Do Felon Voting Rules Affect Criminal Prosecutions?

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Abstract

Felon disenfranchisement laws can alter the composition of the electorate. Accordingly, we argue that in states with such laws, elected prosecutors may be tempted to assign felon status strategically. We test our argument using criminal case data from Virginia and leveraging a surprise gubernatorial announcement: that as of April 22, 2016, all felons would be automatically enfranchised upon completion of their prison or probation sentence. Using well-known racial differences in partisanship as a measure of likely political alignment, we show that the announcement corresponded to an *increase* in criminal charge severity and sentence length for defendants who were politically unaligned with their prosecutors, and a *decrease* for aligned defendants. The results are robust to a range of alternative specifications.

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In recent years, scholars have gained new insight into the American justice system by studying criminal justice officials, such as judges and prosecutors, as political agents motivated by the desire to remain in office. Focusing especially on the incentives of elected judges and prosecutors, scholars have shown that these officials may manipulate criminal charges and sentences in order to signal ideological congruence or competence to voters (e.g., Gordon and Huber, 2002, 2007; Stuntz, 2004; Dyke, 2007; Bandyopadhyay and McCannon, 2014; Gordon and Yntiso, 2021). These results are consistent with a vast formal and empirical literature on political accountability, in which political agents— whether bureaucrats accountable to elected officials (e.g., Gailmard and Patty, 2007, 2012), or elected officials accountable to the public (e.g., Canes-Wrone, Herron and Shotts, 2001; Alt, Bueno de Mesquita and Rose, 2011)—must make policy choices with an eye toward the principals who keep them in office.[1]

In this paper, we argue that elected criminal justice officials are similar to other political actors not merely in having a principal-agent relationship with an electorate, but also in having powers, vis-a-vis their principals, which ordinary agents lack. For example, rather than choosing a set of policies her constituents approve, a legislator may try to choose a new set of constituents which approves her policies, via gerrymandering or the manipulation of voting restrictions such as felon disenfranchisement laws (Bateman, 2018; Helmke, Kroeger and Paine, 2022; Eubank and Fresh, 2022).[2] We argue that elected criminal justice officials may be similarly able to shape the composition of their local electorates to favor themselves or their partisan allies, and disfavor their partisan opponents. The reason is that many states in the U.S. restrict felon voting, allowing officials to shape the electorate through the selective imposition of disenfranchising felony sentences. The temptation to do this may be especially great for elected prosecutors,

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[1]For more comprehensive reviews of this literature, see Ashworth (2012); Gailmard (2014).

[2]Or, in some cases, they may render the composition of the constituency irrelevant by simply replacing elected positions with appointed ones (Komisarchik, 2025).

whose immense discretion gives them an outsize role in the administration of justice (Gordon and Huber, 2009; Stuntz, 2011; Tonry, 2012).

To illustrate our argument more precisely, we construct a simple formal model (presented in full in the Appendix) featuring a prosecutor (he) who may offer a defendant a plea deal or take her to trial, and a defendant (she) who may accept or reject. Trial is costly to both players, and the prosecutor wants to secure a conviction and as high a sentence as possible, while the defendant wants to minimize her expected loss. Consistent with prior work among both political economists and legal scholars (Landes, 1971; Grossman and Katz, 1983; Baker and Mezzetti, 2001; Gordon and Huber, 2009; Stuntz, 2011) we assume that the prosecutor has wide discretion and can therefore select the precise plea offer that maximizes her utility. We show that when the prosecutor also benefits from the outcome of an upcoming election, and the defendant's probability of voting in this election is decreasing in the length of her prison sentence, the prosecutor's optimal plea sentence is increasing (decreasing) in the degree to which he doesn't (does) want the defendant to vote.

To investigate the empirical validity of our argument, we leveraged an unanticipated shock to the relationship between felony criminal sentencing and felon disenfranchisement in the state of Virginia: Governor Terry McAuliffe's unexpected 2016 announcement of the automatic enfranchisement of all felons upon completion of their "sentences of [felony] incarceration" and "supervised release." This announcement heralded a substantial change from the previous regime, in which the length of disenfranchisement and the procedures for rights restoration varied across different types of felony convictions. It had several immediate consequences. First, according to the governor, it resulted in the immediate enfranchisement of approximately 200,000 individuals (Horwitz and Portnoy, 2016). Second, it made the length of a felony prison or probation sentence the key determinant of (dis)enfranchisement going forward.[3] Third, it motivated voter registration drives across

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[3]Commonwealth of Virginia Executive Department, "Order for the Restoration of Rights," Apr. 22, 2016. https://web.archive.org/web/20160502123748/https://

the state, targeting the newly enfranchised felon population.[4] We hypothesize that, as a result, the announcement may have motivated Virginia's elected felony prosecutors to increase charge and sentence severity for felony defendants they perceived as politically opposed, while decreasing them for those defendants they perceived as politically aligned, in order to mitigate changes to the partisan composition of the electorate.

In order to test this hypothesis, we collected all individual felony charges filed or sentenced in Virginia Circuit Courts between 2010 and 2020. Our raw data included over 400,000 circuit (district)-level charges filed or sentenced between 2010 and 2020, each with detailed accompanying information on the name, race, sex, age, and district of the defendant, and on the charge's progress through the justice system from indictment date to the date and nature of the disposition and sentence. From this initial dataset we constructed a case-level dataset by aggregating all charges filed on the same day for the same defendant, and ordering multiple charges within a single case by the severity of their potential sentences. This process yielded a final dataset of 420,856 cases filed or disposed between 2010 and 2020, 46,002 of which were filed or disposed in 2016.

We analyzed the effect of McAuliffe's announcement on charging and sentencing behavior in our data using a nonparametric regression-discontinuity-in-time (RDiT) approach, then checked for robustness using both a local randomization approach and a simple series of pooled differences-in-means. Our main results disaggregate by alignment status, then compare the charging and sentencing outcomes experienced by each group immediately *before* the rule change, to those immediately *after* it. Following Helmke, Kroeger and Paine (2022) and Eubank and Fresh (2022), we assume that in light of racially polarized voting patterns (see, e.g., Kuriwaki et al., 2024; Mason, 2018), a defen-

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commonwealth.virginia.gov/media/5848/order_restoring_rights_4-22-16.pdf;

and https://www.census.gov/library/visualizations/2016/comm/citizen_voting_age_population/cb16-tps18_virginia.html.

[4]See, e.g., https://www.progress-index.com/story/news/crime/2016/06/05/at-voter-rights-restoration-rally/28224272007/.

dant's race is informative—though it need not be determinative—of her likely political affiliation.[5]. Because defendant race is noted on every criminal case file, this information is immediately and costlessly available to prosecutors.

Our results are as follows. First, immediately after the announcement, we find that defendants who were likely politically *unaligned* with their prosecutor experienced increases in charge severity and actual sentence length, while likely-*aligned* defendants experienced decreases. These findings persist across a range of different RDiT specifications, bandwidth choices, and placebo tests, are broadly robust to both pooled difference-in-means regression analyses and local randomization inference, and do not appear to depend on prosecutor's office size or political vulnerability.

Second, these effects appear to have occurred via a range of mechanisms. Shifts in sentence severity persist when we limit our analysis to cases charged prior to the announcement, which suggests that the policy change directly affected prosecutors' sentencing decisions. However, the announcement also appears to have affected prosecutors' initial charging decisions and their willingness to engage in bargaining mid-case—for example, by reducing or dismissing charges. To understand the announcement's total effect on sentence severity, we estimate the pre-announcement relationship between charge severity and sentence severity and use it to predict the indirect effect of post-announcement changes to charge severity on sentencing outcomes. Combining these results with the announcement's direct effect on sentencing, we find that the total effect may have been an average sentence increase of four to seven months for unaligned defendants, and an average decrease of two to three months for aligned defendants.

Why would prosecutors, especially in small districts, bother to manipulate sentences when only a small number of potential voters are affected? There are several possibilities. One is that given the existence of felon voting restrictions, the presence of informative markers of partisanship, and the discretion prosecutors enjoy, such manipulation is cost-

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[5]Racial sorting into parties is a well-known feature of contemporary American political life (e.g., Mason, 2018) which extends to the felon population (Burch, 2012).

less. Accordingly, it is worthwhile, even if the marginal improvement is small. Another explanation is that prosecutors' partisan preferences may motivate them to manipulate felon status in anticipation of state-wide, or even national, races, as well as local contests. McAuliffe's announcement was described by Republicans as an attempt to guarantee Virginia for Democrats in the 2016 presidential election (Horwitz and Portnoy, 2016), and it was accompanied by efforts to boost voter registration among eligible felons.[6] It is possible that the shift we observe is partly due to the perceived salience of felon voting restrictions for the 2016 election. Patterns in criminal case outcomes in the weeks around state and federal elections provide some evidence for this possibility: in almost every year, we observe small decreases (increases) in the number of aligned (unaligned) felony cases decided just before an election, relative to just after, but in 2012 and 2016, presidential election years, we observe a much greater number of unaligned cases decided just before the election and a substantial post-election drop.

A third possibility is that these results are simply attributable to another mechanism: simple electoral responsiveness to voters, or prosecutors' own ideological beliefs. However, we find little evidence for either of these alternatives. Prosecutors never increase or decrease sentence severity against all defendants post-announcement, as would be consistent with simple electoral responsiveness arguments. Moreover, prosecutors of both parties exhibit increased punitiveness towards the unaligned and increased leniency towards the aligned. We also find no evidence that prosecutors' personal opposition to the announcement shaped their behavior: the effect is similar in districts where prosecutors opposed the announcement and those where they did not.

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[6]The state's online restoration of rights announcement linked to a voter registration page (See https://web.archive.org/web/20160501210020/http://commonwealth.virginia.gov:80/judicial-system/restoration-of-rights/) and post-announcement, activists held voter registration rallies for newly enfranchised felons around the state (See https://www.progress-index.com/story/news/crime/2016/06/05/at-voter-rights-restoration-rally/28224272007/.)

Our paper is relevant to the growing literature on justice officials as strategic political actors. Existing work in this field has focused on the incentives for prosecutors and judges to manipulate (and often, maximize) charges, conviction rates, or sentences as a way of signaling their attributes to voters or political principals, with a view to being reelected or promoted (e.g., Gordon and Huber, 2002, 2007, 2009; Mungan, 2017; Bandyopadhyay and McCannon, 2014; Dyke, 2007). Our contribution is to show that elected justice officials may also manipulate charges, convictions, and sentences in order to directly shape the electoral environment.

Our paper is also relevant to scholarship on the attempts by political actors to manipulate the composition of the electorate (Bateman, 2018; Helmke, Kroeger and Paine, 2022; Eubank and Fresh, 2022; Behrens, Uggen and Manza, 2003). The literature in this area has focused primarily on the incentives of legislators and elected leaders to shape the electorate in their favor: with regard to felon disenfranchisement laws, for example, scholars have raised the possibility that these laws may have been strategically imposed (e.g., Helmke, Kroeger and Paine, 2022; Eubank and Fresh, 2022), but have not considered that such laws might also be strategically *implemented*. Here, we contribute by demonstrating that the state-level imposition of such laws may motivate local-level officials to manipulate their application.

Elected Prosecutors and Felon Voting Laws

Consider the interaction between a criminal defendant and a prosecutor. The prosecutor can offer a plea deal which can accepted or rejected by the defendant. If it is rejected, the players go to trial, where with some probability the defendant is convicted and suffers a prison sentence, and with complementary probability she will be acquitted. Trial comes at a privately known cost to the defendant, and her utility is also decreasing in the length of the sentence imposed on her. Trial is also costly to the prosecutor, but his utility is increasing in the length of the sentence imposed on the defendant.

In this context, the prosecutor must trade off a higher sentence in his plea offer against a higher likelihood of being rejected and going to trial (due to the fact that he does not know the defendant's precise cost of going to trial). In equilibrium, he offers a plea sentence that optimally takes into account his own (known) cost of trial, the defendant's (unknown) cost of trial, and the defendant's expected sentence at trial. (In the Appendix, this argument is presented formally and the game is solved in full.)

Now suppose that the prosecutor additionally derives some utility from the outcome of a local election—his own reelection, or simply the election of a copartisan—and that he suspects that the defendant will vote for the opposite party. Suppose further that the probability that the defendant can vote in this election is decreasing in the length of her sentence—as was the case in Virginia after the Governor's announcement. Compared to the previous equilibrium outcome, the prosecutor now adjusts his optimal plea offer *upward*. While the size of the adjustment depends on the importance the prosecutor assigns to decreasing the defendant's vote probability in this fashion, *some* increase is always optimal. If the prosecutor instead suspects that the defendant is a copartisan, he instead adjusts his plea offer downwards.

Now consider how the these intuitions apply in the case of Virginia. First, the Governor's executive order changed the felon disenfranchisement regime to increase the salience of sentence length: before the announcement, multiple factors affected whether or not a defendant was disenfranchised upon conviction, and for how long; after the announcement, only sentence length determined disenfranchisement. Consequently, the importance prosecutors assigned to decreasing (increasing) the defendant's probability of voting via the imposition of longer (shorter) sentences should have increased.

Second, the executive order directly altered the composition of the electorate by immediately enfranchising formerly disenfranchised felons (and motivating widespread efforts to register them to vote). If we expect that pre-announcement, prosecutors were using a wider set of tools to disenfranchise unappealing defendants, this alteration should have resulted in a sudden increase in the share of unaligned voters in each district, which

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should have negatively shocked prosecutors' own electoral viability and that of their co-partisans. These shocks should have heightened the importance prosecutors assigned to decreasing (increasing) the defendant's probability of voting via the imposition of longer (shorter) sentences. These two effects suggest the following hypothesis:

Hypothesis: The unexpected April 22, 2016 changes to felon disenfranchisement policy in Virginia caused the state's elected prosecutors to increase the length of the felony sentences imposed on politically unaligned defendants, and decrease the length of the felony sentences imposed on politically aligned defendants.

Context: Criminal Justice in Virginia

On April 22, 2016, Virginia Governor Terry McAuliffe held a press conference in which he announced an executive order implementing sweeping changes to the state's felon disenfranchisement rules.[7] Before the order, the criteria for voting rights restoration had depended on the offense, with some offenders eligible to be considered for rights restoration upon completion of their sentence provided they were not currently facing felony charges, and others required to complete a 3-year waiting period and submit a written application.[8] After the order, *all* felons who had completed their felony sentences of incarceration or supervised release could expect to have their right to vote immediately and automatically restored.[9] According to the Governor, an immediate result of this change was the automatic enfranchisement of 200,000 felons who had already completed

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[7]Commonwealth of Virginia Executive Department, "Order for the Restoration of Rights," Apr. 22, 2016. https://web.archive.org/web/20160502123748/https://commonwealth.virginia.gov/media/5848/order_restoring_rights_4-22-16.pdf

[8]Archived instructions for the restoration of voting rights in Virginia as of April 17, 2016 are available here: https://web.archive.org/web/20160417134119/http://commonwealth.virginia.gov:80/judicial-system/restoration-of-rights.

[9]Misdemeanor convictions and sentences are never disenfranchising in Virginia.

their sentences—a 3.3% expansion in the statewide voter pool.[10]

For our purposes, the Virginia Governor's announcement provides an excellent opportunity to test whether elected prosecutors attempt to shape the electorate by selectively imposing disenfranchising sentences. This is for three reasons. First, the policy change was unexpected, surprising all but a small group of close political advisors and allies. State legislators, bureaucratic officials—even voter registrars and prosecutors—had been intentionally kept in the dark, to prevent them from attempting to scuttle the changes before they were announced (Zullow and Moomaw, 2016).

Second, it sharply and clearly limited how a felony conviction could result in disenfranchisement. Post-announcement, *all* felons were to be automatically enfranchised after the length of their prison or probation sentence, and any individual convicted of a felony but not sentenced to prison or probation time kept her right to vote. This allows for a clear hypothesis: in the aftermath of the announcement, prosecutors concerned with shaping the local electorate should have shifted their focus to manipulating the severity of the sentences imposed on criminal defendants.

Third, a unique feature of Virginia's criminal justice system makes it an especially useful context in which to study the strategic incentives of elected prosecutors: while in many states, both prosecutors and judges are publicly elected, in Virginia this is not so. Instead, felony prosecutors[11] ("Commonwealth's Attorneys") are chosen in district-level partisan elections every four years—while the Circuit Court judges who handle felony cases are appointed by the legislature to eight-year terms.[12] These judicial appointments proceed along almost consociational lines, beginning with a judge's nomination to the

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[10]See https://www.census.gov/library/visualizations/2016/comm/citizen_voting_age_population/cb16-tps18_virginia.html

[11]Prosecutors' offices have discretion over whether to prosecute misdemeanors (Virginia Code §15.2-1627(B)); offices vary widely in their treatment of misdemeanor offenses.

[12]Criminal cases are distributed across District Courts and Circuit Courts in Virginia. The latter have exclusive jurisdiction over felonies.

Courts Committee by the local Bar Association (often in conjunction with other interested groups), proceeding to unanimous election by all Senators representing a given judicial circuit, and usually ending with unanimous confirmation by the Senate.[13] This suggests that Commonwealth's Attorneys are likely, and Circuit Court judges unlikely, to respond to electoral or partisan considerations.

Data

Our raw data consisted of all Circuit Court charges filed between 2010 and the first months of 2020. The Office of the Executive Secretary at the Supreme Court of Virginia provided access to Circuit court cases, which we complemented with additional sentencing data from Virginiacourtdata.org.[14] These data include, for each charge, the date of indictment; the geographical district in which the charge was brought; the defendant's full name, race, gender, and date and month of birth; the eventual disposition of the charge; and any sentence imposed. Most charges also include information on their movement through the system, including charge reductions and hearings.

We aggregated these charges up to the case level by treating all charges resolved on the same day, for the same individual, as belonging to the same case. This yields a dataset of 650,000 cases between 2010 and 2020, the vast majority of which involve either felony offenses (420,000) or violations of felony probation (200,000). A small number of independent misdemeanor charges — 30,000 over ten years — are also included in our data, but since standalone misdemeanor charges are as a general rule heard in District Court, not Circuit Court, the reasons for their inclusion vary significantly across cases, across

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[13]See, e.g., "A Legislator's Guide to the Judicial Selection Process," *Virginia Division of Legislative Services*, available at http://dls.virginia.gov/pubs/legisguidejudicselect.pdf

[14]This website, run by software engineer Ben Schoenfeld, compiles bulk data on Virginia state court outcomes. See https://virginiacourtdata.org/.

districts, and across time. These reasons include being an appeal from a District Court misdemeanor case, being ancillary to a dismissed felony charge, or, in some districts, because the Circuit Court Clerk and local prosecutors have agreed that sufficiently serious misdemeanors may be prosecuted in Circuit Court (Code of Virginia, section 19.2-190.1. "Certification of ancillary misdemeanor offenses.")

Because misdemeanors do not result in disenfranchisement, and because misdemeanor inclusion rules are specific not only to Circuits but also to local staff, we discard this set of cases. We also exclude cases arising out of probation violations, for two reasons. First, active probationers were disenfranchised both before *and* after the announcement. Second, probation violations often cannot be linked back to the original felony convictions, meaning that we cannot determine the extent to which the severity of probation violation punishments is a function of the original conviction. However, we show in the Appendix that the inclusion of these misdemeanor and probation cases does not alter our results.

Finally, we merged our criminal case records with district-level information on incumbent Commonwealth's Attorney's party membership, election years, and election outcomes, including vote shares, collected from the Virginia Department of Elections website.[15] We supplemented this information with data on the partisan identity of challengers to incumbent prosecutors from Hessick and Morse (2019), data on district vote shares in the 2012 and 2016 presidential elections, also collected from the Virginia Department of Elections, and information on Commonwealth's Attorney's office size from a survey conducted by the Virginia Compensation Board in 2018 and presented in their memorandum, "Workgroup Study of the Impact of Body Worn Cameras on Workload in Commonwealth's Attorneys' Offices."[16]

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[15]Available at https://historical.elections.virginia.gov/.

[16]The survey is available at https://www.scb.virginia.gov/docs/bodycameraworkgroupreport.pdf.

Measurement

Testing our theory required constructing measures of both prosecutor-defendant alignment and case-level charge and sentence severity.

Alignment   We operationalize prosecutor-defendant alignment as follows. As discussed above, race is readily available on every defendant's case file, and is a commonly recognized heuristic for partisan affiliation. In Virginia, where the vast majority of citizens identify their race as white or African-American, 76% of African-Americans describe themselves as Democrats or leaning-Democrat, as compared to 27% of white Virginians (Pew Research Center, 2014).[17] We therefore assume that prosecutors believe that African-American defendants are more likely than white defendants to vote for Democrats. When prosecutors campaign explicitly as Republicans or Democrats, we treat a prosecutor-defendant pair as politically aligned when their parties match. Thus, for example, a white defendant and a Republican prosecutor are treated as politically aligned.

However, almost half of Virginia Commonwealth's Attorneys run for election as Independents. In Virginia, only the Republican and Democratic parties are officially recognized by the state, and Independents are barred from receiving endorsements or funding from the two recognized political parties.[18] As a result, we cannot determine an independent prosecutor's partisan lean from party endorsements or donations. Instead, we

[17]These disparities persist within the felon population: Burch 2012 provides evidence that in North Carolina and Florida more than four-fifths of registered African-American felons, but only around one-third of registered white felons, had registered as Democrats (10).

[18]Virginia Department of Elections. "How to Run for Local Office." June and November 2021 Local Candidates Bulletin. Available at https://www.elections.virginia.gov/media/candidatesandpacs/2021-11-02_Gen_Bulletin_Local_Offices_rev_12-16-2020.pdf.

Figure 1: Challenger Party by Theorized Alignment Status
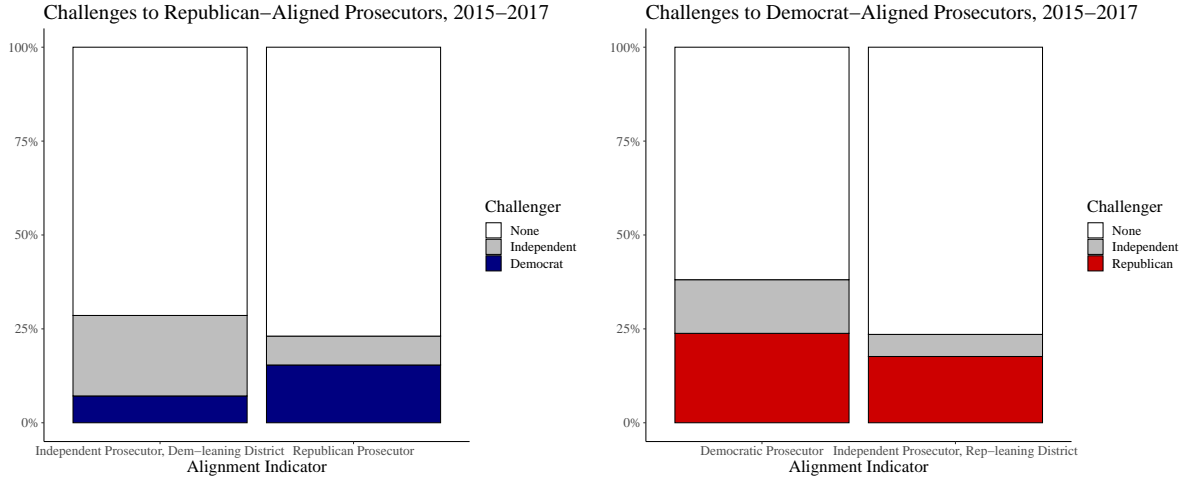
Left panel: party identity of candidates who challenged Independent prosecutors in Democratic-leaning districts versus those who challenged Republicans. Right panel: party identity of candidates who challenged Independent prosecutors in Republican-leaning districts versus those who challenged Democrats.

posit that Independent prosecutors are more likely to be challenged by prosecutors affiliated with the dominant party in the district. Independent prosecutors should therefore build a voter coalition that includes the district's *minority* party. For example, in a district that leans Republican, an Independent prosecutor should expect to be challenged by other Independents or Republicans, and should respond by building a coalition of Independents and Democrats. Data on challenges to incumbent prosecutors 2015-2017 supports this theory: as shown in Figure 1, both Democratic and Independent prosecutors in Republican districts were challenged *only* by Republicans or Independents, while both Republican and Independent prosecutors in Democratic-leaning districts were challenged *only* by Democrats or Independents. We categorize a district as Republican- or Democratic-leaning based on which presidential candidate won the majority in that district in the 2012 presidential election.[19] Table 1 presents our overall operationalization of political alignment among prosecutor-defendant pairs.

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[19]Available online at https://historical.elections.virginia.gov/. Categorizations based on 2016 GOP support produce nearly identical results.

Table 1: Prosecutor-voter alignment

	White felon	Black felon
Republican	aligned	unaligned
Democrat	unaligned	aligned
Independent, Republican county	unaligned	aligned
Independent, Democratic county	aligned	unaligned

Charge and Sentence Severity   Because prosecutors may influence the length of a felony sentence directly, via their choice of recommended sentence, or indirectly, via the charges they file, we constructed measures of both charge and sentence severity. To measure charge severity, we used Virginia's sentencing guidelines to determine the maximum permissible sentence for each charge in a case, then coded the case's charge severity as the maximum sentence attached to the the most severe ('top') charge filed. Permissible maximum sentences in almost all felony cases in our data take on one of five distinct values: five, ten, twenty, or forty years in prison or life imprisonment. This measure of charge severity is a conservative estimate, because it does not account for the severity of *all* charges in a given case, but only the top charge. Because we cannot be certain about the actual length of a 'life' sentence, we recode these sentences downward as forty-year sentences.

To measure sentence severity, we simply calculated the total *felony* prison sentence assigned after conviction in each case. Where multiple charges are sentenced in the same case and the sentences were set to run consecutively, we aggregated the charge-level data by summing each count's sentence length in days. Where sentences were set to run concurrently, we use the longest sentence imposed. This measure is likewise a conservative, *minimum* estimate of actual sentence severity, since it does not include supervised probation time. We exclude probation time for two reasons. First, many probation sentences in the data are 'indefinite.' These sentences feature neither a maximum nor a minimum time: they end when the court actors involved in the case decide they should. Second, even definite terms of probation are indefinite in practice because infractions during

the probation period can be used (at the prosecutor's discretion) to reset the probation clock.[20]

Empirical Strategy

Treating McAuliffe's announcement as an unexpected shock to felon disenfranchisement rules in Virginia, we use several analytical methods to test for discontinuous, alignment-specific shifts in prosecutorial punitiveness in the weeks immediately around the shock. This short-term focus, together with the surprise nature of the announcement, allows us to minimize confounding by unrelated, longer-term patterns in the composition and treatment of criminal cases.

First, taking advantage of our daily district-level data, we use a local-linear regression discontinuity in time (RDiT) approach to test for the effect of the governor's announcement on charge and sentencing outcomes under the assumption that absent the announcement, potential outcomes would have been a smooth function of time. Our data are well-suited to this approach, which permits seasonality in criminal outcomes so long as these outcomes change smoothly in time.[21] However, to address any inferential short-comings in our RDiT analysis, in addition to a set of robustness and placebo tests we also present results from a simple difference-in-means fixed effects regression analysis, which relies on the assumption that the announcement was as-if random. In the Appendix, we also present results from a local randomization analysis, suggested by Cattaneo, Idrobo

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[20]See Code of Virginia § 19.2-304, see also Sentencing Revocation Report and Probation Violation Guidelines (2016) at http://www.vcsc.virginia.gov/worksheets_2015/SRR_booklet2015revised.pdf.

[21]While data lacking cross sectional variation and with observations at few running variable values can be problematic for RDiT designs, especially if the treatment is not quasi-randomly assigned (see, e.g., Hausman and Rapson, 2018), our treatment *is* plausibly quasi-random, and our data contain cases charged and decided every day across numerous districts, suggesting that local-linear RD estimation remains appropriate.

and Titiunik (2024) as a robustness check for data with discrete running variables.

The following local-linear regression model is our main RDiT specification throughout:

$$y_{ijt} = \beta_0 + \beta_1 \mathbb{1}(t > t_{ann.}) + f(t - t_{ann.}) + \alpha_j + X_{it} + \epsilon_{ijt}. \tag{1}$$

Here, y_{ijt} is a measure of prosecutor severity, for example, the total prison sentence for case i in district j at time t, $t_{ann.}$ is the date of the announcement, $f(\cdot)$ is a smooth function of distance from the same, and the parameter of interest, β_1, captures the discontinuous shift in punishment severity following the announcement. In most specifications, we include district fixed-effects (α_j) to account for cross-district differences in sentencing norms, crime, constituency preferences, and prosecutor's office (each district elects one chief felony prosecutor). In some specifications, we also include pre-treatment dummies X_{it}, which capture a defendant's gender and whether the type of crime prosecuted was a violent crime, a drug crime, a property crime, a traffic crime, or some other infraction. Our theoretical prediction is that the sign of the parameter of interest, β_1, should be negative for defendants aligned with their prosecutors, and positive for unaligned defendants. To test this prediction, we estimate Equation (1) separately according to the felon's political alignment vis-a-vis the prosecutor.[22] Throughout, we use MSE-optimal bandwidths with the bias-correction method proposed by Calonico, Cattaneo and Titiunik (2014), and standard errors clustered at the district-level.[23]

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[22]Note that our theory implies that aligned and unaligned defendants are not comparable before *or* after McAuliffe's announcement: while the announcement should have caused a *change* in prosecutors' respective treatment of these two classes of defendant, they should have been treated differently before the announcement as well.

[23]Kolesár and Rothe (2018) and others have shown that clustering on the running variable is inadvisable, even when it is discrete. We therefore first estimate local-linear regression discontinuity results, clustering by district and relying on the large number of unique values in our (daily) running variable, then probe the robustness of our results by conducting additional difference-in-means tests as well as local randomization inference

After reporting our main regression results, we turn to an investigation of the announcement's total effect on sentencing: i.e., its effect on both direct changes to sentence severity and indirect changes to sentence severity through changes in charge severity. We test for a *direct* sentencing effect by estimating the announcement's effect on sentence severity only for cases charged *prior* to the announcement, including when we condition on charge severity. We also examine how prosecutor use of specific sentencing tools shifted post-announcement.

Testing for an *indirect* sentencing effect via changes in charge severity is harder, because cases charged after the announcement are necessarily sentenced after it as well, making it difficult to distinguish the effect of shifts in charge severity from direct sentence manipulation. Our solution is to estimate the pre-announcement relationship between charge severity and eventual sentence length, and then use this relationship to derive bounds on the indirect effect on eventual sentence length of post-announcement charge severity changes.

We conduct a number of robustness and placebo tests to bolster confidence in our results. In later sections, we conduct additional tests to disentangle our explanation from one based on districts' or prosecutors' ideological preferences.

Main Results

We begin with a graphical assessment of the announcement's effect on felony charge severity and felony sentence length. The results are presented in Figure 2. The left-hand column depicts binned averages of the severity of the top charges brought against defendants in Circuit Court in a twelve-week window around the announcement, adjusted for prosecutor fixed effects. The right-hand column depicts binned averages of the lengths of the actual felony prison sentences imposed on defendants in Circuit Court during the same time period, adjusted for prosecutor fixed effects. We separately fit linear predictions in the neighborhood of the threshold (see Cattaneo, Idrobo and Titiunik, 2024).

Figure 2: Announcement Effect on Charge (left column) and Sentence Severity (right column)
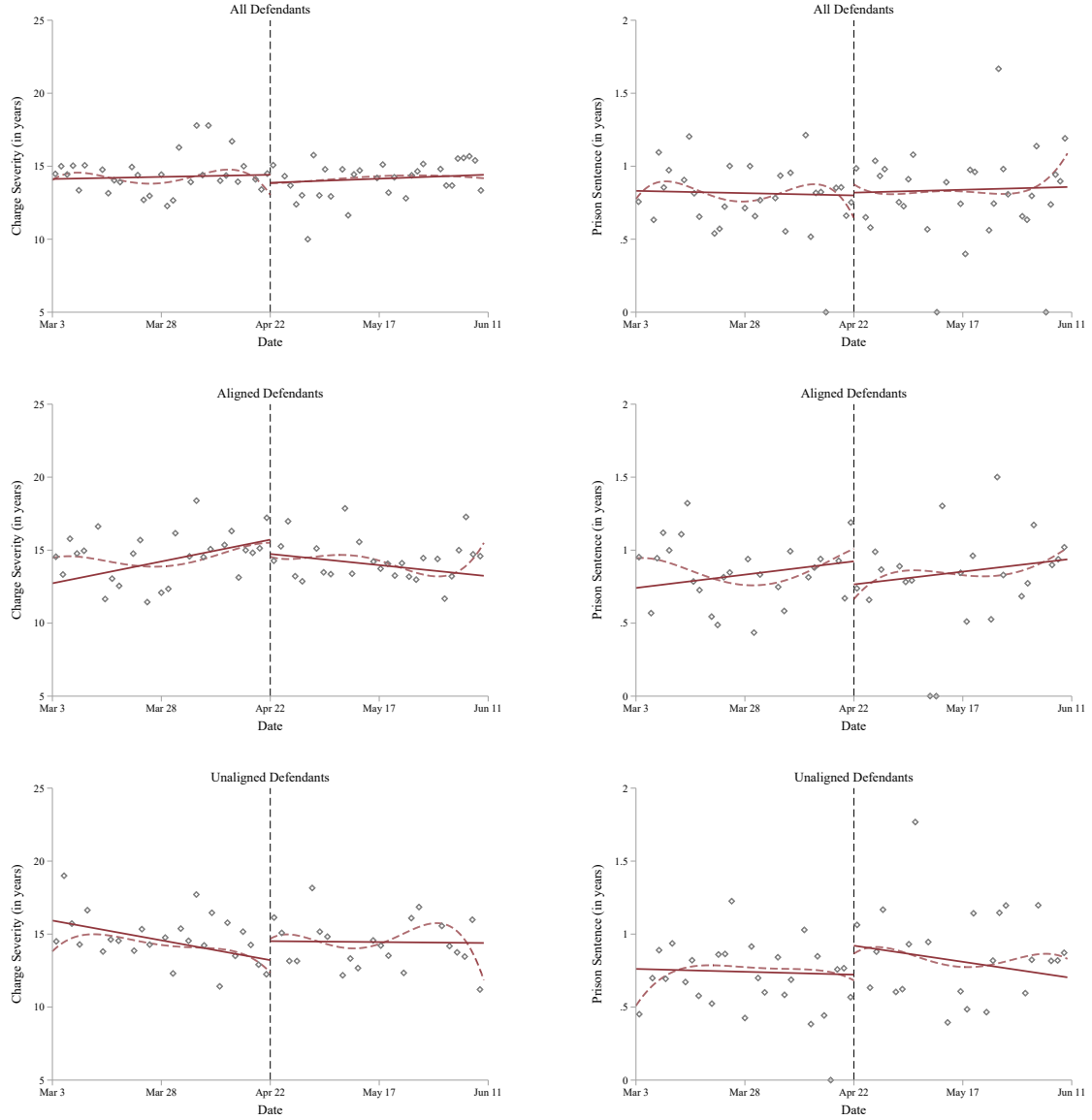
The top row in the figure displays the unconditional, within-district effect of the announcement on average charge severity (left) and prison sentences (right) before and after the announcement. The next two rows disaggregate these results for aligned and unaligned defendants, respectively. The dashed line represents a triangular kernel-weighted polynomial fit. The solid line represents a linear fit.

and kernel-weighted polynomial curves to the data on either side of the announcement. The top row presents the announcement's effect on *all* charges and sentences. The middle and bottom rows present the announcement's effect on charges and sentences for aligned and unaligned defendants, respectively. Consistent with our theory, we observe significant post-announcement shifts by alignment status: sharp increases in average charge and sentence severity for unaligned defendants, and sharp decreases for aligned defendants.

Local-Linear Estimates   Table 2 presents local-linear estimates of the immediate effect of McAuliffe's order on the average severity of the top charges filed (Panel A) and prison sentences imposed (Panel B) in Circuit Court, disaggregated by alignment status. For each defendant type, the first column presents unadjusted results for comparison. The second column adds district fixed effects to take into account variation across districts in crime rate, crime type, demographics, and prosecutor identity. The third column presents results further adjusted for defendant gender, and crime type.[24]

Consistent with the graphical analysis above, we find that across specifications, the announcement consistently drove increases in charge severity and sentence length for unaligned defendants, and decreases for aligned defendants. Note that while the results which do not include district fixed effects are appropriately signed, they not rise to the level of statistical significance. This is unsurprising: given substantial demographic variation across districts as well as variation in criminal activity and prosecutor behavior, these results are highly imprecise. Overall, we find that for unaligned defendants, the executive order led to an average increase in initial charge severity of around 2 years, and an average increase in actual sentence length of around 4 months. Aligned defendants appear to have experienced about a 1.5 year decrease in initial charge severity, and around a 1.5 month decrease in sentence severity, although the last effect is also consistently statistically insignificant at conventional levels.[25]

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[24]In our main specifications, we do not adjust for specific charges or (for sentencing) charge severity, because these are strategic choices by the prosecutor.

[25]Notice from the left-hand intercepts in Table 2 that aligned defendants' baseline

Table 2: Effect of McAuliffe's Order, By Alignment Status

|	Unaligned Defendants			Aligned Defendants		
	Panel A. Charge Severity					
RD Estimate	2.587	1.837	2.643	-1.259	-1.46	-1.556
	(1.494)	(1.003)	(0.93)	(1.636)	(0.932)	(0.821)
Left Side Intercept	14.264	14.314	14.277	16.64	16.887	16.944
	(1.069)	(0.708)	(0.636)	(1.175)	(0.766)	(0.636)
Bandwidth	64.907	73.55	59.248	68.22	71.024	66.513
Eff. Observations	3387	6708	5569	6324	8215	7685
	Panel B. Sentence Severity					
RD Estimate	0.257	0.435	0.376	-0.103	-0.122	-0.121
	(.211)	(0.149)	(0.139)	(.143)	(0.133)	(0.138)
Left Side Intercept	0.777	0.713	0.729	0.881	0.892	0.878
	(0.079)	(0.059)	(0.062)	(0.12)	(0.116)	(0.112)
Bandwidth	75.051	58.761	63.296	69.874	67.059	61.446
Eff. Observations	4478	5131	5622	5275	7574	6885
District FE	N	Y	Y	N	Y	Y
Covariates	N	N	Y	N	N	Y

Charge and sentence severity shown in years. For unaligned/aligned defendants, second and third columns include district-fixed effects, and third columns condition on gender and crime type. Standard errors clustered at the district-level. Only African-American and white defendants (98% of all defendants) included.

These results are broadly robust to a range of alternative specifications, placebo tests, and bandwidth choices. In Appendix Tables A1 and A2, we re-estimate our main models, including probation violations and misdemeanors, and show that the results are similar. In Appendix Table A3, we show that the effects above are also consistent with removing the 60 districts managed by Independent prosecutors, although they are weaker.[26] In Appendix Figure A1, we replicate our analysis but replace the announcement date (April 22, the third Friday in April) with placebo dates on the third Friday of the two months

charges/sentences were more severe than unaligned defendants,' which would be consistent with prosecutors requiring greater seriousness to prosecute aligned defendants at all.

[26]If only Independent prosecutors are included, the results are also weaker.

prior to and following the announcement, respectively. The results are encouraging.[27]
And in Figure A2 below, we present the coefficient estimates for our covariate-adjusted
local-linear regressions beginning with a 10-day, user-set bandwidth on either side of the
announcement and increasing in two-day increments to a 90-day bandwidth. As is clear
from the figure, our results for a wide range of bandwidth specifications closely track
those presented in Table 2.

Figure 3: Replication of Main Results, Variation of Bandwidth
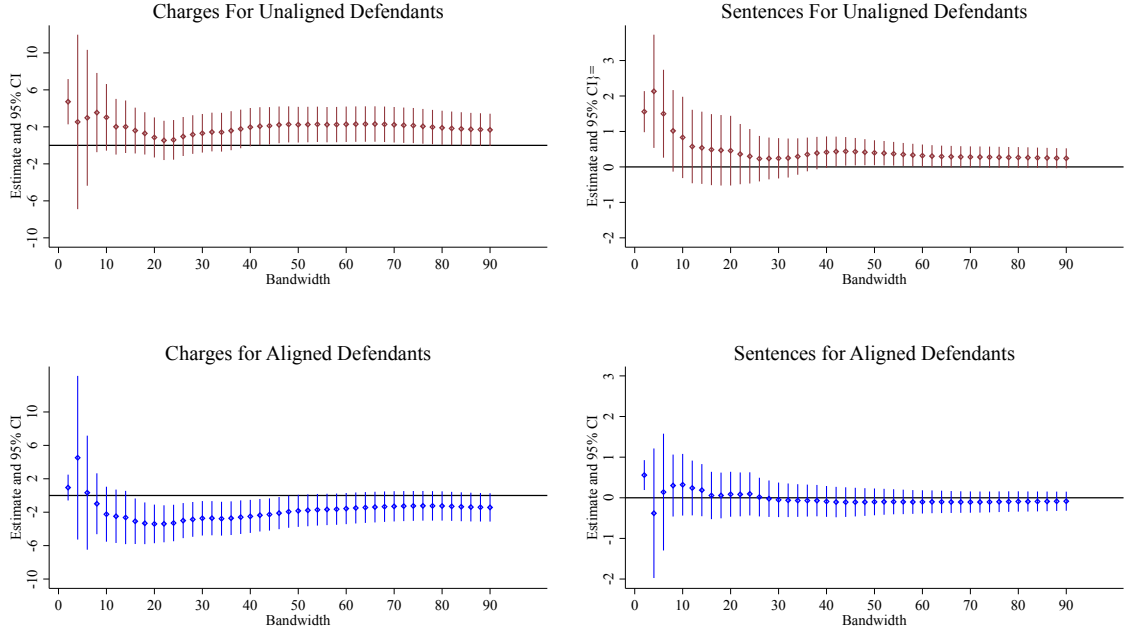
Point estimates and 95% confidence intervals for user-specified bandwidths beginning
at 2 days and increasing to 90 in increments of two. All results include district fixed
effects and pretreatment covariates. In the Appendix, we replicate the analysis without
pretreatment covariates.

Finally, the results are remarkably consistent across prosecutors' offices. Because
elected chief prosecutors generally manage a team of assistant prosecutors, we wondered

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[27]An informative yearly placebo analysis is not possible due both to Governor
McAuliffe's prior revisions of felon disenfranchisement criteria in the spring of 2013, 2014,
and 2015—although these earlier revisions were much less broad in scope and were not a
"surprise"—and due to the fact that primary elections in Virginia take place in June of
odd-numbered years.

whether the announcement's effect might depend on chief prosecutors' control over their agents: in larger districts, we theorized, subordinates might have more independence, and the announcement might have had less of an effect. However, Figure A4 in the Appendix shows little evidence in favor of a size-based effect. Likewise, we wondered whether politically vulnerable prosecutors might be especially prone to voter pool manipulation. This possibility is difficult to test because political vulnerability is endogenous to political aptitude; nevertheless, we code a prosecutor as vulnerable if he faced an opponent who garnered at least 10% of the vote in the previous election, and separately evaluate the announcement's effect on this subgroup of prosecutors and their 'safe' counterparts. As shown in Appendix Figure A3, both groups engaged in similar charging and sentencing behavior post-announcement.[28]

Alternative Specifications The local-linear regression discontinuity analysis above relied on the assumption that the relevant potential outcomes for aligned and unaligned defendants are a smooth function of the running variable. Because the announcement was unexpected, an alternative assumption we might employ is that for a short enough time window around the announcement, the treatment (the announcement) was as-if randomly assigned. Relying on this assumption, we conduct a series of simple, pooled fixed effects regressions for two-to-ten day windows around the announcement. The results are presented in Figure 4. In these estimates, the treatment takes the form of a simple indicator variable which switches from zero to one on the date of the announcement. Despite the narrow time windows, we observe consistent and almost-immediate post-announcement increases in charge and sentence severity for unaligned defendants, and consistent decreases for the aligned. Reassuringly, these effects are very similar to those observed in Table 2.

In the Appendix, we also present the results of a complete randomization inference procedure proposed by Cattaneo, Idrobo and Titiunik (2024). The procedure tests a

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[28]This finding does not depend on the 90% cutoff we selected.

Figure 4: Effect of Announcement, Pooled Difference-in-Means Regressions
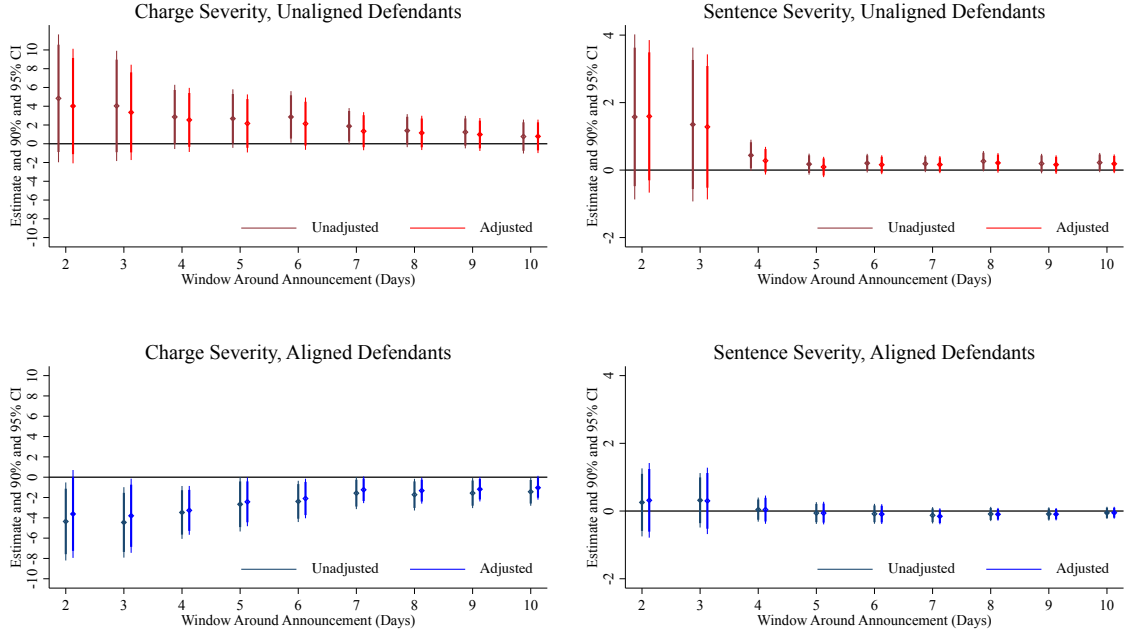
Point estimates and 90/95% confidence intervals for 2-10 day windows around the announcement. All estimates adjusted for district fixed effects; 'adjusted' results adjusted for pretreatment covariates. Standard errors clustered at the district. Clusters for charge severity: 53-90 for unaligned; 61-95 for aligned. Sentence severity clusters: 53-96 for aligned; 61-100 for unaligned.

sharp null hypothesis of no treatment for any unit for a series of small windows around the cut-point by selecting a random subset of possible treatment profiles, and measuring the extremeness of the observed test statistic against those generated from the randomly chosen profiles. We limit our examination to windows (mass points) of one to five days around the announcement, among the subset of districts for which data is available in these windows before and after the treatment. The results are shown in Table A4. Coefficient estimates are consistent, although they are statistically significant only for charge severity.

Scope of Results

We argued above that McAuliffe's announcement caused a decrease in the severity of charges and (to a lesser degree) sentences imposed on aligned defendants, and caused an

increase in charge and sentence severity for unaligned defendants. However, the means through which, and extent to which, prosecutors leveraged sentencing directly versus via charging decisions is unclear. The announcement's total effect on sentencing is likewise unclear. We now turn to a closer examination of the mechanisms behind these changes, and of the announcement's total effect on sentencing.

A Direct Effect on Sentencing An initial question is whether the announcement had an independent effect on sentences, or whether all the effects we observed on post-announcement sentences occurred via a change in charging behavior. To gain some intuition, we re-estimated the announcement's effect on average prison sentences, this time restricting our analysis to cases charged before the announcement. Table 3 presents the results. To address the possibility that pre-announcement variation in indictment severity might have systematically affected the eventual disposition of the case to create the appearance of a direct effect where none existed — i.e., if more severe cases are brought against unaligned defendants, and also take longer to resolve — Columns 3-4 and 7-8 condition on indictment severity.

The results throughout are very similar to those in Table 2: again, there is a statistically significant increase in sentencing severity for unaligned defendants, and a negative but insignificant decrease for aligned defendants. This implies that our main sentencing results do capture some direct effect on sentencing—at least for unaligned defendants.

To further determine the extent to which the announcement directly affected sentencing, as well as the channels through which it may have done so, we identified five tools available to prosecutors which directly shape sentencing outcomes: outright dismissals, charge reductions to misdemeanors, requests for time served sentences, requests for felony supervised probation, and requests for prison time. Ideally, we would examine how prosecutors' use of each tool changed in real time after the announcement. Unfortunately, we lack dates for both informal plea agreements and amendments prior to cases' final resolutions. As a next-best strategy, we conduct a local-linear RD estimation of the

Table 3: Effect of Announcement on Sentence Length (in years) Cases Filed Before Announcement
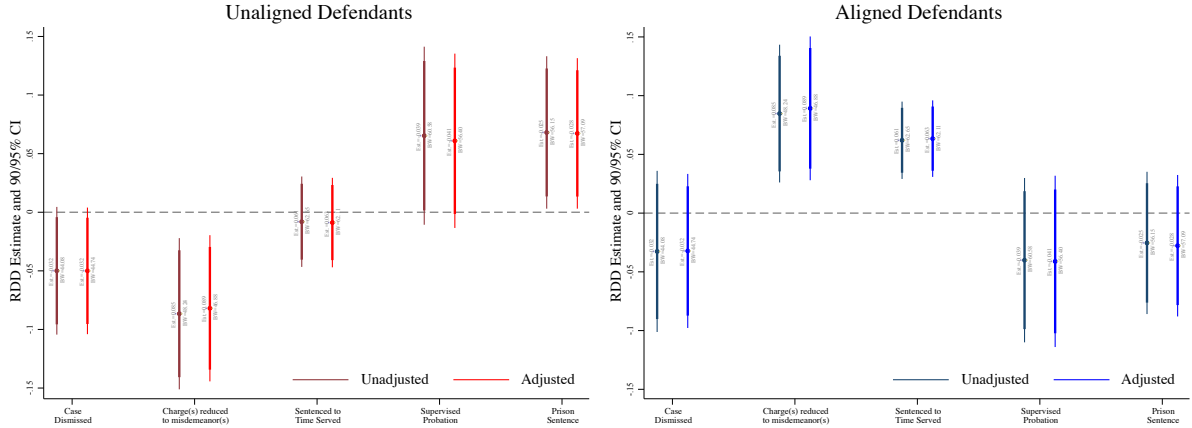
	Aligned Defendants				Unaligned Defendants			
RD Estimate	-0.129	-0.124	-0.063	-0.124	0.386	0.338	0.343	0.307
	(0.132)	(0.138)	(0.138)	(0.138)	(0.143)	(0.138)	(0.144)	(0.14)
Intercept	0.895	0.884	0.828	0.884	0.73	0.747	0.743	0.752
	(0.121)	(0.124)	(0.125)	(0.124)	(0.056)	(0.053)	(0.055)	(0.054)
Bandwidth	67.825	62.11	60.05	62.11	63.45	67.511	64.002	65.791
Eff. Obs.	7317	6746	6590	6746	5441	5788	5509	5598
District FE	Y	Y	Y	Y	Y	Y	Y	Y
Covariates	N	Y	N	Y	N	Y	N	Y
Ind. Sev.	N	N	Y	Y	N	N	Y	Y

Dependent variable is total actual prison time sentenced. Acquittals/dismissals coded as zero. All columns include district-fixed effects. Standard errors clustered at the district level. For theoretical clarity, only African-American and white defendants are included (98% of all defendants).

announcement's effect on the incidence of each outcome (recorded at the conclusion of the case). The results, displayed in Figure 5, show that for unaligned defendants, the announcement decreased the probability that felony charges were dismissed or reduced to misdemeanors, and increased the probability of receiving a prison or probation sentence. For aligned defendants, the announcement had no effect on the probability of receiving a prison or probation sentence, but *did* significantly increase the probability of a charge reduction to a (non-disenfranchising) misdemeanor, and of being sentenced to time served. As we show in Appendix Figure A5, these effects—specifically, a decrease in the likelihood of a case dismissal for unaligned defendants and an increase in the incidence of time served sentences and charge reductions for aligned defendants—persist in a series of pooled fixed effects regressions two to ten days around the announcement.

These results are consistent with a situation in which prosecutors have greater short-term ability to revise sentences *downward* than *upward* — possibly because case outcomes are often the result of a negotiation between a prosecutor and defense attorney, and upward deviations from a negotiated plea deal would not be well received by opposing counsel. We find an especially strong effect on the provision of time served sentences

Figure 5: Effect of Announcement on Sentencing Outcomes

Panels provide RD estimates and (90 and 95%) robust confidence intervals for the post-announcement change in the probability that unaligned and aligned defendants' charges are (from left to right) dismissed, reduced from felonies to misdemeanors, sentenced to time served, sentenced to supervised probation, or sentenced to prison. All estimates adjust for district fixed effects; "adjusted" estimates also control for pre-treatment co-variates.

to aligned defendants, perhaps because these sentences permit prosecutors to convict defendants of felony offenses without—after the announcement—ever disenfranchising them. Because this effect is the strongest, in Appendix Table A5, we also test (and confirm) its robustness to the Fisher complete randomization test we conducted in the previous section.

The Total Sentencing Effect   Understanding the total effect of the announcement on criminal sentences requires determining both its *direct* effect on sentence severity and its *indirect* effect via changes in charge severity. Determining the size of this effect is difficult, because all cases charged post-announcement were necessarily sentenced post-announcement. To gain some purchase on this question, we estimate the pre-announcement relationship between charge severity and sentence severity, then use these estimates to predict the effect of post-announcement charging shifts, accounting for uncertainty in both predictions by performing these calculations for each of 1000 bootstrapped samples clustered at the district level.

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Figure 6: Bootstrapped Estimates of Indirect Sentencing Effect
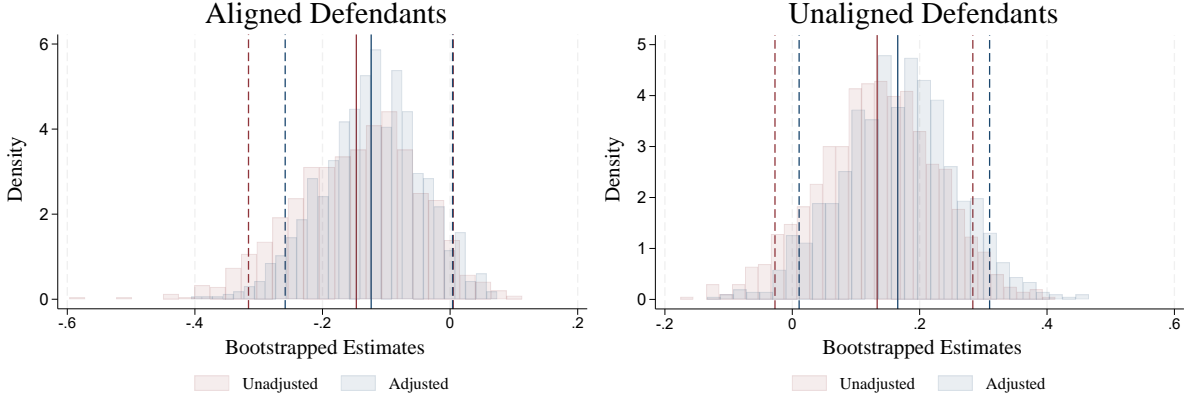
Histograms depict bootstrapped estimates of the discontinuous shift in sentence lengths attributable to the post-announcement shift in charge severity, for aligned (left) and unaligned (right) defendants. The solid vertical lines depict averages, while the dashed vertical lines represent the 5th and 95th percentiles in the distributions of both unadjusted (red) and adjusted (blue) estimates.

Figure 6 displays our estimates of the change in sentence severity induced by post-announcement changes in charge severity, across bootstrapped samples, for aligned defendants (left) and unaligned defendants (right). Among our samples of aligned defendants, we estimate that post-announcement decreases in charge severity led to a 40-50 day decrease in average prison sentences; although the effect skirts conventional levels of statistical significance. For unaligned defendants, we estimate that post-announcement increases in charge severity led to a 50-60 day average increase in sentence length.

Inferring the announcement's total effect on sentencing via our estimates of its direct and indirect effects requires us to assume that the mapping from charge to sentence severity was not too altered by the announcement. Given this assumption, we can place rough bounds on the size of the announcement's effect. Recall that our estimate of the direct effect on sentencing, presented in Table 3, was a prison time increase of 3 to 4 months for unaligned defendants, and a (statistically insignificant) decrease of approximately one and a half months for aligned defendants. If charge severity shifts substitute partially or entirely for sentencing shifts, these changes may approximate the total effect. If charging and sentencing adjustments are complementary to one another, then after the announcement, unaligned defendants may have received sentences as much as seven

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months longer, and the aligned as much as three months less.

Electoral Incentives and Alternative Explanations

In this section, we briefly consider some alternative explanations for our results.

Public Opinion   Prior evidence suggests that prosecutors may alter their charging behavior to align with local-level public opinion (Nelson, 2014). Is it possible that the shifts we observe in charging and sentencing behavior simply reflect post-announcement changes in public support for disenfranchisement? It seems unlikely: a wholesale change in public opinion regarding disenfranchisement should have motivated increases or decreases in prosecutor punitiveness *across the board*, rather than generating differential effects by alignment status.

Racial Attitudes and Disenfranchisement   A more plausible possibility is that our results are generated by the *interaction* of the announcement and cross-district differences in racial attitudes. Our results could obtain, for example, if prosecutors aligned with the Democratic party reduced sentences assigned to (aligned) African-American defendants post-announcement, to demonstrate their opposition to this historical disenfranchisement, while their Republican-aligned counterparts *increased* sentences against (unaligned) African-American defendants post-announcement. We test the validity of this theory by analyzing the effect of McAuliffe's announcement on prosecutor charging and sentencing, disaggregating by both political alignment and race. The left-hand column of Figure 7 displays the results. We find that Democratic party-aligned prosecutors decrease charging and sentencing severity against African-American defendants, while increasing severity for white defendants, while Republican party-aligned prosecutors do the opposite.

Figure 7: Effect of Announcement on Charges and Sentences
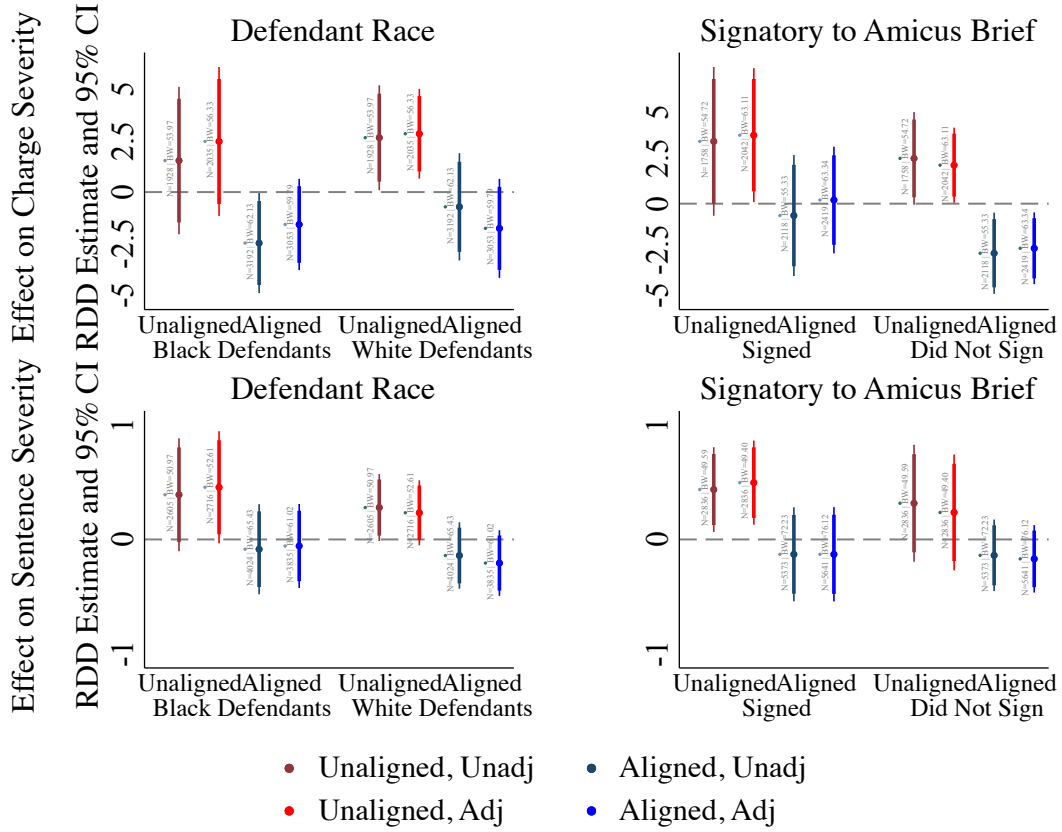
Prosecutor ideology   Another possibility is that, rather than responding to constituent preferences, prosecutors were adjusting charges and sentences to communicate their own views about felon disenfranchisement. These views were varied, even within-party: after McAuliffe's announcement, a bipartisan group of prosecutors signed an amicus brief filed with the state supreme court opposing the executive order.[29] Could prosecutors' punitiveness have shifted simply according to their ideological views on felon disenfranchisement? To answer this question, we separately estimate the announcement's effect on prosecutors who signed the amicus brief and prosecutors who did not. The results, shown in the right-hand panel of Figure 7, show that prosecutors who signed the brief and prosecutors who did not responded to the announcement in the same way, becoming more

[29] "Brief of Amici Curiae Commonwealth's Attorneys In Support Of Petitioners," in *Howell v. McAuliffe*. Available at https://www.brennancenter.org/sites/default/files/analysis/Howell%20v%20McAuliffe%20-%20Prosecutors%20Amicus.pdf

punitive towards unaligned defendants, and less punitive towards aligned defendants.

Some evidence of an electoral motive   Finally, if prosecutors *do* strategically take into account the disenfranchising effect of a sentence in order to further their own electoral prospects or that of their copartisans, we might expect to see them strategically front-load disposition dates for unaligned defendants *before* elections, and hold off on convicting and sentencing aligned defendants until *after* elections. In our data, this would manifest as a post-election *decrease* in the number of daily case dispositions for unaligned defendants, and a post-election *increase* in dispositions for aligned defendants. To explore this possibility, we plotted a simple district-weighted count of felony cases decided for 50 days on either side of each Virginia election between 2012 and 2019, and separately fitted linear predictions and kernel-weighted polynomial curves to either side of the election date.

The results for federal elections (even years) and state elections (odd years) are presented in Appendix Figures A7 and Figure A6, respectively, and are encouraging for our theory. In almost all plots, as predicted, more cases involving aligned defendants are decided in the weeks following the election than in the weeks before it, and the converse for unaligned defendants. The difference is small, however—with two exceptions: in our two presidential election years, 2012 and 2016, we observe very high levels of unaligned cases being decided in the weeks before the election, followed by a substantial post-election drop. This implies that prosecutors may be especially, or even primarily, concerned with the strategic value of felon disenfranchisement when it can be used to assist their preferred presidential candidates in national elections.[30]

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[30]It is possible that prosecutors may also strategically set sentences to *end* just before or after elections, but this is not possible for us to test (and likely difficult for them to do) because we do not know how much time defendants spent incarcerated before sentencing, and this time counts against their total sentence and determines when they are released.

Discussion

Scholars interested in the strategic behavior of elected prosecutors have often focused on their incentives to improve conviction rates, sentencing outcomes, and trial wins in order to signal quality or competence to a fixed pool of voters. Such behavior would arguably be consistent with a prosecutor's responsibility, as an elected official, to perform the will of her constituents — albeit in tension with her obligation as an officer of the court to perform justice. Our findings complicate the story further, suggesting that prosecutors may also act to advance their own likelihood of reelection—or the electoral fortunes of favored candidates at the state or national level— by using the tools of their office to shape the voter pool.

Our findings also contribute to recent scholarship on the ways in which political actors may attempt to 'tilt' electoral outcomes in their own favor. Although existing work focuses on the incentives of state-level politicians to *construct* laws and electoral rules that bend outcomes towards themselves (Helmke, Kroeger and Paine, 2022; Eubank and Fresh, 2022), our findings suggest that the local officials who *implement* these laws may also play an important role. An interesting avenue for future research would be to explore the extent to which local-level and state-level actors have complementary, as opposed to competing, goals. For example, if government is divided at the state and local levels, laws written at the state level with a view to disenfranchising one group might be used, at the local level, to disenfranchise another.

Finally, a major implication of our paper is that the composition of the felon population may depend on the partisan identity of local criminal justice officials. More specifically, the assignment of felon status may be endogenous to the degree of political threat or benefit a defendant poses to a prosecutor. Further research might explore the implications of this endogeneity for broader studies of felon political behavior.

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Appendix

For Online Publication Only

Contents

A Model

Consider a criminal defendant D and a prosecutor, P. The prosecutor can offer D a plea sentence x_P. The defendant can accept it, or refuse it and go to trial at cost c. c is known to D, but P knows only that c is distributed on $U[\underline{c}, \overline{c}]$.

At trial, the defendant will be convicted (given the evidence against her) with probability π. If she is convicted, sentence x_T is imposed.

Suppose for starters that the prosecutor only derives utility from imposing sentences and going to trial; in particular, she gets x from imposing x on the defendant, and pays a cost k from going to trial.

The defendant's utility is the sentence imposed on her plus the cost of trial, if trial occurs. Thus, the defendant gets $-x_P$ from taking the plea deal, $-x_T - c$ if convicted at trial, and $-c$ if acquitted. Putting these together, the defendant accepts the plea deal offered by the prosecutor if

$$x_P \leq \pi x_T + c.$$

Now given the above, the prosecutor' utility from offering x_P is:

$$Pr(x_P - \pi x_T \leq c) \cdot x_P + Pr(x_P - \pi x_T > c) \cdot (-k + \pi x_T)$$

Since c is uniformly distributed on $[\underline{c}, \overline{c}]$ the prosecutor solves:

$$\max_{x_P} \frac{\overline{c} - (x_P - \pi x_T)}{\overline{c} - \underline{c}} x_P + \frac{x_P - \pi x_T - \underline{c}}{\overline{c} - \underline{c}} (-k + \pi x_T)$$

The first order condition is:

$$\frac{1}{\overline{c} - \underline{c}} \left(\overline{c} - 2x_P + \pi x_T - k \right) = 0$$

So the optimal x_P is

$$x_P^* = \frac{\overline{c} + \pi x_T - k}{2}$$

Now suppose the prosecutor also wishes to get reelected, in the the context of a law that says that those convicted of felonies are disenfranchised for the length of their sentences. In particular, the probability that defendant D can vote is decreasing in his sentence x. Suppose the prosecutor suspects that D will vote for her opponent, and let $\lambda \geq 0$ index the degree to which P dislikes letting D vote. Then the prosecutor's utility from x_P becomes:

$$Pr(x_P - \pi x_T \leq c) \cdot (x_P + \lambda x_P) + Pr(x_P - \pi x_T > c) \cdot (-k + \pi(x_T + \lambda x_T))$$

and her optimal plea deal given her dislike of D's voting becomes

$$x_P^{d*} = \frac{(1+\lambda)\bar{c} + 2(1+\lambda)\pi x_T - k}{2(1+\lambda)}$$

Comparing to x_P^* we can show that

$$x_P^{d*} > x_P^* \text{ if } \frac{k}{1+\lambda} < k$$

which is always the case, given $\lambda > 0$. Additionally, the size of the difference is increasing in λ.

Now if the prosecutor wants to get reelected and believes that the defendant will vote *for* her, she likes the defendant to vote and her utility becomes:

$$Pr(x_P - \pi x_T \leq c) \cdot (x_P - \lambda x_P) + Pr(x_P - \pi x_T > c) \cdot (-k + \pi(x_T - \lambda x_T))$$

and her optimal plea offer becomes

$$x_P^{l*} = \frac{(1-\lambda)\bar{c} + 2(1-\lambda)\pi x_K - k}{2(1-\lambda)}.$$

We can similarly show that $x_P^{l*} < x_P^*$ so long as $\lambda > 0$.

B Robustness

Table A1: Announcement's Effect on Sentence, Including Probation Violation Charges

	Unaligned Defendants		Aligned Defendants	
RD Estimate	0.218	0.248	-0.138	-0.145
	(0.11)	(0.113)	(0.104)	(0.107)
Left Side Intercept	0.751	0.749	0.826	0.816
	(0.064)	(0.068)	(0.096)	(0.098)
Bandwidth	59.804	58.579	61.815	58.672
Eff. Observations	7739	7550	10055	9381
District FE	Y	Y	Y	Y
Covariates	N	Y	N	Y

Sentence severity coded in years. Acquittals/dismissals coded as zero. Even-numbered columns condition on gender and crime type. Standard errors clustered at the district-level. Only African-American and white defendants are included (98% of all defendants).

Table A2: Announcement's Effect on Charging and Sentencing, Including Misdemeanors

	Unaligned Defendants		Aligned Defendants		Unaligned Defendants		Aligned Defendants	
	Effect on Charge Severity				*Effect on Sentence Severity*			
RD Estimate	1.618	2.425	-2.138	-1.508	0.406	0.366	-0.135	-0.153
	(0.989)	(0.865)	(0.975)	(0.778)	(0.141)	(0.134)	(0.131)	(0.139)
Left Intcpt	13.294	13.28	16.531	16.204	0.675	0.685	0.847	0.83
	(0.705)	(0.592)	(0.754)	(0.627)	(0.058)	(0.052)	(0.12)	(0.125)
Bandwidth	73.698	65.747	59.913	79.204	60.189	64.09	65.175	58.877
Eff. Obs	7220	6510	7357	9504	5832	6163	7732	6788
District FE	Y	Y	Y	Y	Y	Y	Y	Y
Covariates	N	Y	N	Y	N	Y	N	Y

Charge and sentence severity coded in years. Acquittals/dismissals coded as zero. Even-numbered columns condition on defendant gender, and crime type. Standard errors clustered at the district level. For theoretical clarity, only African-American and white defendants are included (98% of all defendants).

Table A3: Effect of McAuliffe's Order, By Alignment Status, Partisan Prosecutors Only

|	Unaligned Defendants		Aligned Defendants		Unaligned Defendants		Aligned Defendants	
	Charge Severity				Charge Severity			
RD Estimate	0.145	0.437	-1.96	-1.721	0.28	0.23	-0.099	-.056
	(1.15)	(1.008)	(1.077)	(0.896)	(0.212)	(0.207)	(0.152)	(0.164)
Left Intcpt	14.384	14.471	17.105	16.608	0.803	0.814	0.871	0.853
	(0.5)	(0.457)	(0.860)	(0.727)	(0.072)	(0.074)	(0.141)	(0.15)
Bandwidth	62.086	62.815	66.021	75.3	60.637	60.236	70.787	61.052
Eff. Obs	3863	3829	6236	6825	3574	3528	6296	5504
District FE	Y	Y	Y	Y	Y	Y	Y	Y
Covariates	N	Y	N	Y	N	Y	N	Y

For each defendant type, second and third columns include district-fixed effects, and third columns condition on gender, and crime type. Standard errors clustered at the district-level. Only African-American and white defendants included (98% of all defendants).

Figure A1: Replication of Main Results, Placebo Dates
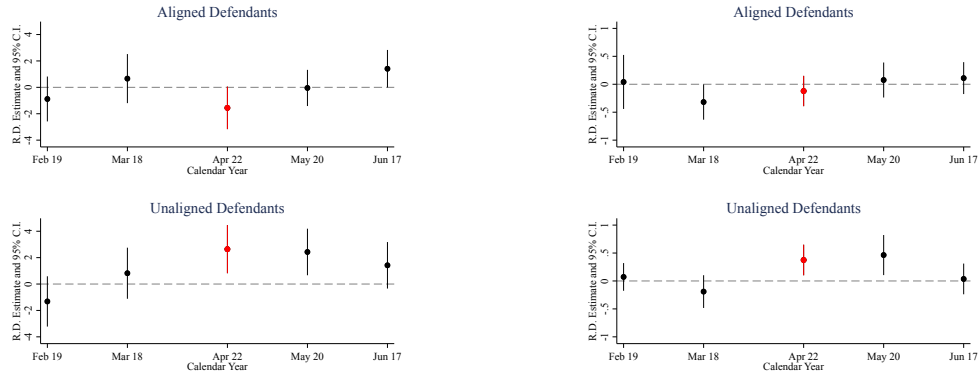
Left: Charge severity. Right: Sentence severity. RDiT estimates presented for placebo dates on the third Friday of the month, one and two months before and after the announcement. All results include district fixed effects and adjust for covariates.

Figure A2: Replication of Main Results, Variation of Bandwidth
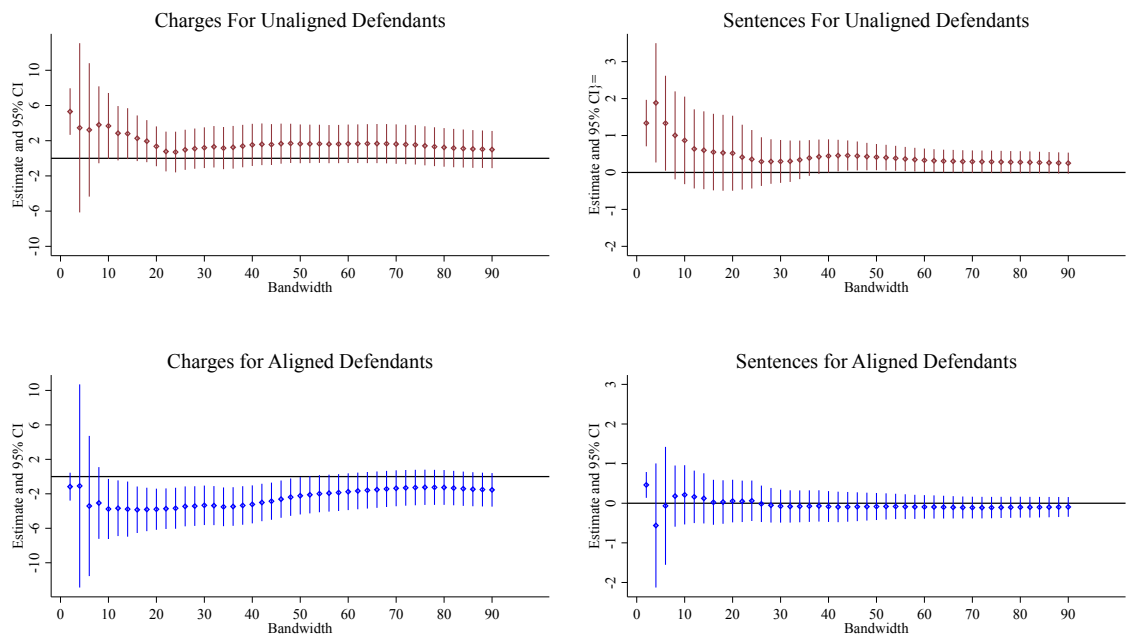
Point estimates and 95% confidence intervals for user-specified bandwidths beginning at 2 days and increasing to 90 in increments of two. Results include only district fixed effects.

Table A4: Effect of McAuliffe's Order on Charge and Sentence Severity By Alignment Status, Local Randomization Difference-in-Means Estimates

	Aligned Defendants				Unligned Defendants			
W	Diff. Means	Finite $P > \|T\|$	Asympt $P > \|T\|$	Eff. Obs.	Diff. Means	Finite $P > \|T\|$	Asympt $P > \|T\|$	Eff. Obs.
Panel A. Charge Severity (Districts Present In Both Windows)								
1	-3.104	0.190	0.182	99	4.697	0.116	0.133	70
2	-4.054	0.036	0.026	160	3.159	0.194	0.203	124
3	-2.552	0.090	0.077	245	1.699	0.242	0.256	222
4	-3.631	0.006	0.002	425	2.047	0.098	0.110	311
5	-3.531	0.000	0.000	505	2.576	0.014	0.028	367
Panel B. Sentence Severity: Districts Present In Both Windows								
1	0.703	0.102	0.091	85	1.558	0.046	0.211	62
2	0.397	0.214	0.249	131	0.923	0.284	0.295	93
3	-0.049	0.838	0.824	368	0.359	0.262	0.324	242
4	-0.086	0.718	0.659	609	0.119	0.574	0.613	430
5	-0.063	0.790	0.740	681	0.171	0.380	0.377	527

Table presents the window used (column 1); the observed difference in means for this window (columns 2 and 5); the p-value for the difference generated via a complete randomization procedure using 1000 randomly drawn randomizations—i.e., the proportion of random assignments to treatment and control for which the test statistic generated was more extreme than that actually observed—(columns 3 and 6); the p-value for the difference corresponding to a test of the average null hypothesis (columns 4 and 7), and the effective number of observations used (column 8). Districts included only if present both before/after announcement and range from 9 (some one-day windows) to 53 (some five-day windows).

B.1 Heterogeneous Effects

Figure A3: Heterogeneous Effects by Electoral Competition
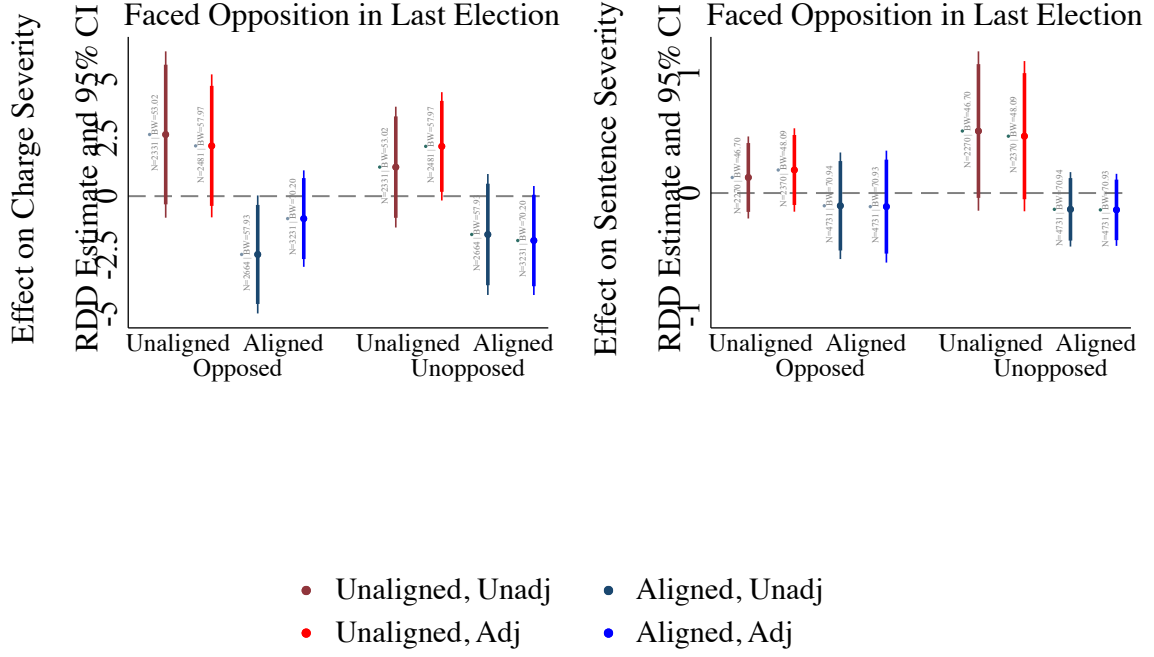
The figure presents separate estimates of the announcement's effect on charge severity (left panel) and sentence severity (right panel) in districts where the prosecutor was threatened in the last election (faced an opponent who won more than 10% of the vote) and those where the prosecutor was secure.

Figure A4: Effect on Charge and Sentence Severity, by Office Size
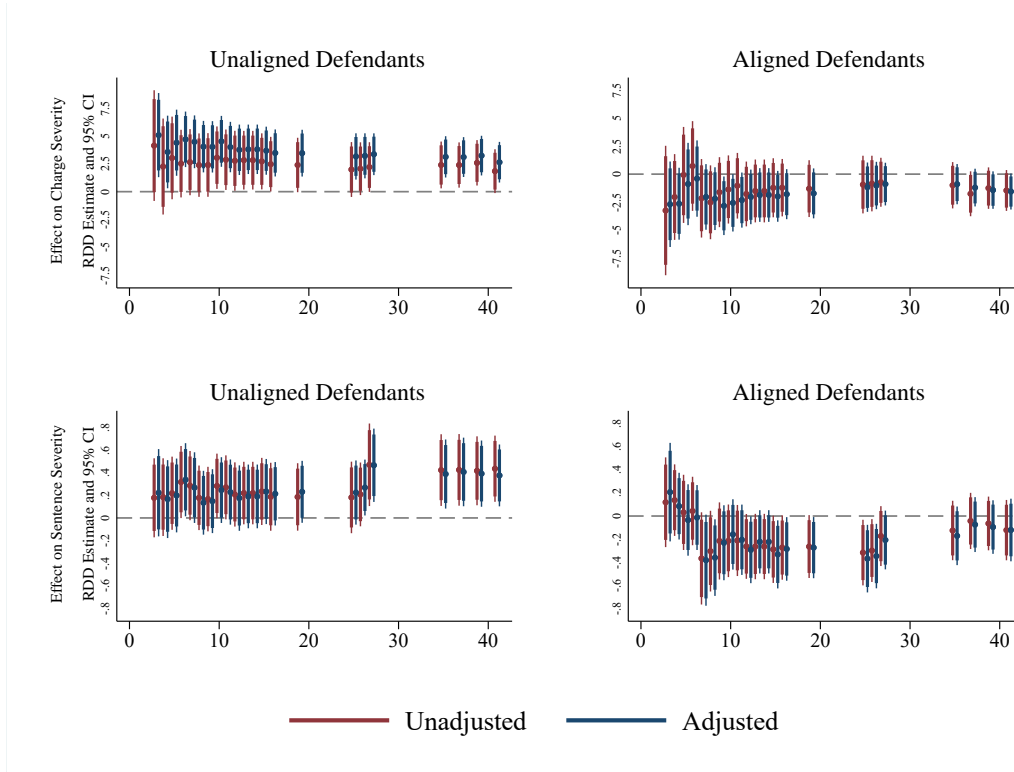
The plots in Figure A4 present the effect of the announcement on charge severity (top row) and sentence severity (bottom row) for aligned (left column) and unaligned (right column) defendants, disaggregating by office size on the X-axis. Specifically, the red/blue coefficient estimates within each plot represent the unadjusted/adjusted effect of the announcement for all offices at or under a given size, beginning at an office size of 2 and increasing to the maximum office size of 41. In general, the results do not vary greatly by office size, especially for the unaligned. For aligned defendants, the announcement's effect on sentence severity seems to be driven primarily by medium-sized offices. We obtain estimates of prosecutor office size from a survey conducted by the Virginia Compensation Board in 2018 and presented in their memorandum, "Workgroup Study of the Impact of Body Worn Cameras on Workload in Commonwealth's Attorneys' Offices," available at https://www.scb.virginia.gov/docs/bodycameraworkgroupreport.pdf.

B.2 Sentencing Strategies

Table A5: Effect of McAuliffe's Order on Incidence of Time Served Sentence, By Alignment Status, Local Randomization Difference-in-Means Estimates

	Aligned Defendants				Unaligned Defendants			
W	Diff. Means	Finite $P > \|T\|$	Asympt $P > \|T\|$	Eff. Obs.	Diff. Means	Finite $P > \|T\|$	Asympt $P > \|T\|$	Eff. Obs.
1	0.096	0.254	0.133	85	0.057	0.652	0.368	62
2	0.135	0.020	0.018	131	0.051	0.594	0.341	93
3	0.063	0.056	0.038	368	0.014	0.864	0.701	242
4	0.056	0.010	0.016	709	0.001	1.000	0.955	430
5	0.046	0.022	0.027	681	0.014	0.634	0.542	527

Column 1 lists the window used for inference, Columns 2 and 5 the observed difference; in means for each window; Columns 3 and 6 the p-value corresponding to a sharp null hypothesis test and generated via a complete randomization procedure using 1000 randomly drawn randomizations. Columns 4 and 7 present the p-value corresponding to a test of the average null hypothesis. Columns 5 and 9 list the effective number of observations used. Only districts with at least one observation in both windows included.

Figure A5: Effect of Announcement on Sentencing Outcomes, Difference-in-Means
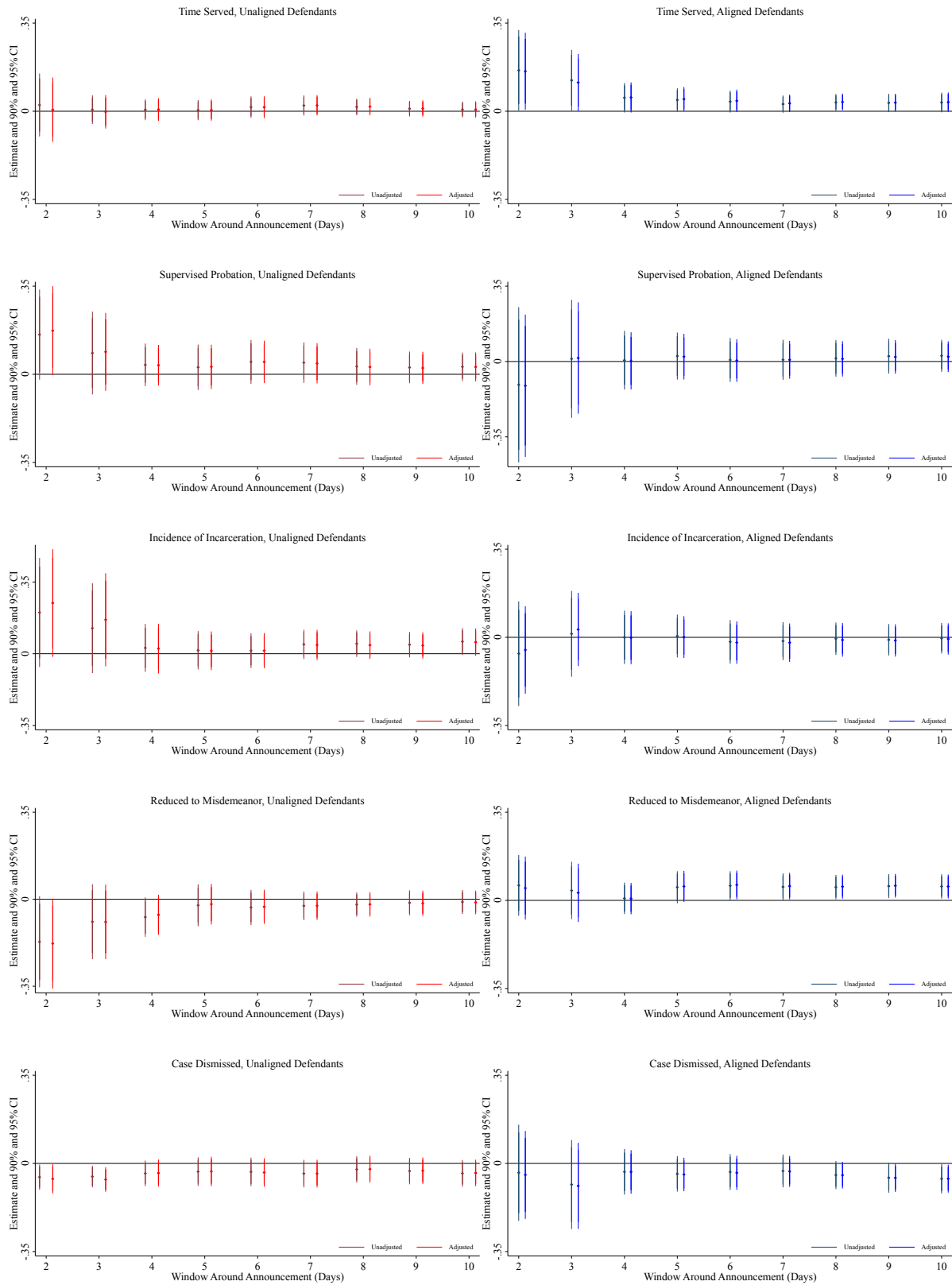
Pooled (district fixed effects) estimates and (90/95%) cluster-robust confidence intervals for change in probability that unaligned/aligned defendants' charges are sentenced to time served, probation, prison, reduced to misdemeanors, or dismissed. Adjusted estimates adjust for gender and crime type. Clusters from 53-96 for aligned, 61-100 for unaligned defendants.
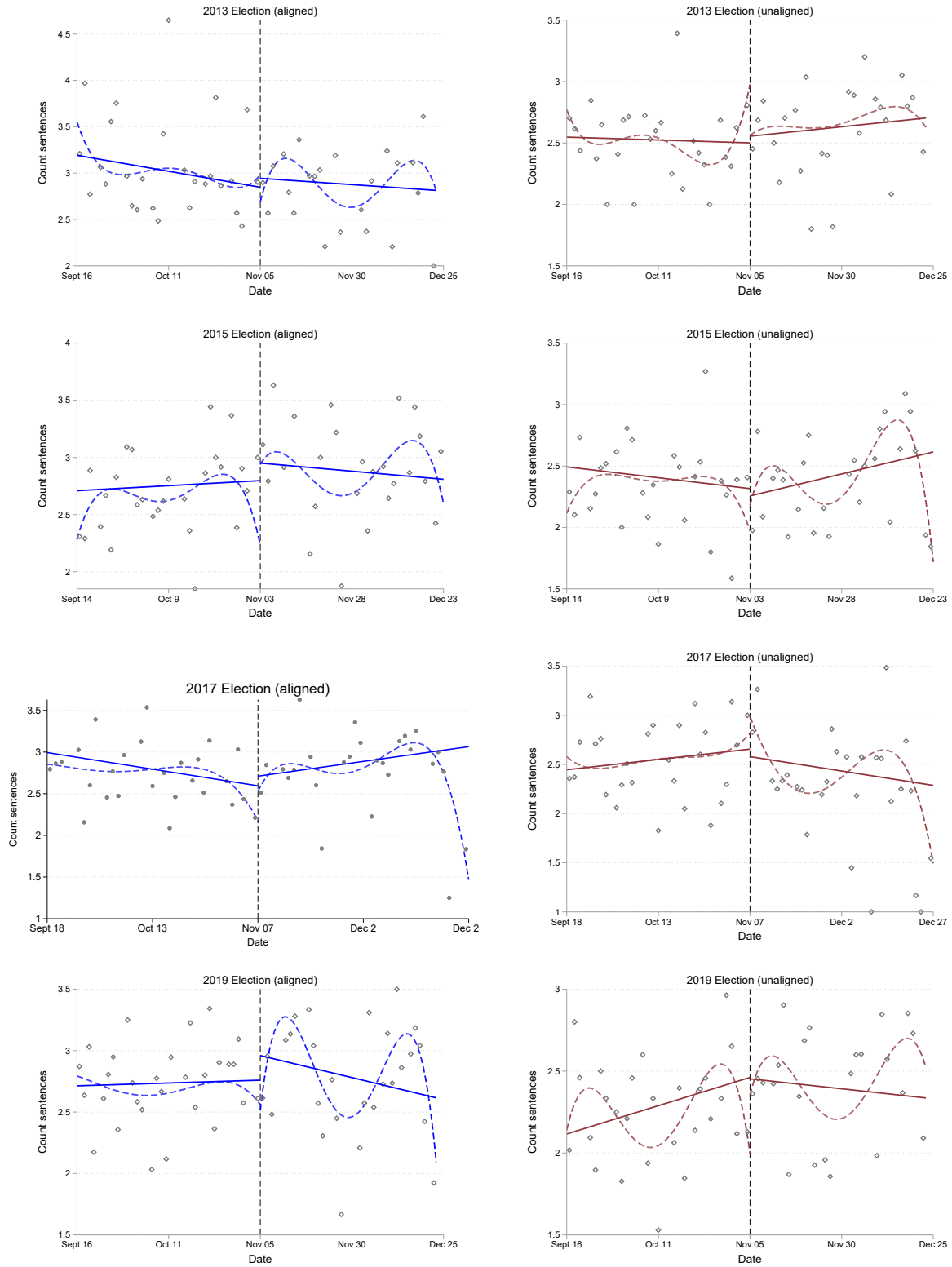
C   Elections

Figure A6: Total cases disposed of for aligned (left) and unaligned (right) defendants before/after state election dates
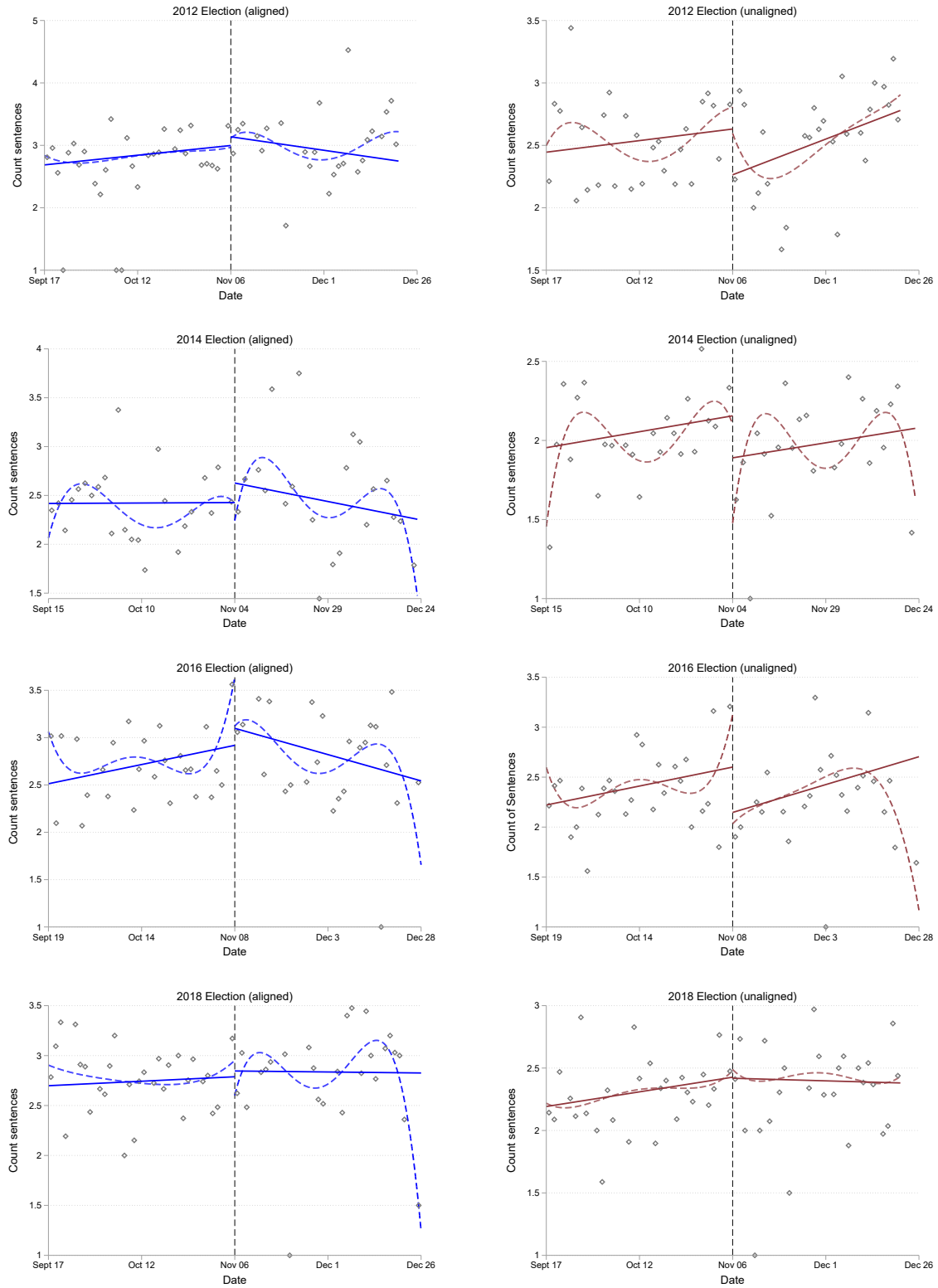
Dashed lines are a kernel weighted polynomial of order 4 and solid lines are linear predictions.

Figure A7: Total cases disposed of for aligned (left) and unaligned (right) defendants before/after federal election dates

Dashed lines are a kernel weighted polynomial of order 4 and solid lines are linear predictions.