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ORIGINAL ARTICLE

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 sudden policy change—unexpectedly announced by the Governor on April 22, 2016—enfranchising all felons 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. Evidence suggests that these changes may have been partially motivated by partisan concerns in the lead-up to the 2016 federal elections.

Keywords: American politics; judicial politics; political economy

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 their ideological congruence or competence to voters (Gordon and Huber, 2002, 2007; Stuntz, 2004; Dyke, 2007; Bandyopadhyay and McCannon, 2014; Gordon and Yntiso, 2022). 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 *et al.*, 2001; Alt *et al.*, 2011)—make policy choices with an eye toward the principals who keep them in office.[1]

In this paper, we contend 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-à-vis their principals, which ordinary agents lack. A legislator, rather than choosing a set of policies her constituents approve, may try via gerrymandering or voting restrictions to choose a set of constituents who approve her policies (Bateman, 2018; Eubank and Fresh, 2022; Helmke *et al.*, 2022). We argue that in a similar fashion, in the many US states that restrict felon voting, elected criminal justice officials may try to shape the composition of the electorate to favor themselves and their copartisans through the selective imposition of disenfranchising felony sentences. The temptation to do this may

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

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

To illustrate this argument, we construct a simple formal model featuring a prosecutor who may offer a felony defendant a plea deal, and a defendant who may accept the deal or reject it and go to trial. 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. We show that when the prosecutor also stands to benefit from the outcome of an upcoming election, and there is a felon disenfranchisement law on the books, the prosecutor's optimal plea offer depends on (1) whether he wants the defendant to vote, and (2) whether disenfranchisement is indefinite, or only for the duration of the felony sentence. If the prosecutor does *not* want the defendant to vote, we show that when disenfranchisement is indefinite, his offer is more lenient (to increase the probability that the defendant accepts), and when it is sentence-dependent, his offer is harsher (to increase the time the defendant spends disenfranchised). If the prosecutor *does* want the defendant to vote, the converse is true. In a "hybrid" regime where either disenfranchisement outcome may occur, any increase in the probability that disenfranchisement is sentence-dependent increases the harshness of the optimal plea when the prosecutor does not want the defendant to vote, and decreases it when he does.

We investigate the empirical validity of our argument by leveraging 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 re-enfranchisement of all felons upon completion of their "sentences of [felony] incarceration" and "supervised release" (Order for the Restoration of Rights, 2016). 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, approximating the "hybrid" regime in our model. It had at least three immediate consequences. First, it made the length of a felony prison or probation sentence the key determinant of (dis)enfranchisement going forward. Second, according to the Governor, it caused the immediate enfranchisement of 200,000 individuals (Horwitz and Portnoy, 2016). Third, it motivated voter registration drives across the state, targeting the newly enfranchised felon population (Vogelsong, 2016). We hypothesize that, as a result, the announcement may have motivated Virginia's elected felony prosecutors to attempt to mitigate changes to the partisan composition of the electorate by increasing charge and sentence severity for felony defendants they perceived as politically opposed, while decreasing them for defendants they perceived as politically aligned.

To test this hypothesis, we collected all criminal charges filed or sentenced in Virginia Circuit Courts between 2010 and 2020. Our raw data featured 896,824 felony and 333,354 misdemeanor charges, each with detailed accompanying information on the name, race, sex, and charging district of the defendant, and on the charge's progress through the justice system from indictment to disposition and sentence. From these data, we constructed a case-level dataset by aggregating all charges filed or sentenced on the same day for the same defendant, and ordering multiple charges within a single case by the severity of their maximum sentences. This process yielded 386,992 felony cases filed or disposed of between 2010 and 2020, of which 56,023 were filed or disposed of 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 defendants by likely political alignment with their prosecutors, then compare the charging and sentencing outcomes experienced by each group immediately *before* the rule change to those immediately *after* it. Following Helmke *et al.* (2022) and Eubank and Fresh (2022), we construct a likely-alignment indicator by assuming that racially polarized voting patterns

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render a defendant's race informative—though not necessarily determinative—of her political affiliation.[2] 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, we find that immediately after the announcement, defendants who were likely-unaligned with their prosecutor experienced increases in felony charge severity and actual sentence length, while likely-aligned defendants experienced decreases. These findings persist across a range of different RDiT specifications and bandwidth choices, are broadly robust to pooled difference-in-means regression analyses and local randomization inference, and do not appear to depend on prosecutor office size. Providing further support for our theory, we show that the announcement had no effect on charging or sentencing for the subset of (non-disenfranchising) misdemeanor cases handled in Circuit Court over the same period.

Second, these effects appear to have occurred via a range of pathways. Shifts in sentence severity persist when we limit our analysis to cases charged prior to the announcement, suggesting that the announcement may have directly affected prosecutors' sentencing decisions. But the announcement also appears to have affected prosecutors' willingness to bargain—for example, by reducing or dismissing charges—as well as their initial charging decisions. Thus, to estimate the announcement's total effect on sentence severity, we estimate the pre-announcement relationship between charge severity and sentence severity, use it to predict the indirect effect of post-announcement changes to charge severity on sentencing, and combine this result with the announcement's direct effect on sentencing. We show that the total effect may have been an average sentence increase of four to seven months for unaligned defendants, and an average decrease of one to three months for aligned defendants.

Third, we find little evidence in favor of alternative explanations, such as electoral responsiveness or prosecutors' personal attitudes toward felon disenfranchisement. 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 toward the unaligned and increased leniency toward the aligned. We also find no evidence that prosecutors' personal attitudes toward felon disenfranchisement shaped their behavior: the effect is similar among prosecutors who opposed the Governor's executive order and those who did not.

We next investigate possible prosecutor motives for strategic disenfranchisement. As with legislators, prosecutors arguably may attempt to manipulate voter status to improve their own chances of reelection, to improve the chances of their favored party, or both. Both motives are plausible: with regard to *personal* incentives, so long as prosecutors can readily infer defendants' likely partisanship and are uncertain about their future margins of victory, manipulation may be worthwhile even when the total number of defendants affected is low. But the low cost of inferring defendant alignment may also tempt prosecutors to manipulate felon status for *partisan* reasons, to tip the scale in other, closer electoral contests. Moreover, McAuliffe's announcement seems likely to have triggered partisan feeling: his changes to Virginia's disenfranchisement law were described by Republicans as an attempt to guarantee Virginia for Democrats in the 2016 presidential election (Horwitz and Portnoy, 2016).[3] Thus, it is possible that prosecutors were especially motivated to manipulate felon status due to the perceived salience of felon voting restrictions for the 2016 presidential election.

We conduct three series of tests to disentangle the personal from the partisan motivation, and find some evidence in favor of the partisan channel. We begin with the conjecture that prosecutors

[2]Racial sorting into parties is a well-known feature of contemporary American political life (Mason, 2018; Kuriwaki *et al.*, 2024) which extends to the felon population (Burch, 2012).

[3]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 (Vogelsong, 2016).

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who wish to shape the outcome of an election may conclude cases pre-election for unaligned defendants, but post-election for aligned defendants. If we observe this behavior around prosecutors' own elections, this suggests a personal motive; if prosecutors engage in this behavior more generally, it suggests partisan concerns. Accordingly, we examine patterns in criminal case outcomes in the weeks around state (odd-year) and federal (even-year) elections in Virginia from 2012 to 2019. We find little evidence of an effect in state election years, but do find some evidence of an effect in federal election years. This is consistent with broader partisan motives.

If partisan feeling motivates prosecutors to manipulate felon status, the announcement's effect on charging and sentencing should be stronger among more enthusiastic partisans. We use prosecutors' donor histories in the Database on Ideology, Money in Politics, and Elections (DIME) (Bonica, 2016) to examine how three measures of partisan political engagement—ideology, donation recency, and donation size—affected prosecutors' responses to the announcement. We find that donors, especially more recent donors, are more responsive to the announcement than non-donors.

Finally, if prosecutors are motivated by their desire to stay in office, more politically vulnerable prosecutors might be expected to have responded more strongly to the announcement. Because vulnerability is endogenous to political aptitude, this possibility is difficult to test. Nevertheless, as a first cut, we code prosecutors as politically "threatened" if their last election featured a challenger who received at least 10% of the vote. We find that political threat, as measured, has no effect on charging and sentencing behavior post-announcement.

Our paper is relevant to the growing literature on justice officials as strategic political actors. Existing work 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; Dyke, 2007; Bandyopadhyay and McCannon, 2014; Mungan, 2017). Our contribution is to show that elected justice officials may also manipulate charges, convictions, and sentences in order to shape the electoral environment directly.

Our paper is also relevant to scholarship on attempts by political actors to manipulate the composition of the electorate (Behrens *et al.*, 2003; Bateman, 2018; Eubank and Fresh, 2022; Helmke *et al.*, 2022). The literature in this area has focused primarily on the incentives of elected officials to shape the electorate in their favor. Thus, scholars have raised the possibility that felon disenfranchisement laws may have been strategically imposed by state officials (e.g., Eubank and Fresh, 2022; Helmke *et al.*, 2022). We contribute to this literature by arguing that such laws might also be strategically *implemented*, and demonstrating that the state-level imposition of such laws may motivate local-level officials to manipulate their application.

1. Elected prosecutors and felon voting laws

Consider an interaction between a felony defendant and a prosecutor. The prosecutor can offer a plea deal which the defendant can accept or reject. Consistent with prior work among political economists and legal scholars (Landes, 1971; Grossman and Katz, 1983; Baker and Mezzetti, 2001; Gordon and Huber, 2009; Stuntz, 2011), suppose the prosecutor has discretion to select the precise plea offer that maximizes his utility. If the defendant rejects the plea, the players go to trial, where with some probability the defendant is convicted and sentenced to prison, and with complementary probability she is acquitted. Trial comes at a privately known cost to the defendant, and her utility is decreasing in the length of her sentence. Trial is also costly to the prosecutor, but his utility is increasing in the length of the defendant's sentence.

In this context, the prosecutor trades off a higher sentence in his plea offer against a higher likelihood of going to trial. In equilibrium, he offers the plea that optimally accounts for his own known

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cost of trial, the defendant's unknown cost of trial, and the defendant's expected sentence at trial. (In the Online Appendix, this argument is presented formally and the game is solved in full.)

Now suppose there is a felon disenfranchisement rule on the books, and the prosecutor is concerned about the outcome of an election. Under the rule, disenfranchisement may take one of two forms: it may be indefinite, or only for the length of a sentence. We show in the Online Appendix that if the prosecutor suspects the defendant will vote for the opposite party and the rule is indefinitely disenfranchising, then the prosecutor adjusts his plea offer *downward* to induce the defendant to accept and lose the right to vote. By contrast, if the rule disenfranchises the defendant only during her sentence, the prosecutor adjusts his plea offer *upward* to inflict a longer sentence. While in both cases the size of the adjustment depends on how much the prosecutor cares about preventing the defendant from voting, some shift is always optimal.

If the prosecutor instead suspects the defendant is a copartisan, then under indefinite disenfranchisement he adjusts the plea offer upward (to discourage the defendant from taking it) and in a sentence-dependent regime he adjusts it downward (to minimize her disenfranchised time). Finally, if the rule is a hybrid—with some probability a convicted felon is indefinitely disenfranchised, and with reciprocal probability she is disenfranchised only during her sentence—we show that an increase in the probability that disenfranchisement is sentence-dependent increases optimal plea offer severity if the defendant is politically unaligned, and decreases it if she is aligned.

So far, we have assumed that the defendant does not consider disenfranchisement in making her plea decision. This is empirically plausible for several reasons: defendants may not know about felony disenfranchisement rules, or may heavily discount the future loss of voting rights relative to their present concerns. Nevertheless, we extend the model to consider the case where the defendant dislikes disenfranchisement. We show that this case exacerbates some of the above effects while mitigating others, but that the prosecutor's incentives to manipulate plea offers as described above are never fully counteracted unless the defendant's disutility from disenfranchisement is very high relative to its benefit (cost) to the prosecutor.

Applying these theoretical findings to Virginia, we can make two conjectures. First, the Governor's executive order effectively transformed felon disenfranchisement in Virginia from a hybrid regime in which multiple factors determined disenfranchisement status, to a fully sentence-dependent regime. Consequently, post-order, prosecutors should have assigned greater importance to manipulating felony sentence length to shape defendants' probabilities of voting. Second, the executive order directly altered the composition of the electorate by immediately enfranchising a large number of formerly disenfranchised felons. 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 should have negatively shocked prosecutors' own electoral viability and that of their copartisans. These shocks should have heightened the importance prosecutors assigned to manipulating defendants' enfranchisement status. These two conjectures 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.

Note that this hypothesis is independent of prosecutors' precise reasons for shaping the electorate. These reasons might be personal and office-seeking, partisan and policy-seeking, or a mix. In a later section, we provide suggestive evidence that prosecutors' attempts to manipulate the electorate are motivated at least in part by a desire to help copartisans.

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2. 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. Before the order, the criteria for re-enfranchisement 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 three year waiting period and submit a written application.[4] 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. According to the Governor, an immediate result of this change was the automatic enfranchisement of 200,000 felons who had already completed their sentences—a 3.3% expansion in the statewide voter pool.[5]

For our purposes, the 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, voter registrars, and prosecutors had been deliberately kept in the dark to prevent them from attempting to head off the changes before their announcement (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: post-announcement, prosecutors concerned with shaping the local electorate should have shifted 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, felony prosecutors[6] ("Commonwealth's Attorneys") are chosen in district-level competitive elections every four years—but the Circuit Court judges who handle felony cases are appointed by the legislature to eight-year terms.[7] These judicial appointments occur in an almost consociational fashion, beginning with a judge's nomination to the Courts Committee by the local Bar Association, proceeding to unanimous election by all state senators representing a given judicial circuit, and usually ending with unanimous confirmation by the Virginia Senate (A Legislator's Guide to the Judicial Selection Process, n.d.). This suggests that Commonwealth's Attorneys are likely, and Circuit Court judges unlikely, to respond to electoral or partisan considerations.

3. Data

Our raw data consists of all Circuit Court charges filed between 2010 and the first months of 2020 in 118 of Virginia's 120 Circuit Courts.[8] The Office of the Executive Secretary at the Supreme Court of Virginia provided access to Circuit Court cases, which we complemented with additional sentencing

[4]For archived instructions for the restoration of voting rights in Virginia as of April 17, 2016, see https://web.archive.org/web/20160417134119/http://commonwealth.virginia.gov:80/judicial-system/restoration-of-rights.

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

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

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

[8]We are missing Circuit Court data from the districts of Alexandria and Fairfax because these districts do not use the state case management system.

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data from Virginiacourtdata.org.[9] These data include, for each charge: the date of indictment; the geographic 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 charge reductions and hearings.

We aggregated criminal charges up to the case level by treating all charges filed or resolved on the same day, for the same individual, as belonging to the same case.[10] This yielded a dataset of 134,695 misdemeanor and 386,992 felony cases charged or sentenced between 2010 and 2020, of which 56,023 felony and 12,264 misdemeanor cases were, respectively, charged or sentenced in 2016.

Because misdemeanors do not result in disenfranchisement, we omit these cases from our main analysis, leveraging them instead to conduct a set of placebo tests.[11] We also exclude a small set[12] of cases coded in our data as probation violations leading to reinstatement of an underlying felony, for two reasons. First, active probationers were disenfranchised both before *and* after the announcement. Second, we were unable to consistently link these cases to the original case and sentence, meaning that we cannot determine the extent to which punishment is a function of the severity of the original conviction.[13]

Finally, we merged our criminal case records with district-level vote shares in the 2012 and 2016 presidential elections and data on incumbent Commonwealth's Attorneys' party membership, election years, and election outcomes (including vote shares),[14] as well as the partisan identities of their challengers (fromHessick and Morse, 2019). We supplemented this information with data on Commonwealth's Attorney office size from a 2018 Virginia Compensation Board survey.[15]

3.1. Measurement

To test our theory, we constructed measures of prosecutor–defendant alignment and case-level charge and sentence severity. We operationalize these concepts as follows.

3.1.1. Defendant political alignment and race

As discussed above, race is indicated on every defendant's case file, and is a common heuristic for partisan affiliation. This heuristic appears empirically plausible in Virginia, where 76% of African-Americans statewide (but only 27% of whites) described themselves as Democrats or leaning-Democrat in 2014 (Pew Research Center, 2014). This heuristic has also been shown to be accurate among felon populations: in North Carolina and Florida, for example, more than 80% of registered African-American felons, but only 33% of registered white felons, were Democrats (Burch, 2012, 10). We therefore assume that Virginia prosecutors view African-American felony defendants as more likely to vote for Democrats, and white felony defendants as more likely to vote for Republicans. Admittedly, this assumption does not account for cross-district variation in the relationship between race and political preferences, e.g., in white support for Democrats. Nevertheless, we believe it is appropriate for two reasons.

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

[10]We excluded from this process separate, procedural charges arising out of underlying cases, such as failures to appear in court or violations of a bail or probation agreement.

[11]These misdemeanors are a subset of all misdemeanors in Virginia, because misdemeanors are generally handled in District, not Circuit Court. The reasons for their presence vary across districts and time, and 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 and local prosecutors agreed to prosecute sufficiently serious misdemeanors in Circuit Court (Code of Virginia, section 19.2-190.1 "Certification of ancillary misdemeanor offenses").

[12]4,655.

[13]However, we show in the Online Appendix that their inclusion does not materially alter our results.

[14]Collected from the Virginia Department of Elections, at https://historical.elections.virginia.gov/.

[15]"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.

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First, there is methodological value in using a simple alignment rule applied equally to all prosecutors as our main specification. While it is possible that prosecutors make more nuanced inferences about defendants' political preferences, these nuances are hard to account for in a principled manner, given data limitations. Second, to the extent that white support for Democrats varies across districts, we argue that even in districts where a randomly selected white *person* would be likelier than not to lean Democratic, this is not the case for a randomly selected white felony defendant, because white support for Democrats is tied to other demographic indicators which are sparse in the felony defendant population. Specifically, support for Democrats tends to be higher among white women, and to increase with education,[16] while white felony defendants are heavily male and tend to have low levels of formal education.[17] Moreover, there is evidence that Democratic Party operatives themselves view white felons as likely-Republican: during a close Governor's race, Washington State's Democratic Party chairman argued that illegal felon voting had tilted the race toward the Republican candidate, because the felon population was "nonunion, blue-collar, male [and] Caucasian ... the prime demographic Republicans target" (Postman, 2005).

3.1.2. *Prosecutor political party and alignment*

Given this assumption on defendant alignment, when prosecutors campaign as Republicans or Democrats, we treat prosecutor–defendant pairs as politically aligned when their parties match: for example, a white defendant and Republican prosecutor would be politically aligned. However, approximately half of Virginia Commonwealth's Attorneys run for election as Independents. Because Virginia does not have an Independent political party, and Independents are barred under state law from receiving endorsements or funding from the Republican and Democratic parties,[18] we cannot determine an Independent prosecutor's voter base from party endorsements or donations. Moreover, Virginia voters who identify as Independent are evenly split between those who lean Democrat and those who lean Republican (Pew Research Center, 2014), suggesting that the label has no inherent partisan association.

Here, we begin with the premise that voters view a politician's party membership as informative of her ideology and policy goals. If this is true, then because Independents are barred from entering into alliances with Virginia's two recognized political parties, in a district with a partisan lean, an incumbent Independent should expect her partisan challengers to hail from the political party with greater voter support in the district. Figure 1, which presents challenges to incumbent prosecutors 2015–2017 by party affiliation (and for Independents, by district partisan lean), shows support for this premise. Independent incumbent prosecutors in blue-leaning districts were *only* challenged by Democrats and other Independents, while those in red-leaning districts were *only* challenged by Republicans and other Independents.

If voters view party membership as informative, an Independent incumbent facing a partisan challenger should not expect the votes of citizens who are dedicated supporters of the challenger's party. By the same logic, she *should* expect the votes of citizens who are dedicated members of the district's minority party: they should prefer her to her opponent. This suggests that while in terms of policy, an Independent incumbent might position herself in the ideological center to build a majority coalition, in terms of electoral strategy, she should treat likely minority-party supporters as "aligned," and likely majority-party supporters as "unaligned."

[16] In Virginia's D.C.-adjacent Democratic strongholds, an overwhelming majority of whites—65–85%—hold a B.A. degree or higher see NIH (2026).

[17] According to one study, only 5% of white males formerly incarcerated in a state prison held a BA degree or higher (Couloute, 2018).

[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.

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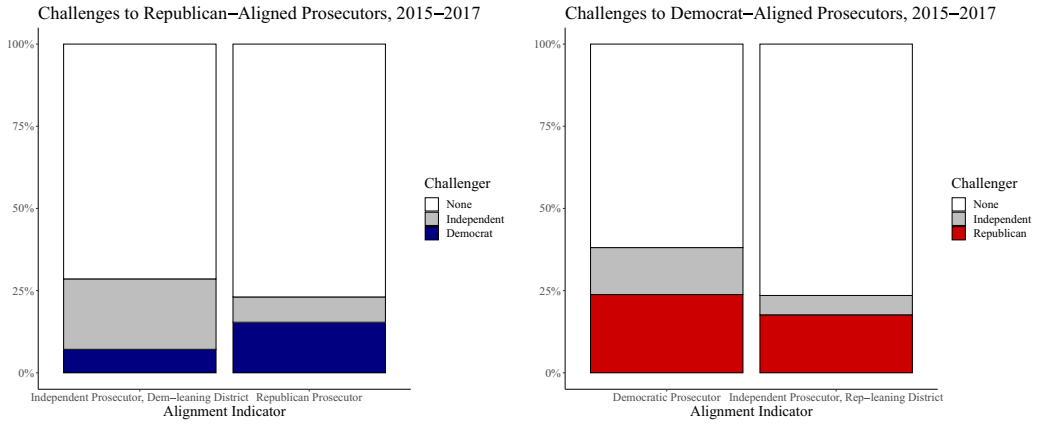


Figure 1. Challenger party by theorized alignment status.
Notes: Left panel: party identity of candidates who challenged Independent prosecutors in Democratic-leaning districts, compared to the party identity of candidates who challenged Republican prosecutors. Right panel: party identity of candidates who challenged Independent prosecutors in Republican-leaning districts, compared to those who challenged Democratic prosecutors. We categorize a district as Republican-leaning or Democrat-leaning based on which presidential candidate won the majority in that district in the 2012 presidential election.[19]

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

Table 1 presents our overall operationalization of political alignment among prosecutor–defendant pairs.

3.1.3. Charge and sentence severity

Because prosecutors may influence the length of a felony sentence directly, via the sentence they recommend, 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 most severe ("top") charge filed. This is a conservative measure of charge severity, because it does not account for the severity of *all* charges in a given case. Nearly all maximum felony sentences in our data take one of five values—5, 10, 20, or 40 years in prison, or life imprisonment—while a small number impose other values, such as 3, 30, or 50 years. Because a life sentence is indeterminate, we recode these sentences as 50-year sentences.

To measure sentence severity, we calculated the total felony prison sentence assigned after conviction in each case. Where multiple charges were sentenced in one case, and the sentences were set to run consecutively, we summed them. Where sentences were set to run concurrently, we used the longest sentence imposed. This measure is a conservative estimate of 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

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

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minimum: they end when the court actors involved in the case decide they should. Second, even definite terms of probation can be indefinite in practice because infractions during the probation period can sometimes be used to reset the probation clock.[20]

3.2. Empirical strategy

Treating Governor McAuliffe's announcement as an unexpected shock to felon disenfranchisement rules in Virginia, we use several methods to test our daily, district-level case data for discontinuous, alignment-specific shifts in prosecutorial punitiveness in the weeks following the shock. First, 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 shortcomings in the RDiT analysis, 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, and (in the Online Appendix) from a local randomization analysis, suggested by Cattaneo *et al.* (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 punishment 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 (each district elects one chief felony prosecutor). In some specifications, we also include pretreatment 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-à-vis the prosecutor.[22] Throughout, we usemean square error (MSE) optimal bandwidths with the bias-correction method proposed by Calonico *et al.* (2014), and standard errors clustered at the district level.[23]

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. We also examine how prosecutor use of specific sentencing tools shifted post-announcement.

[20]See Code of Virginia §19.2-304; 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 (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.

[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' 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 rely on the large number of unique values in our (daily) running variable, and probe robustness by conducting the additional tests described above.

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Table 2. Effect of McAuliffe's order, by alignment status

	Unaligned defendants			Aligned defendants		
			Panel A. Charge severity			
RD estimate	2.945	3.049	2.464	−2.108	−2.174	−1.787
	(1.534)	(1.205)	(1.184)	(1.77)	(1.065)	(1.076)
Left intercept	16.771	16.78	16.694	18.581	18.754	18.805
	(0.983)	(0.716)	(0.741)	(1.349)	(0.874)	(0.846)
Bandwidth	67.053	61.664	51.763	70.306	71.345	57.194
Eff. observations	6,161	5,647	4,699	8,016	8,085	6,593
			Panel B. Sentence severity			
RD estimate	0.378	0.469	0.402	−0.17	−0.169	−0.132
	(0.358)	(0.271)	(0.273)	(0.161)	(0.19)	(0.184)
Left intercept	0.748	0.759	0.759	0.876	0.864	0.87
	(0.097)	(0.084)	(0.087)	(0.12)	(0.152)	(0.137)
Bandwidth	52.6	55.082	54.025	70.994	71.84	73.568
Eff. observations	4,525	4,815	4,728	7,802	7,880	8,057
District FE	No	Yes	Yes	No	Yes	Yes
Covariates	No	No	Yes	No	No	Yes

Notes: Charge and sentence severity shown in years. Left Intercept is the estimated outcome immediately before the announcement. For each defendant type, the second and third columns include district-fixed effects, and the third column conditions on gender and crime type. Standard errors clustered at the district level. Only African-American and white defendants (98% of all defendants) included.

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, then use this relationship to derive bounds on the size of the announcement's effect on sentence severity via charge severity.

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

4. Main results

We begin with a graphical assessment of the announcement's effect on felony charge and sentence severity. 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 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 post-announcement shifts by alignment status: increases in average charge and sentence severity for unaligned defendants, and decreases for aligned defendants.

4.1. 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

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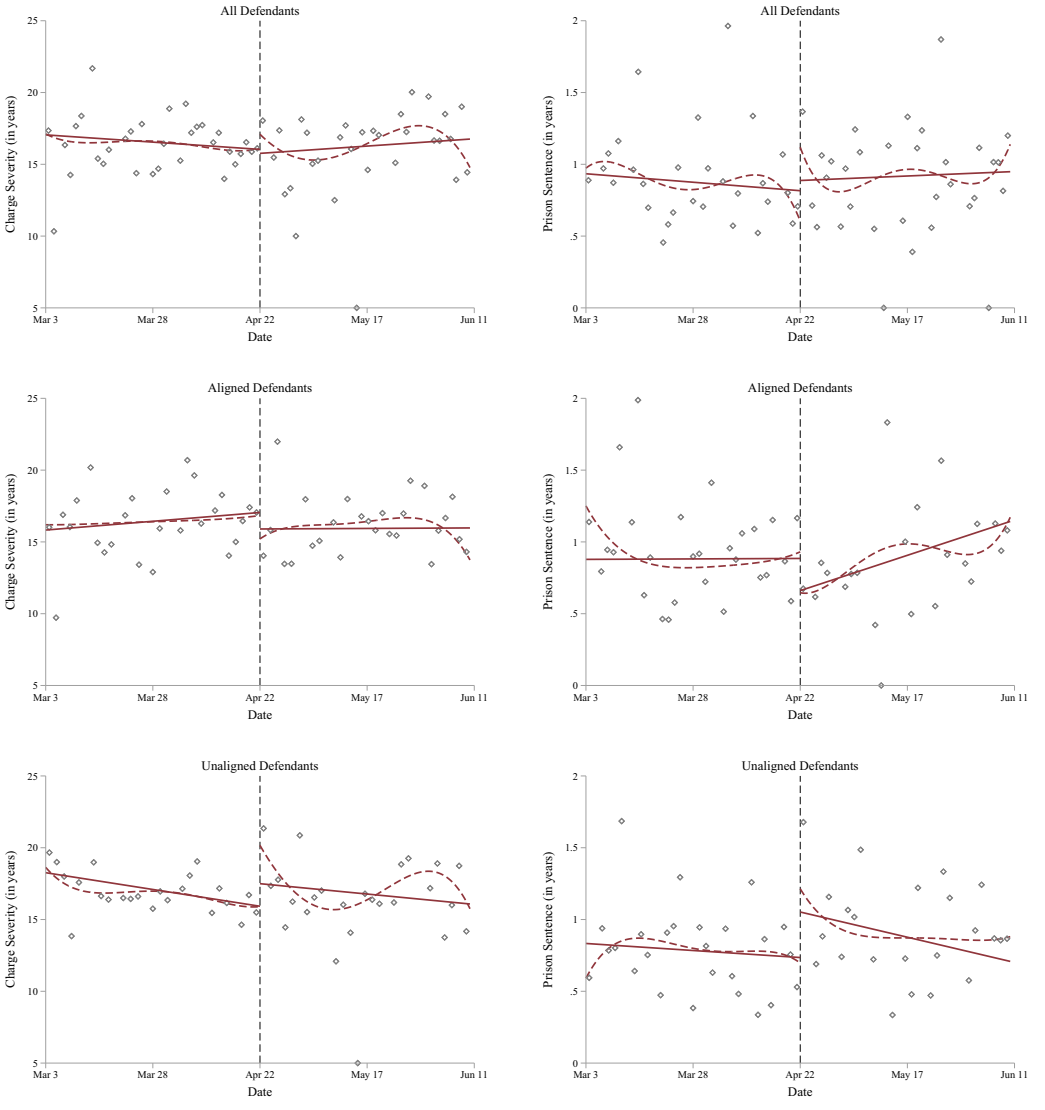


Figure 2. Announcement effect on charge (left column) and sentence severity (right column).
Notes: The top row in the figure displays the unconditional, within-district effect of the announcement on average charge severity (left) and prison sentences (right) before and after the announcement. The next 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.

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 drove increases in charge severity and sentence length for unaligned defendants, and decreases for aligned defendants.[25] Overall, for unaligned defendants, the executive order corresponded to an

[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]While the results which do not include district fixed effects are appropriately signed, they are consistently weaker; this is unsurprising, given substantial demographic variation across districts as well as variation in criminal activity and prosecutor behavior.

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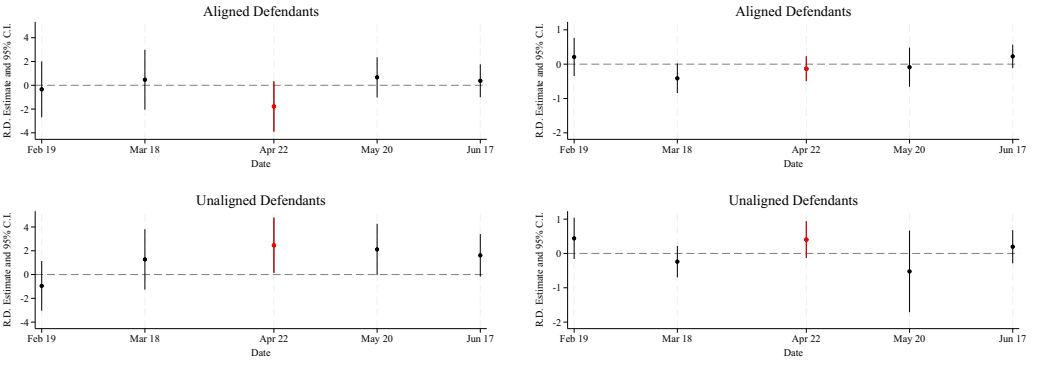


Figure 3. Replication of main results, placebo dates.
Notes: Left: Charge severity. Right: Sentence severity. RDiT estimates presented for placebo dates on the third Friday of the month, one and two months before and after the announcement. All results include district fixed effects and adjust for covariates.

Table 3. Effect of McAuliffe's order, by alignment status, on misdemeanor offenses

	Unaligned defendants		Aligned defendants		Unaligned defendants		Aligned defendants	
	Panel A. Charge severity				*Panel B. Sentence severity*			
RD estimate	0.029	0.025	0.038	0.041	-0.001	-0.006	0.016	0.016
	(0.034)	(0.031)	(0.038)	(0.034)	(0.018)	(0.019)	(0.019)	(0.019)
Left intercept	0.909	0.910	0.900	0.899	0.035	0.035	0.036	0.036
	(0.017)	(0.015)	(0.027)	(0.025)	(0.014)	(0.015)	(0.010)	(0.010)
Bandwidth	58.710	68.743	47.433	49.872	66.748	67.056	77.875	73.996
Eff. observations	1,555	1,881	1,616	1,658	1,803	1,829	2,604	2,514
District FE	Y	Y	Y	Y	Y	Y	Y	Y
Covariates	N	Y	N	Y	N	Y	N	Y

Notes: Charge and sentence severity shown in years. Left Intercept is the estimated outcome immediately before the announcement. For each defendant type, the first column includes district-fixed effects, and the second column also conditions on gender and crime type. Robust standard errors clustered at the district level. Only African-American and white defendants included (98% of all defendants).

average increase in initial charge severity of two to three years, and an average increase in actual sentence length of four to five months. Aligned defendants appear to have experienced about a two-year decrease in initial charge severity, and around a 1.5-month decrease in sentence severity, although the last effect is consistently statistically insignificant at conventional levels.[26]

These results are broadly robust to a range of alternative specifications, placebo tests, and bandwidth choices. First, in Figure 3, 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 prior to and following the announcement, respectively. The results are encouraging.[27] Second, if our hypothesis is correct, only *felony* charges and sentences should have been affected by the announcement, because only felony sentences are disenfranchising. Accordingly, in Table 3, we estimate the announcement's effect on misdemeanor offenses charged or sentenced in Circuit Court in 2016. Consistent with our theory, the resulting estimates are insignificant across all specifications. Third, in Figure 4, we present the coefficient estimates for our covariate-adjusted local-linear regressions beginning with a five-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 track those presented in Table 2.

[26]Notice from the left-hand intercepts in Table 2 that aligned defendants' baseline charges/sentences were more severe than unaligned defendants', which is consistent with prosecutors requiring greater seriousness to prosecute them at all.

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

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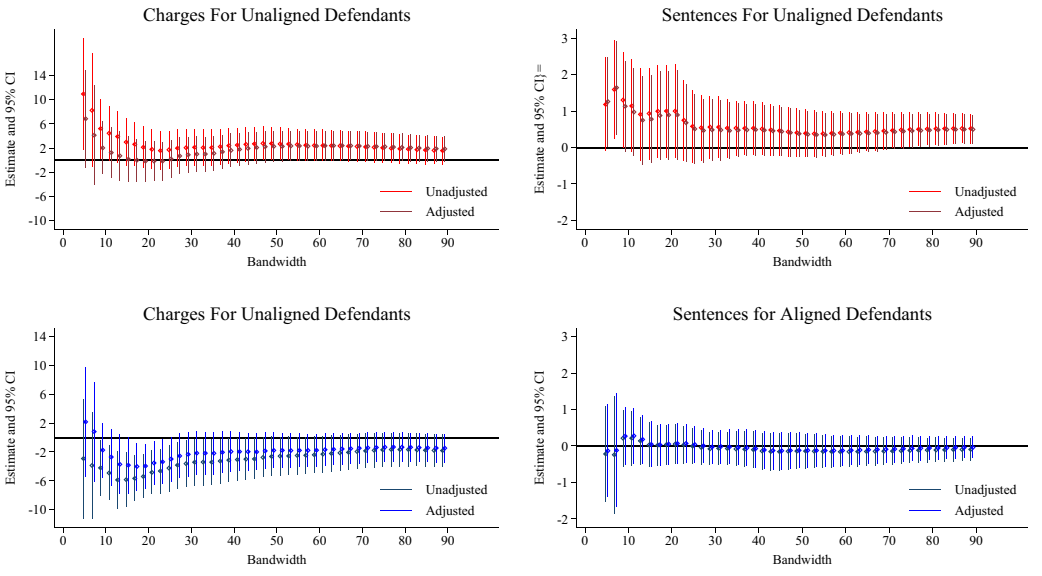


Figure 4. Replication of main results, variation of bandwidth.
Notes: Point estimates and 95% robust confidence intervals for user-specified bandwidths beginning at two days and increasing to 90 in increments of two. All results include district fixed effects; "adjusted" results include pretreatment covariates.

In the Online Appendix, we probe the robustness of our results to different theories of alignment and the inclusion of additional data. First, we re-estimate our main models after removing all districts headed by Independent prosecutors, and show that the results persist, although they are sometimes weaker (Table A1, Figures A1 and A2). Second, we test the robustness of our theory of alignment to an alternative treatment in which both white and African-American felony defendants living in strong Democratic districts (defined as districts with Democratic prosecutors in which at least 70% of the population voted for Barack Obama in 2012) are coded as Democrat-aligned (Table A2). The results are consistently signed and qualitatively similar, but they are weaker. Third, we re-estimate our main models including probation violation reinstatements (Table A3) and show that the results are similar. Finally, because elected chief prosecutors generally manage a team of assistant prosecutors, we explored the possibility that the announcement's effect might depend on chief prosecutors' control over their agents: in larger districts, subordinate prosecutors might have more independence, and the announcement might have had less of an effect. However, we find little evidence of a size-based effect (Figure A4).

4.2. Alternative specifications

The RDiT analysis above relied on the assumption that potential outcomes for aligned and unaligned defendants are a smooth function of the running variable. Because the announcement was unexpected, a plausible alternative assumption is that for a short enough time window around the announcement, the treatment (the announcement) was as-if-randomly assigned. Accordingly, we conduct a series of simple, pooled fixed effects regressions for two to ten-day windows around the announcement. 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. The results are presented in Figure 5. 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. These effects converge to effects similar in size to those in Table 2.

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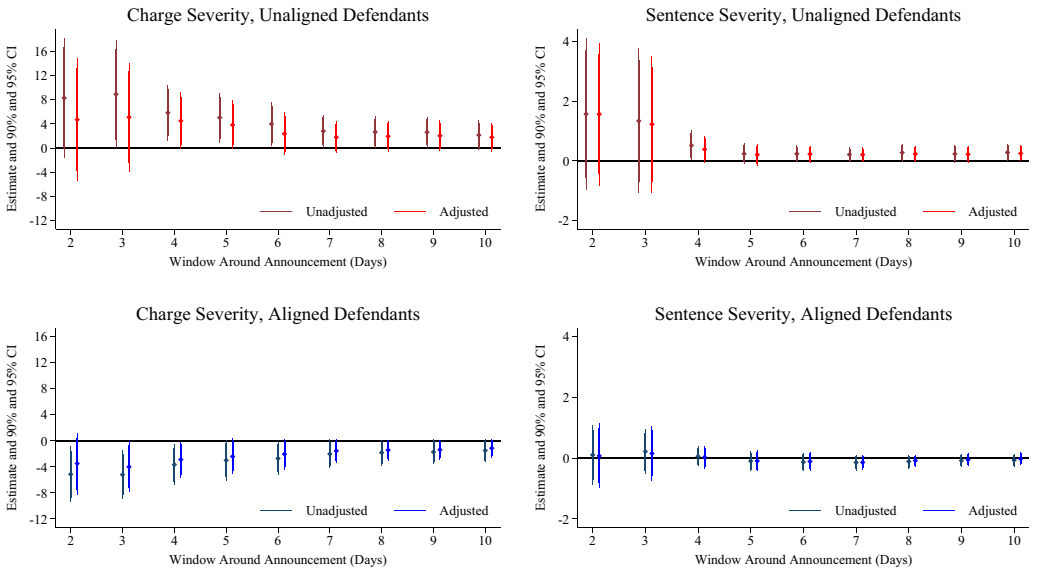


Figure 5. Effect of announcement, pooled difference-in-means regressions.
Notes: Point estimates and 90/95% confidence intervals for two-to-ten-day windows around the announcement. All estimates adjusted for district fixed effects; "adjusted" results adjusted for pretreatment covariates. Standard errors clustered at the district level. Clusters for charge severity: 54–96 for unaligned; 54–94 for aligned. Sentence severity clusters: 54–97 for aligned; 62–98 for unaligned.

In the Online Appendix, we also present the results of a complete randomization inference procedure proposed by Cattaneo *et al.* (2024). The procedure tests a 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 are available in these windows before and after the treatment. The results are shown in Table A4. Coefficient estimates are consistently signed, although they are statistically significant only for charge severity.

4.3. Alternative explanations

Finally, in Section B.4 of the Online Appendix, we consider several alternative explanations for our results. Specifically, we rule out the possibility that our results were generated by prosecutor- or district-level differences in racial attitudes, by showing that results are similar for African-American and white aligned and unaligned defendants. We also test whether prosecutors' personal opposition to the announcement affected their post-announcement charging/sentencing behavior, and show that it did not.

5. Mechanisms

Turning to an examination of the mechanisms behind our results, we begin by asking *how* these results came about. Here, we work to understand the channels through which the announcement affected disenfranchisement status: did prosecutors primarily adjust charges in response to the announcement, or did they directly alter sentences as well—and if so, by what methods?

Next, we consider *why*. Here, we attempt to disentangle two possible motives for charge/sentence manipulation: a personal, office-seeking motive, in which prosecutors seek to maximize the likelihood of their own reelections, and a partisan, policy-seeking motive, in which prosecutors seek to improve the likelihood of reelection for copartisans at other levels of government.

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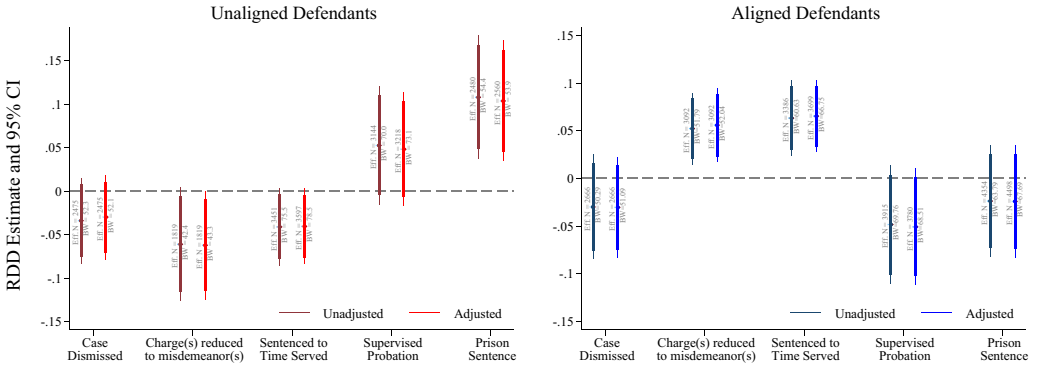


Figure 6. Effect of announcement on sentencing outcomes.
Notes: Panels provide RD estimates and 90/95% robust confidence intervals for the post-announcement change in the probability that unaligned and aligned defendants' charges are (from left to right) dismissed, reduced from felonies to misdemeanors, sentenced to time served, sentenced to supervised probation, or sentenced to prison. All estimates adjust for district fixed effects; "adjusted" estimates also control for pretreatment covariates.

5.1. Direct and indirect effects on sentencing

In this section, we consider the channels through which Governor McAuliffe's announcement may have shaped charge and sentence severity. An initial question is whether the announcement directly affected prosecutors' sentencing choices, or *only* changed their initial charging behavior. We attempt to answer this question in two ways. First, we re-estimate the announcement's effect on average prison sentences, restricting our analysis to cases charged before the announcement. The results (presented in Online Appendix Table A5) are similar to those in Table 2, suggesting that our main sentencing results do capture some direct effect on sentencing.[28]

Second, we examine the announcement's effect on prosecutors' use of five separate tools which directly shape sentencing outcomes: dismissals, charge reductions to misdemeanors, and requesting a time served, supervised probation, or prison sentence. Ideally, we would examine how prosecutors' use of each tool changed in real time after the announcement. Unfortunately, we lack dates for informal plea agreements and amendments prior to cases' final resolutions. As a next-best strategy, we conduct a local-linear RD estimation of the announcement's effect on the incidence of each outcome (recorded at the conclusion of the case). The results, displayed in Figure 6, show that for unaligned defendants, the announcement decreased the probability that felony charges were dismissed, reduced to misdemeanors, or sentenced to time served, and increased the probability of receiving a prison or probation sentence. For aligned defendants, the announcement somewhat decreased the probability of receiving a prison or probation sentence, and significantly increased the probability of a time served sentence or charge reduction to a (non-disenfranchising) misdemeanor. We show in the Online Appendix (Figure A3) that these effects largely persist in a series of pooled fixed effects regressions for two-to-ten-day windows around the announcement. We find an especially strong effect on the provision of time served sentences to aligned defendants, perhaps because these sentences permit prosecutors to obtain convictions on felony offenses without—after the announcement—ever causing disenfranchisement, and show in Table A6 that this result is robust to the Fisher complete randomization test we conducted in the previous section.

5.2. The total sentencing effect

The results above suggest that to understand the announcement's full effect on sentencing (and disenfranchisement) outcomes, we must account for both its direct effect via sentencing strategies and its

[28] In some specifications, we condition on initial charge severity, which attenuates the results. However, the utility of this strategy is uncertain as by our theory, charge severity should be endogenous to alignment before and after the announcement.

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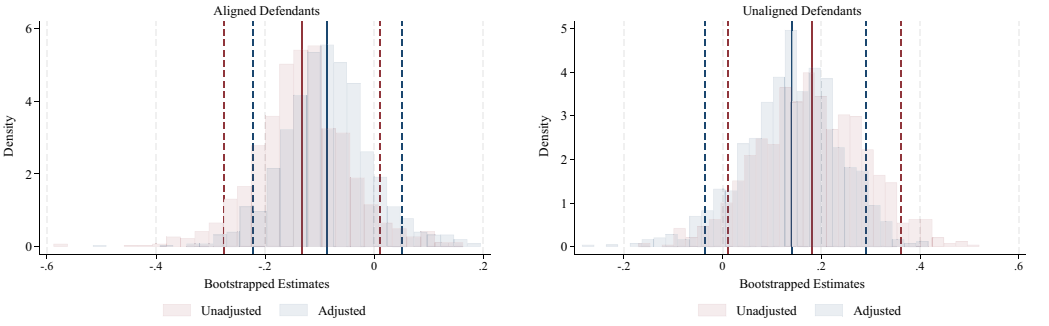


Figure 7. Bootstrapped estimates of indirect sentencing effect.
Notes: Histograms depict cluster-bootstrapped estimates of the discontinuous shift in sentence lengths attributable to the post-announcement shift in charge severity, for aligned (left) and unaligned (right) defendants. The solid vertical 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. Clusters at the district level.

indirect effect via charges. The latter is difficult, because all cases charged post-announcement were necessarily sentenced post-announcement. Our solution is to 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 1,000 cluster-bootstrapped samples.

Figure 7 displays our estimates of the indirect 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 30 to 40-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 the assumption 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 four to five months for unaligned defendants, and a (statistically insignificant) decrease of approximately one to 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 complements, then after the announcement, unaligned defendants may have received sentences as much as seven months longer, and the aligned as much as three months less.

5.3. Personal or partisan motives?

Were Virginia's chief prosecutors manipulating disenfranchisement status to further their personal electoral prospects, or to improve the electoral fortunes of copartisans more broadly? Both motives are theoretically possible. With regard to prosecutors' own races, the negligible cost of manipulation—since defendant partisanship can be readily inferred—may make it worthwhile even when disenfranchisement is unlikely to tip the scale: after all, prosecutors do not know what their margin of victory will be, and a prosecutor who prevents a single vote for his opponent has effectively voted twice. At the same time, Virginia is known as a swing state where statewide (and federal) races are sometimes decided with very small margins, making felon disenfranchisement a potentially valuable tool in shaping outcomes statewide or even nationally—and the possibility that felon

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enfranchisement in Virginia might tip the scale in the 2016 presidential election was explicitly raised post-announcement (Horwitz and Portnoy, 2016).

To disentangle these motives, we begin with the hypothesis that prosecutors wishing to affect the outcome of an election might attempt to front-load disposition dates for unaligned defendants before the election, but hold off on sentencing aligned defendants until after it. This would generate a post-election decrease in the number of daily case dispositions for unaligned defendants, but an increase for aligned defendants. Prosecutors concerned only about their own electoral fortunes should arguably engage in such behavior only around their own elections, while prosecutors more broadly interested in electoral outcomes might be expected to manipulate disposition dates around every election.[29]

To explore this hypothesis, we plotted a simple district-weighted count of felony cases decided for 50 days on either side of each federal (even-year) or state (odd-year) election in Virginia between 2012 and 2019, and separately fitted linear predictions and kernel-weighted polynomial curves to either side of the election date. The results are presented in the Online Appendix (Figures A9 and A10). We find little evidence for an effect in state election years, but in three of four federal elections we observe higher numbers of unaligned cases decided prior to the election, followed by a substantial post-election drop.

If prosecutors engage in strategic disenfranchisement to further partisan goals, more politically engaged prosecutors may be more enthusiastic manipulators of disenfranchisement. Because Commonwealth's Attorneys are elected, they all arguably meet baseline levels of political engagement. To determine whether prosecutors exhibited varying levels of concern for political outcomes besides their own electoral contests, we collected their political donation behavior, matching them by first and last name (and middle initial) to Virginia donors listed in the DIME (Bonica, 2014, 2016). DIME contains biennial information on individual contributions to local, state, and federal elections from 1980 to 2022 as well as common-space ideal-point scores ("cf scores") for donors. We successfully obtained 95 prosecutor-DIME donor matches, nearly all of which we validated against occupation ("prosecutor," "Commonwealth's Attorney") employer information ("County"), and/or addresses. We cross-checked unmatched prosecutors against the OpenSecrets database and the Virginia Public Access ProjectVAData (VPAP) records to confirm that they had indeed made no (recorded) contributions.

We begin by testing whether political donor prosecutors were more responsive to the announcement than non-donors, and whether more recent donors were more responsive than less recent donors. Accordingly, we use DIME's biennial donation information to construct a set of measures of donation recency where the largest measure includes all donations within the entire pre-announcement interval for which we have DIME data (1980–2016), and the smallest includes only donations made between 2014 and 2016. We then separately estimate the announcement's effect on prosecutors who did/did not donate within each interval. Figure 8 shows the district- and covariate- adjusted effect of the announcement on charging and sentencing behavior among prosecutors who did (top row) and did not (bottom row) donate within each interval. As the figure shows, donors (especially more recent donors) appear to respond consistently to the announcement, while non-donors display little change in behavior overall.

Other possible proxies for political engagement are ideological extremism (more extreme prosecutors may care more about electoral outcomes) and donation size (more generous donors may care more about outcomes). Using the DIME dataset, we construct measures of both. To measure ideology, we use common-space DIME scores (which exist only for donors), and label prosecutors as "extreme"

[29]Theoretically, prosecutors might also set sentences to end just before or after elections. This is impossible to test because a defendant's release date depends on how much time he or she spent incarcerated before sentencing, which we do not know.

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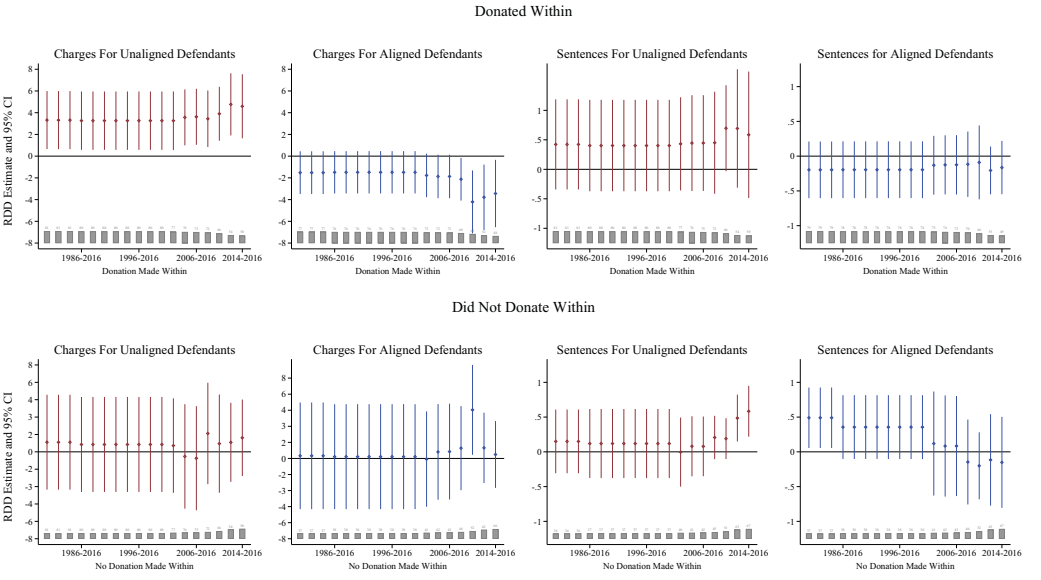


Figure 8. Announcement effect by recency of last donation.
Notes: Point estimates and 95% robust confidence intervals. All estimates adjusted for district fixed effects, crime type, and gender. Estimates unadjusted for crime time and gender are in Appendix Table A6. Clusters vary across specifications and are depicted by gray histograms at the bottom of each figure.

if their cf-scores exceed the median among prosecutors on "their" side of 0.[30] An ideal measure of donation size would adjust for characteristics like wealth and age to avoid conflating these characteristics with political involvement. Unfortunately, we lack information on these characteristics. As a first cut, we used news reports, professional networking sites, and historical election records to hand-code prosecutors' years of tenure as Commonwealth's Attorneys, calculated prosecutors' average annual donations during their time in office, and then treated prosecutors as large donors if their average yearly gifts while in office exceed the median.

The results of this exercise are presented in the Online Appendix (Figure A7). We find no consistent difference in the behavior of "extreme" as opposed to "moderate" prosecutors. However, we do find some differences in the post-announcement behavior of larger, as opposed to smaller, donors: overall, individuals who gave larger average amounts during their tenure as Commonwealth's Attorneys appear to have responded somewhat more strongly to the announcement.

While partisan feeling can arguably be proxied for through measures of political engagement, it is difficult to measure the strength of a prosecutor's personal desire for reelection. However, it seems plausible that politically vulnerable, office-seeking prosecutors might be especially prone to voter pool manipulation. This possibility is difficult to test rigorously, because political vulnerability is endogenous to political aptitude. Nevertheless, as a first cut, 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. We show in the Online Appendix (Figure A8) that both groups engaged in similar charging and sentencing behavior post-announcement.[31]

[30] Thus, prosecutors to the left of 0 were labeled extreme if their scores exceeded the median among all prosecutors to the left of 0.

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

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6. 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 use the tools of their office to shape the voter pool and improve the chance of a favored electoral outcome.

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 toward themselves (Eubank and Fresh, 2022; Helmke *et al.*, 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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