Tipping the Scales?
Testing for Political Influence on Public Corruption Prosecutions

Brendan Nyhan
Dartmouth College
nyhan@dartmouth.edu

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

Oct 2018

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. We document partisan differences in the *timing* of public corruption case filings. Opposition defendants are more likely to be charged immediately before an election rather than afterward. 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 or give favorable treatment to co-partisans. We instead find evidence that co-partisans receive *less* favorable treatment from prosecutors in plea bargaining and sentencing. These partisan disparities in case outcomes disappear when judges gain increased sentencing discretion following the Supreme Court's decision in *U.S. v. Booker*.

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, the Robert Wood Johnson Foundation Scholars in Health Policy Research Program and the Undergraduate Research Opportunity Program at the University of Michigan, and the Social Science Research Council of Canada. We thank Jim Alt, Mauricio Drelichman, Linda Fowler, Sanford Gordon, David Green, Don Green, Scott Hendrickson, Michael Herron, Eitan Hersh, Dave Hopkins, Dean Lacy, Jacob Montgomery, Matthew Stephenson, Francesco Trebbi, and seminar/conference audiences at the Canadian Institute for Advanced Research, Dartmouth College, HEC Montreal, MIT, the Midwest Political Science Association, NBER Summer Institute, the Harvard Political Economy Lunch, the American Political Science Association, the Society for Empirical Legal Studies, 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.

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.

How 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. Only filed cases and their attributes (e.g., sentences) that are themselves the product of discretionary prosecutorial decisions are observed.

We employ two novel empirical strategies to overcome these challenges. First, we focus on one of the earliest prosecutorial decisions 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 and cleaner test of partisan influence.

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 engaging in this behavior is correlated with future political appointments. 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 correspondence is observed among U.S. attorneys elevated to the federal judiciary.

These differences in case timing suggest that prosecutors are not immune from politi-

[1]For 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).

cal pressures and naturally raise concerns that partisan bias could also affect the type and strength of the cases that prosecutors file and how they choose to resolve them. We investigate this and do not find evidence of this using detailed evidence on case processing and resolution. Co-partisan defendants receive *longer* sentences than opposition party defendants. While differential selection (a higher threshold for charging co-partisans per Gordon (2009)) could generate this, we show that opposition and co-partisan defendants are charged with and convicted of crimes of comparable severity. We instead find evidence that these sentencing disparities arise because prosecutors offer less favorable plea deals to co-partisans. Specifically, they are *less* likely to recommend downward sentencing departures, 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 unobserved differences in criminal conduct and case strength, we exploit the sudden 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 sentencing 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 the higher sentences that their more serious crimes warrant after Booker. However, if the cases were comparable, 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. Their 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 control over U.S. attorneys (e.g., Beale 2009), substantial flexibility remains. Directly monitoring prosecutors and constraining their 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 Democrats and Reputblicans. U.S. attorneys are both officers 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, presidents likely 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.[2]

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 party[3] — 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, 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

[2]In 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)

[3]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."

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).[4]

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

[4]Corruption 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.

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),[5] 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 DoJ has 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

[5]Even in the absence of leaks, could undermine the effectiveness of ignoring a solid case. Most of the public corruption offenses charged are also crimes under state law. Law enforcement could pass any refused case onto local authorities for prosecution in state court.

defendants would have been treated differently if they were members of the other party — a counterfactual that is impossible to assess in specific cases (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 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. Both Alt and Lassen (2014) and Gordon (2009) make important contributions to the study of federal corruption prosecutions. 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 in case selection. 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 defendants in question. Defendants were identified using case characteristics including the

[6]Though 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.

[7]Public 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.

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

[8]We 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.

[9]In 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.

[10] We 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.

[11] The "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 the statistical tools developed for testing for manipulation in regression discontinuity (RD) estimation. These 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 θ = -1.66, s.e. = 0.73; $p < .05$) but not same-party defendants (θ = 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).

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

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), 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

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

Online Appendix B).

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

[14]We 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).

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 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]

[15]This 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.

[16]The 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).

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 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

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

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 calendar[18] 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: θ = -0.26, s.e. = 0.19; opposition defendants around placebo elections: θ = 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.

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

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. 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' subsequent 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:

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

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.

[19] We 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.

References

Alt, James E., and David Dreyer Lassen. 2014. "Enforcement and public corruption: evidence from the American states." *Journal of Law, Economics, and Organization* 30 (2): 306–338.

Bandyopadhyay, Siddhartha, and Bryan C. McCannon. 2014. "The effect of the election of prosecutors on criminal trials." *Public Choice* 161 (1-2): 141–156.

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.

Boylan, Richard T. 2005. "What do prosecutors maximize? Evidence from the careers of US attorneys." *American Law and Economics Review* 7 (2): 379–402.

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.

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 Sepetember 13, 2013 from `http://www.nj.com/hudson/index.ssf/2012/01/former_jersey_city_pol_says_co.html`.

Cordis, Adriana S., and Jeffrey Milyo. 2016. "Measuring public corruption in the United States: Evidence from administrative records of federal prosecutions." *Public Integrity* 18 (2): 127–148.

Dominguez, Casey BK. 2011. "Does the party matter? Endorsements in congressional primaries." *Political Research Quarterly* 64 (3): 534–544.

Downey, Mitch. 2016. "Losers Go to Jail: Congressional Elections and Union Officer Prosecutions." Unpublished manuscript. Downloaded March 20, 2017 from `http://econweb.ucsd.edu/%7Epmdowney/pdfs/wp/LosersGoToJail.pdf`.

Eisenstein, James. 1978. *Counsel for the United States: US attorneys in the political and legal systems*. Johns Hopkins University Press Baltimore, MD.

Gailmard, Sean. 2009. "Multiple principals and oversight of bureaucratic policy-making." *Journal of Theoretical Politics* 21 (2): 161–186.

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.

Gordon, S.C. 2011. "Politicizing Agency Spending Authority: Lessons from a Bush-era Scandal." *American Political Science Review* 105 (04): 717–734.

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.

Kriner, Douglas L, and Andrew Reeves. 2015. "Presidential Particularism and Divide-the-Dollar Politics." *American Political Science Review* 109 (1).

Lee, David S., and Thomas Lemieux. 2010. “Regression Discontinuity Designs in Economics.” *Journal of Economic Literature* 48: 281–355.

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.

McCannon, Bryan C. 2013. “Prosecutor elections, mistakes, and appeals.” *Journal of Empirical Legal Studies* 10 (4): 696–714.

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.” `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.

Reeves, Andrew. 2011. “Political disaster: Unilateral powers, electoral incentives, and presidential disaster declarations.” *Journal of Politics* 73 (4): 1142–1151.

Rehavi, M. Marit, and Sonja Starr. 2014. “Racial Disparity in Federal Criminal Sentences.” *Journal of Political Economy* 122 (6): 1320–1354.

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.

Scholz, John T., and B. Dan Wood. 1998. "Controlling the IRS: Principals, principles, and public administration." *American Journal of Political Science* 42 (1): 141–162.

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." September 2008. Downloaded November 23, 2015 from `https://oig.justice.gov/special/s0809a/final.pdf`.

U.S. Department of Justice. 2013. "Report to Congress on the Activities and Operations of the Public Integrity Section for 2013." Downloaded January 21, 2015 from `http://www.justice.gov/criminal/pin/docs/2013-Annual-Report.pdf`.

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.

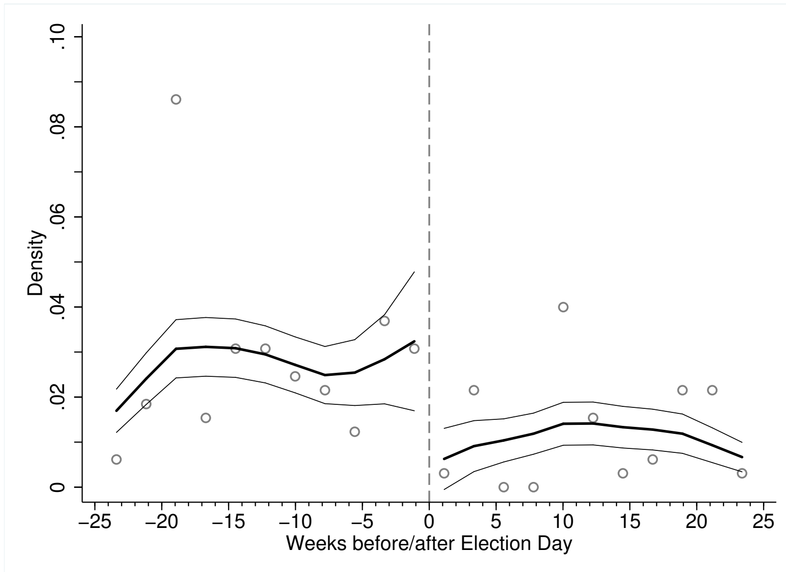
Whitford, Andrew B., and Jeff Yates. 2003. "Policy signals and executive governance: Presidential rhetoric in the war on drugs." *Journal of Politics* 65 (4): 995–1012.

Wood, B. Dan, and Richard W. Waterman. 1991. "The dynamics of political control of the bureaucracy." *American Political Science Review* 85 (3): 801–828.

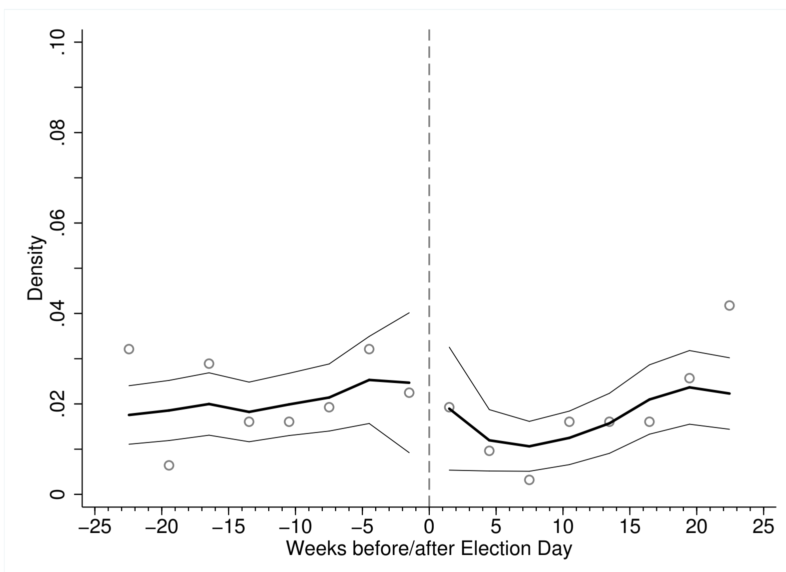
Yalof, David Alistair. 2012. *Prosecution Among Friends: Presidents, Attorneys General, and Executive Branch Wrongdoing*. Texas A & M University Press.

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

(a) *Opposition party*

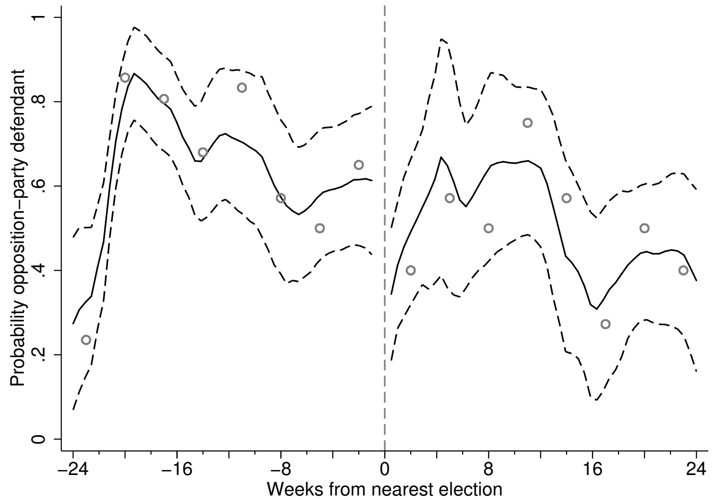


(b) *Same party*



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.

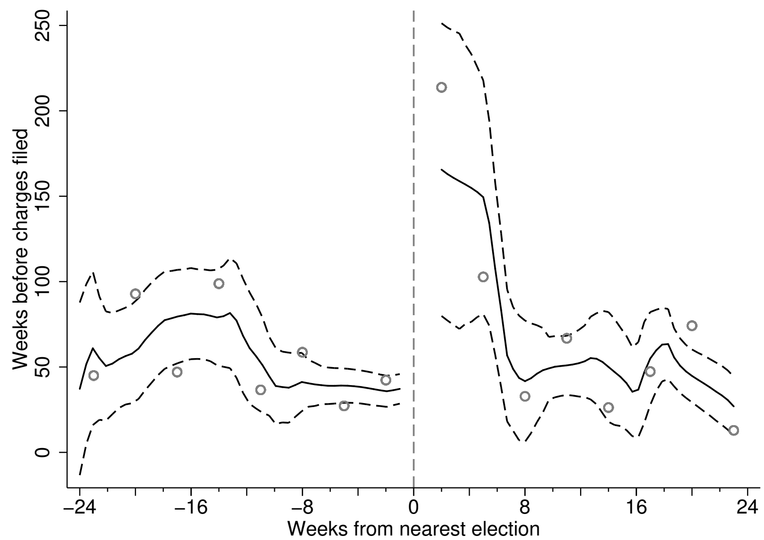
Figure 2: Opposition party prosecution probability over the election cycle



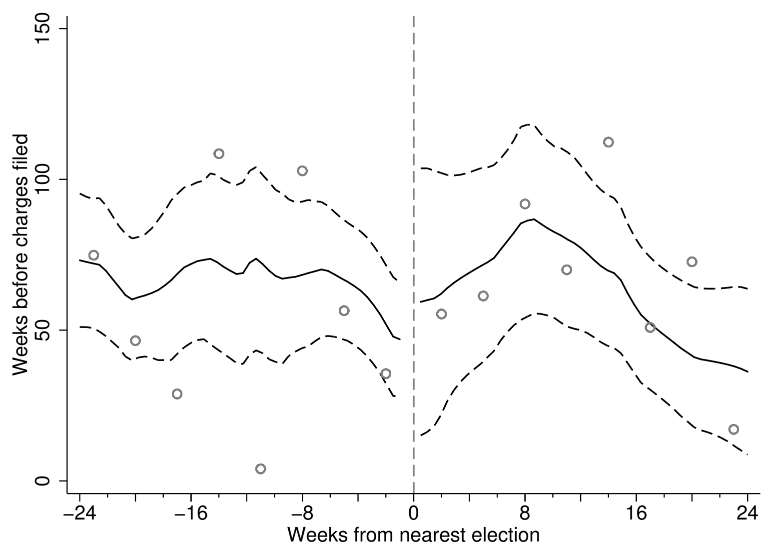
Local polynomial smoothing and 95% confidence intervals calculated using `lpolyci` in Stata (Epanechnikov kernel; rule-of-thumb bandwidth estimator). Bin means of the outcome variable are calculated over three-week intervals. Cases filed less than one week from Election Day are grouped with the intervals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

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

(a) *Opposition party*



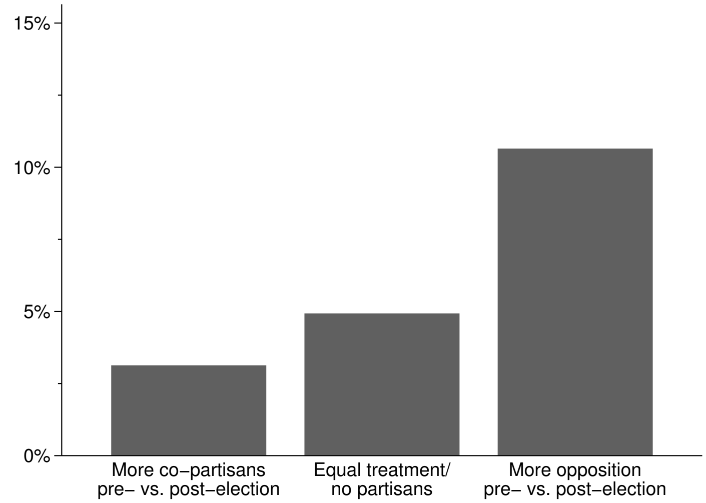
(b) *Same party*



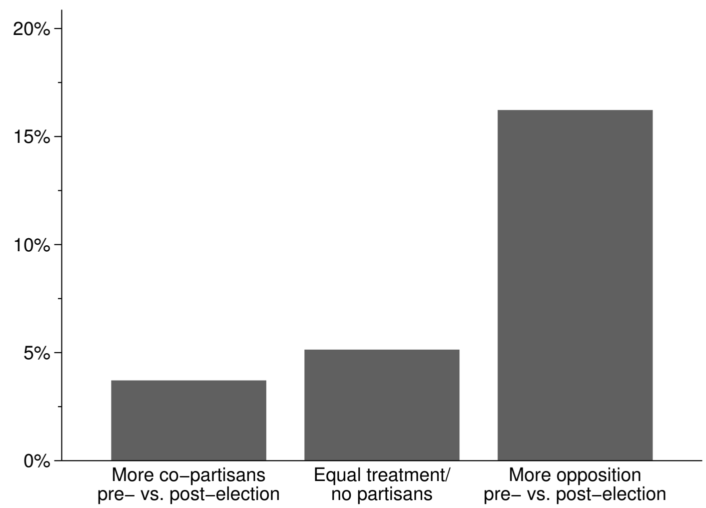
Local polynomial smoothing and 95% confidence intervals calculated using `lpolyci` in Stata (Epanechnikov kernel; rule-of-thumb bandwidth estimator). Bin means of the outcome variable are calculated over three-week intervals. Cases filed less than one week from Election Day are grouped with the intervals 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*	-0.20*	-0.15+	-0.12
	(0.07)	(0.09)	(0.09)	(0.10)
Constant	0.82	0.80	0.85	0.93
	(0.07)	(0.09)	(0.09)	(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
Local linear regression				
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
Flexible polynomial RD (logit)				
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 × 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 ⇒ USA	USA ⇒ judge
Equal treatment/no partisans	0.02	0.01
	(0.02)	(0.04)
More opposition pre-election vs. post-election	0.08+	0.13+
	(0.04)	(0.07)
Constant	0.03	0.04
	(0.02)	(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-*Booker*	-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 × *Booker*	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.

Online Appendix A: Data and coding procedures

Data source and processing

The federal corruption prosecution data come from the 2009 edition of the National Caseload Statistical Data (NCSD), an anonymized database that is regularly released by the Offices of the United States Attorneys at the Department of Justice under the Freedom of Information Act.[1] This dataset includes the universe of federal prosecution files and is effectively a snapshot of the DOJ database of cases (including cases filed and closed in previous years) as of the end of the 2009 fiscal year. We retain the non-appellate criminal cases within the fifty states[2] that were categorized by DOJ as pertaining to state, local, or other public corruption (i.e., federal public corruption cases were excluded[3]). We excluded the 171 charges listed as "opened in error." In order to avoid double-counting charges that were either superseded by a new filing or included in another case, we used the record from the final case that included the defendant in question.[4] Legally, public corruption can range from a government employee stealing office supplies to embezzlement and bribery. To focus on cases that are high-profile enough to have the potential for political repercussions, we follow Gordon (2009, 551) and restrict our attention to the cases coded as national priorities.[5] We therefore exclude 977 defendants who cases were coded as only district priorities, which Gordon (2009, 551) reports "are typically clerical

---

[1] Gordon (2009) uses data from the Transactional Records Access Clearinghouse and the Bureau of Justice Statistics but these secondary sources should be drawn from the raw data we accessed directly.

[2] Cases filed in territories such as Guam and Puerto Rico were excluded because partisan politics might be less salient or operate differently in those areas. Those filed in Washington, D.C. were also excluded because of the differing political environment (cases in D.C. might, for instance, have less direct effects on Congressional or state campaigns than those in the fifty states) and the possibility that cases filed there might come under closer scrutiny from or be more influenced by "Main Justice," the central administration of the Department of Justice.

[3] It is of course possible that U.S. attorneys are also biased in deciding whether to prosecute federal corruption cases that could damage their party. However, few members of the opposition party are presumably charged in such cases. We thus do not examine them here.

[4] The record includes each defendant's full case history. The charges that were eventually superseded are visible in the later case and are still used when analyzing the initial charges filed against the defendant.

[5] These were coded as either a national priority or as both national and district priorities.

workers," as well as the 2041 defendants whose cases were coded as neither a national or district priority or whose priority was undetermined.[6]

Defendant identification

Defendants were identified using the Public Access to Court Electronic Records (PACER) website (`www.pacer.gov`), a fee-based service provided by the federal courts to offer public access to electronic court records. Research assistants initially searched PACER for cases in which the United States was a party that were filed within two calendar days of the case filing date provided in the Department of Justice (DOJ) data. If no matches were found, they expanded the window to four days on either side of the case filing date.

They then matched cases in the DOJ data to PACER when possible using the case filing date, the number and type of charges against the defendant, the case closing date (if any), and the punishment (fine amount and/or months of probation/incarceration).[7] Additional steps were taken to match defendants in the DOJ data to PACER records in multiple defendant cases, including using separate spreadsheets to record information from PACER on all defendants and then match them to the DOJ records. A second research assistant blindly double-coded the most difficult cases, including multiple defendant cases and those for which defendants matched on two identifying variables, and resolved any discrepancies with the first coder and/or the authors

[6]We were concerned that some districts did not appear to use the national or national/district priority codes. As a validation step, we coded all 250 defendants from this group who were charged within 24 weeks of an election in a district that did not use the national or national/district priority codes for any public corruption defendants during an entire presidential administration (either Clinton or Bush). All but twenty of these 250 defendants were charged in New Jersey during the Bush years when then-U.S. attorney Chris Christie launched an unprecedented anti-corruption crusade (e.g., Sampson 2007). Of these, 80 were partisans and all were from New Jersey during the Bush administration. However, we observe no clear partisan patterns in case timing or severity around elections, which likely reflects the intense scrutiny that Christie received due to allegations that his efforts were politically motivated (e.g., Conte 2012).

[7]Due to a lack of case summary information in PACER, it was not possible to identify defendants in the following districts: California Central, Indiana South, Louisiana Middle, Nevada, New York East, Oregon, Texas West, and Virginia West. A lack of case summaries also precluded defendant identification for cases filed between December 16, 1993 and July 20, 1995 in Maryland.

to ensure that defendants were matched properly.[8] After matching the defendant, research assistants copied and pasted a series of fields from the PACER case summary into the data.

Defendant partisanship

Research assistants searched for the defendant in Lexis-Nexis Academic, Google, Google News, Proquest, and the list of federal candidates compiled by Open Secrets. When possible, they identified each defendant's job title or position, city, county, state, and the level of government in which they worked: federal government, state executive branch/bureaucracy, state legislative, local government, private/nonprofit, relative/personal relationship with accused, or a military or postal worker (excluded from federal category).

The research assistants also coded the public partisanship of each defendant and any supervisor, associate, or ally of the defendant who was mentioned in news accounts or official documents about the case using the same data sources used to identify the defendant. When possible, partisan codings were corroborated using data from Gordon (2009). It is important to note that partisan codings do *not* reflect party registration or other private behavior by defendant or their associates. Individuals were only coded as partisans if they were publicly identified as members of the Democratic or Republican party in news accounts or public documents or as associates of prominent partisans.

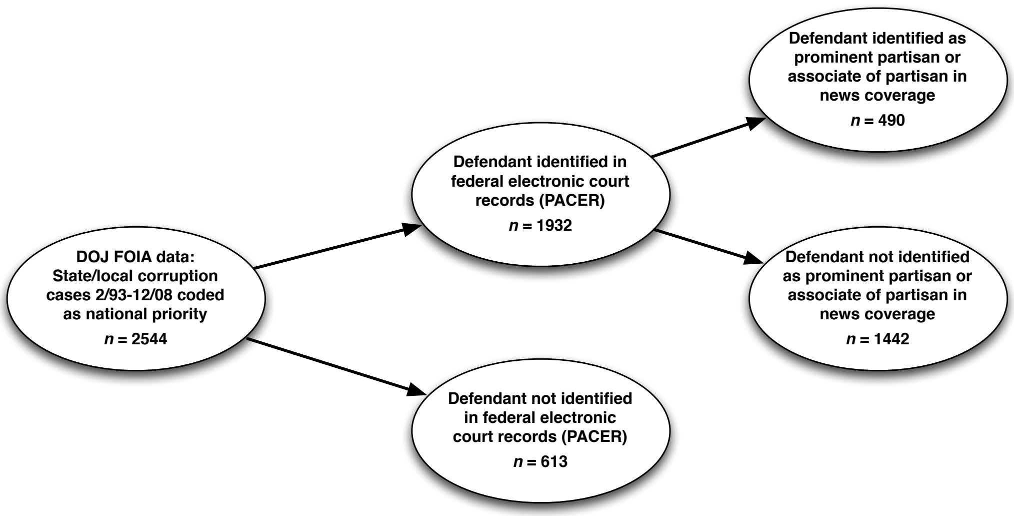
The data employed in the analysis above classifies as partisans both defendants in public corruption cases who were publicly identified with one of the major parties as well as defendants with ties to prominent partisan figures.

[8]Matches were allowed when minor discrepancies existed between the DOJ and PACER data if the PACER defendant data matched the DOJ defendant data on at least two identifying variables either and no other defendant in PACER did so. When too many discrepancies existed or a match could not be found, the identity of the defendant in the DOJ data was coded as missing. Minor date variation (e.g., five days or less) between the DOJ and PACER data was considered to reflect normal bureaucratic imprecision and delays in data entry. Charge/count variation between the DOJ and PACER data sometimes occurs and appears to reflect differences between charges filed (PACER) and those sustained (DOJ at least in some cases). The fact that a charge is listed in PACER but not DOJ is therefore relatively common.

Defendant data: Match rate and reliability

We were able to identify 1931 of the 2544 qualifying defendants (76%) spread across 1177 cases (out of 1336 total).[9] Of the 1932 identified defendants, 489 (25%) were publicly identified with one of the major parties (352 Democrat, 137 Republican) either individually (152), as an associate of a publicly-identified partisan (314), or as both a publicly-identified partisan and an associate of a partisan (23).[10] We illustrate the steps in this process of identifying partisan defendants from the set of qualifying public corruption prosecutions in Figure A1.

Figure A1: Partisan defendant identification procedure



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 or national and local priorities.

As a validation step, we merged our data with the replication files from Gordon (2009) and resolved any unintended discrepancies in defendant identification, party affiliation, or position among partisan defendants. After this step, we matched 94% of his defendants, including 99.4% of the partisans (one defendant appears to be omitted from our DOJ data). The sentencing data corresponds almost perfectly between datasets as

[9]These defendants represented 1903 unique individuals (27 were charged in two cases and one was charged in three). 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 (results available upon request).

[10]There are a total of 480 unique partisan defendants — nine appear in two separate cases.

well (98% on incarceration, probation, and fines among matching defendants). Finally, our coding matches Gordon's very closely on party identification (90% of those defendants who match across datasets) and public/private sector positions (95% of matching defendants).[11] A comparison of defendants we could identify with those that we could not indicates find that the defendants whom we could not identify faced fewer charges and counts and were found guilty less often and of less severe crimes (details available upon request).

One potential concern is that the availability or quality of information on defendants may vary over time. We find no significant relationship between year and the probability of defendant identification once we exclude the apparently anomalous outlier of 1994, which had a 45% match rate (details available upon request). A linear time trend does exist in the probability of partisan identification among the defendants whose names could be identified ($\beta = .01, p < .01$), which could reflect the growing availability of media sources in electronic form during the 1993–2008 period, increasing partisanship in the population of public corruption defendants, and/or partisans being more likely to be targeted by prosecutors in public corruption cases. However, unless partisan identification rates by year are correlated with individual case timing around elections, this time trend should not affect our results.

U.S. attorneys

The public corruption cases in our sample were matched to the U.S. attorney who was serving on the date that the case was filed using data on U.S. attorney tenures during the administrations of Bill Clinton and George W. Bush from the Department of Justice. We observe case filings by 220 U.S. attorneys who took office during the Clinton and Bush administrations in 75 judicial districts.[12] More than 1,000 were filed in ten judicial districts (California East: 81, Florida South: 85, Pennsylvania East: 88, Indiana North: 90, Mississippi South: 93, Michigan East: 104, Massachusetts: 105, Arizona: 115, New York South: 137, and Illinois North: 274). The 489 publicly identified partisans in the data are distributed somewhat more evenly across 60 judicial districts. The most cases against partisans were filed in Illinois North (102), Pennsylvania East

[11] The remaining differences appear to reflect slight variations in coding procedures.

[12] The supervising U.S. attorney could not be identified in 52 cases out of a total of 2545. These cases were filed before the first U.S. attorney nominated by the Clinton administration took office in that district (our data on U.S. attorneys begin with the Clinton administration).

(38), and Indiana North (37). The 483 cases that could be matched to U.S. attorneys[13] were filed by 98 U.S. attorneys, but nearly half (48 of 98) filed cases against only one or two partisan defendants. The remaining 50 U.S. attorneys filed 420 cases against partisan defendants. The most prolific U.S. attorneys were Scott Lassar in Illinois North (43 partisan defendants from 1998–2001), Patrick Fitzgerald in Illinois North (33 from 2001–2010), Joe Van Bokkelen in Indiana North (30 from 2001–2007), Pat Meehan in Pennsylvania East (28 from 2001–2008), and Jim Burns in Illinois North (26 from 1993–1997).

Assistant U.S. attorneys and federal judges

We extract the set of assistant U.S. attorneys (AUSAs) listed in the PACER judicial database as representing the United States in the set of cases that constitute our sample (federal criminal cases targeting state and local public corruption filed between February 1993 and December 2008 that were coded as national priorities and for which the defendant could be determined). For each AUSA who prosecuted a public corruption case, 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. We then compared the balance of prosecutions by party among those cases filed against partisans within 24 weeks of a federal or state election, counting each case for each AUSA who represented the federal government (often more than one was listed). An identical procedure was employed for each U.S. attorney who took office during the Clinton and Bush administrations (see above). Specifically, we compared the partisan balance of all public corruption cases meeting the above criteria that were filed by the U.S. attorney's office under their supervision within 24 weeks of a federal or state or federal election. Data on federal judges was derived from Federal Judicial Center (N.d.). Name matching was used to link individuals across datasets; official biographies and *Who's Who* were consulted to resolve any remaining ambiguities (e.g., inconsistent use of nicknames).

[13] Six were filed before the first Clinton administration U.S. attorneys took office (five in California East, one in Kentucky East).

Election timing

For each case, we calculated the electoral distance variable to the closest election before or after the case filing date in the DOJ data (i.e., the minimum absolute value), which is the one we expect to be most salient.[14] Since most state elections coincide with federal elections, this variable measures the number of weeks until or since the closest federal election except for a subset of cases in the five states with off-year electoral cycles (Kentucky, Louisiana, Mississippi, New Jersey, and Virginia). For those five states, the closest election was a state gubernatorial or legislative election for 153 of 200 defendants. The resulting electoral distance variable ranges from -365 (a case filed on November 8, 1999 in West Virginia South — approximately one year before the 2000 federal elections) to 366 (two cases, including one filed in Missouri East on November 4, 1993— one year and one day after the 1992 federal elections). As described in the main text, we round our electoral distance variable down to the nearest complete week from the election. This week variable ranges from -52 to 52. Cases filed less than 7 days before and after the election were classified as 0.5 and -0.5, respectively.

Figure A2 summarizes the distribution of public corruption case filings over the electoral cycle for those defendants whom we identified as partisans.[15] For visual clarity, we use bins of thirty days (approximately one month) where -1 represents the month before Election Day and +1 represents the month after Election Day.[16]

Figure A3 presents the distribution of these cases for all defendants.

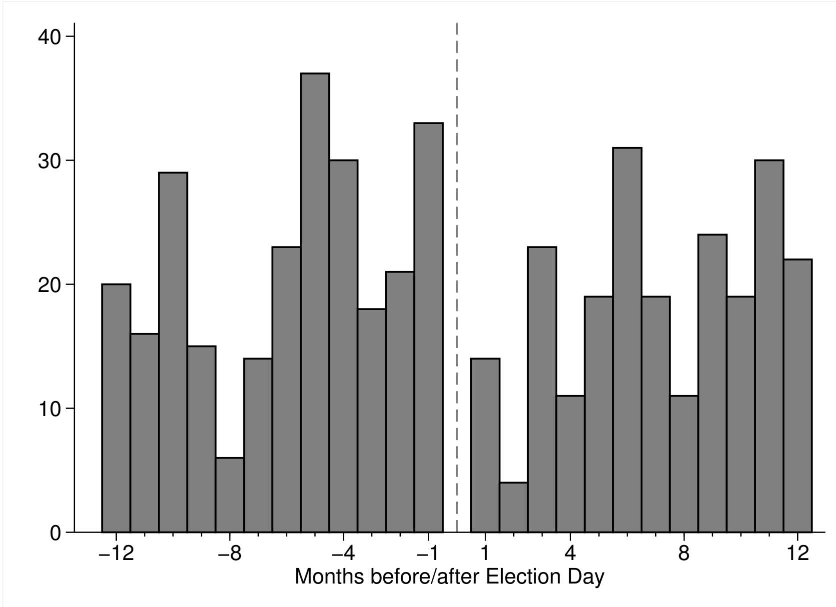
We also construct placebo election dates on the first Tuesday of November in off-years for partisan defen-

[14]The DOJ data only provide one case filing date. For cases with superseding indictments, the filing date may be the date of the most recent filing in the case. These are a relatively small proportion of the sample — 22% of all identified defendants and 24% of partisans have a superseding count in the charges listed in PACER. We expect that the incentives that affect the filing of new charges around an election should also apply to filing additional or updated charges in the period around an election. To the extent that the superseding indictments are otherwise not sensitive or consequential, however, their presence should add noise to the data and make it more difficult for us to find significant results.

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

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

Figure A2: 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.

dants charged with public corruption in the 45 states that hold state elections on the federal election calendar (excludes Kentucky, Louisiana, Mississippi, New Jersey, and Virginia) and estimate the number of weeks to the closest placebo election for these defendants. This measure is constructed analogously to the main electoral distance measure.

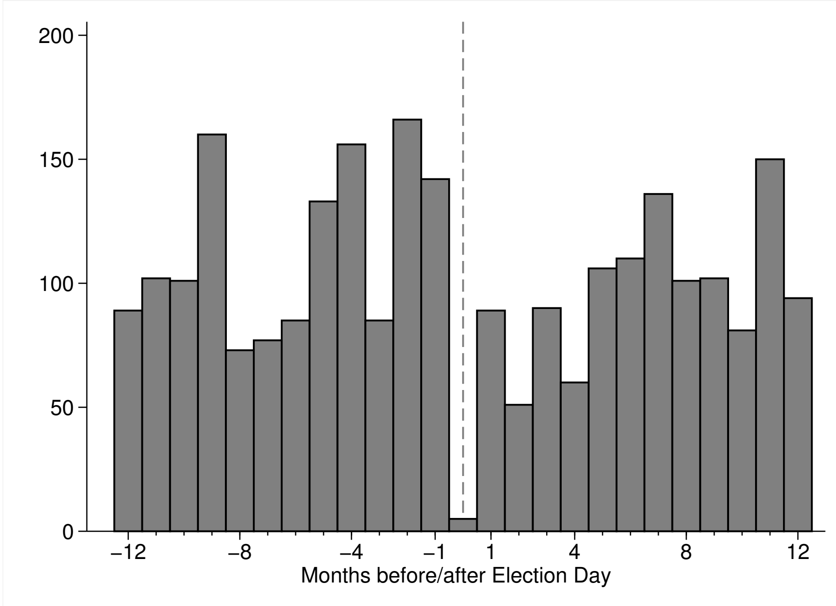
Weeks to file

We calculate the number of days elapsed from the date the case was recorded as being received by DOJ to the date that the prosecutor filed charges. A histogram of this measure, which is rounded to the nearest complete week, appears below (the 19 cases in which more than 300 weeks elapsed are collapsed in the rightmost bin).

Charge severity

In both government databases and court documents, criminal charges are recorded using the exact section of the U.S. Code that the defendant is accused of violating. For example, a charge of 18:1347A refers to Title

Figure A3: Public corruption prosecution case filings over the electoral cycle (all defendants)



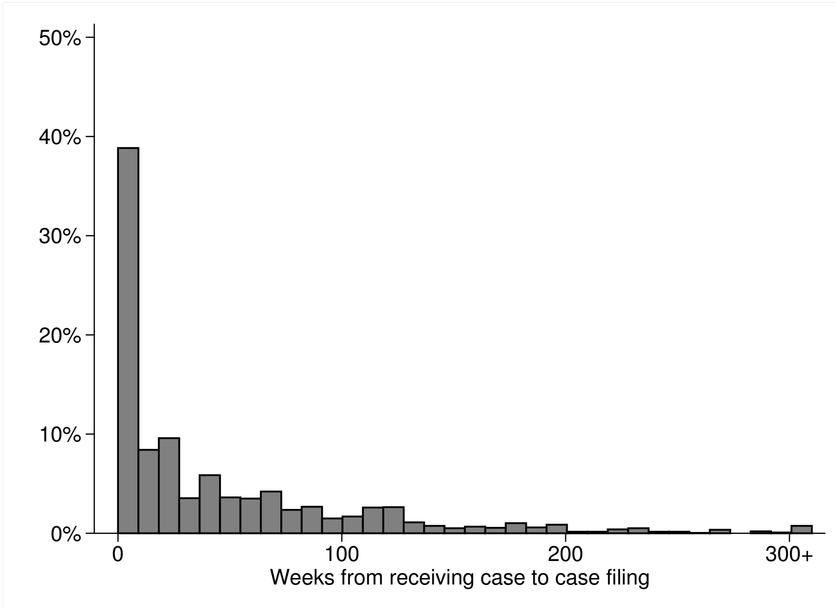
Sample: All federal criminal cases targeting state and local public corruption filed by U.S. attorneys between February 1993 and December 2008 and coded as national priorities. For each case, we calculated the number of weeks from the date the case was filed to the closest election (before or after) at the federal or state level.

(18), Section (1347), Subsection (A) of the U.S. Code. When the relevant code has numerous subsections and paragraphs, the exact reference will be indicated in the charge by an additional series of lower case letters and numbers enclosed in parentheses. Lastly, the category (F for felony or M for misdemeanor) indicates whether the individual was charged with the felony or misdemeanor version of the offense when both exist for that crime. The severity of the individual charges filed against a defendant, the lead charge in the defendant's case, and the individual charges sustained against each defendant were quantified by matching each charge to the the charge severity measures developed by Rehavi and Starr (2014), which provide the maximum potential sentence under the law for every criminal charge used by DOJ since 2000 (i.e., the statutory maximum; see the data appendix in Rehavi and Starr 2014 for a detailed description).

All charges filed/sustained

The NCSD includes detailed information on every charge ever filed against a defendant (including those that were dropped or superseded). Using the charge severity matrix from Rehavi and Starr (2014), we calculated the maximum potential sentence among all charges filed against each defendant as well as the potential

Figure A4: Time to case filings



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 or national and local priorities.

maximum among all charges that were sustained. Because most federal sentences are served concurrently, this measure calculates the maximum severity of the charges filed against each defendant as well as the maximum sustained charge.

Election-related offenses

According to the federal handbook for prosecuting election-related crime (Donsanto and Simmons 2007), the following statutes can be used to charge election-related offenses: conspiracy against rights (18 U.S.C. § 241), deprivation of rights under color of law (18 U.S.C. § 242), false information in and payments for registering and voting (42 U.S.C. § 1973i(c)), voting more than once (42 U.S.C. § 1973i(e)), voter intimidation (42 U.S.C. § 1973gg-10(1), 18 U.S.C. § 594, 18 U.S.C. § 610, 18 U.S.C. § 241 and § 242, 18 U.S.C. § 245(b)(1)(A)), fraudulent registration or voting (42 U.S.C. § 1973gg-10(2)), voting by non-citizens (42 U.S.C. § 1973gg-10(2)), false claims to register to vote (18 U.S.C. § 911), and campaign "dirty tricks" (2 U.S.C. § 441d and 2 U.S.C. § 441h). While used to charge election-related crimes, these statutes can also be applied to non-election-related offenses. The frequencies presented in the main text are thus likely to overestimate the number of public corruption cases stemming from election-related crimes.

Online Appendix B: Robustness checks and additional results

McCrary (2008) density test

The McCrary test is typically used to examine whether the distribution of the "running" variable in regression discontinuity (RD) designs is continuous at the discontinuity. 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 constitutes *evidence* for manipulation of case timing. The null hypothesis for the test is that the log difference in heights of the estimated density is zero at the potential discontinuity. The density estimates are computed by binning the data in histograms on either side of the discontinuity and then smoothing those estimates using local linear regression.

Prosecutorial resources

Offices with more resources might bring more corruption cases (including against opposition defendants), but these resource differentials should not vary sharply according to the electoral calendar and thus would not confound our effect estimates.[17] Moreover, we show in column 2 of Table B4 that there is no evidence that our event study results vary by office resources (the results from Table 2 are presented for comparison in column 1). Specifically, we find no evidence that the relationship between case timing and defendant partisanship varies by whether offices have above-median levels of financial resources in FY 2007–2008 data.[18]

Additional time to charge results

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 after receiving

[17] Federal spending changes each fiscal year, but our falsification test provides no evidence of a seasonal effect (see main text).

[18] Due to data limitations, we are forced to use the only publicly available measures of U.S. attorney office resources for the period of our data, which cover fiscal years 2007 and 2008 (Wilber 2012). We expect that the resource differentials between offices we observe during this period are relatively persistent over time. (We are unable to use the resources measure in Alt and Lassen 2014 because it is proprietary.)

Table B1: Probability of opposition party defendant by election timing: Robustness tests

	Window around election (weeks)			
	24	24	24	24
Post-election	-0.18*	-0.20+	-0.19*	-0.24+
	(0.07)	(0.11)	(0.10)	(0.13)
High office resources		0.01		
		(0.07)		
Post-election × high office resources		0.03		
		(0.13)		
State A.G. from president's party			-0.01	
			(0.07)	
Post-election × state A.G. party match			0.03	
			(0.13)	
Non-presidential election				-0.07
				(0.15)
Post-election × non-presidential election				0.15
				(0.18)
Constant	0.18	0.16	0.19	0.16
	(0.07)	(0.09)	(0.10)	(0.08)
R^2	0.34	0.34	0.34	0.34
N	250	250	250	250
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. Budgets are the logged average of U.S. attorney office budgets for FY 2007–2008 data obtained via a Freedom of Information Act request (Wilber 2012).

them or the charges were the result of a prosecutor-led investigation. Figure B1 presents a simple bar graph demonstrating how the proportion of immediate case filings varies dramatically around elections by defendant partisanship. 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: $p < .01$).

Other case timing robustness tests

The results in Table B2 are virtually identical to Table 2 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.

Neither the non-partisan case filings in Figure B2(a) nor filings around placebo off-year election dates in Figure B2(b) show a discontinuous change in density at Election Day.

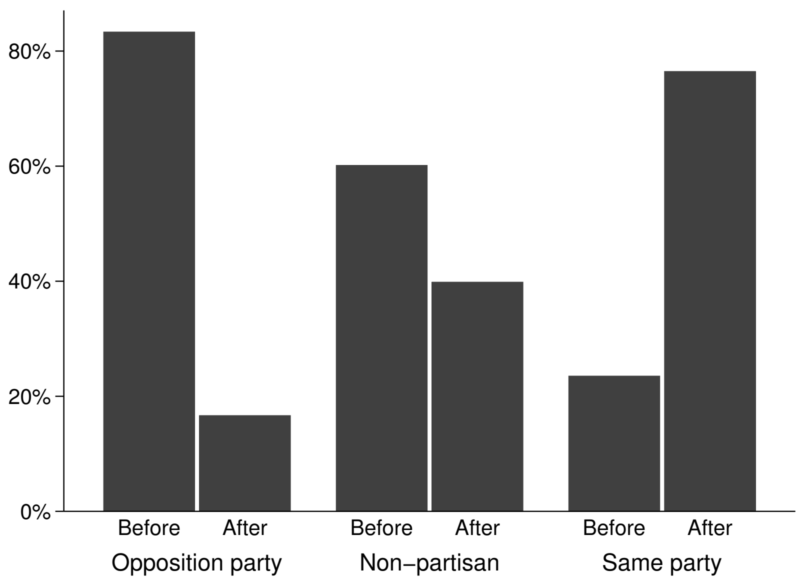
We also find no evidence that our results are driven by the timing of when cases are received by U.S. attorneys from law enforcement agencies. Tables B4 and B3 replicate Tables 2 and 3 in the main text excluding cases received within 12 weeks of an election.[19]

Testing for partisan disparities in case content and outcomes

We find no measurable partisan difference in conviction rates by case timing around elections. Our findings for changes in case resolution after *Booker* are consistent, though less precise, if we only consider cases charged before *Booker* but sentenced afterward. Our results also hold if we only consider cases charged before the U.S. attorneys scandal began.

[19]Unlike in the main text, we do not cluster on election cycle week in these results because the clustered standard errors become much smaller than conventional standard errors — the apparent result of the number of observed unique values of election cycle week dropping below 30 (Cameron, Gelbach, and Miller 2008).

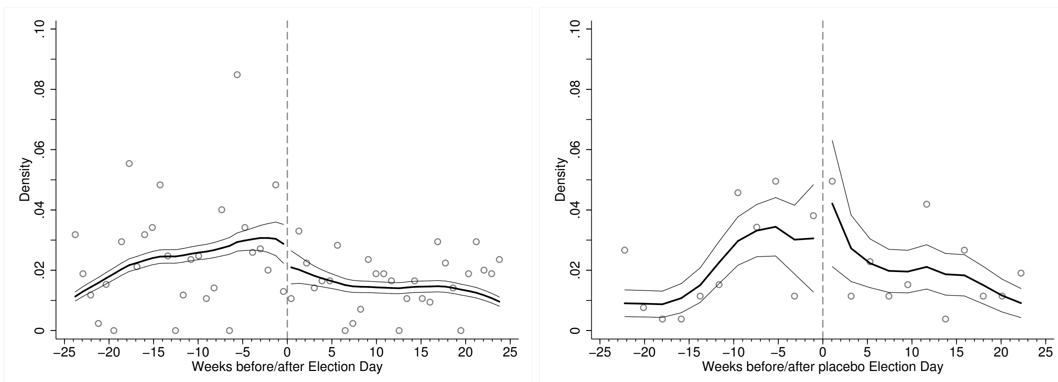
Figure B1: Immediate public corruption prosecutions by election timing



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 B2: Falsification tests for discontinuity in opposition corruption case timing

(a) *Non-partisan defendants* (b) *Opposition: Weeks to placebo elections*



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. The sample for subfigure (a) consists of all defendants who could not be publicly identified as a member of a major party or a prominent associate of a well-known partisan. For each case in this sample, we calculated the number of weeks to the closest election (before or after) the case was received at the federal or state level. The sample for subfigure (b) consists of all defendants in states that hold state elections on the federal election calendar (all but Kentucky, Louisiana, Mississippi, New Jersey, and Virginia) who could be publicly identified as opposition party members or as prominent associates of well-known opposition partisans in state or local politics. For each case in this sample, we calculated the number of weeks to the closest placebo election (early November in off-years) before or after the case was received.

Table B2: Post-election change in probability of opposition-party case

	Window around election (weeks)			
	24	20	16	12
Local linear regression				
Election discontinuity	-0.40*	-0.43*	-0.48*	-0.49*
	(0.19)	(0.19)	(0.19)	(0.19)
LLR 200% optimal bandwidth	9.56	9.25	9.02	8.90
Flexible polynomial RD (logit)				
Election discontinuity	-0.59*	-0.68**	-0.60*	-0.45
	(0.26)	(0.25)	(0.30)	(0.45)
N	250	207	151	113

+, *, and ** denote significance at the 10%, 5% and 1% levels, respectively. Local linear regression estimated in Stata 11 using `rd` (Nichols 2011) with 200% of 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 criminal case for logit models).

Sample consists of all federal criminal cases targeting state and local public corruption filed by U.S. attorneys during the February 1993–December 2008 period and coded as national or national and local 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.

Table B3: Post-election change in probability of opposition defendant excluding cases received near elections

	Window around election (weeks)			
	24	20	16	12
Local linear regression				
Election discontinuity	-0.29	-0.29	-0.60*	-0.59**
	(0.40)	(0.40)	(0.23)	(0.24)
LLR optimal bandwidth	3.23	3.28	4.84	4.52
Flexible polynomial RD (logit)				
Election discontinuity	-0.38	-0.62*	-0.66*	-0.65+
	(0.28)	(0.27)	(0.26)	(0.39)
N	189	157	107	79

+, *, 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.

Table B4: Probability of opposition defendant by election timing excluding cases received near elections

	Window around election (weeks)			
	24	20	16	12
After election	-0.15	-0.20	-0.19	-0.15
	(0.09)	(0.12)	(0.13)	(0.13)
Constant	0.15	0.20	0.19	0.65
	(0.09)	(0.12)	(0.13)	(0.30)
R^2	0.46	0.46	0.43	0.40
N	189	157	107	79
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. Excludes cases received within 12 weeks of an election.

Table B5: Conviction rates (found guilty of one or more charges)

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

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 B6: Case outcomes before and after *Booker* omitting cases charged afterward

	Sentence (months)		Convicted without plea		Govt. departure	
	(1)	(2)	(3)	(4)	(5)	(6)
Opposition party	-9.80**	-9.66**	-0.18**	-0.15*	0.15**	0.15**
	(3.01)	(3.05)	(0.07)	(0.06)	(0.06)	(0.06)
Post-*Booker*	-4.34	-4.51	-0.20**	-0.23**	0.02	0.02
	(4.98)	(5.05)	(0.07)	(0.08)	(0.05)	(0.06)
Opposition party × *Booker*	7.95	8.29	0.27*	0.34**	-0.25**	-0.25**
	(6.24)	(6.36)	(0.11)	(0.10)	(0.09)	(0.09)
Democrat	-3.13	-3.02	0.07	0.09	0.10*	0.10*
	(2.69)	(2.70)	(0.05)	(0.05)	(0.04)	(0.04)
Bush	7.64**	7.83**	0.07	0.11*	0.18**	0.18**
	(2.69)	(2.71)	(0.05)	(0.06)	(0.04)	(0.04)
Proportion same-party judges		-1.83		-0.35**		0.00
		(6.45)		(0.10)		(0.08)
Constant	21.83**	22.49**	0.28**	0.41**	-0.05	-0.05
	(2.85)	(3.90)	(0.06)	(0.07)	(0.04)	(0.06)
R^2	0.03	0.03	0.05	0.08	0.10	0.10
N	321	321	321	321	321	321

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

Table B7: Case outcomes before and after *Booker* omitting cases charged after U.S. attorneys scandal began

	Sentence (months)		Convicted without plea		Govt. departure	
	(1)	(2)	(3)	(4)	(5)	(6)
Opposition party	-9.25**	-9.52**	-0.18**	-0.16*	0.15**	0.14*
	(2.93)	(3.00)	(0.06)	(0.06)	(0.05)	(0.06)
Post-*Booker*	-6.92	-6.57	-0.22**	-0.24**	0.01	0.02
	(4.33)	(4.39)	(0.07)	(0.07)	(0.05)	(0.05)
Opposition party × *Booker*	11.22*	10.64*	0.25**	0.28**	-0.26**	-0.26**
	(5.25)	(5.31)	(0.10)	(0.09)	(0.09)	(0.09)
Democrat	-2.17	-2.35	0.06	0.07	0.10*	0.10*
	(2.46)	(2.46)	(0.05)	(0.05)	(0.04)	(0.04)
Bush	7.07**	6.72**	0.06	0.07	0.17**	0.17**
	(2.46)	(2.47)	(0.05)	(0.05)	(0.04)	(0.04)
Proportion same-party judges		3.71		-0.19*		0.04
		(5.77)		(0.09)		(0.07)
Constant	21.13**	19.77**	0.29**	0.36**	-0.04	-0.06
	(2.71)	(3.68)	(0.06)	(0.06)	(0.04)	(0.05)
R^2	0.02	0.02	0.04	0.05	0.08	0.08
N	412	412	412	412	412	412

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

Table B8: Charge and conviction severity by defendant partisanship before and after *Booker*

	Initial charge severity (maximum possible sentence)		Severity of charges of conviction (maximum possible sentence)	
	(1)	(2)	(3)	(4)
Opposition party	7.64	7.52	-1.27	-0.68
	(18.69)	(18.91)	(21.32)	(21.35)
Post-*Booker*	-2.96	-2.89	-9.18	-9.48
	(22.73)	(22.72)	(30.70)	(30.34)
Opposition party × *Booker*	10.92	10.74	9.21	10.11
	(25.61)	(25.47)	(37.99)	(37.72)
Democrat	8.55	8.50	-2.01	-1.67
	(17.42)	(17.53)	(20.45)	(20.39)
Bush	-3.11	-3.31	-9.36	-8.44
	(17.43)	(17.57)	(20.49)	(20.29)
Proportion same-party judges		1.85		-8.76
		(19.41)		(20.86)
Constant	193.54*	192.84*	178.46*	181.71*
	(18.79)	(19.54)	(20.48)	(21.51)
R^2	0.02	0.02	0.00	0.01
N	486	486	412	412
Mean of dependent variable	189.41	189.41	152.70	152.70

+, *, and ** denote significance at the 10%, 5% and 1% levels, respectively. Robust standard errors from OLS are in parentheses. Maximum possible sentence calculations are estimated using the approach in Rehavi and Starr (2014) and are expressed in months. These represent the maximum sentence allowed by statute for the most severe charge filed against a defendant (initial charge severity) and those for which they were found guilty (severity of charges of conviction). These values are often substantially greater than the sentences typically imposed in practice, but they provide a consistent relative ranking of the statutory severity of the charges that is free from confounds such as defendant demographics.

References

Alt, James E., and David Dreyer Lassen. 2014. "Enforcement and public corruption: evidence from the American states." *Journal of Law, Economics, and Organization* 30 (2): 306–338.

Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller. 2008. "Bootstrap-based improvements for inference with clustered errors." *The Review of Economics and Statistics* 90 (3): 414–427.

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 Sepetember 13, 2013 from `http://www.nj.com/hudson/index.ssf/2012/01/former_jersey_city_pol_says_co.html`.

Donsanto, Craig C., and Nancy L. Simmons. 2007. *Federal Prosecution of Election Offenses*. U.S. Department of Justice, Public Integrity Section.

Federal Judicial Center. N.d. "History of the Federal Judiciary." `http://www.fjc.gov/history/home.nsf/page/export.html`.

Gordon, Sanford C. 2009. "Assessing partisan bias in federal public corruption prosecutions." *American Political Science Review* 103 (4): 534–554.

Imbens, Guido, and Karthik Kalyanaraman. 2012. "Optimal Bandwidth Choice for the Regression Discontinuity Estimator." *The Review of Economic Studies* 79 (3): 933–959.

McCrary, Justin. 2008. "Manipulation of the running variable in the regression discontinuity design: A density test." *Journal of Econometrics* 142 (2): 698–714.

Nichols, Austin. 2011. "rd 2.0: Revised Stata module for regression discontinuity estimation." `http://ideas.repec.org/c/boc/bocode/s456888.html`.

Rehavi, M. Marit, and Sonja Starr. 2014. "Racial Disparity in Federal Criminal Sentences." *Journal of Political Economy* 122 (6): 1320–1354.

Sampson, Peter J. 2007. "Corruption on the run in N.J." *The Hackensack Record*, September 23, 2007.

Wilber, Del Quentin. 2012. "District U.S. Attorney's budget no longer secret." *Washington Post*, August 7, 2012. Downloaded January 23, 2015 from `http://www.washingtonpost.com/blogs/`

crime-scene/post/district-us-attorneys-budget-no-longer-secret/2012/08/07/d0fdf98e-e0a3-11e1-a421-8bf0f0e5aa11_blog.html.