

Tipping the Scales?

Testing for Political Influence on Public Corruption Prosecutions

Brendan Nyhan
Dept. of Government
Dartmouth College
nyhan@dartmouth.edu

M. Marit Rehavi
Dept. of Economics
CIFAR and University of British Columbia
marit.rehavi@ubc.ca

October 14, 2014

Abstract

The need to cultivate political support for nominations to higher office creates a conflict of interest for U.S. attorneys and the prosecutors they supervise in cases involving the two major parties. We find evidence of partisan differences in the timing of public corruption case filings around elections. Relative to the president's co-partisans, opposition party defendants are more likely to be charged immediately before an election than afterward. We find a corresponding decrease in case duration before elections for opposition partisans, suggesting prosecutors are moving more quickly to file cases. These timing differences are associated with greater promotion rates to the federal bench (for U.S. attorneys) and to U.S. attorney (for assistant U.S. attorneys). However, prosecutors do not appear to bring weaker cases against opposition defendants before elections; we find no measurable difference in conviction rates and actually show that co-partisans received *less* favorable treatment in plea bargains and sentencing until recently.

We gratefully acknowledge support from the Arts Undergraduate Research Awards at the University of British Columbia, the Canadian Institute for Advanced Research, the Nelson A. Rockefeller Center at Dartmouth College, and the Robert Wood Johnson Foundation Scholars in Health Policy Research Program at the University of Michigan. We thank Mauricio Drelichman, Linda Fowler, Sanford Gordon, Don Green, Scott Hendrickson, Michael Herron, Eitan Hersh, Dave Hopkins, Dean Lacy, Jacob Montgomery, Francesco Trebbi, and seminar/conference audiences at the Canadian Institute for Advanced Research, Dartmouth College, MIT, the Midwest Political Science Association, the American Political Science Association, and the Canadian Economics Association for helpful comments. Michael Chi, Ester Cross, Hugh Danilack, Brandon DeBot, Eli Derrow, Sasha Dudding, Jacquelyn Godin, Aaron Goodman, Nora Isack, Jessica Longoria, Midas Panikkar, Katie Randolph, Daniel Shack, Urvi Shah, Sabrina Speianu, Evelyn Weinstein, and David Wylie provided excellent research assistance. Finally, we are grateful to Sanford Gordon for generously sharing his data.

The United States Attorney is the representative not of an ordinary party to a controversy, but of a sovereignty whose obligation to govern impartially is as compelling as its obligation to govern at all; and whose interest, therefore, in a criminal prosecution is not that it shall win a case, but that justice shall be done.

—Justice Sutherland, *Berger v. United States* (1935)

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).

Do political pressures like these influence prosecutors or other unelected officials? Despite rules and procedures intended to ensure that they execute their powers faithfully, government officials within the bureaucracy often have substantial discretion to advance their own interests or biases (e.g., Epstein and O’Halloran 1994; Gailmard 2009; Gailmard and Patty 2012; Miller 2005). The potential abuse of delegated authority is an especially serious concern for prosecutors, who have wide latitude in choosing which cases to pursue, which charges to file, when to file them, and how to resolve ongoing cases. In making these choices, they exercise great influence over case selection and outcomes (Kessler and Piehl 1998; Rehavi and Starr N.d.; Starr and Rehavi 2013).

To date, however, little is known about how political incentives influence U.S. attorneys or other prosecutors. The influence of political factors on judges has begun to receive more scrutiny (Berdejo and Chen N.d.; Besley and Payne 2003; Canes-Wrone, Clark, and Park 2012; Gordon and Huber 2007; Huber and Gordon 2004), but only two studies have used rigorous econometric strategies to examine the systematic influence of partisan politics on

prosecutors (Alt and Lassen 2012; Gordon 2009).¹ In particular, it is not known whether partisan considerations influence the timing of charges, as has been alleged (e.g., Belser 1999; Kornacki 2011; U.S. Department of Justice 2008; Yalof 2012), or their resolution.

Concerns about political influence on prosecutorial behavior are particularly acute given the ambitions for ascending to higher appointed or elected offices harbored by many U.S. attorneys and assistant U.S. attorneys (the career prosecutors in their offices). 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.

We therefore test for partisan differences in the timing and resolution of public corruption prosecutions filed immediately before or after elections between 1993 and 2008. 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 members of the president's party. This discontinuity in the timing of charges around elections, which is not observed in falsification tests, corresponds to a discontinuity in the time elapsed before charges are filed for opposition defendants, who are charged more rapidly before elections than afterward.

The career incentives for prosecutors to protect the interests of the party in power appear to be strong. Assistant U.S. attorneys who file relatively more pre-election cases against the opposition defendants than co-partisans are more likely to go on to serve as U.S. attorneys. The correspondence between partisan timing disparities and political appointments is also observed for U.S. attorneys, who are more likely to serve as a federal judge if the prosecutors they supervised filed relatively more pre-election partisan cases against the opposition than co-partisans (relative to cases filed during the immediate post-election period).

Career incentives suggest that prosecutors should be most responsive to partisan incen-

¹See Gordon and Huber 2009 for a review. Shields and Cragan (2007) argue that the Bush administration was disproportionately likely to investigate Democratic officials for corruption, but their methodology cannot prove bias (Gordon 2009). Similarly, the research designs used in Heidenheimer (1990, 583–584) and Meier and Holbrook (1992) cannot determine whether the observed correlations between partisanship and corruption prosecutions are causal.

tives when the reputational costs of doing so are minimized. Case timing decisions may face less external scrutiny due to confidentiality protections. In contrast, the outcome of a case is easily observable and may face greater scrutiny due to the higher stakes involved in conviction and incarceration decisions. Consistent with this, the differences we find in case timing by partisanship are not observed in case outcomes. We find no measurable difference in conviction rates by party. Indeed, the reductions in sentencing that are typically negotiated in plea bargains may make prosecutors vulnerable to accusations of favoritism when the defendant is a co-partisan. Along these lines, we find that co-partisan defendants actually received *longer* sentences than opposition partisans and were *less* likely to accept plea deals or receive favorable sentencing recommendations from prosecutors until recently. These differences in outcomes do not appear to be the result of prosecutors using a higher severity or case strength threshold when deciding whether to file cases against co-partisans. If the co-partisans who were charged faced stronger cases or committed more severe offenses, we would expect judges to punish them with longer sentences. However, the partisan differences in sentencing and case resolution that we observe disappear following the Supreme Court's 2005 ruling in *United States v. Booker*, which as we explain below gave judges greater discretion in sentencing.

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 (often referred to as "Main Justice"). Each U.S. attorney oversees a staff of career prosecutors called 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 (Beale 2009; Green and Zacharias 2008; Whitford 2002; Whitford and Yates 2003), substantial flexibility remains. According 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). 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 federal corruption prosecutions thus appears to be substantial. The cases of state and local public corruption that 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.² Criminal prosecutors also exercise substantial discretion over the number and severity of the charges filed against a defendant, the timing of those charges, and case resolutions such as the terms of plea agreements.

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 officers of the court and political appointees in an executive branch department and thus face an inherent conflict of interest in cases like these (Beale 2009; Eisenstein 1978; Perry 1998). For instance, partisan incentives could affect prosecutorial decisions either consciously or unconsciously. 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. Given the growth in elite and activist polarization in recent years (e.g., Layman et al. 2010; McCarty, Poole, and Rosenthal 2008), the political views of U.S. attorneys are likely to be quite polarized.

Perhaps more importantly, however, U.S. attorneys have strong career incentives to cultivate support among party elites as well as to maintain or improve their status in the legal community. They are typically nominated by the president as a result of support from elected

²The majority of these referrals are not ultimately pursued (Gordon 2009). Prosecutors may also be directly involved in investigations. In such cases, they are likely to also influence how aggressive law enforcement officials are in gathering evidence against a suspect.

officials and other political figures and rely on those allies to maintain favored status within an administration (Eisenstein 1978, 115). Many federal prosecutors also hope to obtain positions as judges and elected officials. To do so, they will likely need support from party elites and activists to do so (e.g., Cohen et al. 2009; Dominguez 2011; Rottinghaus and Nicholson 2010), which could be endangered if they are seen as damaging the interests of the party — a genuine possibility in the case of corruption charges that could be filed near an election. Eisenstein (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.” 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, 201–206).³

It is important to be clear that these forms of influence are likely to center on the potential consequences of a prosecutor’s actions for the party in power rather than individual partisan defendants. 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. By contrast, the timing of charges is likely of greater concern to the party in power, which would presumably prefer to have the opposition party faces corruption charges against a party member in the period before an election. Likewise, the timing of charges against a co-partisan defendant may be more important to party officials than the resolution of the case.⁴

We can thus think of prosecutors’ career trajectories as depending on two sources of capital: political capital within their party (herein partisan capital) and their legal reputations (herein reputational capital). Some actions, such as high-profile prosecutions of terrorists,

³In practice, corruption prosecutions are unlikely to change the outcome of elections unless the defendant is a candidate. For our theory to be valid, it is only necessary that the election context increases the salience of corruption prosecutions and thereby affects the career incentives facing prosecutors.

⁴Indeed, conditional on charges being filed, the party in power may actually prefer to severely punish a corrupt co-partisan to distance themselves from the alleged misconduct. However, it is likely that their preference would be for such charges to be brought at a less salient time.

provide the opportunity to build both types of capital. Ambitious prosecutors should always choose to pursue those activities. They 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 in their publicly observable actions. In these cases, prosecutors are unlikely to take actions that will benefit their party or harm the opposition party if the reputational costs of doing so are sufficiently high. Even the appearance of playing politics with criminal cases could erode a prosecutor's reputational capital (e.g., giving a favorable plea deal to a co-partisan or bringing an unsuccessful case against a political opponent).⁵

These incentives could also affect career prosecutors, who are likely to anticipate or respond to the political incentives facing their supervisor. Many have ambitions of their own and may wish to cater to the preferences of potential allies for career reasons. For instance, approximately one in five federal judges and one in two U.S. attorneys confirmed during the Clinton and Bush 43 presidencies previously served as assistant U.S. attorneys.⁶ Others may simply wish to avoid heightened scrutiny or unpleasant reactions from displeased USAs or their political allies. In this way, political incentives can affect far more cases than U.S. attorneys could possibly handle directly.

Partisan disparities in prosecutions: The limiting role of scrutiny

Given these career concerns, we would expect prosecutors to seek to maintain or build their partisan capital in prosecuting corruption cases only when doing so will not cause greater damage to their reputational capital. This is most likely for actions that are not fully observable such as decisions about case timing. Prosecutors are relatively autonomous in managing

⁵An alternative theory could be developed that modeled partisan disparities as a result of prosecutors' taste for differential treatment by defendant partisanship (e.g., Becker 1957), but such a model would struggle to simultaneously explain the disparities in case timing we observe (which are unlikely to substantially affect defendants) or the changes in sentencing we observe after *Booker* (which suggest that prosecutors were not bringing weaker cases against opposition defendants or offering more favorable deals to co-partisans as a simple taste-based model would predict).

⁶These estimates are derived from biographical data on federal judges (Federal Judicial Center N.d.) and our coding of U.S. attorney biographies from *Who's Who* (details available upon request).

the timing of cases and have substantial flexibility. Slowing down or speeding up a decision about a pending case may be relatively costless for a busy prosecutor with a large caseload.⁷ In addition, cases are typically confidential until charges are filed — the public therefore does not observe when a prosecutor *could* have filed charges but chose not to do so, nor when a case is filed faster or slower than normal.

Even when cases are filed, critics must claim that the defendants would have been treated differently if they were members of the other party (e.g., Conte 2012) — a counterfactual that is impossible to prove in any specific case and is likely unpersuasive when made by or on behalf of defendants in criminal corruption cases. Finally, even prosecutors themselves may not be fully aware of partisan disparities. For instance, U.S. attorneys might unknowingly treat cases differently by party without pursuing obviously weak cases against opposition party defendants.⁸ We therefore expect that the timing of cases against partisan defendants will vary according to the electoral context.

Some anecdotal evidence is consistent with the hypothesis that partisan pressures or incentives affect the timing of political corruption cases. David Iglesias, a former U.S. attorney in New Mexico, alleged that he was pressured by two Republican members of Congress 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 (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. Critics alleged, for instance, that Independent Counsel Lawrence Walsh's decision to file new charges in the Iran-Contra affair

⁷By contrast, a U.S. attorney might find it very costly (both politically and professionally) to demand that subordinates ignore a strong case referral or engage in a meritless prosecution. While the decision not to pursue a criminal case takes place outside of public view, it is witnessed by the law enforcement personnel who investigated the case and the career prosecutors who work under the U.S. attorney. U.S. attorneys also face possible investigation by DOJ's Office of the Inspector General and/or Office of Professional Responsibility, which investigate internal and external allegations of impropriety. Verified allegations can result in scandals or penalties (e.g., internal reprimands and referral to the relevant bar association). Finally, once a case involving partisan defendants is filed, it is likely to receive extensive public scrutiny.

⁸It is easy to rationalize individual decisions to move forward with a case before an election ("Justice can't wait!") or to postpone a case filing until afterward ("Keep politics out!"). As a result, the handling of each case in isolation might seem appropriate even when there is a macro-level pattern of partisan discrepancies.

just days before the 1992 election was politically motivated (Yalof 2012, 30). Conversely, a U.S. attorney for Maryland alleged that he had come under political pressure for investigating associates of the state's Republican governor in the period before the 2004 election (Lichtblau 2007). Similar charges of partisan bias or political influence on the timing of prosecutions are frequently made in state and local public corruption cases (e.g., Belser 1999; *Houston Chronicle* 1996; Murphy 2007; Schultze 2013; Trahan 2009).

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). Prosecutors have substantial discretion in what charges they file and how to resolve cases — most notably, in making plea agreements and requesting downward departures from sentencing guidelines. However, the case filing and resolution process is far more public and vulnerable to external scrutiny than case timing decisions. In particular, the majority of federal cases are resolved by plea agreements in which defendants typically negotiate for a lower sentence than they might otherwise have received. Such cases raise obvious concerns for prosecutors about appearing to offer favorable deals to co-partisans. Unlike case timing, objections to such arrangements do not depend on counterfactuals but instead on the disjunction between a defendant's status as an admitted criminal and the reduction in their sentence relative to the statutory maximum. While such sentencing reductions are standard, they could easily look like favoritism in any particular case.⁹ Further, once an individual is a convicted criminal, it is in the party elite's interest to distance the party from the defendant's conduct 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 indeed treat co-partisans *more* harshly than the opposition. To assess this expectation, we test whether prosecutorial case resolution practices differed by party and how they changed after 2005's *United States v. Booker* decision (discussed further below).

⁹Given the stakes, allegations of politicization of case resolution could also be viewed very negatively within the legal community.

Data

The universe of cases filed by federal prosecutors is released by DOJ under the Freedom of Information Act (FOIA). We extracted all cases classified as targeting state and local public corruption that were filed by U.S. attorneys between February 1993 and December 2008.¹⁰ These data include detailed case information including the history of all charges filed (including those that were superseded or dismissed), key case processing dates (e.g., the date of receipt by DOJ, when charges were filed, etc.) and the ultimate resolution of each charge.¹¹

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 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.).¹² It is these defendants who we refer to as co-partisans or opposition party defendants below.¹³ Further details on the construction of these variables and our coding procedures are provided in Online Appendix A.

In total, we identified 1932 of the 2545 qualifying defendants (76%) spread across 1177

¹⁰We exclude January 1993 since President Clinton did not take office until January 20. It is important to note that this sample is the universe of public corruption cases, but may not include all criminal cases involving politicians. For example, criminal conduct completely unrelated to an individual's public duties or to campaign law (e.g., drug use) would be classified as violating the relevant criminal code, not public corruption.

¹¹Though comprehensive statistics are not available across levels of government, the federal government seemingly conducts most anti-corruption prosecutions in the contemporary United States (Maass 1987). We therefore follow the previous literature (e.g., Alt and Lassen 2012; Gordon 2009) in focusing on federal public corruption prosecutions.

¹²These codings do *not* account for party registration or other private behavior. Individuals were only coded as partisans if they were publicly identified as such in news accounts or public documents.

¹³This terminology refers to the relationship between the partisanship of the administration in which a U.S. attorney serves and that of the defendant in question, not the U.S. attorney's personal partisanship (though it will often coincide with the administration in practice).

cases (out of 1334 total).¹⁴ After defendants were identified, we coded their partisanship following the procedure described above (further details are provided in Online Appendix A). Among the 1932 identified defendants, 490 defendants (25% of those identified) from 286 cases could be coded as having been publicly identified with one of the major parties either individually (153), as an associate of a publicly-identified partisan (314), or as both an individual and as an associate of a partisan (23).¹⁵ A plurality of these defendants were members of local government (39%) while another 19% were part of the state or federal government (non-military). The partisan defendants from federal, state and local government are a mix of elected officials and unelected figures such as political staff and public sector officials. The partisan defendants in the sample who are *not* part of the federal, state, or local government were members of the private sector (34%), family members or personal associates of political figures (3%), or could not be coded (5%). Among the partisans identified, 353 were affiliated with the Democratic Party compared with only 137 Republicans.¹⁶

For each case filing date, we calculated the number of weeks until or since the nearest gubernatorial, state House, state Senate, or federal general election in each state.¹⁷ In spite of the informal norm against charging political cases in the period before an election, there are a considerable number of politically relevant filings in the preceding months. Figure 1 summarizes the distribution of public corruption case filings over the electoral cycle for those defendants whom we identified as partisans.¹⁸ For visual clarity, we use bins of thirty days (approximately one month) where -1 represents the month before Election Day and +1

¹⁴These 1932 defendants represented 1903 unique individuals due to 27 individuals being charged in two cases and one being charged in three separate cases. As we show in Online Appendix A, the defendants who could be identified faced more charges and counts, were more likely to be found guilty, were convicted of more severe crimes, and received longer sentences than those who could not be identified.

¹⁵There are a total of 480 unique partisan defendants — ten appear in two separate cases.

¹⁶In a validation check, we found that our data include 94% of the defendants identified by Gordon (2009), including 99.4% of partisans he identified, and also correspond closely to his party identification coding (90% of defendants present in both datasets were independently coded the same way).

¹⁷We focus on state and federal general elections because those are most consequential to the party members who control the nomination process for higher appointed and elected offices and thus most relevant to the career incentives facing federal prosecutors. Future research should examine primary elections, which we do not consider.

¹⁸An equivalent figure showing the distribution of case timing over the election cycle for the full set of defendants is included in Online Appendix A.

represents the month after Election Day.¹⁹

[Figure 1 about here.]

Among the cases of public corruption involving partisan defendants in our data, nearly two-thirds of those filed within 24 weeks of an election were filed in the 24 week period *before* the election. The median number of partisan public corruption prosecutions per electoral cycle month is 19, but the distribution of public corruption case filings varies over the electoral cycle with a noticeable peak immediately before elections.

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 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. The distribution of time to case filing has a significant peak at 0 for cases filed immediately (21%)²⁰ 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 developed in Rehavi and Starr (N.d.), which estimates the maximum potential sentence under the law for the charges facing each defendant. 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.

Summary statistics for our charge severity, case resolution and time to case filing variables are provided in Table 1.

[Table 1 about here.]

¹⁹The bins for -12 and 12 include cases filed 361–366 days from an election — the maximum electoral distance observed in our data.

²⁰The 25 cases in which defendants were charged before the case was received (i.e., a pre-arrest indictment) are coded as 0 (none were partisans).

In our data, prosecutors take nearly 50% longer to build cases against co-partisan defendants (a mean of 66 weeks versus 45 weeks for opposition defendants; $p < .01$).²¹ Opposition defendants are also charged with 52% more counts but plead guilty to or are found guilty of only 23% more counts. The two groups otherwise appear relatively comparable.²² Charges involving partisan defendants involve slightly more distinct charges than those against non-partisans and have more serious charges sustained (in terms of the statutory maximum sentence for the most serious count that was sustained).

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 regression discontinuity (RD) models, which allow us to evaluate the hypothesis that 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 around elections by party. 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

²¹The median time to charges is even more discrepant — 15 weeks for opposition partisans versus 45 weeks for co-partisans ($p < .01$).

²²While the number of counts may be impressive in a newspaper headline, many federal sentences run concurrently. The sentence of the most serious charge is thus typically a better predictor of the ultimate potential sentence length than than the number of counts.

case outcomes vary by party, using difference-in-differences models to estimate how they vary by party around elections and how they 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 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. However, in the absence of the strategic timing of charges, the arrival of cases that are ready to be filed should vary smoothly over time and not discontinuously change by party on Election Day.

The most natural approach to evaluating manipulation of case timing dates around elections is the McCrary (2008) density test, which is typically used to examine whether the distribution of the “running” or “forcing” variable in regression discontinuity (RD) designs is continuous at the discontinuity.²³ In typical RD applications, finding a discontinuity in the running variable would indicate that agents are sorting around the cutoff or otherwise manipulating the running variable and would invalidate the identification assumptions necessary for causal inference. In our setting, however, such a finding would constitute *evidence* that prosecutors are sorting case filing dates with respect to Election Day. Specifically, we use the McCrary (2008) test to evaluate whether the density of case filings changes around elections for either opposition party defendants or those associated with the president's party.

The results, which are plotted in Figure 2, 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 ($\theta = 0.001$, s.e. = 0.67).

²³For overviews of regression discontinuity designs, see Imbens and Lemieux 2008 and Lee and Lemieux 2010.

[Figure 2 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 members of the administration's party around Election Day using both a simple OLS model and a regression discontinuity-style estimator. The RD model specifically tests for discontinuities in the relative probability of corruption charges being filed by a U.S. attorney against a member of the opposition party rather than a co-partisan after Election Day. We do not expect cases be distributed randomly around the election date. However, if the arrival rate of credible potential cases changes smoothly around elections, then our RD model will provide a valid estimate of the discontinuous post-Election Day change 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 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), the estimates from the models indicate that opposition party defendants were twelve to twenty percentage points less likely to be charged after an election compared with same-party defendants. For instance, 64% of the partisan defendants charged in the 24 weeks before an election were associated with the opposition party compared with 48% of those charged afterward (Fisher's exact test: $p < .05$). The model estimates reported in Table 2 are statistically significant in all but

one case (a window of 12 weeks around elections).

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.

Table 3 reports our estimates from the two predominant approaches to regression discontinuity models employed in the literature (Lee and Lemieux 2010)—local linear regressions and regressions with flexible polynomials (we use logistic regression due to the binary outcome variable).²⁴

[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 public corruption in the period around an election, the probability of a member of the opposition party or an 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 on the criminal case rather than the election cycle week or use 200% of the optimal bandwidth for local linear regression to address possible overfitting on outliers near the discontinuity (see Online Appendix B).

Figure 3 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

²⁴Optimal bandwidths are calculated for each local linear regression following Imbens and Kalyanaraman (2012). The logit models include third order polynomials in the distance from the election estimated separately on each side of the discontinuity. They also use robust standard errors clustered by election cycle week to account for shared shocks or other correlated errors.

all cases filed against partisans within 24 weeks of Election Day.

[Figure 3 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.²⁵

If this discontinuity is the result of prosecutors manipulating case timing, 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 being received by a prosecutor and the date on which charges are filed. One particularly interesting subset of cases are those filed on the same day that they are recorded as being received, which we call “immediate” case filings. Prosecutors either rushed to file these cases on the day they were received or the charges were the result of a prosecutor-led investigation. Figure 4 presents a simple bar graph demonstrating how the proportion of immediate case filings varies dramatically around elections by defendant partisanship.

[Figure 4 about here.]

For opposition defendants who were immediately charged within 24 weeks of an election, 83% ($n=24$) were charged before an election — the time when such charges could be most damaging to their party. By contrast, only 24% of the comparable group of same-party defendants ($n=17$) and 60% of the comparable non-partisan defendants ($n=138$) were charged in the pre-election period. These dramatic differences easily allow us to reject the null of independence between timing and partisanship (Fisher’s exact test: $p < .01$).

²⁵These estimates likely understate the full amount of partisan differences in the timing of charges. As Figure 3 suggests, the differences in the distribution of case timing across parties may not be confined to the period in the immediate vicinity of the election. For instance, it appears that individuals in the opposing party are more likely to be charged in the months leading up to the election. While the estimates above have the advantage of isolating a discontinuous change around a salient date when sharp changes in the underlying distribution of cases are unlikely, they do not capture partisan disparities further away from the election.

The conclusion we draw from the immediate case filings data are supported by the plots presented in Figure 5, which are 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 5 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. 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.²⁶

Table 4 directly 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.²⁷ The post-election shift for opposition defendants, which is estimated as a linear combination of coefficients, is positive and statistically 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

²⁶An alternative interpretation is that prosecutors held some especially sensitive cases that took a long time to develop until after elections to avoid controversy, but this claim is contradicted by the immediate case filing results above.

²⁷We estimate these models using Poisson regression with robust standard errors rather than negative binomial regression despite the presence of overdispersion because the standard negative binomial regression model is not consistent if the variance model is misspecified (Cameron and Trivedi 2010, 577).

to file after elections.

[Table 4 about here.]

These results suggest that prosecutors may be filing cases more quickly against opposition party defendants during the pre-election period when incentives are stronger. The decline in cases filed against opposition defendants in the post-election period (Table 2) and increase in time to filing after elections (Table 4) suggests that prosecutors may be accelerating the timing of cases charged before elections rather than bringing cases that would not otherwise have been filed.²⁸

Partisan disparities in case timing: Threats to inference

Elections create the opportunity for election-related corruption. One might therefore be concerned that the case timing disparities documented above are the product of differences in opportunities for election-related corruption. However, even if corruption does increase around elections, its prevalence should still vary smoothly over time.²⁹ Any alternative account of our results along these lines would thus need to explain why the underlying prevalence of corruption (or arrival rate of available cases) would change discontinuously by party around Election Day. Finally, 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 (see Online Appendix A for details on how these statutes were coded).

We next conduct two falsification tests to address possible concerns that these findings are the spurious result of non-political factors. 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

²⁸We find no measurable difference in conviction rates by election timing and partisanship below, which is consistent with this interpretation.

²⁹Few individuals are arrested and charged in election-related corruption cases before the election that they are trying to affect. Typically, a long chain of events is required to take place before charges can be filed (the identification of a crime and suspect, building a case, making an arrest, etc.). These cases are thus typically filed long after the relevant election has taken place.

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 election calendar³⁰ and estimate 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. However, 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 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 identify the causal effect of partisan timing choices on career outcomes using the available data. However, the *ex post* relationships we observe suggest that prosecutors who engage with partisan case timing patterns that are more favorable to the party in power have higher rates of promotions to appointed office. These associations are presumably observed by prosecutors as well.³¹

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 bench. Documenting both relationships is important because U.S. attorneys supervise their offices and make key management decisions, but AUSAs try the vast majority of cases. As we argue above, partisan career incentives should apply to *both* levels of prosecutors.

We first identify all assistant U.S. attorneys (AUSAs) listed in the PACER judicial database

³⁰The states that do not hold state elections on the federal calendar are Kentucky, Louisiana, Mississippi, New Jersey, and Virginia.

³¹These relationships need not be causal; the fact that such a relationship is observed in practice is all that is necessary for this mechanism to be incentive compatible for prosecutors.

as representing the federal government in the public corruption cases in our sample for which a defendant could be identified. For each of these AUSAs, 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 (essentially, a difference-in-differences estimate, which allows for differences in AUSA preferences over case timing against partisans around elections).

We then classified each AUSA 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 who took office during the Clinton and Bush administrations using the same procedure based on the public corruption cases in our data that were filed under their supervision. 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 further details.)

[Figure 6 about here.]

The raw data, presented in graphical form in Figure 6, 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 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 with categorical indicator variables.

Assistant U.S. attorneys were more than three times as likely to serve as U.S. attorneys if they brought relatively more pre-election cases against opposition defendants compared with those who did 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 serve as a federal judge than those who did the opposite (a 15 percentage point increase in promotion rates; $p < .07$).

Partisan disparities in case content: The role of external scrutiny

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 before elections? We find no evidence to support this claim. If it were correct, we would expect opposition partisans charged immediately before elections to be less likely to be found guilty than those charged immediately afterward, but no such difference is observed. Table 6 presents a difference-in-differences linear probability model of whether partisan defendants charged around elections were convicted of any charges. We find no measurable change in the probability of conviction for partisan defendants of either party around elections, though the estimates are imprecise and we cannot rule out sizable effects in either direction.³²

[Table 6 about here.]

The welfare implications of the apparent partisan influence we observe are unclear, however. Are partisan defendants being treated differently by prosecutors or simply facing opportunistic variation in case timing? 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 assess the effects

³²If prosecutors were masking weak cases by allowing defendants to plead guilty to lesser crimes, we would also expect to observe fewer charges being sustained, but we find no measurable post-election difference in the maximum potential sentence of the charges for which the defendant was found or pled guilty (a measure of case strength that avoids post-*Booker* complications in using sentencing outcomes; results available upon request).

of partisanship on case outcomes outside of the pre-/post-election context, we leverage the Supreme Court’s 2005 decision in *United States v. Booker*, which lifted the previous requirement that federal judges issue sentences within the range specified by the U.S. Sentencing Guidelines.³³

Specifically, we re-examine the Gordon (2009) finding that co-partisans have higher average sentences than members of the opposition party. This discrepancy, which we confirm in our data for the period before 2005, might at first seem to be consistent with a taste-based discrimination model (Becker 1957) in which prosecutors impose a higher crime severity or case strength threshold on potential cases involving co-partisans. As we demonstrate below, the evidence is not consistent with this interpretation. If the gap were a reflection of true differences in crime severity arising from selection into prosecution, judges should have continued to give opposition defendants lower sentences after *Booker*. Instead, however, the disparity disappears. First, models (1) and (2) of Table 7 show that the partisan sentencing gap disappears after *Booker* and that this result is robust to controlling for the proportion of federal judges in a district who were appointed by a president from the same party as the U.S. attorney’s administration.

We instead propose an alternative interpretation in which potentially damaging external scrutiny prevents opposition defendants from being treated more harshly during case resolution decisions by prosecutors. In fact, in the absence of differential selection, the sentencing gap prior to *Booker* would be consistent with prosecutors driving *harder* bargains with co-partisans due to the potential appearance of impropriety.³⁴

[Table 7 about here.]

³³The Guidelines were essentially a point system based on the crime of conviction, the defendant’s criminal history, and any aggravating or mitigating facts present. The point total then assigned each defendant to a specific Guideline cell and a corresponding narrow range of potential sentences. Thus, while the statutory range for a crime could be vast (e.g., 0 to 15 years), the Guideline range for a particular defendant might be much smaller (e.g., an 18-month window).

³⁴An alternative interpretation is that U.S. attorneys might have an incentive to avoid deals with co-partisans that could end up expanding the scope of the case to include other co-partisans. Defendants often provide evidence against other targets in exchange for lenient treatment. If co-partisans are likely to name other co-partisans as fellow conspirators, U.S. attorneys might enmesh even more party allies or public figures in a case.

Consistent with this interpretation, we find that opposition defendants were *more* likely to agree to a plea deal (models (3) and (4)) or receive a prosecutorial recommendation for a favorable departure from sentencing guidelines (models (5) and (6)) than co-partisan defendants pre-*Booker* — precisely the opposite of what we would expect if U.S. attorneys were treating opposition defendants harshly or offering more desirable deals to members of their own party. After *Booker*, however, sentencing guidelines were no longer binding on judges, weakening the relationship between the details of the plea agreement and the sentence. Defendants became less dependent on prosecutors for sentencing reductions; judges could now give lower sentences without favorable sentencing recommendations.³⁵ This change presumably lowered the costs of agreeing to unfavorable sentencing facts, which could explain why the plea gap disappeared post-*Booker* — co-partisan defendants had less incentive to forego a deal and take their chances at trial.³⁶

Conclusion

We provide new evidence of partisan disparities in the timing of public corruption charges by U.S. attorneys and the career prosecutors they supervise around elections. These officials, who depend on partisan elites to support their nominations for higher appointed or elected office, appear to shift the timing of politically sensitive cases in a manner that is 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 opposition 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

³⁵While judges could unilaterally deviate from the guidelines after *Booker*, a prosecutorial recommendation of a departure from sentencing guidelines was still valuable to defendants. Even after *Booker*, such recommendations increase the chances of a favorable sentence and were still present in 9% of post-*Booker* cases in our sample (down from 12% pre-*Booker*).

³⁶The overwhelming majority of cases in the federal system are resolved by plea agreements. However, plea rates are lower for public corruption cases.

when defendants do not share their partisanship. The career paths prosecutors observe are consistent with the theory we propose — 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.

We do not find any evidence that prosecutors are bringing weaker corruption cases against opposition partisans, however. There is no measurable difference in the probability of conviction for partisans of either party who are charged around elections. Furthermore, prosecutors do not appear to actively favor co-partisan defendants in case resolutions. In fact, prior to a recent increase in judges' sentencing discretion, defendants from the president's party received *higher* sentences and were less likely to agree to plea bargains than opposition party members — a finding that is consistent with prosecutors seeking to avoid the appearance of impropriety when they could face more serious scrutiny. These differences disappeared after the *Booker* decision when judges came to play a greater role in case resolution, suggesting that the previous discrepancies in case outcomes were the result of prosecutorial discretion rather than taste-based discrimination in case selection.

As with any research, this study has limitations that should be noted. First, 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. We are additionally constrained by the relatively small number of partisan public corruption defendants and limited case details provided in the DOJ's public data. Finally, our findings of course depend on the validity of the assumptions that underlie our research designs, including the McCrary (2008) test and the regression discontinuity and difference-in-differences models.

Nonetheless, these results deepen our understanding of the role of politics in prosecutorial behavior — an important topic for future research — and have potentially significant policy implications as well. Most notably, our results indicate the difficulty of countering political influence using regulatory or enforcement approaches. Even if U.S. attorneys were required to recuse themselves from public corruption prosecutions against partisans in the pre-election period, the career prosecutors that they supervise have incentives to curry favor

with their supervisor or external political elites. Moreover, those who lack political ambitions might still wish to avoid the potential repercussions that could ensue if they were seen as harming the interests of their supervisor or his or her political allies. None of these influences are easily detected by outsiders; proving that a prosecutor has violated ethical or legal standards is even more difficult. A better approach might therefore concede the inevitability of political influence in the charged pre-election environment and instead enforce 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.

References

- Alt, James E, and David Dreyer Lassen. 2012. "Enforcement and Public Corruption: Evidence from the American States." *Journal of Law, Economics, and Organization* Advance Access published November 18, 2012.
- Beale, Sara Sun. 2009. "Rethinking the Identity and Role of United States Attorneys." *Ohio State Journal of Criminal Law* 6: 369–439.
- Becker, Gary S. 1957. *The Economics of Discrimination*. University of Chicago Press.
- Belser, Ann. 1999. "Prosecutor implicates Washington County district justice in video gambling operation." *Pittsburgh Post-Gazette*, October 6, 1999.
- Berdejo, Carlos, and Daniel L. Chen. N.d. "Priming Ideology: Electoral Cycles Among Unelected Judges." Unpublished manuscript.
- Besley, Timothy, and Abigail Payne. 2003. "Judicial accountability and economic policy outcomes: Evidence from employment discrimination charges." Unpublished manuscript.
- Cameron, A. Colin, and Pravin K. Trivedi. 2010. *Microeconometrics using Stata*. Stata Press.

- Canes-Wrone, Brandice, Tom S. Clark, and Jee-Kwang Park. 2012. "Judicial Independence and Retention Elections." *Journal of Law, Economics, and Organization* 28 (2): 211–234.
- Cohen, Marty, David Karol, Hans Noel, and John Zaller. 2009. *The party decides: Presidential nominations before and after reform*. University of Chicago Press.
- Conte, Michaelangelo. 2012. "Former Jersey City pol says corruption charges against him part of effort to elect Christie governor." *The Jersey Journal*, January 26, 2012. Downloaded September 13, 2013 from http://www.nj.com/hudson/index.ssf/2012/01/former_jersey_city_pol_says_co.html.
- Dominguez, Casey BK. 2011. "Does the party matter? Endorsements in congressional primaries." *Political Research Quarterly* 64 (3): 534–544.
- Eisenstein, James. 1978. *Counsel for the United States: US attorneys in the political and legal systems*. Johns Hopkins University Press Baltimore, MD.
- Houston Chronicle*. 1996. "State briefs." February 16, 1996.
- Epstein, David, and Sharyn O'Halloran. 1994. "Administrative procedures, information, and agency discretion." *American Journal of Political Science* 38 (3): 697–722.
- Federal Judicial Center. N.d. "History of the Federal Judiciary." <http://www.fjc.gov/history/home.nsf/page/export.html>.
- Gailmard, Sean. 2009. "Multiple principals and oversight of bureaucratic policy-making." *Journal of Theoretical Politics* 21 (2): 161–186.
- Gailmard, Sean, and John W. Patty. 2012. "Formal Models of Bureaucratic Politics." *Annual Review of Political Science* 15 (1).
- Gordon, Sandy C., and Gregory A. Huber. 2007. "The effect of electoral competitiveness on incumbent behavior." *Quarterly Journal of Political Science* 2: 107–138.

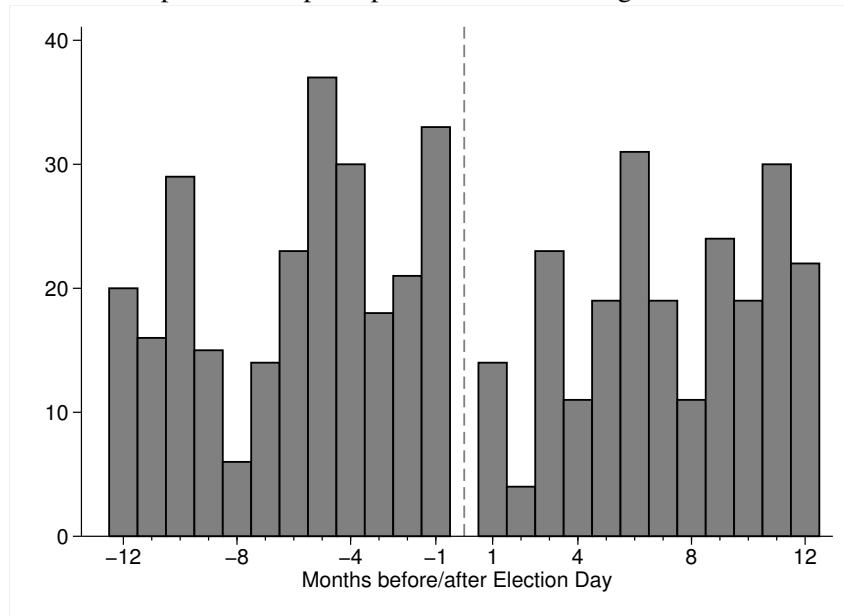
- Gordon, Sanford C. 2009. "Assessing partisan bias in federal public corruption prosecutions." *American Political Science Review* 103 (4): 534–554.
- Gordon, Sanford C., and Gregory A. Huber. 2009. "The Political Economy of Prosecution." *Annual Review of Political Science* 5: 135–156.
- Green, Bruce A., and Fred C. Zacharias. 2008. "US Attorneys Scandal and the Allocation of Prosecutorial Power, The." *Ohio State Law Journal* 69: 187.
- Heidenheimer, Arnold J. 1990. "Problems of Comparing American Political Corruption." In *Political Corruption: A Handbook*, ed. Arnold J. Heidenheimer, Michael Johnston, and Victor T. LeVine. Transaction Books.
- Huber, G., and S.C. Gordon. 2004. "Accountability and Coercion: Is Justice Blind When It Runs for Office?" *American Journal of Political Science* 48 (2): 247–263.
- Imbens, Guido, and Karthik Kalyanaraman. 2012. "Optimal Bandwidth Choice for the Regression Discontinuity Estimator." *The Review of Economic Studies* 79 (3): 933–959.
- Imbens, Guido, and Thomas Lemieux. 2008. "Regression Discontinuity Designs: A Guide to Practice." *Journal of Econometrics* 142: 615–635.
- Johnston, David. 2007. "Inquiry Into Ouster of U.S. Attorneys Moves Toward Subpoenas at Justice Department." *New York Times*, March 8, 2007.
- Kessler, Daniel P., and Anne Morrison Piehl. 1998. "The role of discretion in the criminal justice system." *Journal of Law, Economics, and Organization* 14 (2): 256–276.
- Kornacki, Steve. 2011. "Why Bob Menendez really can't stand Chris Christie." Salon.com, September 30, 2011.
- Layman, Geoffrey C., Thomas M. Carsey, John C. Green, Richard Herrera, and Rosalyn Cooperman. 2010. "Activists and conflict extension in American party politics." *American Political Science Review* 104 (2): 324–346.

- Lee, David S., and Thomas Lemieux. 2010. "Regression Discontinuity Designs in Economics." *Journal of Economic Literature* 48: 281–355.
- Lichtblau, Eric. 2007. "Former Prosecutor Says Departure Was Pressured." *New York Times*, March 6, 2007.
- Maass, Arthur. 1987. "U.S. Prosecution of State and Local Officials for Political Corruption: Is the Bureaucracy out of Control in a High-Stakes Operation Involving the Constitutional System?" *Publius: The Journal of Federalism* 17 (3): 195–230.
- McCarty, Nolan, Keith T. Poole, and Howard Rosenthal. 2008. *Polarized America: The dance of political ideology and unequal riches*. MIT Press.
- McCrary, Justin. 2008. "Manipulation of the running variable in the regression discontinuity design: A density test." *Journal of Econometrics* 142 (2): 698–714.
- Meier, Kenneth J., and Thomas M. Holbrook. 1992. "'I seen my opportunities and I took 'em': Political corruption in the American states." *Journal of Politics* 54 (1): 135–155.
- Miller, Gary J. 2005. "The political evolution of principal-agent models." *Annual Review of Political Science* 8: 203–225.
- Murphy, Bruce. 2007. "A Saint No More." *Milwaukee Magazine*, November 7, 2007.
- Nichols, Austin. 2011. "rd 2.0: Revised Stata module for regression discontinuity estimation. rd 2.0: Revised Stata module for regression discontinuity estimation." <http://ideas.repec.org/c/boc/bocode/s456888.html>.
- Perry, H.W. 1998. "United States Attorneys: Whom Shall They Serve?" *Law and Contemporary Problems* 61 (1): 129–148.
- Rehavi, M. Marit, and Sonja Starr. N.d. "Racial Disparity in Federal Criminal Sentences." Forthcoming, *Journal of Political Economy*.

- Rottinghaus, Brandon, and Chris Nicholson. 2010. "Counting Congress In: Patterns of Success in Judicial Nomination Requests by Members of Congress to Presidents Eisenhower and Ford." *American Politics Research* 38 (4): 691–717.
- Schultze, Steve. 2013. "Ex-Walker aide Timothy Russell sentenced to 2 years in prison for veterans theft." *Milwaukee Journal-Sentinel*, January 22, 2013.
- Shields, Donald C., and John F. Cragan. 2007. "The Political Profiling of Elected Democratic Officials: When Rhetorical Vision Participation Runs Amok." ePluribus Media. Downloaded August 23, 2012 from http://www.epluribusmedia.org/columns/2007/20070212_political_profiling.html.
- Starr, Sonja B., and M. Marit Rehavi. 2013. "Mandatory Sentencing and Racial Disparity: Assessing the Role of Prosecutors and the Effects of Booker." *Yale Law Journal* 123 (1): 2–81.
- Trahan, Jason. 2009. "Southern Dallas ministers ask Washington for fresh review of City Hall bribery case." *Dallas Morning News Crime Blog*, June 18, 2009.
- U.S. Department of Justice. 2008. "An Investigation into the Removal of Nine U.S. Attorneys in 2006." Released September 2008.
- U.S. Department of Justice. 2012. "United States Attorneys' Mission Statement." Downloaded August 23, 2012 from <http://www.justice.gov/usao/about/mission.html>.
- Whitford, Andrew B. 2002. "Bureaucratic discretion, agency structure, and democratic responsiveness: The case of the United States attorneys." *Journal of Public Administration Research and Theory* 12 (1): 3–27.
- Whitford, Andrew B., and Jeff Yates. 2003. "Policy signals and executive governance: Presidential rhetoric in the war on drugs." *Journal of Politics* 65 (4): 995–1012.

Yalof, David Alistair. 2012. *Prosecution Among Friends: Presidents, Attorneys General, and Executive Branch Wrongdoing*. Texas A & M University Press.

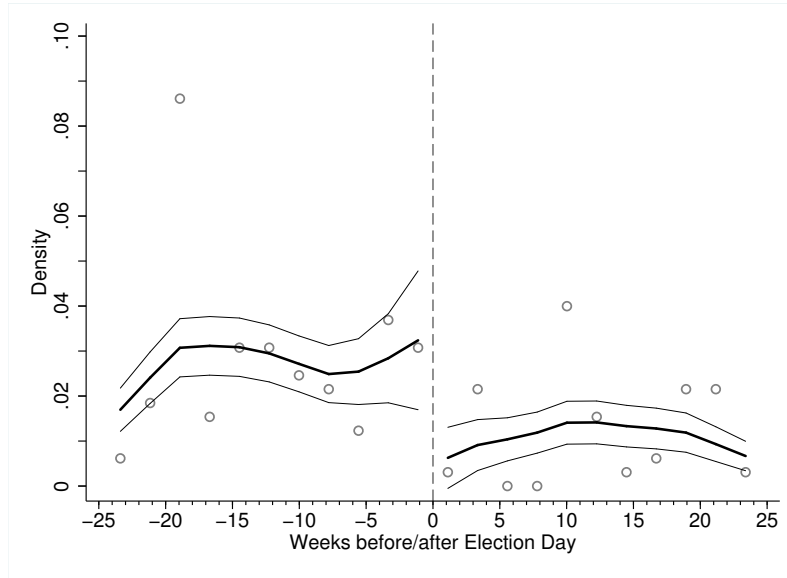
Figure 1: Partisan public corruption prosecution case filings over the electoral cycle



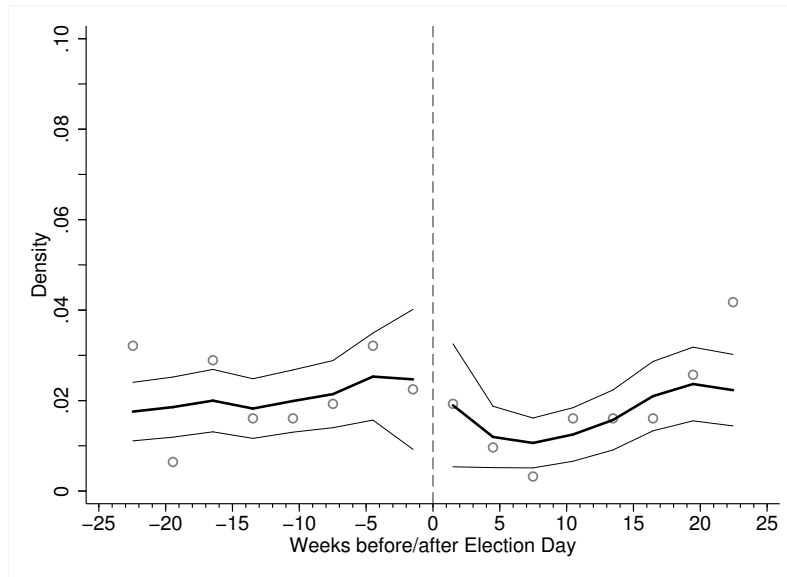
Sample: All federal criminal cases targeting state and local public corruption filed by U.S. attorneys between February 1993 and December 2008 and coded as national priorities in which the defendants were publicly identified as a member of a major party or a prominent associate of a well-known partisan. For each case, we calculated the number of weeks from the date the case was filed to the closest election (before or after) at the federal or state level. See Online Appendix A for further details.

Figure 2: Partisan differences in corruption case timing over the electoral cycle

(a) *Opposition party*



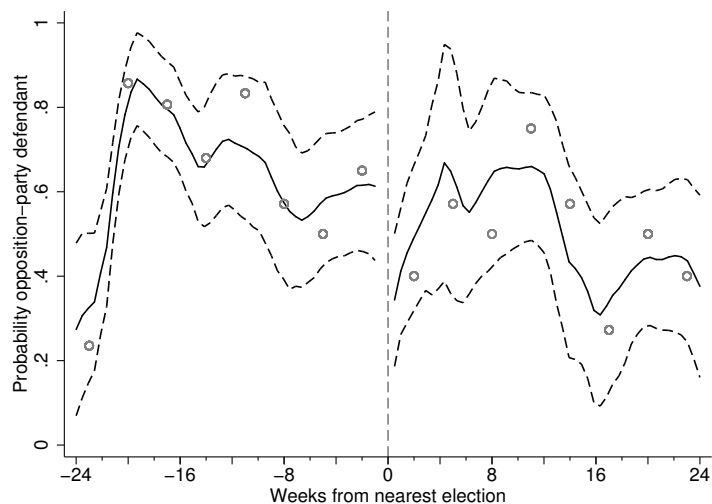
(b) *Same party*



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.

Sample: All federal criminal cases targeting state and local public corruption filed by U.S. attorneys between February 1993 and December 2008 within 24 weeks of an election and coded as national priorities in which the defendants were publicly identified as a member of a major party or as a prominent associate of a well-known partisan. For each case, we calculated the number of weeks to the closest election (before or after) the case filing at the federal or state level. See Online Appendix A for further details.

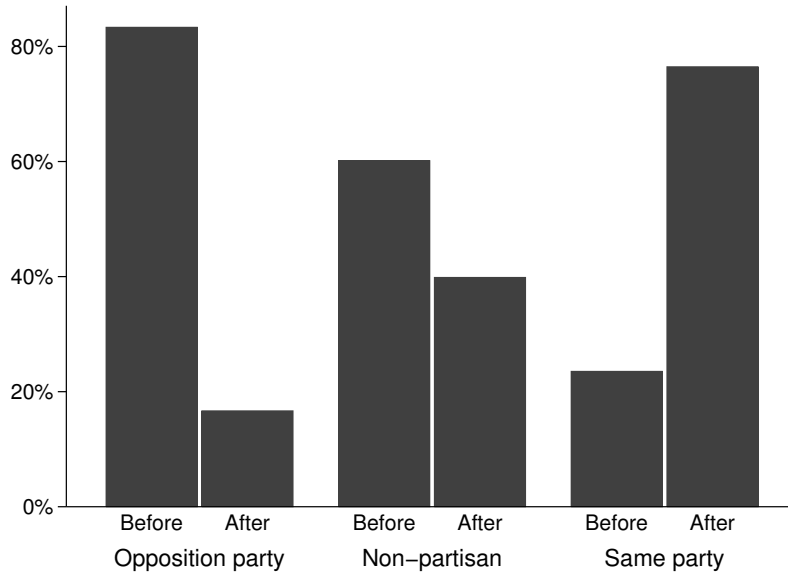
Figure 3: Opposition party prosecution probability over the election cycle



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.

Sample: All federal criminal cases targeting state and local public corruption filed by U.S. attorneys within 24 weeks of a state or federal election 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. See Online Appendix A for further details.

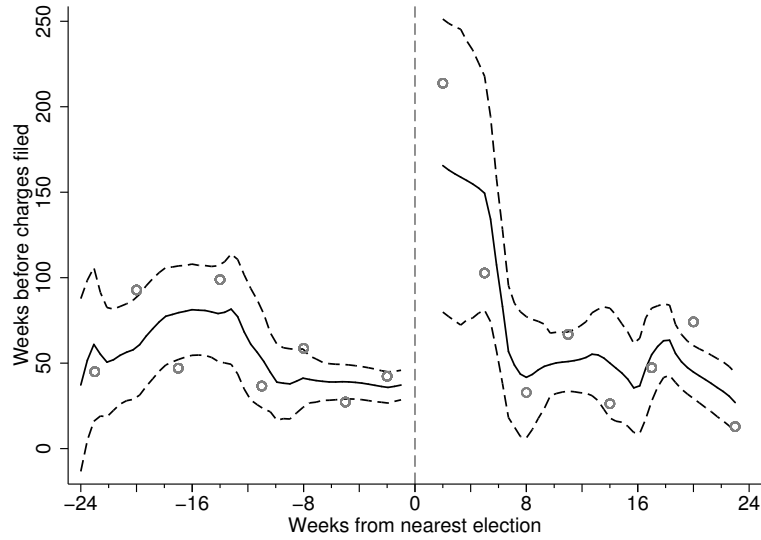
Figure 4: Immediate public corruption prosecutions by election timing



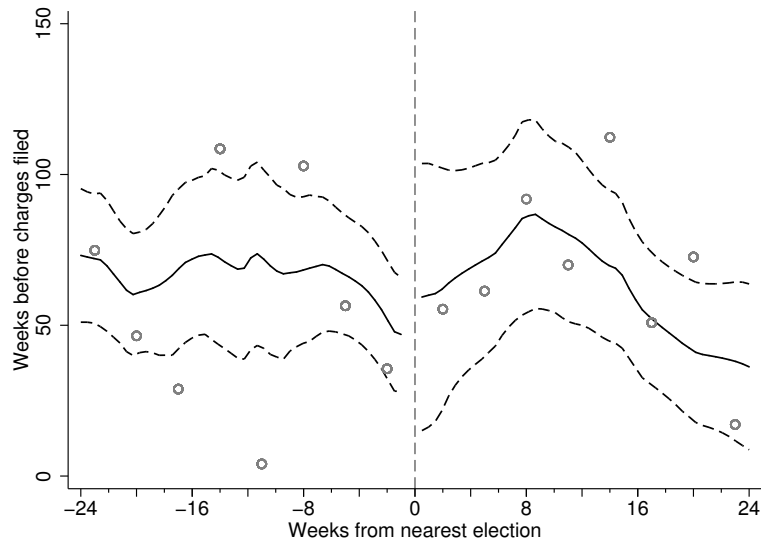
Sample: All federal criminal cases targeting state and local public corruption filed by U.S. attorneys between February 1993 and December 2008 within 24 weeks of an election and coded as national priorities in which charges were filed on the same date that the case was received. Opposition party case frequencies: 20 before, 4 after; non-partisan: 82 before the election, 55 after; same party: 4 before, 13 after. Partisan defendants are those who publicly identified as a member of a major party or a prominent associate of a well-known partisan. For each case, we calculated the number of weeks from the date the case was filed to the closest election (before or after) at the federal or state level. See Online Appendix A for further details.

Figure 5: Time to case filing by party over the electoral cycle

(a) *Opposition party*



(b) *Same party*

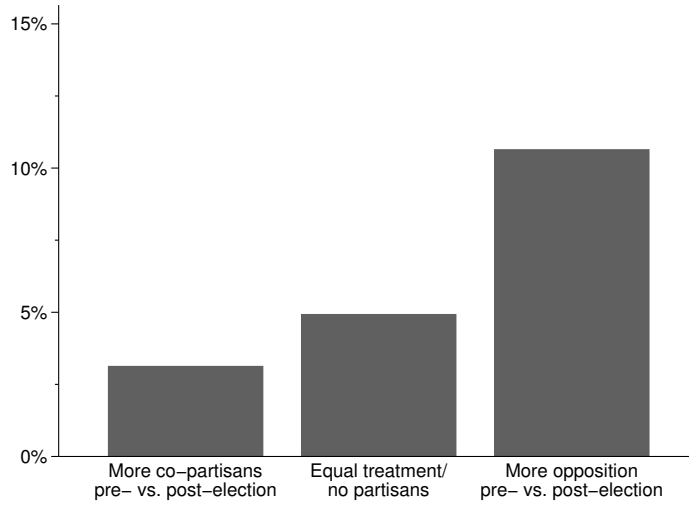


Local polynomial smoothing and 95% confidence intervals calculated using `lpolyci` in Stata (Epanechnikov kernel; rule-of-thumb bandwidth estimator). Bin means of the outcome variable are calculated over three-week intervals. Cases filed less than one week from Election Day are grouped with the intervals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

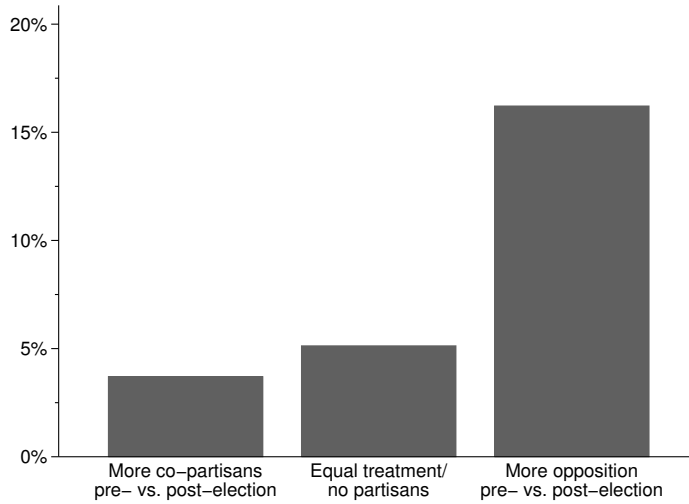
Sample: All federal criminal cases targeting state and local public corruption filed by U.S. attorneys between February 1993 and December 2008 within 24 weeks of an election and coded as national priorities in which the defendants were publicly identified as a member of a major party or as a prominent associate of a well-known partisan. For each case, we calculated the number of weeks to the closest election (before or after) the case filing at the federal or state level. See Online Appendix A for further details.

Figure 6: 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) listed in the PACER judicial database as representing the government in federal criminal cases targeting state and local public corruption filed between February 1993 and December 2008 that were coded as national priorities and the supervising U.S. attorneys in those cases. For each AUSA in the sample, we determined the number of cases they prosecuted against defendants who were publicly identified as a member of a major party or a prominent associate of a well-known partisan and compared the balance of prosecutions by party among those filed within 24 weeks of a federal or state election. An identical procedure was employed for cases filed under U.S. attorneys who took office during the Clinton and Bush administrations. Data on federal judges is from the Federal Judicial Center (N.d.). See Online Appendix A for further details.

Table 1: Summary statistics

	Same party		Opposition party		Non-partisan	
	Mean	SD	Mean	SD	Mean	SD
Charge characteristics						
Number of distinct charges	2.423	[1.437]	2.436	[1.613]	1.920	[1.349]
Total counts	5.572	[7.763]	8.464	[15.28]	4.893	[16.57]
Statutory max: most serious charge (months)	159.5	[80.37]	163.8	[83.98]	160.8	[79.13]
Case resolution						
Guilty of any charge	0.896	[0.307]	0.820	[0.385]	0.796	[0.403]
Number distinct charges pled guilty	0.881	[0.903]	0.865	[0.927]	0.846	[0.873]
Number counts pled guilty	1.886	[3.837]	2.318	[5.239]	2.117	[9.032]
Statutory max: most serious guilty plea (months)	171.6	[86.46]	168.1	[88.40]	148.3	[112.3]
Months of incarceration	20.19	[24.63]	17.84	[26.98]	18.96	[47.22]
Plea agreement	0.716	[0.452]	0.789	[0.409]	0.833	[0.373]
Sentencing departure	0.085	[0.279]	0.118	[0.323]	0.058	[0.235]
Timing						
Weeks from case received to filed	66.18	[79.04]	45.03	[64.77]	43.71	[58.50]
Number of defendants	201		289		2053	

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 (N.d.), 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)	-0.16+ (0.09)	-0.12 (0.10)
Constant	0.82** (0.07)	0.80** (0.09)	0.84** (0.09)	0.93** (0.07)
R ²	0.34	0.38	0.33	0.31
N	251	208	152	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.

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. For each case, we calculated the number of weeks to the closest election (before or after) the case filing at the federal or state level. See Online Appendix A for further details.

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*
	(0.21)	(0.22)	(0.22)	(0.22)
LLR optimal bandwidth	4.78	4.62	4.51	4.45
<u>Flexible polynomial RD (logit)</u>				
Election discontinuity	-0.59*	-0.68**	-0.61*	-0.45
	(0.28)	(0.23)	(0.29)	(0.35)
N	251	208	152	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 for logit models).

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. For each case, we calculated the number of weeks to the closest election (before or after) the case filing at the federal or state level. See Online Appendix A for further details.

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	0.34	0.18	0.20	0.08
	(0.21)	(0.24)	(0.26)	(0.26)
Opposition party \times post-election	0.16	0.46	0.58+	0.90**
	(0.26)	(0.30)	(0.33)	(0.32)
Constant	3.77**	3.93**	3.91**	2.87**
	(0.21)	(0.24)	(0.26)	(0.88)
N	251	208	152	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.

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. For each case, we calculated the number of weeks from the date the case was filed to the closest election (before or after) at the federal or state level. Weeks to file calculated from date case was received to charges being filed. See Online Appendix A for further details.

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)	0.06** (0.02)
More opposition pre-election vs. post-election	0.08+ (0.04)	0.15+ (0.08)
Constant	0.03 (0.02)	0.00 (0.00)
R ²	0.01	0.01
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) listed in the PACER judicial database as representing the government in federal criminal cases targeting state and local public corruption filed between February 1993 and December 2008 that were coded as national priorities and the supervising U.S. attorneys in those cases. For each AUSA in the sample, we determined the number of cases they prosecuted against defendants who were publicly identified as a member of a major party or a prominent associate of a well-known partisan and compared the balance of prosecutions by party among those filed within 24 weeks of a federal or state election. An identical procedure was employed for cases filed under U.S. attorneys who took office during the Clinton and Bush administrations. Data on federal judges is from the Federal Judicial Center (N.d.). See Online Appendix A for further details.

Table 6: Conviction rates (found guilty of one or more charges)

	Window around election (weeks)			
	24	20	16	12
Opposition party	-0.05 (0.07)	-0.05 (0.09)	-0.01 (0.09)	-0.00 (0.10)
Post-election	-0.04 (0.08)	-0.02 (0.10)	0.01 (0.11)	0.04 (0.13)
Opposition party \times post-election	-0.07 (0.09)	-0.07 (0.12)	-0.03 (0.13)	-0.11 (0.15)
Constant	1.04* (0.08)	1.02* (0.10)	0.99* (0.11)	0.78* (0.23)
R ²	0.13	0.14	0.12	0.10
N	251	208	152	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.

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 one of the major parties or as a prominent associate of a well-known partisan in state or local politics. For each case, we calculated the number of weeks from the date the case was filed to the closest election (before or after) at the federal or state level. See Online Appendix A for further details.

Table 7: Case outcomes before and after *Booker*

	<u>Sentence (months)</u>		<u>Plea deal</u>		<u>Govt. departure</u>	
	(1)	(2)	(3)	(4)	(5)	(6)
Opposition party	-9.46**	-10.05**	0.17**	0.17*	0.15**	0.14**
	(2.89)	(2.98)	(0.06)	(0.07)	(0.05)	(0.05)
Post- <i>Booker</i>	-7.15+	-6.81+	0.18*	0.18*	0.00	0.00
	(3.80)	(3.81)	(0.07)	(0.07)	(0.05)	(0.05)
Opposition party \times <i>Booker</i>	10.30*	9.44*	-0.19+	-0.20*	-0.27**	-0.27**
	(4.73)	(4.75)	(0.10)	(0.10)	(0.08)	(0.08)
Democrat	-2.72	-3.01	-0.06	-0.07	0.09*	0.09*
	(2.33)	(2.33)	(0.05)	(0.05)	(0.04)	(0.04)
Bush	6.97**	5.98*	-0.04	-0.05	0.16**	0.16**
	(2.33)	(2.35)	(0.05)	(0.05)	(0.04)	(0.04)
Proportion same-party judges		9.19+		0.10		0.03
		(5.52)		(0.09)		(0.06)
Constant	21.60**	18.12**	0.71**	0.68**	-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	490	490	490	490	490	490

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

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 one of the major parties or as a prominent associate of a well-known partisan in state or local politics. Sentence is measured in months of incarceration. Plea deal is a binary indicator for whether a plea agreement was not reached. Government departure is an indicator that the government requested a downward departure from guidelines at sentencing. See Online Appendix A for further details.