

Tipping the Scales?

Testing for Political Influence on Public Corruption Prosecutions

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Abstract

Political ties and the need to cultivate support for nominations to higher office create a conflict of interest for U.S. attorneys and the prosecutors they supervise in political corruption cases. How severe is this problem? We document partisan differences in the *timing* of public corruption case filings. Opposition defendants are more likely to be charged immediately before an election than afterward (relative to the president's co-partisans). We find a corresponding difference in case duration, suggesting prosecutors move more quickly to file cases against opposition partisans. These timing differences, which we attribute to the career incentives facing prosecutors, are associated with greater promotion rates to appointed office. However, prosecutors do not appear to bring weaker cases against opposition party defendants before elections. We instead find evidence that co-partisans received *less* favorable treatment from prosecutors in plea bargaining and sentencing. These partisan disparities in case outcomes disappear following the Supreme Court's decision in *U.S. v. Booker*.

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A scandal broke out in 2006 when it was revealed that the Bush administration had sought to replace nine U.S. attorneys. Some of these officials, who serve as the chief federal prosecutors in each judicial district, had investigated Republicans for corruption or declined to bring corruption or voter fraud cases against Democrats (Johnston 2007), leading to allegations that the requests were politically motivated. An internal Department of Justice investigation concurred, finding that “political partisan considerations were an important factor in the removal of several of the U.S. attorneys” (U.S. Department of Justice 2008). More recently, concerns about impartiality forced Attorney Generals Lynch and Sessions to recuse themselves from investigations into Senator Clinton and President Trump, respectively.

Do political pressures like these influence prosecutors or other unelected officials? Despite rules and procedures intended to ensure that they execute their powers faithfully, bureaucratic officials often have substantial discretion to advance their own interests or biases (e.g., Miller 2005; Gailmard 2009). The potential for bias is an especially serious concern for prosecutors, who have wide latitude over the timing and content of charges and their resolution (Kessler and Piehl 1998; Rehavi and Starr 2014; Starr and Rehavi 2013).

Concerns about political influence on prosecutorial conduct are particularly acute at the federal level given the ambitions for ascending to higher appointed or elected offices harbored by many federal prosecutors. Their prospects for career advancement depend on both their legal reputations and their relationships with political elites in their party — two factors that may come into conflict in public corruption cases involving partisan defendants, especially around elections when elites perceive the stakes to be especially high. As the Department of Justice itself notes (2013), a public corruption case “always has the potential of becoming a high-profile case simply because its focus is on the conduct of a public official. In addition, these cases are often politically sensitive because their ultimate targets tend to be politicians or government officials appointed by politicians.” Therefore, like Gordon (2009) and Alt and Lassen (2014), we focus our analysis on public corruption cases, which are most likely to attract political attention and thus to generate partisan pressure on prosecutors.

The key challenge in any empirical test for prosecutorial bias is that the researcher only

observes the cases prosecutors *choose* to pursue. The true underlying amount of corrupt conduct and even the unpursued cases brought to the prosecutor’s attention are unobserved. In addition, exogenous measures of the severity of the alleged conduct and the strength of the case are not available. The researcher only observes cases and their attributes (e.g., sentences) that are themselves the product of discretionary prosecutorial decisions.

We employ two novel empirical strategies to overcome these challenges. First, we focus on a much earlier prosecutorial decision in each case — the *timing* of the filing of charges, an area of prosecutorial discretion that has not been previously studied despite numerous allegations of political influence (e.g., Belser 1999; Kornacki 2011; U.S. Department of Justice 2008; Yalof 2012).¹ The incentives to shift the timing of cases involving partisans from within the administration and the party in power are likely to be especially strong before elections, a period of great concern for risk-averse political elites. Concerns about or hope for the political return to the announcement of charges should, however, fall sharply on Election Day. We therefore employ regression discontinuity-style tests for partisan differences in the timing of corruption prosecutions filed immediately before and after elections from 1993 to 2008, a novel test of partisan influence that is less vulnerable to concerns about confounds like those described above.

Our results indicate that cases against defendants associated with the opposition party are more likely to be filed *before* elections rather than afterward (relative to co-partisans of the president). This discontinuity in case timing around elections corresponds to a discontinuity in the time elapsed before charges are filed for opposition defendants, who are charged more rapidly before elections than after. Neither effect is observed in falsification tests. We also show that the reward structure facing U.S. attorneys appears to encourage this behavior. Assistant U.S. attorneys who file relatively more pre-election cases against opposition defendants than co-partisans are more likely to go on to serve as U.S. attorneys. A similar

¹For instance, as we note below, the former U.S. attorney David Iglesias alleged that two Republican members of Congress pressured him to file corruption charges against a Democrat before the 2006 elections (U.S. Department of Justice 2008, 53).

correspondence is observed among U.S. attorneys elevated to the federal judiciary.

The differences we find in case timing might seem to suggest that partisan bias would also affect the type and strength of the cases that prosecutors file, but we do not find evidence of such an effect using detailed evidence on case processing and resolution. Co-partisan defendants *do* face longer sentences than opposition party defendants, which might seem to suggest that the threshold for charging them is higher per Gordon (2009). However, we show that opposition party defendants are charged with and convicted of crimes of comparable severity to co-partisans. We instead find that prosecutors are *less* likely to recommend downward sentencing departures for co-partisans, which may reflect fears that such standard concessions will create the appearance of favoritism. Consistent with being offered less attractive deals, co-partisans choose to accept plea agreements at lower rates — the opposite of what we would expect if prosecutors were treating them more favorably.

To verify that these partisan sentencing differences do not reflect underlying differences in criminal conduct and case strength, we also exploit the increase in judges' sentencing discretion created by the Supreme Court's decision in *U.S. vs Booker*. *Booker* immediately increased judges' sentencing discretion, allowing them to unilaterally deviate from federal guidelines, which reduced prosecutors' influence over sentencing. If cases against co-partisans were objectively stronger or the underlying crimes more severe, then the partisan sentencing gap should be unaffected by *Booker* — judges should choose to continue to give co-partisans higher sentences after *Booker*. However, if the cases were comparable between parties, then we would expect the sentencing gap to disappear with judges' increased sentencing discretion. Consistent with the latter account, we find that the partisan gap in plea bargaining and sentencing disappeared following *Booker*.

Ultimately, our results underscore the profound challenge of effectively insulating the decisions of appointed officials and the civil servants who work for them from political influence in a highly polarized democracy. Previous research shows that political and electoral considerations can creep into essential functions of government ranging from disaster relief to contracts and spending (e.g., Gordon 2011; Reeves 2011; Kriner and Reeves 2015). Our

findings suggest that the career incentives that partisanship creates can even influence the behavior of appointed and career officials in seemingly less politicized domains like the law.

Political corruption prosecutions: Theory and context

The risk of partisan disparities in prosecutions

U.S. attorneys are the chief federal prosecutors in the 94 geographically-defined federal judicial districts. Each of these districts has a U.S. attorney who is appointed by the president and confirmed by the Senate to represent the federal government's interests in that district. These duties include overseeing federal criminal prosecutions and implementing the policies and priorities of the Department of Justice. Each U.S. attorney oversees a staff of career prosecutors, assistant U.S. attorneys, who help her carry out these tasks.

Though the president and Department of Justice (DOJ) do seek to exert some control over U.S. attorneys (e.g., Beale 2009), substantial flexibility remains.² Directly monitoring prosecutors and constraining their use of this discretion is difficult given ambiguities in the law and the subjective nature of the decisions that prosecutors must make. The scope for discretion in the federal corruption prosecutions we focus on thus appears to be substantial. The cases U.S. attorneys handle typically begin with a referral from an investigating agency (most frequently, the F.B.I.). U.S. attorneys and their staff must then choose whether to pursue the case and seek an indictment of the target. If a prosecutor chooses to proceed, she then exercises substantial discretion over the number and severity of the charges filed, the timing of those charges, and the resolution of the case, which includes the terms of plea agreements and any associated sentencing recommendations.

There is particular reason for concern about partisan disparities in corruption cases concerning prominent members of the two major political parties. U.S. attorneys are both of-

²According to DOJ, "Each United States Attorney exercises wide discretion in the use of his/her resources to further the priorities of the local jurisdictions and needs of their communities" (U.S. Department of Justice 2012).

ficers of the court and political appointees and thus face an inherent conflict of interest in these cases (Beale 2009; Eisenstein 1978; Perry 1998). Partisan factors could affect prosecutorial decisions either consciously or unconsciously. First, it is likely that presidents nominate U.S. attorneys who share their political views. These prosecutors may be instinctively sympathetic toward partisan allies or antagonistic toward political foes. U.S. attorneys also have strong career incentives to cultivate support among party elites within and outside the administration they serve (while also maintaining or improving their status in the legal community). They are typically nominated by the president as a result of support from elected officials and other political allies and rely on those allies to maintain favored status (Eisenstein 1978, 115). Many federal prosecutors also hope to obtain positions as judges and elected officials. Boylan (2005) found that nearly 30% of the U.S. attorneys he tracked immediately moved to another appointed or elected public position following their service as U.S. attorneys.³

Prosecutors with ambitions for appointed or elected office will likely need support from party elites and activists (e.g., Dominguez 2011; Rottinghaus and Nicholson 2010). Such support could be endangered if they are seen as damaging the interests of the party⁴ — a genuine possibility in the case of corruption charges filed near an election. While direct attempts to influence specific cases appear to be relatively rare, prosecutors are likely to anticipate the reactions of their allies to a case, to be responsive to signals from those allies, and to be more open to influence from and consultation with supporters (Eisenstein 1978,

³In Boylan's sample of 570 US attorneys who served between 1969 and 2000, "9.12% became federal judges immediately...9.47% took another appointed position in the federal government; 7.9% became state judges or took an appointed state or local government position...1.93% took elected office" (Boylan 2005, 383)

⁴Eisenstein (1978, 199–200), for example, writes of a U.S. attorney who received a call "from an important individual in his political party's state organization" saying "that if an indictment was returned against a major political figure, he would never realize his ambition to become a federal judge."

201–206).

These forms of influence are likely to center on *perceptions* of potentially damaging consequences of a prosecutor’s actions for the party in power. The overwhelming majority of defendants are not themselves on the ballot and are likely less concerned about the exact timing of the charges than the outcome of the case. The timing of charges is likely of greater concern to the party in power, which we expect to be highly attuned to upcoming elections and to seek every advantage in trying to acquire and maintain power. Elections are likely to be perceived as consequential by party elites in the state for both presidential elections and midterm/off-year state elections (where there may be more of a focus on Congressional or state campaigns). Conditional on charges being filed, elites from the party in power would presumably prefer to have opposition party members face corruption charges in the period before an election and for co-partisans to be charged afterward. Politicians’ fears that pre-election scandals will affect the outcome may have merit. There is some evidence to suggest that charges filed against political allies (union supporters) do indeed influence election results for the elected officials whom they support (Downey 2016).⁵

These political incentives can affect far more cases than U.S. attorneys could possibly handle directly. First, U.S. attorneys could select politically sympathetic or ambitious assistant U.S. attorneys to handle partisan public corruption cases. In addition, the incentives facing U.S. attorneys could also affect staff lawyers who simply wish to please their supervisors and avoid any risk of increased scrutiny of their work or negative performance reviews, which could harm their career prospects even if they plan to move into private practice or remain at DOJ. Finally, some assistant U.S. attorneys have political ambitions — for instance, approximately one in five federal judges and half the U.S. attorneys confirmed during the Clinton and Bush 43 presidencies previously served as assistant U.S. attorneys (details available upon request). Prosecutors who hope to receive political appointments might therefore

⁵Corruption prosecutions need not change election outcomes for our theory to be valid. It is only necessary that the election context increases the salience of corruption prosecutions to party elites and thereby affects the career incentives facing prosecutors.

cater to the preferences of potential political allies in making case timing decisions.

Partisan disparities in prosecutions: The limiting role of scrutiny

We can think of prosecutors as making tradeoffs on the margin between political capital within their party (herein partisan capital) and their legal reputations (herein reputational capital). Some actions, such as high-profile prosecutions of terrorists, provide the opportunity to build both types of capital. Prosecutors should also be eager to take actions that build one type of capital at no cost to the other (e.g., private signals of party loyalty). In public corruption cases with partisan defendants, however, prosecutors face difficult tradeoffs between the two types of capital. Even the appearance of playing politics with criminal cases could destroy a prosecutor's reputational capital. Scrutiny is likely to be highest — and the reputational risk thus most acute — for publicly observable outcomes that can be assessed in absolute terms for a given defendant. A U.S. attorney could find it very costly both politically and professionally to ignore a strong case referral (which could generate leaks to the media from law enforcement or career prosecutors), to engage in a meritless prosecution of a political opponent (which can generate unfavorable case outcomes or even reprimands), or to give a sweetheart plea deal to a co-partisan (which could damage their standing in the legal community and their viability as a political appointee).

Given these career concerns, we would expect prosecutors to seek to maintain or build their partisan capital in the aspects of corruption cases where the risk to their reputational capital is minimized. Partisan influence is thus most likely for actions that are not fully observable such as decisions about case timing around elections. Prosecutors are relatively autonomous in managing the timing of cases and have substantial flexibility. On average prosecutors have each public corruption case in our sample for nearly a year before initial charges are filed. Slowing down or speeding up a decision about a pending case may not be costly to a busy prosecutor with a large caseload. Even when cases are filed, critics must claim that the defendants would have been treated differently if they were members of the other party — a counterfactual that is impossible to assess in specific cases and is likely

unpersuasive when made by criminal defendants (see, e.g., Conte 2012).

Anecdotal evidence is consistent with the hypothesis that partisan pressure is exerted over and may affect the timing of political corruption cases. David Iglesias, a former U.S. attorney for New Mexico, alleged that two Republican members of Congress pressured him to file corruption charges against a Democrat before the November 2006 elections (U.S. Department of Justice 2008, 53). Similarly, allies of Senator Bob Menendez, a Democrat, accused then-U.S. attorney Chris Christie (a Republican who was subsequently elected governor) of pursuing a politically-motivated ethics investigation against Menendez during his 2006 election campaign (Kornacki 2011). Such allegations are not new; charges of partisan bias or political influence on the timing of prosecutions are frequently made in public corruption cases (e.g., Belser 1999; Murphy 2007; Schultze 2013; Trahan 2009; Yalof 2012).

Another potential mechanism for prosecutorial disparities is the use of differing standards for pursuing or resolving cases against co-partisans and opponents, which could result in sentencing differences by party (Gordon 2009). Conditional on filing, however, the content of cases filed and the case resolution process are far more vulnerable to external scrutiny. Most federal cases are resolved by negotiated plea agreements that result in reduced sentences. Such cases raise obvious concerns for prosecutors about appearing to offer favorable deals. These concerns could be especially acute for politically sensitive cases involving co-partisans. Moreover, once an individual is a convicted criminal, the political incentives reverse — party elites have an incentive to distance themselves and the party from the defendant to the extent possible. Our expectation is therefore that U.S. attorneys will be more sensitive to the appearance of partisan bias in these matters and may even treat co-partisans *more* harshly than the opposition. Below we test below whether prosecutorial case resolution practices differed by party and how those changed after 2005's *U.S. v. Booker* decision increased judges' sentencing discretion.

Existing evidence of political bias in prosecution

Previous studies have investigated responsiveness to political incentives in the executive branch (e.g., Wood and Waterman 1991; Scholz and Wood 1998) and among judges (Huber and Gordon 2004; Canes-Wrone, Clark, and Park 2012). Yet, relatively few empirical studies have investigated how politics affects prosecutorial behavior. There is some evidence that elected prosecutors prosecute cases more aggressively and make more reversible errors when up for re-election (McCannon 2013; Bandyopadhyay and McCannon 2014). However, relatively little is known about how political factors influence appointed and career federal prosecutors (e.g., Whitford 2002 and Whitford and Yates 2003; see Gordon and Huber 2009 for a review). Moreover, those studies that have examined the potential for partisan influence among prosecutors are typically correlational and do not provide causal evidence of bias (e.g., Meier and Holbrook 1992; Shields and Cragan 2007).

A handful of studies have used rigorous econometric strategies to examine the systematic influence of partisan politics on federal prosecutors' behavior. First, both Alt and Lassen (2014) and Gordon (2009) make important contributions to the study of federal corruption prosecutions but also face significant inferential limitations. For instance, Alt and Lassen (2014, 333) find that recent administrations invested more effort in prosecuting corruption within areas that favored the other party, but cannot disentangle prosecutorial priorities and the geographic distribution of the crimes in question. Likewise, Gordon (2009) finds that sentences in public corruption cases tended to be relatively shorter for opposition-party defendants than same-party defendants, which he interprets as evidence of taste-based discrimination. Unfortunately, final sentences are not an exogenous objective measure of criminal severity or case strength. They are the culmination of multiple decisions throughout a case, many of which are made by prosecutors. This partisan sentencing gap could thus be the result of other factors such as differences in case resolution and sentencing rules. As we show below, though the severity of initial charges is comparable, prosecutors appear to offer co-partisans *less* favorable plea bargains to avoid the appearance of favoritism. Consistent with

this interpretation, we find that the sentencing disparity disappears as judges' sentencing discretion increases.

While not focusing on public corruption specifically, Downey (2016) exploits the exogenous variation in political control arising from close elections. He finds that union leaders are significantly more likely to be indicted by the Department of Labor's Office of Labor-Management Standards following the close loss of a Congressional candidate whom they endorsed.

Data

The universe of cases filed by federal prosecutors was obtained from DOJ under the Freedom of Information Act (FOIA).⁶ We extracted all cases classified as targeting state and local public corruption that were filed by U.S. attorneys in the fifty states between February 1993 and December 2008.⁷ Each case includes detailed information such as the date the case was first received by DOJ, the history of all charges filed, key case processing dates, and the ultimate resolution of each charge. These detailed administrative data are vastly superior to the reports from the DOJ's Public Integrity Section that have been used in past studies. Those reports rely on retrospective surveys, lack detailed case-level information, and have other quality problems (Cordis and Milyo 2016).

Defendant names are not included in the DOJ data released under FOIA. We therefore searched electronic federal court records from the relevant judicial district to identify the

⁶Though comprehensive statistics are not available, the federal government conducts most anti-corruption prosecutions (Maass 1987). We therefore follow previous studies (e.g., Gordon 2009; Alt and Lassen 2014) in focusing on federal corruption cases. Prosecutions of political figures for crimes unrelated to their public office or its resources (e.g., driving under the influence or solicitation) do not fall under DOJ's public corruption classification.

⁷Public corruption cases brought against federal officials were excluded because the federal executive branch is affiliated with the President's party and therefore generally does not include opposition party defendants.

defendants in question. Defendants were identified using case characteristics including the filing date, sentence, sentence date, and charges filed. We then used news coverage and other public information to determine whether the defendants were publicly identified as members of the Democratic or Republican parties (e.g., an elected official or staff member) or were prominently associated with well-known partisans in state or local politics (e.g., a subordinate, family member, co-defendant, etc.).⁸ It is these defendants whom we refer to as co-partisans or opposition party defendants below depending on whether they share the partisanship of the presidential administration. (Further details on the construction of these variables and our coding procedures are provided in Online Appendix A.)

In total, we identified 1931 of the 2544 qualifying defendants (76%) spread across 1177 cases (out of 1336 total). After defendants were identified, we coded their partisanship following the procedure described above (352 were Democrats and 137 as Republicans).⁹ Among the 1931 identified defendants, 489 defendants (25% of those identified) from 286 cases were publicly identified with one of the major parties either individually (152), as an associate of a publicly-identified partisan (314), or as both an individual and as an associate of a partisan (23). Many were part of local government (39%) while another 19% were part of the state or federal government (non-military). These are a mix of elected officials and unelected figures such as political staff and public sector officials. Other partisan defendants were members of the private sector (34%), family members or personal associates of political figures (3%), or could not be coded (5%).

For each case filing date, we calculated the number of weeks until or since the nearest

⁸We do *not* account for party registration. Individuals were only coded as partisans if they were publicly identified as such in news accounts or public documents.

⁹In a validation check, we found that our data include 99% of the partisan defendants identified by Gordon (2009) (see Online Appendix A for further details).

gubernatorial, state House, state Senate, or federal general election in each state.¹⁰ Despite the informal norm against charging political cases soon before an election, there are a considerable number of politically relevant filings in that period. Among the partisan public corruption cases in our data, nearly two-thirds of those filed within 24 weeks of an election were filed *before* the election. The median number of these prosecutions per electoral cycle month is 19, but the distribution varies over the electoral cycle with a noticeable peak immediately before elections (see Online Appendix A).

Elections are typically held on Tuesdays and cases are generally filed on weekdays, creating systematic “holes” with no cases filed in a day-level electoral distance measure. We therefore round our electoral distance variable to the nearest complete week and use that as the running variable in the regression discontinuity-style models reported below.

The DOJ data also include the date the case was received and the date on which it was filed, which enables us to directly measure the time elapsed before charges were filed.¹¹ The distribution of time to case filing has a significant peak at 0 (21%) for cases filed immediately or before the case was received (i.e., a pre-arrest indictment) and a long right tail (the median is 21 weeks and the mean is 45.6 weeks; see Online Appendix A for the full distribution).

In addition, we tabulated the charges and counts against each defendant that were filed and sustained and calculated the severity of the charges at both stages using the approach employed by Rehavi and Starr (2014). Finally, we examined case resolution, including sentencing (months of incarceration), whether a plea agreement was reached, and whether the government requested a favorable departure from sentencing guidelines.

¹⁰We focus on state and federal general elections held in November (including state elections held in odd-numbered years in applicable states) because those are most consequential to party elites and thus most relevant to the incentives facing federal prosecutors. However, future research should consider examine local and primary elections as well.

¹¹The “received date” is the date that the case first appeared in the DOJ computer system. It is not necessarily the date on which DOJ chose to accept the case and could be well before any such determination was made.

Table 1 summarizes the charge severity, case resolution, and time to case filing variables for identified defendants in the data we analyze. We find that prosecutors take much longer to file cases against co-partisan defendants — the median time to file charges is 15 weeks for opposition partisans versus 45 weeks for co-partisans ($p < .01$). Opposition defendants are also charged with 51% more counts but plead guilty to only 22% more counts. The two groups otherwise appear relatively comparable across a number of measures, including the severity of the initial charges they face and the probability of a conviction. It therefore does not appear that prosecutors are prosecuting opposition defendants for more minor crimes or filing unsustainable charges against them.

[Table 1 about here.]

Finally, in order to study how partisan case timing affects the career trajectories of prosecutors, we identified all assistant U.S. attorneys (AUSAs) who represented the federal government in the public corruption cases in our sample for which a defendant could be identified. For each prosecutor, we computed the relative balance of cases filed pre-election versus post-election against prominent partisans within 24 weeks of a state or federal election. We then placed prosecutors into one of three categories: those who brought more cases against opposition party defendants pre- versus post-election than they did against co-partisans; those who brought relatively more pre-election cases against co-partisans; and those who had no partisan differential or did not bring any cases against partisans around elections. We then categorized every U.S. attorney from the Clinton and Bush 43 administrations using the same procedure. Finally, we identified the AUSAs who went on to serve as USAs and the USAs who later served as federal judges (see Online Appendix A for details).

Estimation and results

In the analyses below, we first test for shifts in case timing around elections using the McCrary (2008) density test, which allow us to test whether the density of case filing dates is continuous at Election Day for both opposition and co-partisan defendants. We then test for

differences in case timing between parties around elections using event study and a regression discontinuity-style estimation approach (RD), which allow us to test if the probability of opposition partisans being charged with public corruption is higher immediately before elections than afterward. Next, we assess the mechanism for this effect using difference-in-differences models of the time elapsed between when a case is received and charged, which estimate how average case duration varies by party around elections. To establish that the case timing differentials we observe are not spurious, we subsequently perform falsification tests for both non-partisan defendants and dummy elections in non-election years. We then show that U.S. attorneys and assistant U.S. attorneys who bring relatively more pre-election cases against opposition defendants are more likely to receive political appointments to higher office (to be a federal judge or U.S. attorney, respectively), which is consistent with our theory of the career incentives facing prosecutors. Finally, we test whether case outcomes vary by party, using difference-in-differences models to estimate how those outcomes vary by party around elections and how this differential changed after *U.S. v. Booker*.

Partisan disparities in case timing and duration

We begin our analysis by testing for partisan differences in the timing of charge filings relative to Election Day (conditional on charges being filed). We first examine whether the timing of corruption case filings varies around elections depending on the defendant's party affiliation. Timing should not be confounded with case selection, the severity of the crime, plea bargaining strategies, or changes in criminal sentencing law. The prevalence of corruption by party and the number of cases that could be filed against either party in any given time period are, of course, unobserved. The RD estimate represents the causal effect of the change in political incentives on prosecutors' filing decisions if the partisan mix of cases that are ready to be filed varies smoothly over time and does not discontinuously change on Election Day (absent strategic timing of charges). This is the identifying assumption in a standard RD design (see, e.g., Imbens and Lemieux 2008). If it holds, any changes in the availability of partisan corruption cases resulting from events in the world or unobserved

case selection processes will be filtered out by the estimator, which identifies the discontinuous change in the relative probability of filing charges by party at Election Day.

The most natural approach to evaluating manipulation of case timing dates around elections is the McCrary (2008) density test, which we use to evaluate whether the density of case filings changes around elections for either opposition party defendants or those associated with the president's party.¹² The results, which are plotted in Figure 1, show that the density of case filings declines significantly after Election Day for opposition defendants (log difference in height $\theta = -1.66$, s.e. = 0.73; $p < .05$) but not same-party defendants ($\theta = 0.01$ s.e. = 0.67).

[Figure 1 about here.]

The discontinuity around Election Day suggests a shift in the distribution of cases for opposition party defendants to the weeks immediately preceding elections. This finding is consistent with the hypothesis that opposition defendants are more likely to be charged before elections than afterward.

Next, we compare the timing of case filings between parties, directly evaluating whether the probability of filing public corruption cases against opposition party defendants varies relative to administration party defendants around Election Day. We use both a simple OLS model and a regression discontinuity-style estimator that specifically tests for a discontinuous change at Election Day in the relative probability of corruption charges by defendant partisanship. We do not expect cases to be distributed randomly around the election. We instead identify our model by assuming that the arrival rate of credible potential cases varies smoothly around elections. If it does, our RD models will provide valid estimates of the discontinuous change at Election Day in the probability that opposition party defendants will be charged with public corruption relative to co-partisan defendants (conditional on being charged in the period around an election).

We first directly estimate the magnitude of the partisan difference in case timing around

¹²See Online Appendix B for further discussion of this test and how we employ it here.

elections for all partisan public corruption defendants charged in a relatively narrow window around elections of 12–24 weeks. Table 2 presents event study estimates from linear probability models with a simple indicator variable for the post-election period.

[Table 2 about here.]

Relative to the period before the election (the omitted category), opposition party defendants were twelve to twenty percentage points less likely to be charged after an election compared with same-party defendants. For instance, 70% of cases against opposition defendants charged in the 24 weeks around an election were filed before the election in question compared with 55% of cases against co-partisans (Fisher’s exact test: $p < .05$). The estimates reported in Table 2 are stable and are statistically significant in all but one case (a window of 12 weeks around elections) and remain consistent when we account for resource differentials between offices (see Table B1 in Online Appendix B).

To more rigorously test for an election-specific partisan differential in case timing, we examine whether the likelihood that an opposition party defendant will be charged with public corruption varies discontinuously around elections. Specifically, we estimate the change in the probability of an opposition party defendant at Election Day among the partisan defendants who were charged within 24 weeks of an election using an RD-style approach, which assumes that the availability of partisan corruption cases that are ready to be filed should vary smoothly across the discontinuity in the electoral calendar at Election Day (and thus be absorbed by the flexible RD models we employ). If this assumption is met, it is unnecessary to control for covariates — the only role for additional controls would be to improve the precision of the estimates (Imbens and Lemieux 2008; Lee and Lemieux 2010).

Table 3 reports our RD-style estimates of the discontinuous change in the probability of an opposition defendant being charged at Election Day among those partisans charged within 24 weeks of the election. We estimate these models using the two predominant approaches in the literature (Imbens and Lemieux 2008; Lee and Lemieux 2010) — local linear regressions and regressions with flexible polynomials. In both sets of models, time (the “running” or “forcing” variable in RD terms) is measured in terms of weeks before or after the nearest

election in order to prevent weekends and holidays from creating holes in the density, which can otherwise create estimation problems. Due to the binary outcome variable, we specifically estimate logistic regression models that include third order polynomials in the distance from the election. These polynomials are estimated separately on each side of the discontinuity in order to absorb and filter out the effects of any relevant factors that vary smoothly over time. Table 3 reports the marginal effect of the post-election indicator at Election Day, which represents the discontinuous change in the probability of an opposition defendant being charged at that time (our primary quantity of interest). The local linear regressions use an even more flexible functional form to accomplish the same goal of filtering out smoothly varying changes in factors that affect the likelihood of opposition prosecutions, allowing us to again estimate the discontinuous change in the probability of an opposition prosecution among the set of partisans charged.¹³

[Table 3 about here.]

With one exception, the results in Table 3 consistently estimate a negative and statistically significant discontinuity at Election Day. Conditional on a partisan being charged with corruption near an election, the probability of an opposition party member or associate being charged before the election decreases dramatically after Election Day relative to a member of the president's party or an associate. The local linear regression results, which are more stable and less sensitive to the boundaries of our window around Election Day, provide point estimates of a decrease of approximately 50 percentage points (95% confidence interval using results from the model estimated with a 24-week window: -0.92, -0.08). Our results are virtually identical if we cluster the logistic regression results on criminal cases rather than election cycle weeks or use 200% of the optimal bandwidth for local linear regression to address possible overfitting of outliers near the discontinuity (see Table B2 in Online Appendix B).

¹³See Imbens and Kalyanaraman (e.g., 2012) for a more extensive exposition of local linear regression.

Figure 2 presents the graphical analogue of the flexible polynomial estimates in the first column of results in Table 3. It contains local polynomials with mean smoothing of the probability over time of charging an opposition party defendant rather than a co-partisan for all cases filed against partisans within 24 weeks of Election Day.

[Figure 2 about here.]

These estimates are consistent with those in Table 3 above—the figure provides graphical evidence of a substantial discontinuity. The probability of an opposition party defendant being charged with public corruption relative to a same-party defendant decreases dramatically after Election Day. Conversely, our data suggest that same-party defendants are more likely to be charged after Election Day than before relative to opposition party defendants.

If this discontinuity is the result of prosecutors manipulating case timing (rather than case referral), the elapsed time before charges are filed should vary by defendant party affiliation and the temporal distance from elections. We therefore calculate the interval between the date on which a case is recorded as received by a prosecutor and the date charges are filed. Table 4 therefore estimates the post-election change in average weeks to file charges for both opposition and same-party defendants. Given the relatively small number of defendants charged in these partisan subsamples, we estimate a simple difference-in-differences model. Our results, which we estimate using Poisson regression with robust standard errors due to the presence of immediate case filings (zeroes) and skewness in the dependent variable (a maximum of 345 weeks in the partisan defendant sample with a 24-week window around elections), indicate that time to file charges tends to be shorter for opposition defendants before elections than same-party defendants but increases dramatically for opposition party defendants charged after elections.¹⁴ The post-election shift for opposition defendants, which is estimated as a linear combination of coefficients, is positive and sta-

¹⁴We estimate these models using Poisson regression with robust standard errors because the standard negative binomial regression model is not consistent if the variance model is misspecified (Cameron and Trivedi 2010, 577).

tistically significant at $p < .01$ for windows of 12, 16, and 20 weeks around elections and $p < .05$ for a 24-week window. No evidence is found of an equivalent post-election shift among same-party defendants; we cannot reject the null hypothesis of no change in weeks to file after elections.

[Table 4 about here.]

Figure 3 illustrates this finding using local polynomials with mean smoothing of the average number of weeks elapsed before a case was filed among those cases filed against partisans within 24 weeks of Election Day.

[Figure 3 about here.]

We observe a substantial discontinuity around Election Day for opposition defendants. Among this group, cases filed immediately before elections were held for a much shorter period of time than those filed immediately after.¹⁵ This discrepancy suggests that cases were generally brought more quickly against opposition defendants in the period before elections. Pulling those cases forward would then inflate the average time to case filing among the remaining cases that were charged afterward.¹⁶

Partisan disparities in case timing: Threats to inference

We next consider potential threats to identification of the case timing results. One concern is that the partisan disparities in case filings could be the result of actions taken by law

¹⁵This finding is also consistent with Figure B1 in Online Appendix B, which shows that immediate case filings are significantly more common for opposition party defendants before elections versus after relative to same-party defendants.

¹⁶The decline in cases filed against opposition defendants after elections (Table 2) suggests that this discontinuity is the result of prosecutors accelerating the timing of cases charged before elections rather than bringing cases that would not otherwise have been filed. We find no measurable difference in conviction rates by election timing and partisanship, which is consistent with this interpretation (see Table B5 in Online Appendix B).

enforcement agencies rather than prosecutors. If these agencies provided prosecutors with stronger or more numerous cases against the opposition just prior to an election, we would expect both more pre-election filings and shorter times until filing even in the absence of prosecutorial bias. To address this concern, we re-estimate the case-timing results excluding the 86 partisan cases referred to DOJ within 12 weeks of the election. Though the estimates are in some cases less precise due to the reduction in statistical power, our point estimates are robust to this exclusion (see Tables B3 and B4 in Online Appendix B). This finding suggests that the partisan timing differential documented above is not driven by cases received by prosecutors in the months leading up to the election.

Elections can of course create the opportunity for election-related corruption. One might therefore be concerned instead that the case timing disparities documented above are the product of partisan differences in opportunities for election-related corruption. However, few individuals are actually charged in election-related corruption cases near the election they are trying to affect. These cases are typically filed long after the relevant election. Election-related corruption prosecutions are exceedingly rare in our data — we observe only three partisan defendants who were charged under statutes related to election crimes within 24 weeks of the nearest election.¹⁷

Election-related crimes are only one example of the more general concern that these findings are a spurious result of non-political factors that vary around the first Tuesday in November. We conduct two falsification tests to address these concerns. First, we test for a discontinuous break in the density of case filings of non-partisan defendants around elections, which could result if there were a more general election effect on case timing that also affects defendants who are not publicly associated with a major party. We also construct placebo election dates on the first Tuesday of November in off-years for partisan defendants charged with public corruption in the 45 states that hold state elections on the federal elec-

¹⁷See Online Appendix A for details on how these statutes were coded.

tion calendar¹⁸ and calculate the number of weeks to the closest placebo election for these defendants. If our results are a seasonal artifact of U.S. general elections being held on the first Tuesday in November, then we should observe a discontinuity in the density of opposition party case filings around that date in off-years as well. Neither test reveals a statistically significant discontinuity using the McCrary (2008) approach (non-partisan defendants: $\theta = -0.26$, s.e. = 0.19; opposition defendants around placebo elections: $\theta = 0.50$, s.e. = 0.50). Graphs of these falsification tests are provided in Figure B2 in Online Appendix B.

Career incentives

We have argued that prosecutors have strong career incentives to maximize their partisan capital in case timing choices. It is unfortunately not possible to directly identify the causal effect of partisan timing choices on career outcomes using the available data. However, observing such a correlation in practice is all that is required for this mechanism to be incentive-compatible for prosecutors. The *ex post* relationship between prosecutors' partisan case timing patterns and appointment rates to higher office is presumably observed by prosecutors as well. We examine two types of promotion: assistant U.S. attorneys' (career prosecutors) nominations and confirmations as U.S. attorneys (a politically appointed office) and U.S. attorneys' nominations and confirmations to the federal judiciary. Both relationships are important because U.S. attorneys supervise their offices and make key management decisions, but ostensibly non-partisan AUSAs try the vast majority of cases. As we argue above, partisan career incentives could affect *both* levels of prosecutors.

[Figure 4 about here.]

The results, which we present in Figure 4, are striking. Ten of the 94 assistant U.S. attorneys who brought relatively more cases against opposition partisans in the immediate pre-election period went on to serve as U.S. attorneys (10.6%) compared with only 26 of the

¹⁸The states that do not hold state elections on the federal calendar are Kentucky, Louisiana, Mississippi, New Jersey, and Virginia.

528 with a neutral record (4.9%) and 2 of the 64 who brought relatively more pre-election cases against co-partisans (3.1%). Likewise, six of the 37 U.S. attorneys who prosecuted relatively more opposition defendants in the immediate pre-election period were elevated to the federal bench (16.2%), while only 8 of 156 with a neutral record (5.1%) and 1 of 27 of those who filed relatively more co-partisan cases (3.7%) became federal judges.

[Table 5 about here.]

As Table 5 shows, these differences in promotion are statistically significant in simple linear probability models of political promotions. Assistant U.S. attorneys were more than three times as likely to go on to serve as U.S. attorneys if they brought relatively more pre-election cases against opposition defendants compared with the converse (an eight percentage point increase in the probability of promotion; $p < .06$). Similarly, U.S. attorneys who prosecuted relatively more opposition defendants in the immediate pre-election period were more than four times as likely to go on to serve as a federal judge than those who did the opposite (a 13 percentage point increase in promotion rates; $p = .08$).

Partisan disparities in case content

The partisan disparities in case timing that we observe raise questions about whether the *content* of cases also differs around elections. Are opposition defendants being targeted with weaker cases or minor offenses before elections? If so, we would expect opposition partisans charged immediately before elections to be less likely to be found guilty than those charged immediately afterward. However, we find no measurable change in the probability of conviction for partisan defendants of either party around elections (though the estimates are imprecise; see Table B5 in Online Appendix B).

The welfare implications of the apparent partisan influence on case timing that we observe are thus unclear. Are defendants actually being treated differently by prosecutors in other respects? The timing of charges is just one of many decisions that prosecutors make during a case. Political pressures and career concerns could also influence prosecutors' sub-

sequent decisions when prosecuting partisans. We therefore test for political influence on three aspects of the case resolution process: sentence length, sentencing recommendations, and the willingness of defendants to accept plea agreements.

Without an external measure of evidentiary support or data on cases that were not filed, we must rely on indirect tests for political influence in case outcomes and infer influence from changes in those outcomes. To that end, we leverage the Supreme Court’s 2005 decision in *U.S. v. Booker*, which decreased prosecutors’ influence over sentencing by lifting the requirement that federal judges follow the U.S. Sentencing Guidelines.

Specifically, we estimate regressions of the following form:

$$Y_{it} = \alpha_{it} + \beta_1 \text{Opp}_{it} + \beta_2 \text{Post}_t + \beta_3 \text{Opp}_{it} \times \text{Post}_t + \varepsilon_{it} \quad (1)$$

where Y_{it} is the case outcome for defendant i sentenced at time t , Opp_{it} is an indicator for whether the defendant was publicly affiliated with the party in opposition to the president’s administration, and Post_t is an indicator for whether the sentence was issued after the Supreme Court’s *Booker* decision.

First, we re-examine the Gordon (2009) finding that opposition party members have lower average sentences than members of the president’s party. This disparity is also present in our data for the period before *Booker* in 2005 ($\beta_1 > 0$). In theory, this disparity could be generated by a taste-based discrimination model (Becker 1957) in which prosecutors have a distaste for prosecuting co-partisans (or enjoy prosecuting opposition party defendants). However, if the gap were a reflection of true differences in crime severity and case strength arising from biased selection into prosecution, one would expect judges to continue to give opposition defendants lower sentences after *Booker* ($\beta_3 = 0$). Instead, the difference-in-differences models in columns 1 and 2 of Table 6 show that the partisan sentencing gap changed significantly after *Booker* ($\beta_3 > 0$). The gap among partisan defendants disappears entirely after the decision ($\beta_1 + \beta_3 = 0$) — a result that is robust to controlling for the proportion of co-partisan judges in the district and the administration in power (column 2).

This finding cannot be easily explained as an artifact of selection — the partisan differential also disappears among the set of cases that were sentenced after *Booker* but filed during the period before the decision (though we have less statistical power so the estimates are less precise; see Table B6 in Online Appendix B).¹⁹

[Table 6 about here.]

We therefore propose an alternative interpretation in which potentially damaging external scrutiny prevents opposition defendants from being treated more harshly during case resolution decisions by prosecutors. We show that pre-*Booker* differences in sentences between co-partisans can be explained by differences in case resolution. Co-partisans and opposition defendants were charged with and convicted of crimes of comparable severity — a pattern that does not change measurably around *Booker* (Table B8 in Online Appendix B). Instead, sentencing gaps appear to emerge because co-partisans received less favorable treatment in case resolutions. Specifically, prosecutors were more likely to recommend downward sentencing departures for opposition defendants (Table 6, columns 5-6) — perhaps due to fear that favorable sentencing recommendations for co-partisans would create the appearance of favoritism. Consistent with prosecutors offering less desirable terms and being unwilling to offer standard plea inducements, co-partisans were in turn less likely to accept plea agreements than opposition defendants prior to *Booker* (Table 6, columns 3-4). These case resolution results are the exact opposite of what we would expect if U.S. attorneys were treating opposition defendants more harshly or favoring members of their own party by offering them more desirable deals. Our results are thus inconsistent with a taste-based discrimination model.

¹⁹We also show in Table B7 of Online Appendix B that the result holds in the set of cases charged before the U.S. attorneys scandal began in December 2006 (a possible confound).

Conclusion

Federal prosecutors depend on party elites to support their nominations for higher appointed or elected office. How much do these incentives affect the way they handle corruption cases against partisans? Contrary to previous research (Gordon 2009), we find no evidence that U.S. attorneys and the career prosecutors they supervise bring weaker corruption cases against opposition partisans or favor co-partisan defendants in case resolutions. By contrast, we provide new evidence of partisan disparities in the *timing* of public corruption charges around elections that are favorable to the party in power. Our results indicate that opposition party defendants are more likely to face corruption charges immediately before elections than afterward. This differential in the timing of partisan case filings around elections is not observed for non-partisan defendants and is not the result of seasonal effects. We find instead that cases against opposition defendants — but not same-party defendants — are filed sooner after being received before elections compared to afterward, suggesting that prosecutors pursue cases more quickly when defendants do not share their partisanship.

We attribute these results to the competing incentives prosecutors face to enhance their standing within the party and to protect their professional reputations. Case outcomes are directly observable, creating a threat to prosecutors' standing in the legal community that appears to restrain the effects of partisan factors on how defendants are treated. By contrast, case timing decisions are not directly observable — cases are confidential unless and until they are filed, making it more difficult to assess when charges could have been brought in a counterfactual scenario. Under these circumstances, the perceived consequences of their decisions for allied partisans should have a more significant influence on the choices that prosecutors make. The career paths prosecutors observe are consistent with this account — U.S. attorneys and assistant U.S. attorneys with a record of filing relatively more cases against opposition defendants pre-election versus post-election compared to co-partisans are more likely to be appointed to higher office.

As with any research, this study of course has limitations that should be noted. First,

our findings of course depend on the validity of the assumptions of the research designs that we employ. Second, we cannot observe the cases that are not filed by prosecutors nor the underlying prevalence or severity of public corruption in any given time or place. A third limitation is sample size. Though we consider a much longer time period than previous studies, we are constrained by the limits of available public corruption data. Future studies should seek to test the assumptions we employ when possible; expand the universe of filed cases under consideration; identify other crimes or settings in which defendant partisanship might affect the timing of prosecutions; and measure other indicators of prosecutorial disparities like the timing or publicity given to announcements that charges will not be filed against prominent partisans.

Nonetheless, these results highlight the difficulty of containing partisan influence on the administration of government, which is likely to be a particularly significant concern during periods with high levels of polarization. Parties help organize political competition and ensure democratic accountability, but the incentives they create can distort the practice of government and the administration of justice in fundamental ways. Creating procedural safeguards and delegating authority to career public servants may not be enough; it is difficult to insulate the exercise of power from political influence using regulatory or enforcement approaches. A better approach might therefore concede the inevitability of political influence and instead restrict the scope or effects of that influence — for instance, by enforcing the informal norm against charging politically salient cases around elections. There may be no way to keep politics out of the prosecutor’s office, but cases are likely to be handled more equitably after the fervor of campaign season has subsided.

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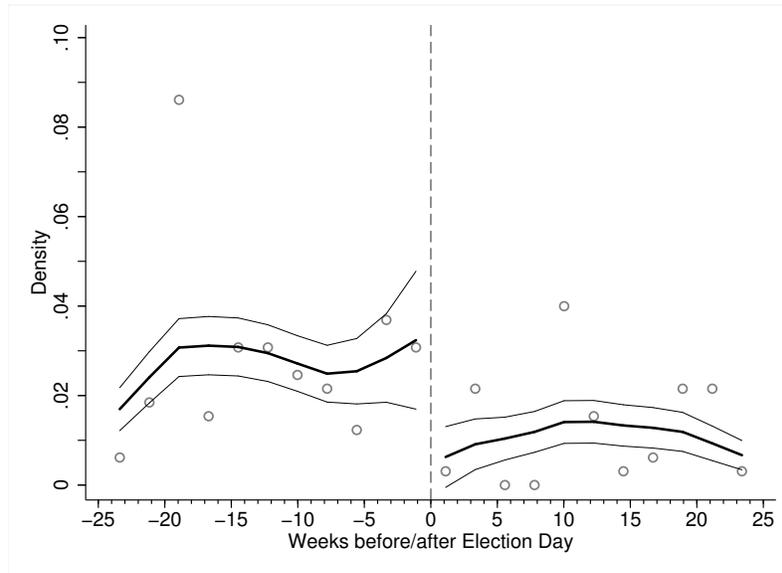
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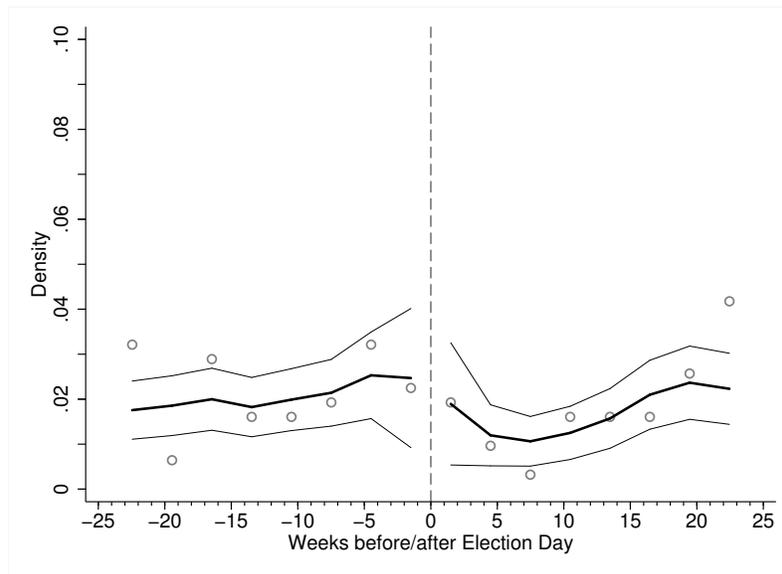
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Figure 1: Partisan differences in corruption case timing over the electoral cycle

(a) *Opposition party*

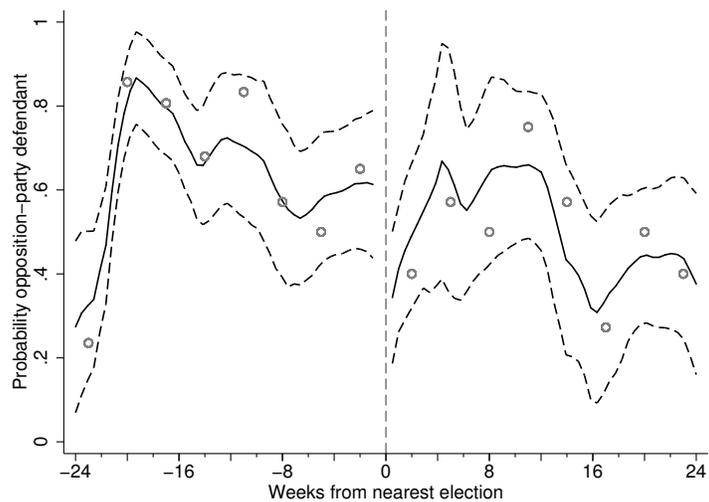


(b) *Same party*



Plots calculated using the McCrary (2008) density test in Stata with default bin size and bandwidth calculations; thick lines represent density estimates, while thin lines represent 95% confidence intervals.

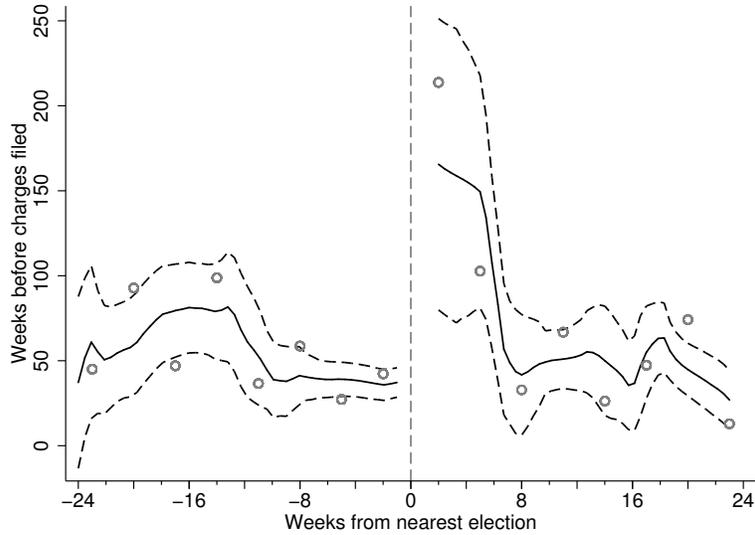
Figure 2: Opposition party prosecution probability over the election cycle



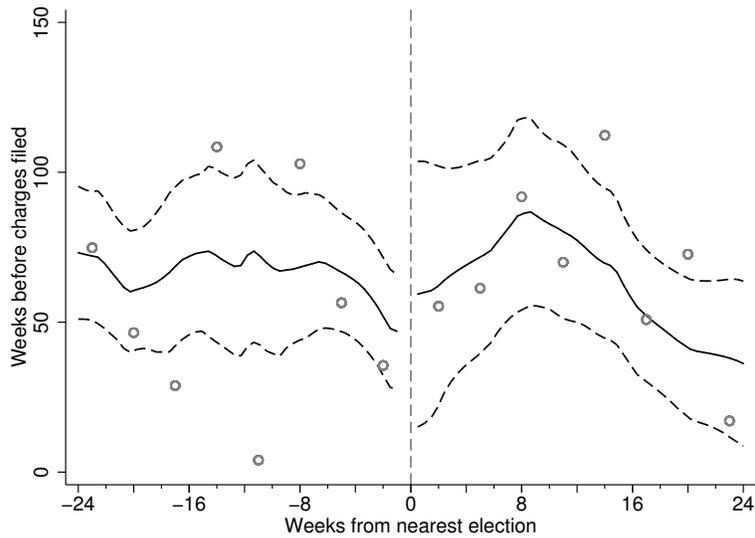
Local polynomial smoothing and 95% confidence intervals calculated using `lpolyci` in Stata (Epanechnikov kernel; rule-of-thumb bandwidth estimator). Bin means of the outcome variable are calculated over three-week intervals. Cases filed less than one week from Election Day are grouped with the intervals on the corresponding side of the discontinuity.

Figure 3: Time to case filing by party over the electoral cycle

(a) *Opposition party*



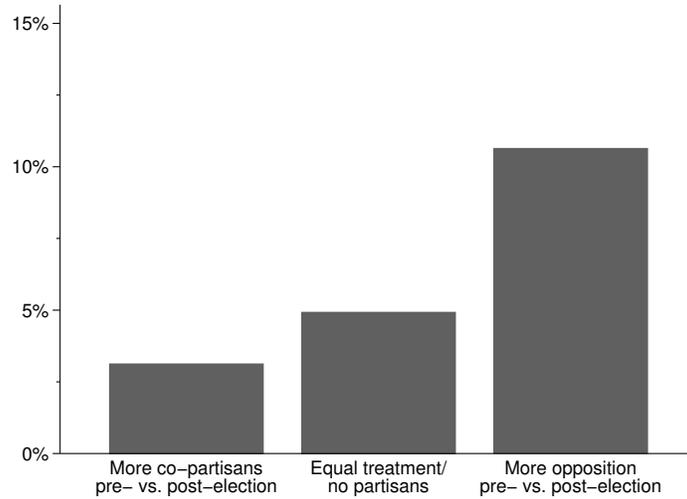
(b) *Same party*



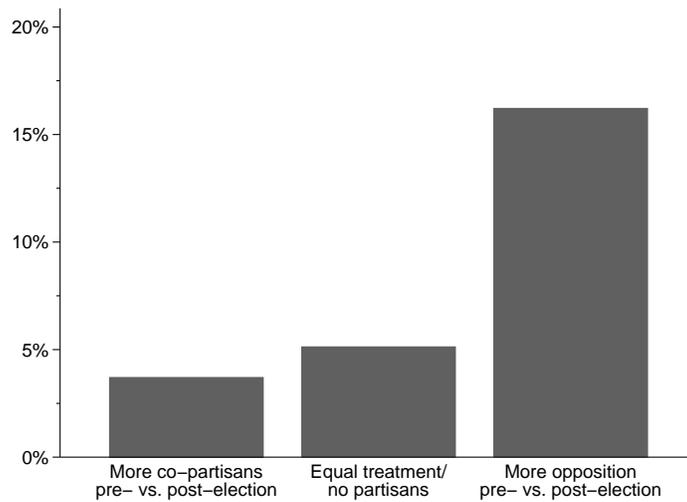
Local polynomial smoothing and 95% confidence intervals calculated using `lpolyci` in Stata (Epanechnikov kernel; rule-of-thumb bandwidth estimator). Bin means of the outcome variable are calculated over three-week intervals. Cases filed less than one week from Election Day are grouped with the intervals on the corresponding side of the discontinuity.

Figure 4: The role of career incentives in public corruption prosecutions

(a) Assistant U.S. attorney promotion rate to U.S. attorney



(b) U.S. attorney promotion rate to federal judge



Sample: Assistant U.S. attorneys (AUSAs) in federal criminal cases targeting state and local public corruption filed from February 1993 to December 2008 that were coded as national priorities and the supervising U.S. attorneys in those cases.

Table 1: Summary statistics

	Same party		Opposition party		Non-partisan	
	Mean	SD	Mean	SD	Mean	SD
Charge characteristics						
Number of distinct charges	2.430	[1.437]	2.436	[1.613]	1.920	[1.349]
Total counts	5.595	[7.776]	8.464	[15.28]	4.893	[16.57]
Statutory max: most serious charge (months)	159.1	[80.37]	163.8	[83.98]	160.8	[79.13]
Case resolution						
Guilty of any charge	0.895	[0.307]	0.820	[0.385]	0.796	[0.403]
Number distinct charges pled guilty	0.885	[0.903]	0.865	[0.927]	0.846	[0.873]
Number counts pled guilty	1.895	[3.844]	2.318	[5.239]	2.117	[9.032]
Statutory max: most serious plea (months)	171.2	[86.55]	168.1	[88.40]	148.3	[112.3]
Months of incarceration	20.29	[24.64]	17.84	[26.98]	18.96	[47.22]
Plea agreement	0.615	[0.488]	0.609	[0.489]	0.629	[0.483]
Sentencing departure	0.085	[0.280]	0.118	[0.323]	0.058	[0.235]
Timing						
Weeks from case received to filed	65.765	[79.02]	45.03	[64.77]	43.71	[58.50]
Number of defendants	200		289		2055	

Sample: All federal criminal cases targeting state and local public corruption filed by U.S. attorneys between February 1993 and December 2008 and coded as national priorities in which the defendants were publicly identified as a member of a major party or a prominent associate of a well-known partisan. Charge severity measures were calculated using the approach developed in Rehavi and Starr (2014), which estimates the maximum potential sentence under the law for every criminal charge used by the Department of Justice. Weeks to file were calculated from the date the case was received to the date on which charges were filed (the 25 cases in which defendants were charged before the case was received due to a pre-arrest indictment are coded as 0; none were partisans). Number of defendants represents totals in the data; individual cell sample sizes vary slightly due to missing data. See Online Appendix A for further details.

Table 2: Probability of opposition party defendant by election timing

	Window around election (weeks)			
	24	20	16	12
After election	-0.18* (0.07)	-0.20* (0.09)	-0.15+ (0.09)	-0.12 (0.10)
Constant	0.82 (0.07)	0.80 (0.09)	0.85 (0.09)	0.93 (0.07)
R ²	0.34	0.38	0.33	0.31
N	250	207	151	113
Year fixed effects	Yes	Yes	Yes	Yes

+, *, and ** denote significance at the 10%, 5% and 1% levels, respectively. Robust standard errors from OLS are in parentheses.

Table 3: Post-election change in probability of opposition party defendant

	Window around election (weeks)			
	24	20	16	12
<u>Local linear regression</u>				
Election discontinuity	-0.50*	-0.49*	-0.48*	-0.48*
	(0.20)	(0.19)	(0.19)	(0.19)
LLR optimal bandwidth	4.78	4.62	4.51	4.45
<u>Flexible polynomial RD (logit)</u>				
Election discontinuity	-0.59*	-0.68**	-0.60*	-0.45
	(0.28)	(0.24)	(0.30)	(0.35)
N	250	207	151	113

+, *, and ** denote significance at the 10%, 5% and 1% levels, respectively. Local linear regression estimated in Stata using rd (Nichols 2011) with bandwidth calculated using the approach in Imbens and Kalyanaraman (2012). Flexible polynomial estimator includes third order polynomials estimated using logistic regression. Standard errors in parentheses (clustered by election cycle week).

Table 4: Weeks to charge around elections

	Window around election (weeks)			
	24	20	16	12
Opposition party	-0.34*	-0.33+	-0.31	-0.59**
	(0.16)	(0.19)	(0.20)	(0.21)
Post-election	0.32	0.17	0.18	0.08
	(0.22)	(0.24)	(0.26)	(0.26)
Opposition party \times post-election	0.21	0.48	0.60+	0.90**
	(0.27)	(0.30)	(0.33)	(0.32)
Constant	3.80	3.94	3.93	2.87
	(0.22)	(0.24)	(0.26)	(0.88)
N	250	207	151	113
Year fixed effects	Yes	Yes	Yes	Yes

+, *, and ** denote significance at the 10%, 5% and 1% levels, respectively. Robust standard errors from Poisson models in parentheses.

Table 5: Promotion rates by partisan case timing in public corruption prosecutions

	AUSA \Rightarrow USA	USA \Rightarrow judge
Equal treatment/no partisans	0.02 (0.02)	0.01 (0.04)
More opposition pre-election vs. post-election	0.08+ (0.04)	0.13+ (0.07)
Constant	0.03 (0.02)	0.04 (0.04)
R ²	0.01	0.03
N	686	220

+, *, and ** denote significance at the 10%, 5% and 1% levels, respectively. Robust standard errors from OLS are in parentheses. The omitted category is an indicator for those officials who prosecuted more co-partisans in the immediate pre-election period versus afterward relative to opposition party defendants.

Sample: Assistant U.S. attorneys (AUSAs) in federal criminal cases targeting state and local public corruption filed from February 1993 to December 2008 that were coded as national priorities and the supervising U.S. attorneys in those cases.

Table 6: Case outcomes before and after *Booker*

	Sentence (months)		Convicted without plea		Govt. departure	
	(1)	(2)	(3)	(4)	(5)	(6)
Opposition party	-9.35**	-9.93**	-0.18**	-0.17**	0.15**	0.14**
	(2.90)	(2.98)	(0.06)	(0.06)	(0.05)	(0.06)
Post- <i>Booker</i>	-6.75+	-6.38	-0.20**	-0.21**	0.00	0.01
	(3.90)	(3.90)	(0.07)	(0.07)	(0.05)	(0.05)
Opposition party \times <i>Booker</i>	9.90*	9.00+	0.22*	0.23*	-0.27**	-0.27**
	(4.80)	(4.83)	(0.10)	(0.10)	(0.08)	(0.08)
Democrat	-2.54	-2.82	0.05	0.05	0.09*	0.09*
	(2.35)	(2.35)	(0.05)	(0.05)	(0.04)	(0.04)
Bush	6.80**	5.79*	0.06	0.07	0.16**	0.16**
	(2.35)	(2.37)	(0.05)	(0.05)	(0.04)	(0.04)
Proportion same-party judges		9.24+		-0.10		0.03
		(5.53)		(0.09)		(0.06)
Constant	21.49	17.98	0.30	0.33	-0.04	-0.05
	(2.65)	(3.63)	(0.06)	(0.06)	(0.04)	(0.05)
R ²	0.02	0.02	0.03	0.04	0.07	0.07
N	489	489	489	489	489	489

+, *, and ** denote significance at the 10%, 5% and 1% levels, respectively. Robust standard errors from OLS are in parentheses.