states, as RDDs rely on a large number of observations near the threshold (Calónico, Cattaneo and Titiunik, 2014; De la Cuesta and Imai, 2016). Nonetheless, each state affords unique advantages for my analysis. While Texas and Virginia use partisan primary elections, prosecutorial behavior under Washington’s top-primary system allows me to assess the influence of partisanship in a setting in which primary electorates are less divergent. In addition, while the charging records available from most state courts do not include

¹²These two courts utilize separate case management systems.

¹³Similarly to Alexandria and Fairfax in Virginia, Kings County utilizes a different case management system that was unavailable at the time of writing.

¹⁴Before falling by 8 percent during the pandemic in 2020. Source: https://www.txcourts.gov/media/1451853/fy-20-annual-statistical-report_final_mar10_2021.pdf

¹⁵Source: http://courts.state.va.us/courtadmin/aoc/judpln/csi/sjr/2019/state_of_the_judiciary_report.pdf

¹⁶Source: <https://www.courts.wa.gov/caseload/content/archive/superior/Annual/2019.pdf>

information about the initial arrest or filing decision, the CCH records include nearly all arrests made in Texas for offenses more severe than a Class C misdemeanor. I link indictment charges with arraignment charges from the CCH to assess shifts in the probability of indictment and selection into arrest.

Because of substantial differences in prosecutorial areas of jurisdiction and responsibilities across states, I exclude misdemeanors from my sample. In Texas, voters elect district attorneys and county attorneys with concurrent jurisdiction. The county attorney handles only criminal misdemeanors cases in county courts, in addition to civil suits affecting the county.¹⁷ Next, Virginia Code §15.2-1627(B) requires Commonwealth’s Attorneys to prosecute felonies but affords the same prosecutors discretion over whether to prosecute misdemeanors. Since funding and staffing procedures do not account for misdemeanor prosecutions, several Commonwealth’s Attorneys either prosecute select misdemeanors only or none at all. Finally, the data available in Washington do not include misdemeanor cases.

To construct a comparable set of cases within and across states, I include all felony cases that appear or would appear in the trial courts of general jurisdiction in each state. The final dataset includes 5.9 million individual cases, nearly representing the universe of felony cases heard in each state’s trial courts from 2000-2020. The primary outcome of interest is charge severity (measured as the upper base of the statutory range) for the charges selected by the prosecutor.¹⁸ To generate this variable, I applied each state’s sentencing guideline procedures to the indictment crimes, given the defendant’s prior history when available. The final dataset includes charge severity for each observation regardless of eventual case disposition.¹⁹ See Appendix Appendix C for a discussion of the data construction. Appendix 8 displays the summary statistics for the main variables by state.

¹⁷In 28 states, prosecutors are elected to jurisdictions that match commonly used political boundaries (counties and cities). In another 18 states, prosecutors are elected at the judicial or prosecutorial district level. Several states in the latter group elect district and county prosecutors with overlapping jurisdictions.

¹⁸The literature has operationalized punitiveness in several ways, including the probability of conviction, the probability of conviction on the top-line charge, the probability of dismissal or conviction by jury trial (Dyke, 2007; Bandyopadhyay and McCannon, 2014) as well as the average sentence length (Bandyopadhyay and McCannon, 2014). Since conviction rates are normalized by the number of charges filed, certain outcomes cannot differentiate punitiveness from manipulations of the pool of initial charges. Sentence lengths sidestep this issue, but both outcomes raise concerns that judicial discretion drive effects.

¹⁹While charge severity is solely at the discretion of the prosecutor, a judge must eventually approve all convictions.

Electoral Data. Like nearly all other states that allow for prosecutorial elections, Texas and Virginia hold partisan elections every four years. In Texas, 164 elected district attorneys oversee felony cases. The chief prosecutor may represent one county (as a County Attorney or a District and County Attorney) or multiple counties (as a Criminal District Attorney). I collected electoral returns from the Texas Secretary of State’s office (available for all district-elected attorneys) and the remaining returns from county elections offices.

In Virginia, 95 counties and 25 cities elect a single Commonwealth’s Attorney to oversee felony cases. I secured electoral returns from the Virginia Department of Elections (available from 2000 onward). In addition, I collected electoral returns for incumbents serving in 2000 by contacting county and city elections offices. Independents (prosecutors affiliated with neither major party) are common in Virginia, winning 42.8% of the elections in my sample. To preserve statistical power while minimizing extraneous assumptions, I code the running variable using all elections in which a Republican candidate appeared on the ballot. As illustrated in Table 10, pooling Independents and Democrats in this manner does not affect my findings.

In Washington, voters elect thirty-nine prosecuting attorneys (representing one county each). In 2004, voters in Washington approved Initiative 872, replacing the open primary system with a top-two nonpartisan blanket primary. Under the new system, all candidates compete in the primary, and the top two candidates advance to the general. I collected electoral data from 2010-onward from the Washington Secretary of State. I secured pre-2010 elections from newspapers and county elections offices. The sample used for the regression discontinuity analysis below follows the same procedure as in Virginia but further excludes races in which general election candidates shared the same partisan affiliation.

4.2 Empirical Approach

My empirical analysis consists of four components. The first component tests for aggregate partisan differences in charging severity using a Regression Discontinuity Design (RDD) estimator. To assess whether prosecutors treat racial groups differently, I examine treatment effect heterogeneity by estimating partisan effects separately by defendant race. Next, I examine the extent to which

the RDD estimates are an artifact of selection into arrest or indictment. The third component examines the role of electoral incentives on partisan effects. Finally, I examine the influence of prosecutorial partisanship on imprisonment outcomes.

Main RDD Estimates. Isolating the causal effect of partisanship alone is complicated by unobservable differences in the sample of cases that might appear in different jurisdictions. For example, punitive voter preferences may jointly affect charge severity and the electoral prospects of Republican prosecutors. To circumvent this challenge, I apply a Regression Discontinuity Design (RDD) on charge outcomes in counties that barely elect a Democratic or Republican prosecutor. Specifically, the RDD corresponds to following local-linear regression model:

$$\log(y_{ijt}) = \beta_0 + \beta_1 \mathbb{1}(r_{jt} > 0.5) + f(r_{jt} - 0.5) + \alpha_{st} + \epsilon_{ijt} \quad (1)$$

where $\log(y_{ijt})$ is indictment severity (measured as the upper base of the statutory range) for case i in district j at time t , r_{jt} is the Republican percentage of the two-party vote share in the last election and $f(\cdot)$ is a smooth function of the same. The log transformation is useful because sentencing guidelines are structured so that inputs (offense severity and offender prior criminal history) have multiplicative effects on sentencing (e.g. maximum prison penalties for felonies in Texas are 2 years, 10 years, 20 years and 99 years). I include state-by year-fixed effects (α_{st}) to account for considerable cross-state and over-time differences in average severity. When estimated alongside state-by year-fixed effects, β_1 captures the within state-year shift in indictment severity associated with partisanship. Equation (1) is estimated in MSE-optimal bandwidths with the bias-correction method proposed by Calonico, Cattaneo and Titiunik (2014), and standard errors clustered at the election-level.

The RDD model is estimated separately by defendant race throughout. Although this research design does not rely on conditional independence assumptions, heterogeneous treatment effects in this context may reflect numerous non-race factors. For example, if Republican prosecutors prefer harsher charges for defendants with a prior criminal history, differences in prior history by race would confound the cross-race comparison. Accordingly, I adjust for defendant sex, prior criminal

history, and crime type (whether violent, drug, property, or other) in the primary analyses. In an additional analysis, I also adjust for the severity of the underlying arrest offense.

The parameter of interest, β_1 , is identified under the assumption that potential outcomes (the severity of indictment charges under Republican or Democratic control) vary smoothly near the 50% vote threshold. While not directly testable, this assumption implies smoothness of pre-treatment covariates and the density of the running variable. I report both tests in the Appendix Table 9. I find no noticeable discontinuities in demographic characteristics (population share by race and total population), defendant characteristics (gender or prior criminal history), arrest characteristics (logged count of arrests in multiple samples), or lagged Republican vote share. I also fail to find a discontinuous shift in the empirical density of the running variable using binned means or the nonparametric density estimator proposed by Cattaneo, Jansson and Ma (2020).

Accounting for Selection Bias. To date, most state courts only provide information about cases that have been filed. We therefore typically do not observe the outcome of interest when the police fail to make an arrest or the prosecutor declines initial charges. To the extent that these decisions may be shaped by the prosecutor’s party, the set of observed cases under Democratic and Republican jurisdictions will vary systematically. While the estimates in Appendix Table 9 provide some support for the validity of the research design, the standard RDD model assumes that the probability of selection into arrest or indictment is smooth at the RDD threshold. I assess each threat to inference in turn, beginning with sample selection in indictments.

Formally, let I be an indicator equal to one if the prosecutor seeks an indictment. A case’s potential outcomes under Republican ($I_i(1)$) and Democratic ($I_i(0)$) prosecutors are defined accordingly. Following the principal stratification framework of Frangakis and Rubin (2002), there are four principal strata. *Always-taker* cases include severe offenses (e.g. assault) for which the prosecutor indicts arrestees regardless of party ($I_i(1) = I_i(0) = 1$). *Complier* cases include minor crimes (e.g. possession of drug paraphernalia) for which the prosecutor indicts only if Republican ($I_i(1) = 1$ and $I_i(0) = 0$). *Defier* cases include crimes for which the prosecutor indicts only if Democratic ($I_i(1) = 0$ and $I_i(0) = 1$). Finally, there are *never-taker* cases ($I_i(1) = I_i(0) = 0$).

Generally, the RDD effect on charge severity for filed cases ($I_i = 1$) will not correspond to a true causal effect as this sample will include a non-comparable set of indictments in Republican jurisdictions (containing some mix of *always-taker* and *complier* cases) and Democratic jurisdictions (containing *always-taker* and *defier* cases).

We can decompose the total RDD effect into two causal effects - the extensive and intensive margins. The extensive margin is the RDD effect on the indictment decision ($E[I_i(1) - I_i(0)|r_{jt} = 0.5]$), which I estimate using data on arrests that failed to produce an indictment in Texas. The intensive margin is the RDD effect on charge severity ($E[Y_i(1)|I_i(1) = 1, r_{jt} = 0.5] - E[Y_i(0)|I_i(0) = 1, r_{jt} = 0.5]$), regardless of changes in the composition of cases. Following Dong (2019), I compute the intensive margin using the difference in the conditional expectations of charge severity estimated from a fuzzy RDD estimator on either side of the cutoff (for example, on the right side, $1\{r_{jt} \geq 0.5\} \times Y \times I$ is the outcome and $I \times 1\{r_{jt} \geq 0.5\}$ is the treatment).

In assessing selection into indictment, I adjust for the severity of the underlying arrest offense. Nonetheless, if the arrest decision is itself post-treatment, the probability of inclusion into the arrest sample may not be smooth at the RD threshold. Furthermore, unlike the decision to file, I have no data about suspects who were not arrested.

Fortunately, results from Knox, Lowe and Mummolo (2020) imply that selection will downwardly bias estimates under plausible assumptions. In the present context, the first assumption (mandatory reporting) requires that charge severity is zero if no arrest is made. The second assumption, mediator monotonicity, requires that suspects are more likely to be arrested if the prosecutor is a Republican. This assumption follows from Republican prosecutors' lower threshold for indictment. If the police face an opportunity cost for making arrests, they will be deterred from doing so for less severe charges when they expect that a (lenient) prosecutor is unlikely to seek indictment or conviction. I provide empirical estimates consistent with this assumption in Appendix Table 9.²⁰ The third assumption (relative non-severity of tough on crime stops) requires that charge

²⁰Panel B of Appendix Table 9 examines the RDD effect on a number of arrest outcomes. First, I examine the probability of arrest conditional on a traffic stop for a large sample of states. As traffic arrests largely fall under the *tough-on-crime* arrest strata (i.e., arrests that only occur if the prosecutor is Republican), one would expect a positive RDD effect. One would similarly expect a positive RDD effect on the number of stops for the larger sample and the number of arrests in Texas. Finally, I would expect that Republican prosecutors should receive less severe arrests on average (in Texas). While I fail to reject the null across all tests, the direction of most of the estimates is

severity is more severe for *always-arrests* (arrests made irrespective of the prosecutor’s party) than *tough-on-crime* arrests (arrests made if and only if the prosecutor is Republican). This assumption similarly follows from the lower threshold for arrest in jurisdictions electing Republican prosecutors. The final assumption, treatment ignorability, follows from the research design.

To estimate the size of the selection bias (which may differ by defendant race), I turn to the causal risk ratio approach described by Zhao et al. (2020). This approach sidesteps the need to compute the share of observed arrests that only occur if the prosecutor is Republican ($Pr(A_i(0) = 1|D_i = 1, A_i = 1)$), where D_i is the treatment indicator and A_i is the arrest indicator) which Knox, Lowe and Mummolo (2020) use to construct bounds on the LATE.²¹ Specifically, I estimate the product of the naive risk ratio and a bias factor:

$$RR = \frac{E[Y_i(1)]}{E[Y_i(0)]} = \underbrace{\frac{E[Y|D = 1, A = 1]}{E[Y|D = 0, A = 1]}}_{\text{naive risk ratio}} \underbrace{\frac{Pr(D = 1|A = 1)}{Pr(D = 0|A = 1)} \frac{Pr(D = 1)}{Pr(D = 0)}}_{\text{bias factor}} \quad (2)$$

The bias factor corrects for the over-representation of cases from a specific party in the data. To compute the bias factor, I use the distribution of cases and prosecutors within a neighborhood of the cutoff (as the constituent probabilities are not defined at the cutoff).

Interaction between Electoral Selection and Incentives. A discontinuous shift in charge severity is consistent with either electoral selection or incentives accounts. To assess whether the overall RDD estimate conflates intrinsic preferences and cross-party differences in electoral concerns, I leverage the fact that electoral incentives are increasing in temporal proximity to the election date (Huber and Gordon, 2004; Bandyopadhyay and McCannon, 2014). In particular, I report specifications where the partisan effects are estimated separately by years to reelection. If electoral pressures do not exert differential influence on prosecutors, I would expect partisan effects by race to remain similar over time.

consistent with the mediator monotonicity assumption.

²¹Furthermore, the latter approach relaxes the mediator monotonicity and relative non-severity of racial stops assumptions.

Influence of Political Geography. Two patterns in the data would be consistent with an impact of local electoral considerations on the racial charging gap. First, we would expect differences in the distribution of political support between areas where African Americans defendants reside and areas where White defendants live. Descriptively, I assess whether African American defendants are more likely to reside in precincts where Republican candidates perform poorly. Second, we would expect the partisan effect to be concentrated in misaligned precincts. Specifically, I examine whether Democratic-leaning precincts in Republican jurisdictions drive the main racial heterogeneity result. At the time of writing, precinct-level prosecutor electoral results are only available for a handful of elections. Based upon the strong correlation between local-level and presidential vote share (Warshaw, 2019), I leverage presidential electoral returns in those states where defendant addresses could be geo-located (Texas and Virginia).

Ultimate Effects. The final component of my analysis turns to the effect of partisanship on incarceration. In particular, I estimate the RDD effect on probability of conviction and the (logged) sentence length.

5 Empirical Findings

5.1 Descriptive Results

I begin by assessing descriptively whether prosecutors select more severe charges against minority defendants. Prior studies demonstrate considerable racial disparities in charging. Evidence from federal courts indicates that Black defendants are twice as likely to face a mandatory minimum charge and receive charges that are 6-9% more severe than white defendants (Rehavi and Starr, 2012). A study of Wisconsin Circuit Courts indicates that Black defendants are also twenty-five percent less likely to have charges dropped or reduced (Berdejó, 2018).

I restrict attention to Black and White defendants only, as these two groups account for 97% of defendants for whom race was coded. Given enormous differences in the composition of racial

groups and average severity across states, I present difference-in-means estimates by state.

The first panel of Table 1 reports racial disparities on charge severity in Texas. Conditional on indictment, defendants coded as Black receive charges that would carry 13% longer incarceration terms. Differences in defendant location cannot explain racial disparities; adjusting for district- and year- fixed effects in the second column produces a comparable estimate. To identify whether Black and White arrestees caught engaging in the same criminal conduct receive different charges, the third column adjusts for the 1500 arrest charge fixed effects, prior criminal history, and sex. Conditional on the underlying criminal conduct, Black defendants receive 9.7% more severe charges. Columns 4-7 report estimates of the effect of defendant race on the probability of indictment. Across multiple specifications, the average effect of defendant race is both statistically and substantively indistinguishable from zero.

Estimates in the second and third panels imply that Black defendants in Virginia and Washington similarly face charges that are 4-15% more severe on average. In all states, the unadjusted estimates are smaller than the district-adjusted estimates, suggesting differences in racial disparities across districts. In Washington, adjusting for crime type reduces much of the within-district racial charging gap.

As a first cut at whether Republican prosecutors pursue systematically different charges than their Democratic counterparts, I present estimates from a two-way fixed effects estimator (adjusting for district and year). In the first panel of Table 2, the unadjusted estimate in Texas implies that Republican prosecutors seek 2.5% *less* severe charges on average. This effect is imprecisely estimated and may reflect differences in the composition of arrest - Republican prosecutors seek 1.5% more severe charges after adjusting for arrest crime (Column 2). Panel (B) reports estimates of the effect of partisanship on the probability of indictment. Surprisingly, I find that Republican prosecutors are 0.4% less likely to file charges. Although statistically insignificant, this pattern persists whether or not one adjusts for arrest crime. While the estimates from Virginia are similar to the adjusted estimates from Texas (1.4%), the estimates from Washington indicate that partisan affiliation is associated with a significant increase in severity (5.6-7.2%).

Table 1: Descriptive Evidence of Racial Disparities in Charging

	(logged) Charge Severity			Probability of Indictment		
	<i>A. Texas</i>					
Black	0.133 (0.029)	0.161 (0.011)	0.097 (0.021)	0.015 (0.017)	0.007 (0.004)	0.001 (0.002)
Observations	3.158m	3.158m	3.157m	3.844m	3.844mm	3.835m
	<i>B. Virginia</i>					
Black	0.142 (0.025)	0.153 (0.009)	0.115 (0.007)	-	-	-
Observations	0.762m	0.762m	0.762m	-	-	-
	<i>C. Washington</i>					
Black	0.115 (0.111)	0.162 (0.034)	0.039 (0.024)	-	-	-
Observations	0.601m	0.601m	0.601m	-	-	-
Year FE	N	Y	Y	N	Y	Y
District FE	N	Y	Y	N	Y	Y
Def. Characteristics	N	N	Y	N	N	Y

Robust standard errors clustered by district are presented in parentheses. Defendant characteristics include dummies for prior criminal history, crime type (violent, property, or drug) and sex across all panels. Estimates in Texas further include 1,500 arrest charge fixed effects as defendant covariates.

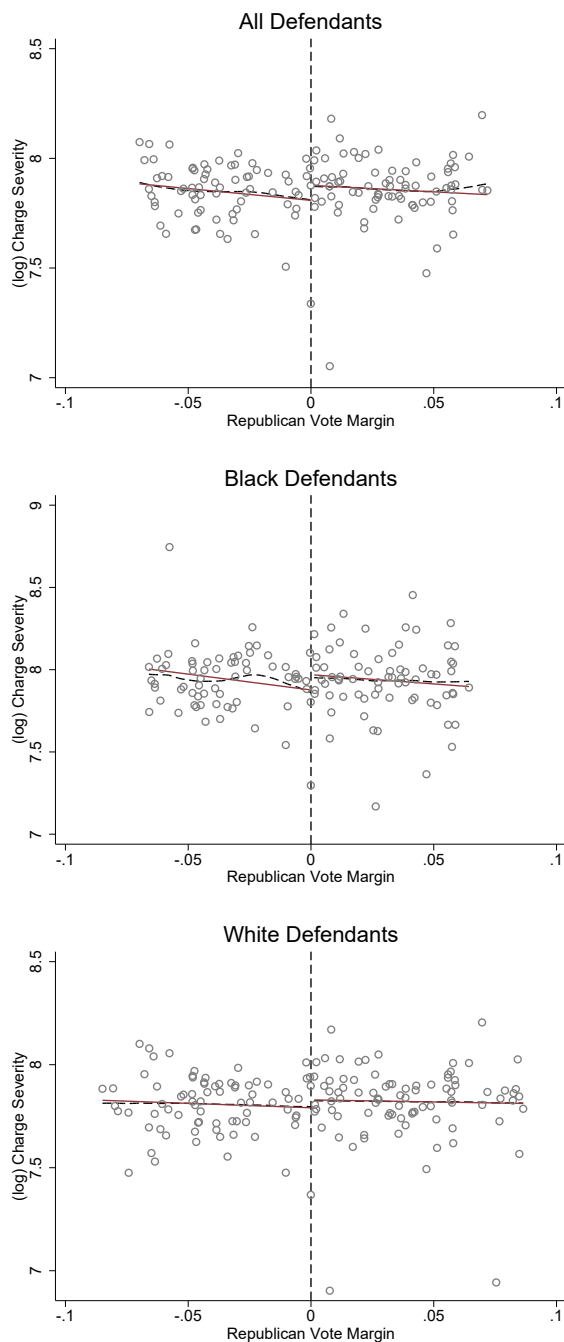
5.2 Local-Linear Estimates of Partisanship

To assess partisan differences more rigorously, I turn next to the RDD specification. As the identifying assumption (continuity) relies on a sizeable number of observations near the cutoff, I pool observations from all states. Owing to considerable cross-state differences in severity highlighted above, I focus on an adjusted estimator in the section. The primary dependent variable is (logged) charge severity adjusted for state-by-year fixed effects.

Figure 1 illustrates binned averages of the charge severity against Republican vote share, with local-linear predictions, fit separately on either side of the cutoff. The evidence of partisan effects presented in Table 2 also appears at the Republican margin cutoff.

Table 3 presents the corresponding local linear regression estimates. Column (1) reports the estimate for the pooled sample. Although imprecisely estimated, I find that narrowly elected Republicans seek charges that carry 7% greater incarceration years on average. Column (2) further adjusts for district fixed effects to ensure that this effect is not driven by comparisons across districts. Column (3) adjusts for defendant sex, prior criminal history and crime type (whether

Figure 1: Effect of Partisan Affiliation on Indictment Severity



Binned means of (logged) charge severity. Estimates on the right residualize using district-fixed effects. A local-linear (maroon) and local polynomial smoother (grey) are fit on either side of the Republican vote margin cutoff.

Table 2: Descriptive Evidence of Partisan Effects in Charging

	TX		VA		WA	
	<i>A. (logged) Charge Severity</i>					
Republican	-0.025 (0.017)	0.015 (0.013)	0.014 (0.018)	0.014 (0.015)	0.058 (0.023)	0.072 (0.029)
Observations	3.088m	3.087m	0.768m	0.767m	0.691m	0.689m
	<i>B. Probability of Indictment</i>					
Republican	-0.004 (0.011)	-0.004 (0.010)	-	-	-	-
Observations	3.768m	3.759m	-	-	-	-
Def. Characteristics	N	Y	N	Y	N	Y

Two-way fixed effects estimates (each column adjusts for district- and year- level fixed effects). Robust standard errors clustered by election are presented in parentheses. Defendant characteristics include dummies for prior criminal history, crime type (violent, property, or drug) and sex across all panels. Estimates from Texas further include 1,500 arrest charge fixed effects as defendant covariates.

violent, drug, property or other). The adjusted estimates range from 2-3% in magnitude and are similarly indistinguishable from zero.

The second panel indicates that partisan affiliation induces prosecutors to seek charges that would carry significantly longer sentences for Black defendants. In the unadjusted specification, I find a 21% discontinuous shift in severity. Adjusting for district dummies and defendant characteristics, the effect remains substantively large (10-14%) and statistically significant. Finally, the corresponding estimates for White defendants are small (0.4-2.7%) and estimated imprecisely.

Robustness. The data-driven MSE-optimal bandwidths employed in Table 3 are standard in the literature. Nevertheless, this procedure may produce knife-edge results in practice. To assuage this concern, I replicate the unadjusted and fully adjusted estimates in Table 3 employing various bandwidths from 2 to 25 percent. Appendix Figure 5 indicates that the unadjusted estimate for the pooled sample is larger on either side of the MSE-optimal bandwidth (depicted as a red line). In particular, the estimate is significant for bandwidths between 6-14 percentage points. The estimated effect for Black defendants is robust to bandwidths ranging from 2 percent to 16 percent. Nonetheless, the corresponding effect for White defendants is small and is centered around zero for larger bandwidths.

Table 3: Effect of Partisan Affiliation on Indictment Severity, by Race

	<i>A. All Defendants</i>		
RD estimate	0.077 (0.053)	0.018 (0.040)	0.038 (0.047)
Left-hand side intercept	7.757 (0.033)	7.728 (0.039)	7.715 (0.044)
Bandwidth	0.056	0.064	0.065
Eff. Observations	433,905	617,368	694,985
Eff. Elections	120	135	139
	<i>B. Black Defendants</i>		
RD estimate	0.214 (0.081)	0.101 (0.049)	0.147 (0.055)
Left-hand side intercept	7.782 (0.070)	7.783 (0.047)	7.722 (0.046)
Bandwidth	0.057	0.063	0.066
Eff. Observations	159,472	158,184	164,386
Eff. Elections	120	135	140
	<i>C. White Defendants</i>		
RD estimate	0.027 (0.047)	0.007 (0.038)	0.005 (0.042)
Left-hand side intercept	7.715 (0.035)	7.665 (0.028)	7.680 (0.032)
Bandwidth	0.067	0.100	0.089
Eff. Observations	313,547	627,893	566,127
Eff. Elections	142	179	165
District FE	N	Y	Y
Defendant Characteristics	N	N	Y

The dependent variable is (logged) indictment charge severity, adjusted by state-and year- fixed effects. Defendant characteristics include dummies for prior criminal history, crime type (violent, property, or drug) and sex. Standard errors are clustered at the election level.

One might be similarly concerned that the choice to cluster standard errors at the level of treatment (the election), although standard, maybe anti-conservative. Panel (A) of Table 10 reports substantively similar results when I cluster at the district level. To check whether my estimates are overly dependent on the procedure by which I aggregate charges to cases (a state-by-state function of prior criminal history, number of charges and sentencing enhancements), I construct a simpler version of severity - the sum of the maximum penalties for all indictment charges. Panel (B) of Table 10 reports estimates that are nearly identical to estimates in Table 3 for Black defendants. Owing to the large number of Virginian elections in which Independent candidates are competitive (42.8%), the sample pools contests between Democrats and Republicans and contests between Independents and Republicans. Panel (C) reports estimates where Independents are excluded; while reducing the effective number of elections by 20%, the estimates remain unaffected by this choice.

While state-year fixed-effects ensure that comparisons between states do not drive the estimated effect, one might be concerned that the within-state effect is driven by a specific state or group of districts. Table 11 replicates the primary analyses excluding one state at a time. While the estimated effect of partisanship on Black defendants remains similar in magnitude across samples (20.1%-24%), the effect is more precisely estimated in samples that include Texas.

5.3 Accounting for Sample Selection Bias

Sample selection bias (post-treatment changes in the composition of indictments or arrests) is a primary threat to inference in studies of prosecutorial behavior. This is because any change in severity at the cutoff could reflect actual changes in the disposition of filed cases or changes in case composition. This section leverages additional arrest records from Texas to assess the direction and magnitude of sample selection bias (which may vary by defendant race). To preview the findings, I find that Republican prosecutors are moderately more likely to file charges overall (by 2.8%) and this effect is larger in cases involving Black defendants (6.1%). Thus, selection tends to downward bias estimates of the RDD effect on charge severity (by 2-4%). Furthermore, the distribution of observed arrests imply a similar pattern with respect to sample selection in arrests. Sample selection into arrest reduces the RDD effect on the probability of indictment, a pattern disproportionately

driven by cases involving Black defendants.

5.3.1 Sample Selection in Indictments

I begin with estimates of the extensive margin and intensive margin effect on charge severity. Recall that the extensive margin is the RDD effect on the probability of indictment. The intensive margin is the difference in the conditional means of charge severity accounting for the compositional change in indictments. For comparison, I also report the naive (conditional-on-indictment) intensive margin effect which corresponds to RDD estimates in Table 3. To account for police behavior, I report specifications that adjust for the severity of arrest crime (a function of the arrest offense and defendant priors).

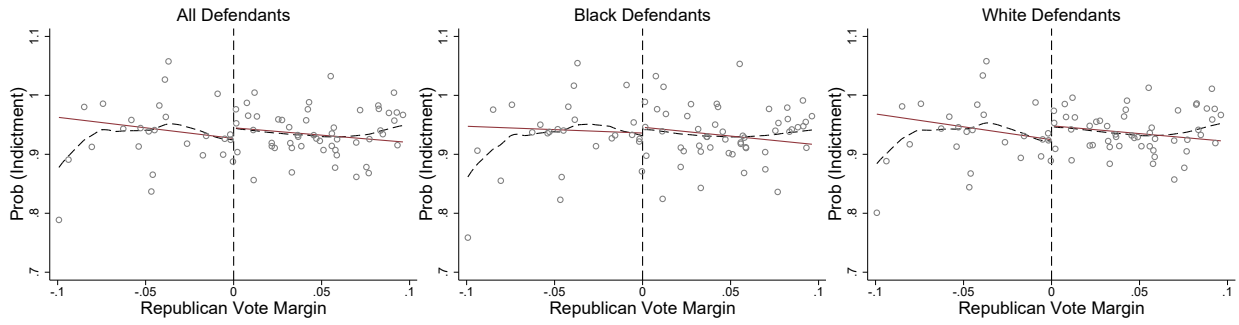
The top plot in Figure 2 illustrates the probability of indictment by Republican vote margin, adjusting for district- and year- fixed effects as well as defendant characteristics. I find a small upward shift in indictment rates at the cutoff, although this pattern does not appear to vary significantly by defendant race. The bottom plot demonstrates a larger shift in charge severity conditional on indictment. Descriptively, this effect appears largest in the middle panel which restricts attention to Black defendants.

The corresponding local-linear estimates are reported In Table 4. Panel (A) indicates that marginally elected Republican prosecutors are 2.8-3.5% more likely to seek an indictment for all defendants. Partisan affiliation is associated with a larger increase (by 6.1-7.1%) in the probability of indictment for Black defendants. Nonetheless, both the overall effect and the effects by race are imprecisely estimated.

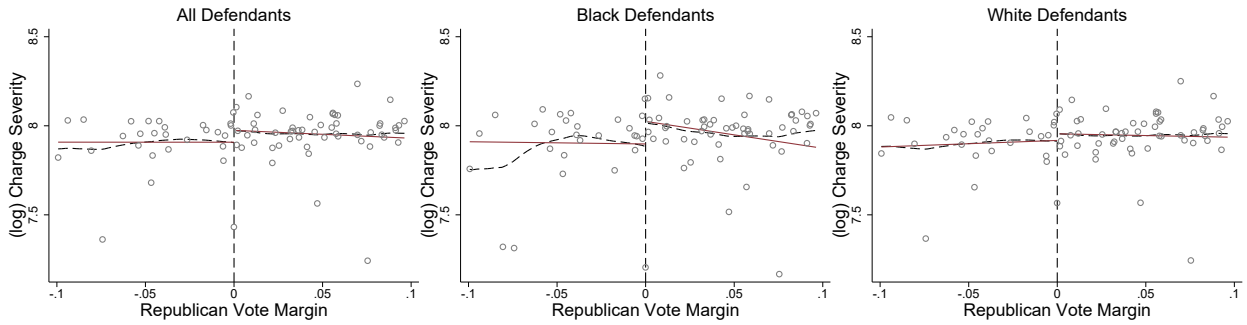
Panel (B) reports the intensive margin estimate with bootstrapped standard errors. Narrowly elected Republicans select charges that would carry -0.3-7.7% longer incarceration terms. Consistent with Table 3, partisan effects are concentrated in cases involving Black defendants (11.9-14.1%). By comparison, the RDD effect on White defendants is small and varies in direction. Finally, Panel (C) reports the conditional-on-indictment estimates for this sample. Comparing these results to the intensive margin estimates, the former are substantively similar but 2-3% smaller in magnitude. This would imply that accounting for changes in the composition of indictments does not

Figure 2: Effect of Partisan Affiliation on Indictment Outcomes in Texas

A. Extensive Margin: Prob. of Indictment



B. Naive Intensive Margin: (log) Charge Severity



Binned means of indictment outcomes – the probability of indictment on the top and (log) charge severity conditional on indictment on the bottom. Estimates in each figure residualize using district and year-fixed effects, as well as defendant sex, prior criminal history and arrest offense dummies. A local-linear (maroon) and local polynomial smoother (grey) are fit on either side of the Republican vote margin cutoff.

significantly alter my conclusions.

Table 4: Accounting for Selection into Indictment: RD Estimates

	All		Black		White	
	A. Extensive Margin					
RD estimate	0.035 (0.112)	0.028 (0.112)	0.071 (0.185)	0.061 (0.182)	0.032 (0.086)	0.032 (0.086)
Left-hand side intercept	0.852 (0.104)	0.870 (0.104)	0.841 (0.181)	0.854 (0.177)	0.851 (0.074)	0.871 (0.074)
Bandwidth	0.062	0.063	0.062	0.063	0.068	0.069
Eff. Observations	0.54m	0.54m	0.21m	0.22m	0.39m	0.39m
Eff. Elections	59	60	59	60	60	60
	B. Intensive Margin					
RD estimate	-0.003 (0.016)	0.077 (0.043)	0.119 (0.017)	0.141 (0.010)	-0.049 (0.030)	0.045 (0.011)
	C. Naive Intensive Margin					
RD estimate	-0.006 (0.044)	0.056 (0.069)	0.090 (0.048)	0.115 (0.057)	-0.059 (0.051)	0.023 (0.067)
Left-hand side intercept	7.975 (0.043)	7.917 (0.069)	8.022 (0.046)	7.928 (0.055)	7.926 (0.049)	7.904 (0.065)
Bandwidth	0.056	0.061	0.048	0.056	0.064	0.087
Eff. Observations	0.37m	0.37m	0.15m	0.13m	0.22m	0.36m
Eff. Elections	51	59	45	51	60	73
Defendant Covariates	N	Y	N	Y	N	Y

Standard errors clustered at the elections level in Panels (A) and (C); constructed from the nonparametric bootstrap in Panel (B). The dependent variable is the decision to file in Panel (A) and (log) charge severity in Panels (B) and (C). Each column adjusts for district-and year- fixed effects. Defendant covariates include the severity of the underlying arrest offense, prior criminal history, and sex.

Robustness: To generate the indictment outcome, I coded cases where an arrest record did not match a corresponding prosecution record as zeros (non-indictments). Yet, this procedure may fail to identify all indictments if administrative record keeping by prosecutors is poor. To the extent that poor record keeping correlates with partisanship, our estimates of the extensive margin and intensive margin will be biased. To tackle this concern, I employ Manski bounds, a popular approach to handle missingness in outcome variables. In particular, I consider extreme scenarios wherein all unmatched arrest records would be prosecuted if and only if (a) the prosecutor is a Democrat or (b) the prosecutor is a Republican. Table 12 replicates the fully adjusted specifications in Panels (A) and (B) of Table 4. Unsurprisingly, the bounds on the extensive margin estimate cover zero for

both Black defendants ($[-0.115, 0.096]$) and White defendants ($[-0.105, 0.09]$). Nonetheless, even if all unmatched arrests only produced indictments under Democratic prosecutors, the intensive margin estimate would remain considerably larger for Black defendants.

5.3.2 Sample Selection in Arrests

Turning to selection into arrest, Table 5 reports naive and bias-corrected risk ratio estimates by defendant race. The naive risk ratio estimates reproduce the extensive margin estimates in Panel (A) of Table 4 as a risk ratio. The bias-corrected estimates are generated under the assumption that the estimated LATE is the ATE within the optimal bandwidths.

Column (1) indicates that across all defendants, Republican prosecutors are 1.291 times more likely to seek an indictment (just under 25% larger than the naive estimate reported in Panel B). This suggests a discontinuous increase in the probability of arrest, which would be consistent with a lower threshold for arrest and an over-representation of minor crimes in Republican jurisdictions. Columns (2)-(3) indicate that selection into arrest is slightly larger for Black defendants (50% larger than the naive estimate) relative to White defendants (13% larger than the naive estimate). In sum, the bias-corrected estimates indicate that failing to account for selection produces downwardly biased estimates. The pattern of differential selection by race would indicate that the treatment effect heterogeneity on indictment outcomes at the RDD threshold is also downwardly biased.

Table 5: Accounting for Selection into Arrest: Risk Ratio Estimates

	All	Black	White
	(A) Bias-Corrected Risk Ratio		
RR Estimate	1.291	1.586	1.165
	[1.277, 1.305]	[1.535, 1.620]	[1.159, 1.172]
	(B) Naive Risk Ratio		
RR Estimate	1.034	1.076	1.036
	[1.030, 1.038]	[1.063, 1.089]	[1.030, 1.041]
Adjusted	Y	Y	Y

Estimates adjust for district- and year- fixed effects as well as defendant characteristics (the severity of the underlying arrest offense, prior criminal history, and sex). Bootstrapped 95% confidence intervals reported in parenthesis.

5.4 Local-Linear Estimates by Electoral Cycle

Next, I examine whether electoral concerns exacerbate or mitigate partisan differences in criminal prosecutions. To assess the effect of both reelection incentives (which may vary by defendant race) and selection, I allow the LATE to change as a function of both the electoral calendar and defendant race. Figure 3 reports estimates of the effect of partisanship by year to re-election (black lines). During non-reelection years, partisan differences in indictment charge severity are small (3-6%) and imprecisely estimated. Approaching reelection, narrowly elected Republican prosecutors seek charges that carry on average 24% greater years of incarceration. The difference-in-RDD estimate (differencing the reelection year effect from the pooled effect for other years) is substantively large (20.27%) and statistically significant at the 10% level. An effect of partisanship on prosecutorial severity that increases during periods of electoral concern is consistent with the view that differences in the electoral value of charge severity, not primitive differences in preferences, drive the overall partisan difference.

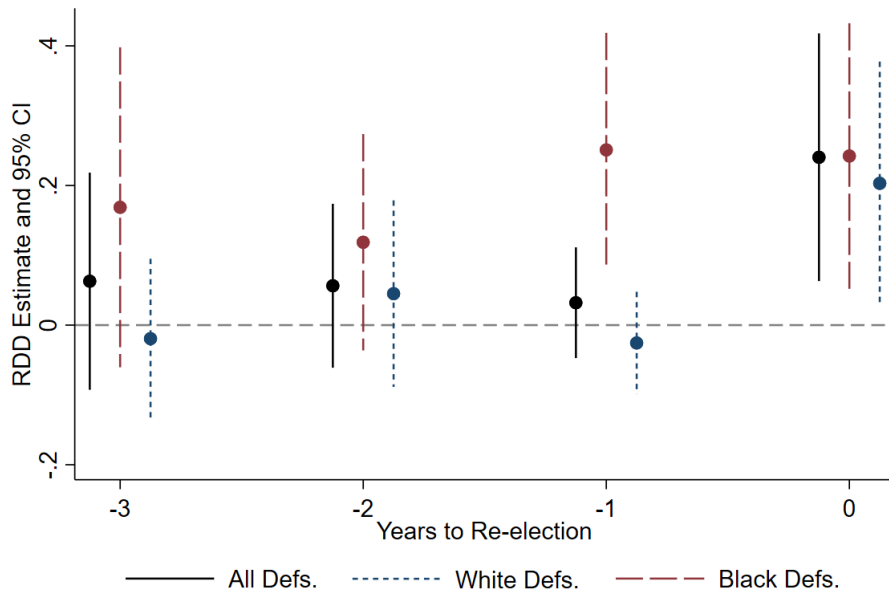
Figure 3 also presents the RDD estimates by reported race (Black defendants depicted in red; White defendants represented in blue). In non-reelection years, narrowly elected Republicans select charges that would induce 11-25% longer incarceration terms for Black defendants. Partisan effects are statistically significant in the reelection year and the year preceding the reelection year for this group.

In contrast, partisan affiliation has a small and insignificant effect (-1-4.5%) on severity for White defendants during non-reelection years. In reelection years, the effect of partisanship on case severity increases dramatically, bringing the size of RDD estimate for White defendants (20.3%) on par to Black defendants (24.22%). Due to the smaller baseline for White defendants, the difference-in-RDD estimate indicates that reelection incentives induce a more significant relative increase in severity for White defendants (18.44%, significant at the 10% level) compared to Black defendants (8.42%).

In sum, narrowly elected Republican prosecutors increase overall charge severity during reelection years (relative to Democrats). Furthermore, the racial treatment effect heterogeneity is larger when prosecutors are insulated from electoral pressure. That is, electoral incentives increase the

overall partisan difference without increasing racial differences across parties.

Figure 3: Effect of Reelection and Partisanship on Racial Burden



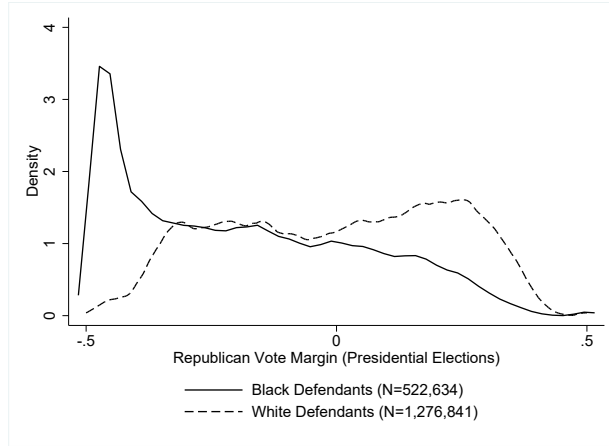
Each point corresponds to the RDD effect on (logged) indictment charge severity. Standard errors are clustered at the election level.

5.5 Influence of Local Political Geography.

Figure 4 plots presidential Republican vote margin in the precincts where African American and White defendants reside. We see that the overwhelming majority of African American defendants (75.08%) are located in precincts where a majority of voters selected Democratic presidential candidates. By comparison, White defendants are approximately evenly distributed between Democratic-leaning (48.03%) and Republican-leaning (51.87%) precincts. This difference is driven by districts electing Republican prosecutors, where 73.26% of Black defendants and 39.46% of White defendants reside in Democratic-leaning precincts. In contrast, in districts electing Democratic prosecutors, 21.15% of Black defendants and 36.18% of White defendants live in Republican-leaning precincts.

Racial differences in the distribution of political support alone would not explain the main heterogeneity result. To examine whether local political geography drives that finding, Table 6 reports the primary RDD estimate by defendant race and precinct partisanship. Precincts are

Figure 4: Distribution of Minority Communities across Precincts



Precinct-level presidential vote margin in Texas and Virginia.

classified as Democratic- or Republican-leaning based on whether a majority of voters selected the Republican candidate in the previous Presidential election. We see that prosecutor partisanship has a significant and positive effect on charge severity only on cases involving African American defendants in Democratic-leaning precincts. Given the evidence of racial segregation in Figure 4, it is perhaps no surprise that the partisan effect for African American defendants in Republican-leaning precincts is imprecisely estimated. For White defendants, the RDD estimate is negative but imprecisely estimated across precincts.

Table 6: Effect of Local Political Geography and Partisanship on Racial Burden

	Dem-Leaning Precincts			Rep-Leaning Precincts		
	All	Black	White	All	Black	White
RD estimate	0.051 (0.063)	0.100 (0.047)	-0.020 (0.059)	-0.040 (0.040)	0.025 (0.043)	-0.079 (0.043)
Left-hand side intercept	7.963 (0.062)	8.024 (0.044)	7.928 (0.054)	7.915 (0.036)	7.996 (0.040)	7.902 (0.039)
Bandwidth	0.077	0.047	0.080	0.068	0.093	0.066
Eff. Observations	0.28m	0.06m	0.14m	0.084m	0.02m	0.06m
Eff. Elections	126	83	126	118	140	115

Standard errors clustered at the elections level. The dependent variable is(log) charge severity. Each column adjusts for state-by year- fixed effects.

5.6 Local-Linear Estimates of Partisanship on Incarceration

Turning to sentence lengths, Table 7 reports the effect of partisan affiliation in Texas. I restrict attention to Texas to capture the full impact of prosecutorial discretion and adjust for arrest severity. Marginally elected Republican prosecutors are significantly more likely to attain conviction (by 17.5%). Disaggregating by race, I find that the probability of conviction increases by a similar magnitude for both groups (19% for Black defendants and 15.9% for White defendants). The RDD effect on sentence severity is also positive (5.7%) but indistinguishable from zero. Nonetheless, there are significant racial differences on this margin – partisanship affects the length of incarceration for Black defendants (15.5%) while having little impact on White defendants (1.2%).

Table 7: Effect of Partisanship on Incarceration (Texas)

	Prob(Conviction)			(log) Sentence Severity		
	All	Black	White	All	Black	White
RD estimate	0.175 (0.037)	0.19 (0.029)	0.158 (0.036)	0.057 (0.041)	0.155 (0.045)	0.012 (0.036)
Adjusted	Y	Y	Y	Y	Y	Y
Bandwidth	0.047	0.035	0.053	0.059	0.058	0.058
Eff Obs	0.5m	0.21m	0.28m	0.56m	0.24m	0.32m

Estimates adjust for district- and year- fixed effects as well as defendant characteristics (the severity of the underlying arrest offense, prior criminal history, and sex). Standard errors are clustered at the election level.

6 Discussion

Are racial disparities in criminal justice exacerbated by differential treatment on the part of local law enforcement officials? This article examines the influence of partisan affiliation on the behavior of elected district attorneys in three U.S. states. I find that narrowly elected Republicans seek charges that would carry longer incarceration terms, but only for cases involving Black defendants. Additional tests conclude that observable defendant characteristics (including sex, prior criminal history, crime type, or arrest offense) or selection into arrest cannot explain this result. Prosecutor partisanship is not merely expressive - narrowly elected Republicans attain significantly longer prison sentences but only for cases involving Black defendants.

Significant partisan differences in this electoral environment can arise from persistent policy-related differences (selection mechanisms) or differences in electoral motivations across parties (incentives mechanisms). Consistent with one incentives mechanism, I find that reelection concerns differ across parties – relative to their Democratic counterparts, Republican prosecutors seek more severe charges only during periods of electoral scrutiny. This finding indicates that electoral selection and incentives can interact strategically; future work might address how to extend the standard research design toolkit to account for such strategic interactions.

More broadly, this result implies that criminal justice reforms focused on who is in office without altering the discretionary power afforded to prosecutors may be insufficient. In 2016, prosecutors running on progressive decarceration platforms began to win elections across the country.²² As partisan effects are insignificant during non-reelection years (pooling all defendants together), the impact of prosecutorial leniency on courtroom proceedings may be limited for much of the electoral term. While electoral incentives appear to bind Republican prosecutors more, the recent experience of progressive prosecutors in competitive districts (such as Harris County District Attorney’s Kim Ogg) illustrate that Democratic prosecutors are not insulated from electoral concerns. Accordingly, it may be more fruitful to evaluate progressive candidates on their willingness to address failures in criminal justice (e.g., wrongful conviction review) and limit prosecutorial discretion (e.g., commitments to bail reform, relaxing mandatory minimums, etc.)

Nonetheless, the increase in partisan effects during periods of electoral scrutiny is concentrated in cases involving White defendants, implying that this mechanism cannot fully account for the main racial heterogeneity result. Instead, my findings point to a mismatch between the political preferences of the local communities where Black defendants reside and their elected prosecutors. This finding implies that the design of political boundaries can cause officeholders to fail to internalize the disparate costs of incarceration. Understanding how the geographic distributions of crime, race and partisanship jointly affect the behavior of elected law enforcement officials is an important topic for future research.

²²All told, as many as 10 percent of people in the U.S. may reside in jurisdictions with a progressive prosecutor. Source: <https://theappeal.org/the-successes-and-shortcomings-of-larry-krasners-trailblazing-first-term/>.

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Appendix

Appendix A Descriptive Analyses and Statistics

Table 8:
Summary Statistics by State

	Texas		Virginia		Washington	
	Mean	SD	Mean	SD	Mean	SD
(log) Indictment Severity (days)	7.948	1.409	8.210	1.202	5.944	1.065
Prob (Indictment)	0.909	0.288	-	-	-	-
(log) Arrest Severity (days)	8.103	1.301	-	-	-	-
Republican Vote Margin	0.020	0.073	-0.001	0.111	0.037	0.083
Reelection Year	0.261	0.439	0.281	0.449	0.260	0.439
Black	0.282	0.450	0.417	0.493	0.110	0.312
White	0.709	0.454	0.562	0.496	0.745	0.436
Female	0.211	0.408	0.246	0.431	0.219	0.414
Violent Crime	0.191	0.393	0.142	0.349	0.145	0.352
Drug Crime	0.331	0.471	0.259	0.438	0.258	0.438
Property Crime	0.101	0.302	0.251	0.434	0.217	0.412
Total Observations	3,663,669		1,319,148		794,610	

Table 9: RDD Validity Tests

	Estimate	Std. Error	Eff. Obs	Eff. Elections
<i>A. Pretreatment Covariates</i>				
Black population share	0.053	(0.069)	160	160
(log) Total population	0.133	(0.560)	185	185
Republican Vote Share _{t-1}	0.014	(0.039)	44	44
<i>B. Defendant Characteristics</i>				
Female	0.003	(0.011)	0.778m	147
Prior Criminal History	0.041	(0.060)	1.308m	165
<i>C. Arrest Characteristics</i>				
Prob(Arrest Traffic Stop)	-0.002	(0.004)	3.494m	77
(log) Count of Traffic Stops	0.339	(1.353)	75	75
(log) Arrest Charge Severity (Texas)	-0.107	(0.071)	0.594m	63
(log) Number of Arrests (Texas)	0.234	(1.015)	85	85
<i>D. Discontinuity in Density of Running Variable</i>				
McCrary Density Test	0.074	(0.226)		
CJM (2020) Estimator	1.761	(1.207)		

Panels (A) - (C) report bias-corrected RDD estimates with standard errors clustered on the election. In Panel (D), the first column reports the estimated discontinuity in the running variable using a binsize of 0.014; the second column reports the estimated discontinuity using the nonparametric density estimator of Cattaneo, Jansson and Ma (2020) (CJM). Population data drawn from the 2000 and 2010 Censuses. Data on patrol stops were collected from the Stanford Open Policing project (Source: <https://openpolicing.stanford.edu/>). The traffic stops sample consists of those states with arrest indicators in state traffic stop records, and at least three competitive prosecutor elections in recent years. Specifically, the sample includes five states (FL, MI, NC, OH, WI) with varying coverage between 2004-2018, and encompasses 106 competitive elections and 36 million stops.

Appendix B Additional Results

Figure 5: RD Estimates, Varying Bandwidths

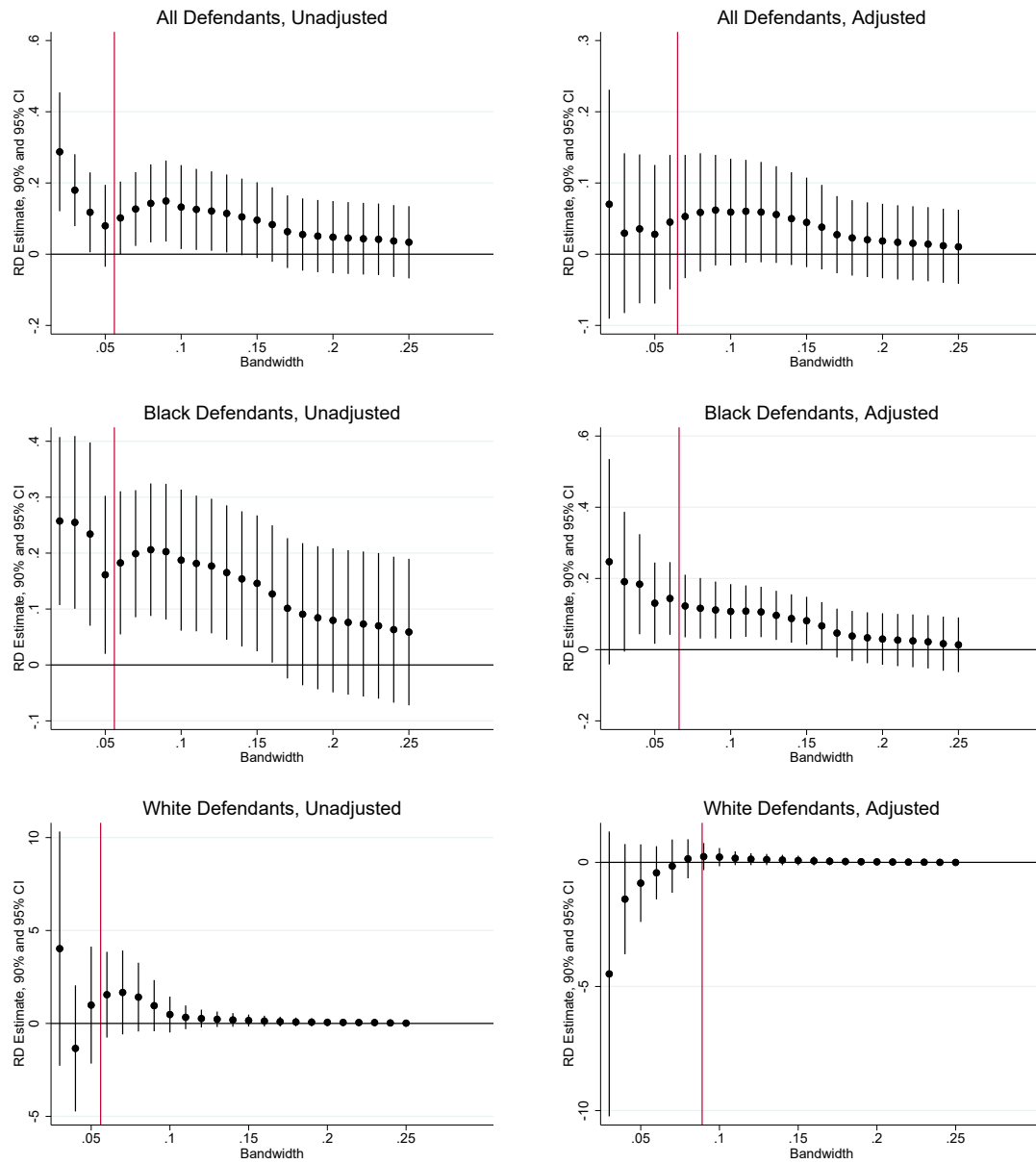


Table 10: Effect of Partisan Affiliation: Alternative Specifications

	All		Black		White	
	A. (log) Charge Severity					
RD estimate	0.076 (0.039)	0.039 (0.029)	0.235 (0.075)	0.146 (0.046)	0.030 (0.040)	0.001 (0.036)
Left-hand side intercept	7.755 (0.030)	7.713 (0.026)	7.780 (0.067)	7.720 (0.038)	7.705 (0.034)	7.681 (0.025)
Bandwidth	0.052	0.066	0.060	0.070	0.064	0.084
Eff. Observations	0.43m	0.55m	0.16m	0.16m	0.25m	0.52m
Eff. Elections	115	142	132	146	136	159
	B. (log) Charge Severity of All Offenses					
RD estimate	-0.001 (0.053)	0.045 (0.055)	0.106 (0.060)	0.143 (0.068)	-0.056 (0.060)	-0.020 (0.052)
Left-hand side intercept	8.087 (0.048)	8.008 (0.052)	8.118 (0.049)	8.034 (0.058)	8.051 (0.055)	7.985 (0.044)
Bandwidth	0.069	0.075	0.060	0.067	0.072	0.082
Eff. Observations	0.54m	0.85m	0.16m	0.16m	0.36m	0.53m
Eff. Elections	143	149	132	142	147	156
	C. Excluding Independents					
RD estimate	0.086 (0.053)	0.029 (0.049)	0.231 (0.080)	0.170 (0.064)	0.030 (0.048)	-0.004 (0.042)
Left-hand side intercept	7.754 (0.033)	7.717 (0.046)	7.772 (0.070)	7.702 (0.055)	7.707 (0.038)	7.680 (0.034)
Bandwidth	0.052	0.062	0.049	0.057	0.059	0.085
Eff. Observations	0.42m	0.68m	0.16m	0.16m	0.25m	0.54m
Eff. Elections	91	109	87	96	106	130
District FE	N	Y	N	Y	N	Y
Defendant Covariates	N	Y	N	Y	N	Y

Standard errors are clustered on the district level in Panel (A) and at the elections level in Panels (B) and (C). The dependent variable is adjusted by state-and year- fixed effects throughout. Defendant covariates include dummies for prior criminal history, crime type (violent, property, or drug) and sex.

Table 11: Effect of Partisan Affiliation: Excluding States

	All	Black	White
<i>A. Excluding TX</i>			
RD estimate	0.041 (0.149)	0.201 (0.147)	-0.057 (0.150)
Left-hand side intercept	7.573 (0.128)	7.563 (0.141)	7.620 (0.130)
Bandwidth	0.071	0.079	0.070
Eff. Observations	115,936	27,363	69,674
Eff. Elections	146	152	146
<i>B. Excluding VA</i>			
RD estimate	0.087 (0.051)	0.240 (0.070)	0.032 (0.047)
Left-hand side intercept	7.757 (0.033)	7.774 (0.061)	7.713 (0.037)
Bandwidth	0.053	0.051	0.069
Eff. Observations	404,888	149,445	272,293
Eff. Elections	116	112	143
<i>C. Excluding WA</i>			
RD estimate	0.120 (0.042)	0.226 (0.078)	0.057 (0.040)
Left-hand side intercept	7.739 (0.025)	7.780 (0.068)	7.700 (0.032)
Bandwidth	0.056	0.057	0.075
Eff. Observations	399,302	157,412	234,424
Eff. Elections	118	120	149

The dependent variable is (logged) indictment charge severity, adjusted by state-and year- fixed effects. Standard errors are clustered at the election level.

Table 12: Bounds on the Intensive and Extensive Margins

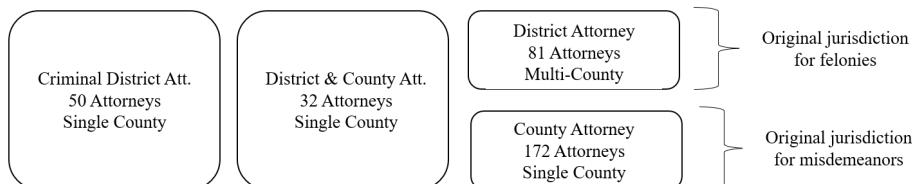
	All	Black	White
	(A) Extensive Margin: Prob. of Indictment		
RD Estimate	0.028	0.061	0.032
	[-0.113,0.091]	[-0.115,0.096]	[-0.105,0.09]
	(B) Intensive Margin: (log) Charge Severity		
RD Estimate	0.077	0.141	0.045
	[0.066,0.074]	[0.112,0.157]	[0.033,0.046]
Adjusted	Y	Y	Y

Upper and lower bounds constructed for cases where an arrest could not be matched to an indictment. Standard errors are clustered at elections level. The dependent variable is adjusted by district-and year-fixed effects throughout. Defendant covariates include dummies for prior criminal history, crime type (violent, property, or drug) and sex.

Appendix C Data Construction

Texas

Figure 6: Criminal Prosecutors in Texas



This figure does not include city prosecutors who file cases in municipal or justice courts. City prosecutors, who can be elected or appointed, try cases punishable by fine only.

Role of Prosecutors: Each judicial district elects either a Criminal District Attorney or a District Attorney (Texas Government Code Chapters 43 and 44). Generally, these offices are responsible for felony criminal prosecutions in state district courts. These offices also represent state agencies when the Attorney General’s Office does not do so. A county attorney is elected from each county in which there is not a resident criminal district attorney or district attorney who serves the same purpose. Generally, the county attorney handles only criminal misdemeanors cases in county courts, in addition to civil suits affecting the county (Texas Code of Criminal Procedure Act §2.02) and litigation involving county and local government agencies. In 32 counties, district attorneys handle duties typically assigned to counties attorneys (i.e., misdemeanor prosecutions).

Vacancies in a criminal district attorney’s, district attorney’s or county attorney’s office are filled by the county’s Commissioners Court (Local Government Code §87.041). All prosecutors may be removed by a district judge for incompetency, official misconduct, or intoxication (Local Government Code §87.012-87.013).

Data: The Computerized Criminal History (CCH) is a multi-agency database maintained by the Crime Records Services of the Texas Department of Public Safety (DPS). Any police department, Sheriff’s Office or other law enforcement entity that arrests a person in Texas for a Class B misdemeanor or more severe offense must report the arrest to the DPS. As a result, the CCH contains a record of every prosecutor’s decision to accept, reject or change charges.

Charge Severity Construction: The main dependent variable, charge severity, is the maximum

possible penalty for the most serious charge. The Texas Penal Code (TPC) classifies felonies into five categories (State Jail, Third Degree, Second Degree, First Degree and Capital Felonies), where each offense category is associated with a different maximum punishment (2 years, 10 years, 20 years, 99 years and life imprisonment/death penalty, respectively, see TPC §12.32-12.35).²³ Sentencing enhancements may include penalties for repeat and habitual offenders. With certain exceptions, preparatory offenses (criminal attempt, solicitation, etc) carry a punishment one category lower than the intended offense (TPC §15.01-15.03).

In Texas, judges are allowed to “stack” sentences under two circumstances. First, judges may demand consecutive sentences for charges arising out of a single criminal episode (TPC §3.03). TPC §3.03 applies only to a handful of crimes, including intoxication, assault/manslaughter, and sex crimes against children. If a case involves a §3.03 crime, I define case severity as the maximum severity of non-§3.03 charges plus the sum of maximum punishments for the §3.03 crimes. Otherwise, the case severity is simply the maximum permissible sentence for the most serious charge.

Texas Code of Criminal Procedure §42.08 further permits judges to stack sentences arising from multiple criminal episodes/cases. Under this provision, judges have broad discretion to determine whether confinement on subsequent offenses are imposed or suspended and, if imposed, whether they are to run concurrently or consecutively. Defendants can also move to have charges “severed,” or prosecuted separately - if, for example, they suspect that evidence admitted for one count may be prejudicial in the context of another. Nonetheless, if the facts show that charges arise from the same criminal episode, judges cannot order consecutive sentences, and doing so may be grounds for appeal - see *Drain v. State*, 540 S.W.3d 637 (Tex. App. Amarillo 2018).²⁴ Accordingly, I do not further aggregate unconsolidated cases in which either the prosecution or defense has moved to severe charges arising from the same arrest.

²³Misdemeanors are classified into three categories (Class A, Class B and Class) with maximum punishments of one year, 180 day and fines.

²⁴According to TPC §3.01, a single criminal episode encompasses either (a) all offenses committed pursuant to a single transaction/ connected transactions or (b) similar offenses that were repeatedly committed.

Virginia

Role of Prosecutors: Each county and city elects a single Commonwealth’s Attorney. Virginia Code §15.2-1627(B) requires Commonwealth’s Attorneys to prosecute felonies but affords the same prosecutors discretion over whether to prosecute misdemeanors. Since funding and staffing procedures do not account for misdemeanor prosecutions, several Commonwealth’s Attorneys either prosecute select misdemeanors only or none at all. In response, many localities either select their own misdemeanor prosecutors or permit police departments to handle misdemeanor prosecutions.

Data: The Office of the Executive Secretary at the Supreme Court of Virginia Circuit provided access to Circuit, General District and Juvenile court charges and dispositions from 2000-2020. Circuit courts in Virginia have general jurisdiction over felony cases, and appellate cases stemming from misdemeanor offenses heard in lower courts (General District or Juvenile Delinquency courts).

I exclude 723,858 probation revocation charges from the data. The statutory maximum sentence for probation violations is the same as the most serious offense for which the offender was originally convicted. From the prosecutor’s perspective, although the statutory maximum sentence is taken into account by earlier charging decisions, the decision to pursue probation violation charges may constitute an important source of prosecutorial discretion. To this point, 59% of probation revocation charges appear alongside other felony charges. Nonetheless, only 35% of probation violation charges could be consistently linked to a specific previous charge and hence severity level. I also exclude 167,000 charges that either have a duplicate within the dataset or were transferred to another jurisdiction.

Outcome Variable Construction: Virginia uses a complex sentencing guideline system, wherein a case’s recommended sentence depends upon the combination of charges, the defendant’s prior history (generally and for related charges), and case characteristics, such as weapon use. Unfortunately, many of these characteristics are unavailable, and linking defendants to priors within the data would require further assumptions.

To construct charge severity, I use the penalty range, which is available for most charges. Judges in Virginia are allowed to run any sentence consecutively with other sentences. According to the Sentencing Guidelines Commission, most judges (although not at all) correctly stack mandatory

minimum sentences occurring in the same case. A case's severity is, therefore, the sum of the maximum penalties for which charge.

Washington

Role of Prosecutors: Each county elects a single Prosecuting Attorney. These attorneys prosecute all criminal (felony and misdemeanor) and civil actions in which the state or the county may be a party (Revised Code of Washington (RCW) 36.27.020). Municipalities elect additional municipal prosecutors to prosecute municipal criminal codes (Auburn v. Gauntt, 174 Wn.2d 321 (2012); RCW 39.34.180(1)). Municipal criminal violations carry up to a one year jail sentence, and may include crimes that are "incorporated by reference" from state criminal laws. For example, a city's code may penalize indecent exposure by including language referencing the equivalent state criminal law (RCW 9A.88.010).

Data: The Washington State Administrative Office of the Courts provided access to the entire Superior Court caseload from 2000-2020, excluding Kings County.

Outcome Variable Construction: Washington criminal law classifies criminal offenses into three felony offense categories (A, B and C) carrying maximum sentences of life imprisonment, ten years and five years respectively and two misdemeanor offense categories (Misdemeanor and Gross Misdemeanors) carrying maximum sentences of 90 days and one year respectively (RCW 9A.20.021). Since 1984, courts have imposed sentences based on a sentencing range derived from sentencing grids (RCW Section 9.94A), which are a function of the offense level and the offender score (i.e. prior criminal history). Departures from the standard presumptive ranges must be based on compelling reason and may be appealed.

To compute case severity, I applied general sentencing forms to construct offender scores,²⁵ and matched offenses to the appropriate offense level. I then computed the sentencing range from the appropriate sentencing grid and extracted the most serious offense. Next, I identified charges involving any of 10 sentencing enhancements including enhancements related to drug use, gang membership, deadly weapon use, sex crimes, etc (RCW 9.94A.533). As judges are allowed to issue

²⁵see Section 6 of the Adult Sentencing Manual: http://www.cfc.wa.gov/PublicationSentencing/SentencingManual/Adult_Sentencing_Manual_2020.pdf

consecutive sentences only for cases involving two or more serious violent convictions (RCW RCW 9.94A.589), I aggregate charge severity to case severity depending upon whether such charges are present.

Finally, I checked whether the case severity (generated from the sentencing range maximum) exceeds the statutory maximum of the most serious offense. If there are no sentencing enhancements and the sentencing range exceeds the statutory maximum, then the case severity is the statutory maximum. If there are sentencing enhancements, the case severity is given by the sentencing range (see *State v. Cyr*, Wn.2d, 97232-7 (2020)).