Is justice blind? Evidence from federal corruption convictions

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Abstract

Are federal prosecutors influenced by partisan political concerns? We examine that question by analyzing 40 years of federal corruption convictions at the state and federal district levels. Our key finding is that state-level federal corruption convictions fall by roughly 9% in years when a state's governor belongs to the same party as the president who appointed local US Attorneys, a measure of state-federal political alignment. The result is robust to controls for the state political environment, election cycles, party tenure in the executive branch, public sector employment, federal aid to states, a state's electoral importance, and the changes in Honest Services law, the statutory basis for many federal corruption cases. Our results are consistent with a significant level of partisan prosecutorial bias on the part of US Attorneys. In a placebo test, we find no evidence that state-federal political alignment affects the total number of federal criminal convictions. That finding provides support for the mechanism that we propose, namely the partisan exercise of prosecutorial discretion, rather than the partisan allocation of prosecutorial resources across federal districts.

Keywords Partisanship · Separation of powers · Federal courts · Corruption · US attorneys · Political economy · Political rents · Political appointments

JEL Classification K14 · D73 · P48

1 Introduction

On March 10, 2017, Attorney General Jeff Sessions requested the resignations of 46 United States (US) Attorneys. Although half of the 94 Obama-appointed federal prosecutors already had resigned, the remaining 46 expressed shock, frustration, and irritation at the sudden announcement. Behind the attorney general's request, however, was a decades-old, distinctly political reality: it was the president's prerogative to appoint his own US Attorneys, and every president since Reagan has done just that when his predecessor belonged

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to a different party. President Reagan replaced 89 of Jimmy Carter's appointees, President Clinton in turn replaced them, and President George H. W. Bush replaced 88 of Clinton's appointees during his first 2 years in office. In a May 2009 statement announcing plans to nominate a batch of Obama nominees, Attorney General Eric Holder stated traditional presidential privilege in no uncertain terms, saying "elections matter—it is our intention to have the US Attorneys that are selected by President Obama in place" (Gerstein 2009).

The partisan tilt of the US Attorney[1] appointment process invites a series of important questions about the role of partisanship in the day-to-day activities of federal prosecutors. As political appointees, federal prosecutors occupy positions in which their actions are, in expectation, nonpartisan, but their own career ambitions towards elected office or federal judgeships might be aided by demonstrably partisan activity. However, US Attorneys' wide scope of discretion in determining which defendants to pursue, the charges filed against them, and the delay between investigative referrals and the pursuit of an indictment provides them with significant prosecutorial autonomy (Gordon 2009). The questions of whether, where, and to what extent US Attorneys allow partisan considerations to influence their prosecutorial decision-making motivate the study we present here.

This paper serves as a first step towards a broader investigation of *partisan prosecutorial bias*, which we define as prosecutorial behavior that is consistent with partisan political considerations. Such biases may arise for any of several reasons, including prosecutors' own personal political preferences, feelings of loyalty to the sitting president or other elected officials from a prosecutor's own party, or calculations of long- or short-term opportunities for career advancement that might result from the partisan pursuit of justice. Prosecutors' own career ambitions towards elected office, federal judgeships, or even lucrative positions in the private sector might be aided by demonstrably partisan activity. Owing to the size of the federal judiciary, it may be nearly impossible to draw a direct causal line from presidential wishes to the actions of his US Attorneys. However, it seems reasonable to assume that US Attorneys are aware that their president would benefit from selective enforcement, corruption cases could serve as a powerful means of demonstrating partisan loyalties—by vigorously prosecuting a member of the opposing party, or choosing not to prosecute co-partisans. US Attorneys occupy a position that is, "in a formal sense, plainly political: US Attorneys are appointed by the president with the advice and consent of the Senate, and they serve at the president's pleasure" (Beale 2009, p. 370). They are no doubt privy to *political* maneuvering, and therefore understand that convictions are valuable, but certain convictions may be more valuable than others, both to the president and to state-level elected officials.

We join a small but growing body of academic literature that began to identify evidence of partisan bias in the Justice Department's public corruption prosecutions in the wake of a 2007 scandal in which the Bush administration allegedly dismissed eight US Attorneys for failing to pursue corruption investigations against prominent Democrats. That scandal highlighted the fact that corruption cases can serve as a powerful means of demonstrating partisan loyalties—by vigorously prosecuting a member of the opposing party, or choosing

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[1] According the United States Department of Justice, United States ("US") Attorneys "serve as the nation's principal litigators under the direction of the Attorney General." Appointed by and serving at the discretion of, the President of the United States, US Attorneys are confirmed with the advice and consent of the United States Senate to serve four-year terms. Pursuant to 28 USC. § 457, US Attorneys have three statutory responsibilities, including "the prosecution of criminal cases brought by the Federal Government, the prosecution of civil cases in which the United States is a party, and the collection of debts owed the Federal Government which are administratively uncollectible."

not to prosecute co-partisans. Charges of corruption could prove particularly useful in painting the opposing party in a negative light, and the possibility that such charges would be pursued selectively is an important potential outcome of the prosecutorial appointment system.

Public corruption cases offer a useful window into the partisan exercise of prosecutorial discretion for two key reasons. First, federal prosecutors have a clear *incentive* to pursue a partisan version of justice in federal corruption cases. Public corruption cases are among the most politically salient types of legal proceedings, and have "the potential of becoming … high-profile … simply because [their] focus is on the conduct of a public official" (Justice Department 2013, p. 1). Research shows that US Attorneys prioritize career advancement over crime reduction (Glaeser et al. 2000) and pursue high-profile cases when possible (Gordon and Huber 2002). In addition, corruption charges can have significant electoral implications (Peters and Welch 1980; Welch and Hibbing 1997), suggesting that prosecutors may have incentives to exhibit partisan bias in such cases to curry political favor. That power can be leveraged to further individual career goals, including pursuit of elective office or federal judgeships. Moreover, Boylan (2005, p. 383) reports that many federal prosecutors pursue careers in which political capital matters. In a sample of 570 US Attorneys serving between 1969 and 2000, 9.12% found their next jobs as federal judges, 9.47% as other appointed federal officials, 7.9% as state officials, including judges, and 1.93% secured elective office. Thus, incentives for career advancement may lead to partisan prosecutorial bias in federal corruption cases.

Second, US Attorneys also have the *opportunity* to pursue partisan justice. The substantial literature on prosecutorial discretion establishes the significant autonomy afforded federal prosecutors in determining whether to pursue investigations, file charges against officials suspected of corruption, bring cases to trial, or allow defendants to strike plea bargains.[2] The vast majority of case referrals originate from the Federal Bureau of Investigation (FBI), but the decision to take up the case or drop it altogether lies with the US Attorney in each district.[3] Furthermore, the district attorneys retain jurisdiction over all public corruption cases, regardless of whether the defendant is a member of state or local government. Finally, while public corruption cases historically have been—and continue to be—a top priority of the Justice Department, which retains its own division—the Public Integrity Section (PIN)—within "Main Justice" in Washington to assist in the pursuit of corruption cases, district-level federal prosecutors have wide discretion in those cases, and PIN usually gets involved only in cases involving particularly sensitive material or defendants from multiple districts (Gordon 2009). As the arbiters of federal law within their districts, US Attorneys play important roles in policy implementation, "profoundly shap[ing] the number, type, and effectiveness of federal criminal prosecutions and federal law enforcement generally" (Eisenstein 1978, p. 14).

Existing work has examined partisan bias in the context of sentencing (Gordon 2009) and case file timing (Nyhan and Rehavi 2017). Both of those papers consider case-level

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[2] While many corruption cases are, in practice, handled by Assistant US Attorneys, that delegation of prosecutorial authority does not alter the basic logic of our argument, since US Attorneys control case assignments and could either retain control over politically sensitive cases or assign them to politically motivated Assistant US Attorneys.

[3] There are 94 federal judicial districts in the United States and its territories. Each of the 50 states has at least one district, as does the District of Columbia and Puerto Rico. A US Attorney is assigned to each district. The geographic composition of each judicial district is detailed in 28 USC. § 81–131.

Fig. 1 Federal corruption convictions and state-federal political alignment
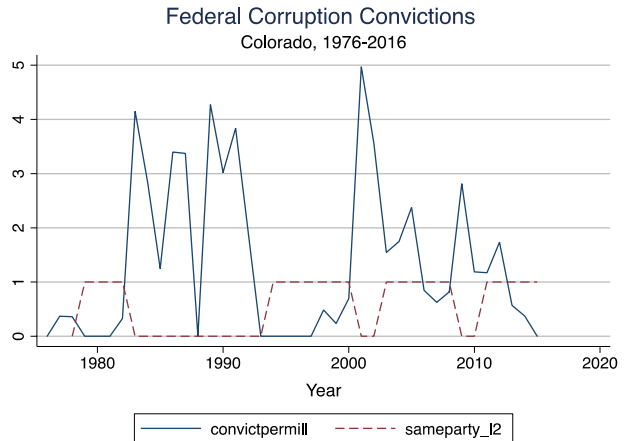
outcomes and identify partisan behavior by considering the party affiliations of the prosecutor and the defendant. Our investigation focuses instead on the extensive margin, the total number of federal public corruption convictions in a given state and year, and identifies the incentive for partisan behavior by considering a measure of state-federal political alignment, as determined by whether a state's governor belongs to the same political party as the US President. For presidents, governors can be powerful allies (or enemies) when it comes to policy implementation. In addition, gubernatorial appointment powers can influence party control of various state agencies and offices. Gubernatorial alignment with the White House may therefore be a powerful indicator of prosecutorial incentives in public corruption cases because well-placed corruption convictions may be potent political tools, benefitting co-partisan US Attorneys and governors alike. Alternately, strategic lenience by prosecutors may help to protect the party in power at the state level when its interests align with the DOJ and its appointees. Thus, we expect the number of federal public corruption convictions to fall when a state's governor belongs to the same political party as the US president.

Figure 1 illustrates this relationship for a single state. It shows federal corruption convictions per million inhabitants for Colorado from 1976 to 2015, along with a dummy variable for whether the governor of the state and the US President belong to the same political party. The indicator of state-federal political alignment is lagged 2 years to account for post-election administrative delays in the appointment of US Attorneys and the time needed for the prosecution of cases. Casual inspection suggests a negative correlation between federal corruption convictions and state-federal political alignment and, indeed, on average, 2.60 convictions per million are observed in years when the indicator is zero, versus 0.65 convictions per million in years when it is one, a difference that is statistically significant ($p = 0.000$). However, the pattern is only suggestive of partisan bias in federal corruption data. It might fail to hold for other states or be explained equally well by other factors.

We test our hypothesis using a panel of data on state-level federal public corruption convictions from 1976 to 2015. Data on the number of public corruption convictions are available at the district level from the *Annual Reports* of the Public Integrity Section of the Justice Department. We aggregate convictions data to the level of the state, since that is the unit of observation of local politics. We investigate the hypothesized relationship

using Poisson and negative binomial econometric models, which are designed to handle possibly "over-dispersed" and to be explained count data. In each case, we enter state and period fixed effects to control for the influence of unobserved heterogeneity across states and times.

Our primary finding is of an economically and statistically significant relationship between state-federal political alignment and the number of federal corruption convictions in a state. In particular, the number of public corruption convictions falls by roughly 9% when a state's governor belongs to the same political party as the president who appointed a state's US Attorneys. The result is robust to controls for a variety of measures of state political environments, including measures of public sector employment by state, federal aid to states, the political composition of state congressional delegations and of state legislatures, presidential vote share, and a measure of a state's importance in presidential elections. Our key finding also is robust to controls for a variety of time-varying factors that might influence public corruption convictions, including 2- and 4-year election cycles, the tenure of the presidential administration, and changes in the interpretation of the Honest Services law, which provides a legal standard used in many federal corruption cases. In addition, we investigate whether prosecutorial bias differs significantly across political parties. In all specifications, the effect is large, negative and significant for Republican administrations. In contrast, in most specification the effect for Democratic administrations is not estimated sufficiently precisely to draw strong conclusions. Finally, we conduct a placebo test, showing that state-federal political alignment does not predict the total number of criminal convictions in a state in a given year. That finding helps both to substantiate our primary claim and, as discussed further later, provides additional support for partisan prosecutorial bias as the mechanism underlying the empirical relationship we document.

The next section reviews the relevant literature. Section 3 introduces the data and discusses our econometric methodology. Section 4 presents our result; Sect. 5 concludes and summarizes the policy implications of our findings.

2 An overview of the relevant literature

Our examination of partisanship in federal corruption convictions contributes to a number of areas of active scholarly research. The literature on partisan prosecutorial bias presents strong, but inconclusive evidence that US Attorneys color their prosecutorial decisions in pursuit of their own ambitions, or out of strategic partisan loyalties. Most notably, federal prosecutors have been shown to pursue weaker cases against members of the opposite party (Gordon 2009) and to exhibit partisan differences in the timing of case filings (Nyhan and Rehavi 2017).

Developing a model of interactions between corrupt officials and potentially biased prosecutors, Gordon (2009) argues that an outcome-based approach can identify partisan bias by examining sentence length. His model assumes that US Attorneys prefer to pursue high-profile cases and more serious crimes, and therefore base their decisions as to whether to pursue particular cases on three considerations—the opportunity cost of pursuing a given case, the career benefit obtained from pursuing a noteworthy case, and the political benefit from pursuing a case against a member of the opposing party. The net theoretical result of Gordon's empirical estimates is that prosecutors tend to pursue weaker cases against out-party officials, resulting in shorter sentences for members of the opposing party relative to prosecutors' copartisans.

Gordon ([2009](#)) argues that his model identifies evidence of corruption in the Justice Departments of George W. Bush and Bill Clinton, but he also acknowledges potentially confounding issues within his data, including what appear to be a disproportionate targeting of Democrats under both administrations. Gordon speculates that such an effect could be a result of more urban-concentrated Democrats' relatively easier access to opportunities for corruption, but stops short of any geographic analysis. One reason that may be particularly significant is that disparities exist in available district-level prosecutorial resources (Alt and Lassen [2014](#)), and similar disparities in prosecutorial *effort.* As we will discuss in the following section, such geographic disparity is an issue we aim to address with district-level controls.[4]

Gordon's model succeeds, however, in identifying the importance of perceived political benefits. To the extent that prosecutors perceive that it is valuable to keep helping the party that put them in power for their own personal, political and career advancements, it would be prudent for politically minded prosecutors to be *sure* to charge members of the opposing party, and bury cases against their copartisans wherever and *whenever* possible. The time dimension of that relationship is an important one. Nyhan and Rehavi ([2017](#)) assess the timing of corruption charges filed close to elections as evidence of partisan bias, using a quasi-discontinuity design in which corruption charges against partisans *not* in the president's party immediately *before* elections indicate evidence of partisan bias. Identifying nearly 2000 partisan defendants, they find that, relative to the president's copartisans—and, presumably, most of his US Attorneys—defendants from the opposing party are more likely to be charged with corruption before an election. While Nyhan and Rehavi find no evidence that US Attorneys or their assistants bring weaker cases against out-party defendants, which conflicts with Gordon's ([2009](#)) results, they do find that charges are filed more slowly against copartisans.

Gordon ([2009](#)) utilizes data from the Bureau of Justice Statistics (BJS) as well as Syracuse University's Transactional Records Access Clearinghouse (TRACFed) database, a collection of detailed information about federal case filings obtained through Freedom of Information Act (FOIA) requests. Nyhan and Rehavi ([2017](#)), on the other hand, rely on data collected directly from FOIA requests for case-level information. For practical reasons, both studies examine cases only wherein charges actually were filed, and therefore overlooked potentially invaluable instances of partisan prosecutorial bias—cases opened for which charges eventually were *not* filed so as to protect copartisans. We contend—and Gordon likely would concur—that *less* evidence of copartisan sympathy can be found in cases wherein charges were filed largely because corruption was particularly egregious. A prosecutor's political return from *not* pursuing a case against a copartisan might be negligible or even punitive if the case against that copartisan is strong enough effectively to require that charges be filed, but we suspect that a significant number of observable cases still can be found in which federal prosecutors elected not to file corruption charges against their copartisans. Using conviction data from all 50 states, dating back four decades, we argue that prosecutors strategically choose not to pursue cases or obtain convictions when doing so is politically inexpedient.

Bologna Pavlik ([2017](#)) argues that federal prosecutors have an incentive to show that their party is tough on corruption prior to elections, providing a link between a state's importance to presidential elections and the number of federal corruption convictions. To measure a state's importance to presidential elections, Bologna Pavlik ([2017](#)) constructs a

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[4] We use the GeoLytics ([2013](#)) database to develop district-level demographic controls by aggregating up from the census tract level to the judicial district level.

three-component variable: the closeness of the most recent presidential election, the number of a state's electoral votes, and the standard deviation of the Democratic presidential candidate's vote share.[5] In some specifications, Bologna Pavlik also controls for whether the president and a state's governor belong to the same party, which is closely related to the variable we develop to measure state-federal political alignment. However, Bologna Pavlik neither presents nor discusses regression results for that variable, concentrating instead on the effect of electoral importance on a state's federal corruption convictions and whether that effect differs across political parties. As a result, our paper is the first explicitly to estimate, present, and discuss the effects of state-federal political alignment on federal corruption convictions.

Other scholarship has examined alternative causes of corruption beyond party alignment alone. Leeson and Sobel (2008) examine the relationship between FEMA disaster relief and public corruption. Identifying a positive correlation between FEMA relief funding and public corruption convictions, Leeson and Sobel (2008) suggest that corruption in certain regions of the country is driven partly by weather. In effect, frequent natural disasters beget disaster relief, which creates additional opportunities for corruption through the mismanagement of funding and resources that flow into states in the wake of hurricanes, tornadoes, and other major weather events. Following Leeson and Sobel (2008), we incorporate data on federal aid per capita and public sector employment by state in our specifications.

Glaeser and Saks (2006) argue that education, income inequality, racial fractionalization, and even economic development are correlated with public corruption, suggesting that robust political institutions reduce corruption. They focus on three theories that detail the causes of corruption. The first theory, following Lipset (1960), is that higher levels of income and education are associated with lower levels of corruption because wealthier, more educated voters will be more vigilant in monitoring their public officials, and therefore more responsive to and less tolerant of corruption. Glaeser and Saks find "significant support" for that hypothesis, noting that both richer and more educated states are less corrupt, with a "stronger and more robust" impact from education than from income. The authors also report evidence supporting a second hypothesis, following Mauro (1995) and Alesina et al. (2003), that demographic heterogeneity—measured as the extent of income inequality and racial diversity in a state—increases corruption. The suggested relationship here holds that more diverse voting blocs inevitably will come to focus more on policies of redistribution, and therefore less on government transparency and efficiency. The third theory they consider—that "places with more government revenues or regulations will have higher levels of corruption, as these places will have more assets to steal and more rules to subvert"—offers the least in terms of empirical support, finding no statistically significant effects once other factors limiting growth and correlated with corruption were controlled for. We draw primarily on Glaeser and Saks's first and second theories, entering education data into our specifications to address the role of scale effects in corruption convictions, and demographic data at the state level to control for additional differences across state residents.

A closely related literature examines whether the institutional mechanism for selecting judges and prosecutors affects legal outcomes. Popular election of state judges and prosecutors often is thought of as a critical check on misconduct in the criminal justice system, but the efficacy of such electoral accountability remains in dispute. Research shows that the increases in conviction rates and reductions in corruption that follow electing, rather than

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[5] The standard deviation of the presidential vote share was first proposed and used by Wright (1974).

appointing, prosecutors and judges (Alt and Lassen 2007; Bandyopadhyay and McCannon 2014; Gordon and Huber 2002) may come at the expense of judicial independence (Huber and Gordon 2004). Overall, electing prosecutors and judges may be thought of as "politically unassailable but insidious in its potential for compromising judicial independence" (Huber and Gordon 2004, p. 247). However, our study contends that the appointment system for federal prosecutors may have serious problems of its own, aligning with existing research supporting the assumption that prosecutors are likely to pursue politically advantageous cases (Boylan 2005; Glaeser et al. 2000; Gordon and Huber 2002).

Overall, the present paper joins a growing body of work that questions the use of corruption conviction data as a reliable proxy for criminal activity by public officials (Boylan and Long 2003; Cordis and Milyo 2016). Our results suggest that prosecutorial discretion and partisan political considerations may play key roles in determining the cases that ever make it past the investigation stage. To explore that question further, we will require detailed case-level data, but the work we present here provides compelling evidence that state and district-level public corruption convictions are sensitive to political alignment with the White House. Corruption convictions, therefore, cannot tell us how corrupt a district might be. Rather, they may tell us more about corruption within the justice system itself, particularly because we show that variations in public corruption convictions over time and across space are not a function of total criminal convictions. Rather, as we discuss in Sect. 5, political alignment tends to be associated with a reallocation of convictions *away* from public corruption cases.

3 Data and methodology

We construct a panel of annual state-level data on federal corruption convictions from 1976 to 2016. Since we are concerned primarily with the effects of state-federal political alignment, our measure of corruption is annual public corruptions convictions measured at the state level, *convictions*$_{it}$, where i and t index states and years respectively. Conviction data are available from the US Justice Department's Public Integrity Section (PIN). Every March, PIN surveys US Attorneys, asking them to summarize their corruption cases over the past year. The responses are compiled into annual reports to Congress, which provide up to 15 years of data at the federal district level. We gathered convictions data individually from PIN's *Annual Reports* from 1978 to the present. We aggregate district level data to create an annual measure of the number of federal public corruption convictions within each state.[6]

Our measure of partisan prosecutorial bias is a state-level aggregate, rather than a case-level measure, and is based on convictions rather than indictments. Each of those aspects of the data deserves some comment. Consider, first, the choice to focus on convictions rather than indictments. While indictments may be a cleaner measure of prosecutorial incentives,

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[6] Because convictions data for a given year are available from several *Annual Reports*, occasional discrepancies crop up across them. The convictions data for 1979 were revised substantially in the 1981 *Annual Report*. As the 1981 numbers were not revised further in subsequent *Annual Reports,* we take the later numbers to be correct. In addition, convictions data for 2001 were misreported in the 2001 *Annual Report*, and the 2003 version omits one column of convictions data. Some minor changes in the naming conventions of certain districts occurred in the 1990s. We have accounted for all such changes in our data. See Cordis and Milyo (2016) for further discussion.

since they reflect prosecutorial discretion more narrowly, convictions may provide a better measure of the sustained allocation of prosecutorial resources. In addition, and what is more important for our purposes, convictions are significantly more costly for both the accused individuals and society than are indictments. As such, a convictions-based measure provides a more meaningful measure of the *social cost* of politically motivated prosecutorial bias.

A focus on the social cost of prosecutorial bias also motivates the use of an aggregate measure of partisan prosecutorial behavior. Case-level data provide insight into how political considerations affect the outcomes of particular cases. We believe that our approach, which focuses on the extensive margin, is complementary to case-based analyses, and the use of aggregate data also has two distinct advantages. First, it captures some potentially important forms of prosecutorial bias, such as the decision not to prosecute cases against copartisans, that are missed in case-level measures. Second, and even more important, an aggregate measure focusing on the total number of corruption convictions, is more revealing as a gauge of the social cost of partisan prosecutorial bias.[7]

In spite of its advantages, aggregate measures of federal corruption convictions involve two ambiguities that deserve comment. First, partisan prosecutorial bias could alter the number of corruption convictions in two distinct ways. Partisan prosecutors could decline or dismiss cases against copartisans, or they could pursue, or prosecute more aggressively, cases against members of the political opposition. While we are not able to identify the relative contributions of those two factors in giving rise to the relationship between state-federal political alignment and the number of corruption convictions, we still consider the difference in corruption convictions across politically aligned and non-aligned states to be an important measure of the social cost of partisan political bias, as both processes represent serious miscarriages of justice.

Second, using an aggregate conviction measure means that we are not able directly to observe the party affiliations of individual defendants, raising an ambiguity regarding our interpretation of the results as evidence of partisan bias.[8] That said, the findings of studies like Gordon (2009) and Nyhan and Rehavi (2017), which provide microeconomic, case-based evidence of partisan prosecutorial bias using information on the party identities of defendants, increases our confidence in interpreting our results in terms of partisan prosecutorial bias. Furthermore, we provide some evidence that weighs against alternative interpretations.

3.1 State-federal political alignment

Our focal dependent variables measure state-federal political alignment based on various measures of state politics and the party of the president who appointed the serving US

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[7] A similar consideration weighs against distinguishing between corruption convictions of private citizens and of state, federal or local public officials. Not only would a disaggregated approach obscure the social cost of prosecutorial bias, but such distinctions maybe overly fine for our purposes. In particular, corruption convictions of private citizens have important political ramifications, as in Kady (2008) and Neumeister (2018); evidence of local corruption also can spill over into electoral politics, as in Allen (2008).

[8] As pointed out by Bologna Pavlik (2017, p. 349), however, information on the party affiliations of defendants is available for less than half of the sample used by Gordon (2009) and only a quarter of the sample used by Nyhan and Rehavi (2017), which suggests that an aggregate measure of the partisan identities of defendants would be quite costly to construct.

Attorneys. The variable *Republican US Attorney* is an indicator that equals one if the US Attorneys in a state were appointed by a Republican president. Using data compiled by the US Senate, which covers the appointment and confirmation of. S Attorneys from Reagan onward, we find that the average delay in the appointment of new political appointees is 1.47 years.[9] Given that delay in the confirmation process, in the first year of a new administration's tenure, we code *Republican US Attorney* according to the party of the previous administration, and in an administration's second year, we base it on the average of the current and previous administration's partisan identities. For example, *Republican US Attorney* equals one in 1993, the first year of a transition from a Republican to a Democratic administration, 0.5 in 1994, and zero in 1995. During the remaining years of an administration's tenure, we code *Republican US Attorney* according to the president's party.[10] The variable *Republican US Attorney* is used to construct measures of state-federal political alignment.

Our primary measure of state-federal political alignment, *Same Party Governor*, is an indicator of party alignment between a state's governor and the party that appointed its US Attorneys. We also construct five additional measures of state-federal political alignment based on a state's politics and the party that nominated US Attorneys. The variables *Same Party, State House Majority* and *Same Party, State Senate Majority*, reflect party control of a state's legislative bodies, *Same Party, US House Majority* and *Same Party, US Senate Majority* reflect the share of a state's congressional delegation that belongs to the party that appointed the US Attorney; *Same Party, Majority Presidential Vote* is based on the president's share of a state's two-party vote in the most recent national election. Our data on state legislators and governors come from Carl Klarner's (2013) databases of state partisan balance and governors, available online from the Harvard Dataverse.[11] We also augmented that data, which reports gubernatorial data only through 2010 and partisan balance data only through 2011, with our updated information derived from state websites, news reports, and data from the National Council of State Legislatures. Information on voting in presidential elections also is available from the Harvard Dataverse.[12]

3.2 Additional political variables

For our purposes, the first 2 years of a new administration's term of office represent something of an interregnum, in which the previous administration's US Attorneys may have resigned, but the new administration's appointees have not yet been confirmed.[13] It follows

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[9] Data on the confirmations of US Attorneys are available here: https://www.congress.gov/searc h?searchResultViewType=expanded&q=%7B%22source%22%3A%22nominations%22%2C%22sea rch%22%3A%22united+states+attorney%22%2C%22congress%22%3A%22all%22%2C%22nomination-status%22%3A%22Confirmed+by+Senate%22%7D.

[10] The approach has two advantages over using data on confirmation dates of new US Attorneys. First, the confirmation data may miss some appointments, e.g., if a US Attorney moves from one district to another. Second, the confirmation delay for a particular district is endogenous, and may reflect partisan political considerations.

[11] Available at: https://dataverse.harvard.edu/dataverse/cklarner.

[12] Presidential vote share data by state, candidate, and party are available here: https://dataverse.harva rd.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/42MVDX.

[13] Adding to the ambiguity of the interregnum, some US Attorneys agree to serve in acting capacities following their formal resignations, while others leave, with their offices transitioning to Assistant US Attorneys. The latter likewise are political appointees, but not subject to Senate confirmation and, thus, seen generally as less political than US Attorneys.

that the effect of state-federal political alignment on corruption convictions may be weaker over the interregnum period. To test that and related hypotheses, we define *tenure* as the number of years the White House has been in a particular party's hands. Thus, for example, the maximum value of *tenure* occurs in the final year of the George H. W. Bush presidency, at which point the Republican party had controlled the White House for 12 years.

Following Bologna Pavlik (2017), we also construct a measure of a state's political importance to presidential politics. The measure combines state-level information on the closeness of the most recent presidential election, the number of a state's electoral votes, and the standard deviation of the Republican presidential vote share for the 1976-2012 elections, i.e., $importance = \left(1 - 4(gopvoteshare - .5)^2\right)(electoralvotes)\left(\sigma_{demvoteshare}\right)$.

Bologna Pavlik (2017) finds a linear relationship between that variable and federal corruption convictions. In the regressions below, we enter the natural log of *Importance*, as our regression model posits an exponential relationship between the regressors and the number of corruption convictions.

Following Glaeser and Saks (2006), we control for the log of state per capita income and the share of a state's population with a bachelor's degree or higher. We enter two variables, the natural logs of state population size and government employment, to address the role of scale effects in federal corruption convictions. Leeson and Sobel (2008) scale public corruption convictions by the level of government employment. State-level data on public sector employment are available from the US Census Bureau's Annual Survey of Public Employment and Payroll.[14] In addition, in some specifications, we control for the natural log of federal aid to states, as in, e.g., Bologna Pavlik (2017). Federal aid to states is available for 1981 through 2010.[15]

Table 1 presents summary statistics for selected variables. Our sample consists of all federal corruption convictions reported by the Justice Department's Public Integrity Section (PIN) from 1976 to 2015, in all 50 states. We note considerable variation in the number of convictions across states. A surprising amount of parity is observed in terms of partisan alignment; on average, partisan alignment between Democrat chief executives is only slightly more common than Republican alignment in our sample, which includes 20 years of Democratic presidents and 21 years of Republican presidents. In addition, we note that most states experience multiple changes in the party affiliations of their governors. All states experience at least one change in party control of the governor's mansion, with an average of 3.76 changes in party control within the time frame covered by our sample. Our primary measure of state-federal political alignment also varies significantly across and within states. The average level of *Same Party, Governor* ranges from 0.1625 in Virginia to 0.8125 in Missouri. The standard deviation of *Same Party, Governor* exceeds 0.35 for all but three states.

3.3 Empirical methodology

OLS estimators are not consistent when the dependent variable consists of count data, so we base our analysis on estimators designed specifically to estimate models in which

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[14] We collected data by state and function for 1992 to 2018 from https://www.census.gov/programs-surveys/apes.html.

[15] Data on federal aid to states are from *Federal Expenditures by State* for fiscal years 1981-1997 and from *Federal Aid to States* for 1998-2010; available here: https://www.census.gov/library/publications/time-series/cffr.html.

Table 1 Summary statistics

Variable	Obs.	Mean	SD	Min	Max
Dependent variables					
Convictions	1982	15.94,147	21.86,634	0	166
Total cases	1950	847.6492	1444.179	0	16,297
Political variables					
Same party governor	2000	0.434695	0.4606085	0	1
Republican governor	2000	0.479155	0.4958349	0	1
Republican majority, US house	2000	0.37981	0.4853394	0	1
Republican majority, US senate	2000	0.421905	0.4937521	0	1
Additional variables					
ln(Population)	2000	15.00648	1.015342	12.88186	17.48278
ln(Income)	2000	9.978742	0.572005	8.494334	11.1339
College	2000	20.28498	6.107828	7.11	41.5
ln(Federal aid to states)	1500	7.9056	1.08486	4.691348	11.10593
ln(Public sector employment)	1149	12.0922	1.012119	9.324026	14.42859
ln(Politically important state)	2000	− 0.8129948	0.7212376	− 2.25054	1.405782
Honest services law	2000	0.525	0.4994995	0	1
Tenure	2000	4.9	2.879956	1	12
Alternative measures of state-federal political alignment					
Same Party, State House Majority	2000	0.42081	0.4609418	0	1
Same party, state senate majority	2000	0.433905	0.4630461	0	1
Same party, US house majority	2000	0.496051	0.3055792	0	1
Same party, US senate majority	2000	0.51375	0.3827652	0	1
Same party, majority presidential vote	2000	0.574014	0.1611508	0.2203482	0.9999539
Same party governor (Democrat)	2000	0.214675	0.3849839	0	1
Same party governor (Republican)	2000	0.22002	0.3980709	0	1

the dependent variable contains count data. Our discussion of count data model draws heavily on that in Cameron and Trivedi (2001). Such models reflect the fact that count data inherently are non-linear, censored at zero, distributed with positive mass at non-negative integers, and skewed to the left. The starting point for count data empirical models is the Poisson regression, which assumes that the underlying data, in our case *convictions$_{st}$*, are distributed according to a stochastic Poisson process, with a common mean and variance, $E(convictions_{st}) = V(convictions_{st}) = \mu_{st}$. Assuming an exponential mean, the Poisson model is parameterized as $\mu_{st} = \exp(\beta X_{st})$ which typically is estimated using maximum likelihood techniques.

In practice, the assumptions underlying the Poisson regression are often rejected. In particular, the estimated variance of the dependent variable often exceeds the mean, a condition known as overdispersion, which violates the assumption that the underlying variable is distributed according to a Poisson process. The fixed effects variant of the Poisson estimator provides some additional flexibility, as it allows the dispersion parameter to vary across panels, but it still requires the mean and variance of the underlying data to be equal for each panel (Allison and Waterman 2002).

The negative binomial regression provides a parsimonious flexible alternative to Poisson regression.[16] Here, we consider a common variant of the negative binomial in which the variance is modeled as a quadratic function of the mean, $V(convictions_{it}) = \eta + \alpha\mu^2$ (Cameron and Trivedi 2001). In such a case, Poisson regression is rejected in favor of the negative binomial regression, provided that one can reject the assumption of $\alpha = 0$. That is the case with our data and, as a result, the empirical analysis is conducted using negative binomial regressions.

We address the unobserved heterogeneity across states by estimating the conditional fixed effects negative binomial specification originally developed by Hausman et al. (1984), in which a state fixed effect enters the model by allowing the dispersion to vary arbitrarily across states. In our baseline model, we assume that $X_{it} = \beta_1 alignment_{it} + \gamma Z_{it} + \phi_t$, where $alignment_{it}$ is a measure of state-federal political alignment, Z_{it} is a vector of time-varying state-level variables reflecting political and socioeconomic characteristics, and ϕ_t is a vector of annual dummy variables, which control for the impact of national shocks to federal corruption convictions, such as might arise owing to changes in the national political environment, such as presidential administrations and variations in the political returns to corruption convictions over the electoral cycle, and changes in the legal basis for corruption convictions, such as the 1988 introduction the Honest Services law, which provides the statutory basis for many federal corruption cases, and its re-interpretation in 2010.[17]

4 Results

We begin by considering the relationship between partisanship and state-level corruption convictions. The dependent variable is $convictions_{it}$, the number of federal public corruption convictions is a given state and year. Our dependent variable of interest is *Same Party Governor*, which takes a value of one when the state's governor and the US President belong to the same political party.[18] If US Attorneys are vulnerable to partisan bias, we would expect the coefficient on *Same Party Governor* to be negative, indicating that fewer corruption convictions are observed in states when its governor belongs to the same party as the US President. Our baseline specification includes controls for the state's political environment, including indicator variables for the party affiliation of the governor and party control of the state's legislative chambers. In each case, we enter indicator variables that take values of one for the Republican Party and zero for Democratic Party.[19] When the

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[16] Ver Hoef and Boveng (2007) point out that the Poisson model may be adjusted with quasi-likelihood assumptions to fit overdispersed count data, resulting in a model that is similar to the negative binomial other than the manner in which observations are weighted. Some statisticians prefer the negative binomial on theoretical grounds, as overdispersion arises organically in a count data model from a well-understood statistical process.

[17] Entering annual fixed effects raises the possibility of an incidental parameters problem. However, Allison and Waterman (2002, p. 248) examine the conditional mean negative binomial model using simulated data, finding that it "does not appear to suffer from any 'incidental parameters' bias, and is generally superior to the Poisson estimator."

[18] Our primary finding of a negative relationship between state-federal political alignment and federal corruption convictions is robust to the use of per capita and logged measures of corruption. Results are available from the authors.

[19] It is worth noting that including measures of party control of state legislative bodies may result in post-treatment bias in the coefficient on same party governor, as the party of a state's governor may affect the electoral success of down-ticket candidates, such as races for state legislatures. We believe that potential

relevant office is held by an independent, we assign the variable a value of 0.5. We also enter the log of state per capita income, the share of a state's population with a bachelor's degree, the natural log of state population plus state and year fixed effects. Those variables control respectively for key socioeconomic characteristics, scale effects in crime and the administration of justice, time-invariant state-level omitted variables, and national shocks to corruption convictions, including variations in the national political environment and the relevant body of federal law.

Table 2 presents results for our baseline model. As seen in column 1, our focal dependent variable is negative and significant at the 1% level. The results of alternative specifications are shown in Table 2. In both models, our focal dependent variable is significant at the 1% level. That variable also is economically significant, as the point estimate indicates that state-federal political alignment reduces the number of federal corruption convictions by 8.72%. In addition, we find evidence that the number of a state's federal corruption convictions is increasing in its population and decreasing in the share of its population with a four-year college degree.

Next, we consider variations on the baseline model in which we adjust for two possible factors that may influence the timing of the relationship between state-federal political alignment and federal corruption convictions. First, the average duration of a federal corruption case, from referral to conviction, is approximately 1.4 years for federal officials and 1.7 years for state and local officials (Cordis and Milyo 2016). In columns 2 through 4, we lag the focal dependent variable by one to 3 years to account for the time involved in prosecuting corruption cases. Our results indicate a negative and statistically significant relationship between federal corruption convictions and *Same Party Governor* lagged one or 2 years, although that relationship is not significant with a three-year lag. We note also that the coefficient on *Same Party Governor* is quite stable across the first three columns. Given that evidence, in the remaining analyses we enter the contemporaneous measure of state-federal political alignment.

The next four columns address the potential effects of the delay in appointing and confirming US Attorneys. To do that, we restrict the sample by omitting the first one to four initial years of a party's tenure in the White House. Our results indicate that the relationship between state-federal political alignment and federal corruption convictions increases as we omit the first 3 years of a party's tenure, with the coefficient on *Same Party Governor* increasing in both magnitude and statistical significance; the estimated impact rises to 12.0% in column 7. The results in column 8 are similar to those in column 7. Going forward, we estimate most regressions using the full sample, but we also consider a subsample that omits the first 2 years of a party's tenure, which corresponds to the interregnum during which a new president's US Attorneys are being confirmed.

The final two columns of Table 2 consider specifications that add two measures of the role played by the government of a state. The first is the natural log of state government employment, which Leeson and Sobel (2008) use to scale public corruption convictions. As seen in column 9, that variable is not significant, and its inclusion increases both the magnitude and significance of *Same Party Governor*. Second, in column 10, we enter the log of the level of a state's federal aid, which, as suggested by Bologna Pavlik (2017), is plausibly linked to state-federal political alignment. While this variable is itself significant,

Table 2 Corruption convictions and state-federal political alignment (2019 07 19)

Variables	(1) Full sample Convictions	(2) Full sample Convictions	(3) Full sample Convictions	(4) Full sample Convictions	(5) Tenure >1 Convictions	(6) Tenue >2 Convictions	(7) Tenure >3 Convictions	(8) Tenure >4 Convictions	(9) Full sample Convictions	(10) Full sample Convictions
Same party governor	−0.0913*** (−2.739)				−0.0986*** (−2.801)	−0.114*** (−3.084)	−0.128*** (−3.120)	−0.125*** (−2.671)	−0.140*** (−3.433)	−0.0838** (−2.108)
L.Same party governor		−0.0842** (−2.519)								
L2.Same party governor			−0.0898*** (−2.694)							
L3.Same party governor				−0.0403 (−1.210)						
Republican governor	0.0143 (0.462)	0.00840 (0.271)	0.00702 (0.228)	−0.000916 (−0.0296)	0.0152 (0.461)	0.0343 (0.956)	0.0367 (0.937)	0.0231 (0.511)	0.0154 (0.409)	−0.00562 (−0.154)
Republican majority, US House	0.0632 (1.594)	0.0705* (1.774)	0.0785** (1.981)	0.0826** (2.067)	0.0594 (1.402)	0.0752 (1.631)	0.0822* (1.656)	0.0839 (1.498)	0.0783* (1.693)	0.0774* (1.733)
Republican Majority, US Senate	−0.0119 (−0.411)	−0.00928 (−0.321)	−0.00931 (−0.324)	−0.00585 (−0.202)	−0.0221 (−0.714)	−0.0221 (−0.662)	−0.0391 (−1.069)	−0.0219 (−0.530)	−0.0366 (−1.113)	−0.00779 (−0.244)
ln(Population)	0.578*** (12.00)	0.576*** (11.83)	0.587*** (11.92)	0.578*** (11.62)	0.557*** (10.81)	0.540*** (9.519)	0.557*** (8.965)	0.512*** (7.438)	0.649*** (2.589)	0.161 (1.149)
ln(Income)	0.434 (1.607)	0.367 (1.347)	0.355 (1.292)	0.413 (1.485)	0.438 (1.540)	0.591* (1.939)	0.628* (1.865)	0.818** (2.142)	0.401 (1.193)	0.867** (2.327)
College	−0.0352*** (−3.376)	−0.0335*** (−3.205)	−0.0330*** (−3.146)	−0.0337*** (−3.182)	−0.0356*** (−3.263)	−0.0378*** (−3.258)	−0.0419*** (−3.250)	−0.0548*** (−3.754)	−0.0243** (−2.104)	−0.0541*** (−3.674)
ln(Public Sector Employment)									−0.0829 (−0.323)	

Table 2 (continued)

Variables	(1) Full sample Convictions	(2) Full sample Convictions	(3) Full sample Convictions	(4) Full sample Convictions	(5) Tenue > 1 Convictions	(6) Tenue > 2 Convictions	(7) Tenue > 3 Convictions	(8) Tenue > 4 Convictions	(9) Full sample Convictions	(10) Full sample Convictions
ln(Federal Aid to States)										0.493*** (3.255)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	−12.11*** (−5.123)	−11.44*** (−4.755)	−11.45*** (−4.659)	−11.76*** (−4.687)	−11.82*** (−4.721)	−12.87*** (−4.762)	−13.38*** (−4.466)	−14.20*** (−4.198)	−11.43*** (−3.303)	−12.80*** (−3.599)
Observations	1982	1933	1884	1834	1733	1484	1235	988	1143	1488
Number of states	50	50	50	50	50	50	50	50	50	50

z-statistics in parentheses

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

its inclusion does not alter our estimate of the impact of state-federal political alignment materially. Because of its theoretical link to state-federal political alignment, however, we consider specifications including the level of federal aid as a robustness check below.

Our initial set of results provide broad support for the hypothesis that the level of federal corruption convictions is sensitive to a state's political alignment with the federal government. The estimated effect also is economically significant, with federal corruption convictions falling roughly 9% in years when a state is politically aligned with the sitting US President. The effect is similar in models using different lags of the focal dependent variable and is somewhat stronger when we omit the early years of a party's White House tenure. In the remaining analysis, our baseline model is that in column 1, which considers the contemporaneous effect of *Same Party Governor*. As robustness tests we consider the specifications in column 6, which drop the first 2 years of a party's White House tenure, and column 9, which controls for the natural log of a state's federal aid.

4.1 Alternative measures of state-federal political alignment

Next, we consider the robustness of our key finding to specifications that incorporate alternative measures of state-federal political alignment. In particular, we test the importance of measures of state-federal political alignment based on political control of the state's legislative bodies, the political composition of its congressional delegation, and the president's vote share in the state's most recent national election. In column 1 we enter the indicator variables *Same Party, State House Majority* and *Same Party, State Senate Majority*, variables that take the value of one if a state's legislative bodies are controlled by the same party that appointed a state's US Attorneys. In column 2, we include *Same Party, US House* and *Same Party, US Senate*, which measure the share of the state's US Senators and US House Representative who belong to the party that appointed a state's US Attorneys. In column 3, we control for *Same Party Vote*, a state's share of the most recent presidential vote going to the party that appointed the US Attorneys and, in column 5, we control for all five additional measures of state-federal political alignment simultaneously. None of the alternative measures is significant in any specification. What is more important, entering those variables does not appreciably affect the magnitude or significance of the coefficient of our primary measure of state-federal political alignment, *Same Party Governor*.

The final two columns of Table 3 summarize two additional robustness checks, adding Federal Aid to States and restricting the sample to years in which party tenure exceeds two. Neither change has much effect on the size or significance of our key coefficient, though in column 6, with $p = 0.051$, it narrowly misses the conventional threshold for statistical significance.

Econometrically, the robustness of our finding for *Same Party Governor* probably owes something to the relatively small correlation between that variable and the other measures of state-federal political alignment, which ranges between 0.079 and 0.21. While our empirical work does not indicate why our particular measure of state-federal political alignment matters, our results may reflect the importance of state governors as political allies, relative to other state legislative leaders and congressional representatives, including factors such as the roles of state executive branches in administering elections, and the relationship between gubernatorial party and the ascendance of like-minded state bureaucrats and career officials, all of whom easily could become defendants in the cases in our database.

Table 3 Alternative measures of political alignment (2019 07 19)

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Full sample	Full sample	Full sample	Full sample	Full sample	Full sample	Tenure > 2
	Convictions	Convictions	Convictions	Convictions	Convictions	Convictions	Convictions
Same party governor	−0.0913***	−0.0876**	−0.0934***	−0.0873***	−0.0867**	−0.0785*	−0.117**
	(−2.739)	(−2.562)	(−2.768)	(−2.586)	(−2.527)	(−1.955)	(−2.372)
Same party, state house majority		−0.0225			−0.0300	−0.0350	−0.0138
		(−0.650)			(−0.776)	(−0.818)	(−0.284)
Same party, state senate majority		0.00932			0.0115	−0.00411	−0.00500
		(0.300)			(0.368)	(−0.115)	(−0.120)
Same party, majority presidential vote				−0.157	−0.352	0.142	−0.0802
				(−0.763)	(−1.304)	(0.474)	(−0.223)
Same party, US house majority			0.0287		0.0892	0.0497	0.0202
			(0.412)		(1.121)	(0.567)	(0.195)
Same party, US senate majority			0.00461		0.0361	0.0166	0.0173
			(0.115)		(0.808)	(0.344)	(0.311)
Republican Governor	0.0143	0.0127	0.0159	0.0123	0.0137	−0.0100	0.0201
	(0.462)	(0.404)	(0.509)	(0.397)	(0.436)	(−0.272)	(0.424)
Republican majority, US house	0.0632	0.0638	0.0638	0.0636	0.0658*	0.0756*	0.0899
	(1.594)	(1.603)	(1.607)	(1.603)	(1.652)	(1.670)	(1.611)
Republican majority, US senate	−0.0119	−0.0144	−0.0104	−0.0133	−0.0129	−0.00784	−0.00920
	(−0.411)	(−0.493)	(−0.359)	(−0.458)	(−0.442)	(−0.235)	(−0.222)
ln(Population)	0.578***	0.579***	0.579***	0.578***	0.582***	0.167	0.0875
	(12.00)	(12.01)	(12.00)	(11.99)	(12.05)	(1.194)	(0.536)
ln(Income)	0.434	0.437	0.427	0.445	0.433	0.841**	1.148**
	(1.607)	(1.614)	(1.578)	(1.643)	(1.598)	(2.252)	(2.461)
College	−0.0352***	−0.0356***	−0.0344***	−0.0369***	−0.0360***	−0.0521***	−0.0619***
	(−3.376)	(−3.400)	(−3.255)	(−3.457)	(−3.380)	(−3.517)	(−3.479)

Table 3 (continued)

Variables	(1) Full sample	(2) Full sample	(3) Full sample	(4) Full sample	(5) Full sample	(6) Full sample	(7) Tenure > 2
	Convictions	Convictions	Convictions	Convictions	Convictions	Convictions	Convictions
ln(Federal aid to states)						0.492****	0.515****
						(3.248)	(2.887)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	−12.11***	−12.14***	−12.08***	−12.10***	−12.01***	−12.75***	−13.80***
	(−5.123)	(−5.132)	(−5.111)	(−5.114)	(−5.077)	(−3.582)	(−3.125)
Observations	1982	1982	1982	1982	1982	1488	1089
Number of states	50	50	50	50	50	50	50

z-statistics in parentheses ***$p<0.01$, **$p<0.05$, *$p<0.1$

Table 4 Political importance

Variables	(1) Full sample Convictions	(2) Full sample Convictions	(3) Full sample Convictions	(4) Tenure > 2 Convictions	(5) Tenure > 2 Convictions	(6) Tenure > 2 Convictions
Same party governor	−0.0938***	−0.139***	−0.106**	−0.116***	−0.171***	−0.149***
	(−2.810)	(−3.770)	(−2.420)	(−3.132)	(−4.180)	(−2.787)
ln(Imports)	0.139	0.205	0.0275	0.174	0.261*	0.0600
	(1.060)	(1.556)	(0.187)	(1.143)	(1.716)	(0.350)
Same party governor x ln(imports)		−0.112***	−0.0551		−0.133***	−0.0674
		(−2.800)	(−1.221)		(−3.036)	(−1.339)
Republican governor	0.0157	0.0155	−0.00505	0.0355	0.0355	0.0238
	(0.508)	(0.500)	(−0.138)	(0.992)	(0.993)	(0.506)
Republican majority, US house	0.0668*	0.0685*	0.0793*	0.0803*	0.0834*	0.0913*
	(1.676)	(1.725)	(1.775)	(1.733)	(1.812)	(1.692)
Republican majority, US senate	−0.0120	−0.0206	−0.0118	−0.0214	−0.0321	−0.0167
	(−0.415)	(−0.709)	(−0.366)	(−0.645)	(−0.962)	(−0.441)
ln(Population)	0.482***	0.477***	0.167	0.419***	0.406***	0.0651
	(4.683)	(4.632)	(1.008)	(3.479)	(3.367)	(0.341)
ln(Income)	0.406	0.425	0.860**	0.556*	0.608**	1.140**
	(1.500)	(1.567)	(2.298)	(1.818)	(1.986)	(2.455)
College	−0.0352***	−0.0358***	−0.0539***	−0.0375***	−0.0383***	−0.0613***
	(−3.375)	(−3.438)	(−3.657)	(−3.226)	(−3.300)	(−3.485)
ln(Federal aid to states)			0.490***			0.525***
			(3.242)			(2.956)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Constant	−10.34***	−10.39***	−12.80***	−10.65***	−10.87***	−13.46***
	(−3.572)	(−3.595)	(−3.185)	(−3.193)	(−3.262)	(−2.738)

Table 4 (continued)

Variables	(1) Full sample Convictions	(2) Full sample Convictions	(3) Full sample Convictions	(4) Tenure > 2 Convictions	(5) Tenure > 2 Convictions	(6) Tenure > 2 Convictions
Observations	1982	1982	1488	1484	1484	1089
Number of states	50	50	50	50	50	50

z-statistics in parentheses

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

4.2 State importance to presidential elections

Bologna Pavlik (2017) finds more corruption convictions in states that are more impor-
tant to presidential elections, where political importance reflects the closeness of recent
presidential election, the number of a state's electoral votes, and the standard deviation
of a party's share of the two-party presidential vote. In Table 4 we consider a variety
of specifications designed to account for the potential explanatory powers of this vari-
able. Because negative binomial regressions posit an exponential relationship between
regressors and the dependent variable, we enter the natural log of *Political Importance*
(specified above), rather than its unlogged value.

In the first column of Table 4, we include the natural log of political importance
as a regressor. While this variable is not statistically significant, the relationship
between state-federal political alignment and federal corruption convictions is robust
to the change in specification. Next, we augment the model by entering the interaction
between *ln(Political Importance)* and *Same Party Governor*. The interaction term tests
whether state-federal political alignment matters more in politically important states. As
seen in column 2, both *Same Party Governor* and the interaction term are significant at
the 1% level. Our estimates indicate that state-federal political alignment plays a larger
role in federal corruption convictions in politically important states. However, as seen
in column 3, the statistical significance of the interaction term is not robust to the inclu-
sion of the natural log of federal aid.

As reported in columns 4 through 6, we get similar results when we consider these
specifications using a sample restricted to the last 2 years of a party's presidential tenure.
In particular, while *Same Party Governor* is significant in all regressions, indicating the
robustness of our primary hypothesis, evidence that state-federal political alignment mat-
ters more in politically important states is fragile.

4.3 Political asymmetry

Both Gordon (2009) and Bologna Pavlik (2017) find evidence of asymmetry in partisan
prosecutorial bias across political parties, with Democratic administrations being worse
offenders. A potential problem with that finding is that both studies rely on rather lim-
ited data. Gordon (2009) relies on case-level data from just two administrations, those of
presidents Clinton and George W. Bush, while Bologna Pavlik (2017) also enters data from
the first 2 years of the Obama administration. As a result, it is unclear whether the results
reflect the policies of these particular administrations or hold for Democratic and Republi-
can administrations more broadly. In contrast, the PIN corruption data, on which we rely,
spans 40 years, including at least part of seven presidential administrations: Ford, Carter,
Reagan, Bush I, Clinton, Bush II and Obama.

We begin our investigation by augmenting our baseline model to include a new variable,
Same Party Governor (Democrat), which takes a value of one if a state's governor and the
US President both are Democrats. Entering that variable allows us to test whether the effect
of state-federal political alignment differs across the two political parties. As seen in the
first column of Table 5, however, the coefficient on *Same Party Governor (Democrat)* is
not significantly different from zero, while our general indicator of state-federal political
alignment remains significant at the 1% level. As a result, we cannot reject the hypothesis
that partisan prosecutorial bias is the same across the two major parties.

Table 5 Evidence on partisan asymmetry

Variables	(1) Full sample Convictions	(2) Full sample Convictions	(3) Full sample Convictions	(4) Full sample Convictions	(5) Tenure > 2 Convictions	(6) Tenure > 2 Convictions
Same party governor	−0.112**		−0.118**		−0.423***	
	(−2.016)		(−2.050)		(−3.630)	
Same party governor (democrat)	0.0418	−0.0705	0.0779	−0.0398	0.592***	0.169
	(0.470)	(−1.273)	(0.820)	(−0.598)	(2.798)	(1.569)
Same party governor (republican)		−0.112**		−0.118**		−0.423***
		(−2.016)		(−2.050)		(−3.630)
Republican governor	0.0289	0.0289	0.0181	0.0181	0.319***	0.319***
	(0.660)	(0.660)	(0.390)	(0.390)	(2.966)	(2.966)
Republican majority, US house	0.0634	0.0634	0.0767*	0.0767*	0.0792*	0.0792*
	(1.599)	(1.599)	(1.720)	(1.720)	(1.722)	(1.722)
Republican majority, US senate	−0.0116	−0.0116	−0.00797	−0.00797	−0.0234	−0.0234
	(−0.401)	(−0.401)	(−0.250)	(−0.250)	(−0.704)	(−0.704)
ln(Population)	0.579***	0.579***	0.161	0.161	0.549***	0.549***
	(12.01)	(12.01)	(1.147)	(1.147)	(9.665)	(9.665)
ln(Income)	0.430	0.430	0.861**	0.861**	0.609**	0.609**
	(1.590)	(1.590)	(2.313)	(2.313)	(1.999)	(1.999)
College	−0.0351***	−0.0351***	−0.0541***	−0.0541***	−0.0386***	−0.0386***
	(−3.357)	(−3.357)	(−3.667)	(−3.667)	(−3.331)	(−3.331)
ln(federal aid to states)			0.497***	0.497***		
			(3.280)	(3.280)		
State Fe	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Constant	−12.09***	−12.09***	−12.78***	−12.78***	−13.16***	−13.16***
	(−5.115)	(−5.115)	(−3.593)	(−3.593)	(−4.863)	(−4.863)

Table 5 (continued)

Variables	(1) Full sample Convictions	(2) Full sample Convictions	(3) Full sample Convictions	(4) Full sample Convictions	(5) Tenure > 2 Convictions	(6) Tenure > 2 Convictions
Observations	1982	1982	1488	1488	1484	1484
Number of States	50	50	50	50	50	50

z-statistics in parentheses

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

To continue our investigation, we consider a specification including both *Same Party Governor (Democrat)* and *Same Party Governor (Republican)*, an indicator variable that equals one if both the president and a state's governor are Republicans. As seen in column 2, the coefficient on *Same Party Governor (Republican)* is negative and significant at the 1% level, while the coefficient on *Same Party Governor (Democrat)* is, again, insignificant. Thus, we also cannot reject the hypothesis that partisan prosecutorial bias occurs only under Republican administrations.

As seen in columns 3 and 4, controlling for the natural log of federal aid does not alter these results. Finally, in columns 5 and 6, we restrict the analysis to the subsample that omits the first 2 years of a party's presidential tenure. In column 5, we can reject the hypothesis that the effects of state federal political alignment are symmetrical across political parties at the 1% level. As indicated in column 6, however, the effect for Democratic administrations is not significantly different from zero.

In summary, we find consistent evidence of partisan prosecutorial bias among Republican administrations. In contrast, our results for Democratic administrations are weaker. In only one of six specifications that we consider do we find evidence that the partisan prosecutorial bias under Democratic administrations is either different from zero or different from that under Republican administrations. While not entirely conclusive, these results contrast strongly with those reported by Gordon (2009) and Bologna Pavlik (2017), who found evidence of greater prosecutorial bias under Democratic administrations.

4.4 Time variation in corruption convictions

Next, we consider whether our findings are robust to controls for a number of issues related to the time variation in corruption convictions. The first hypothesis we examine has to do with the timing of national elections. As noted in Sect. 3, Nyhan and Rehavi (2017) find that politically valuable, cross-party indictments for public corruption rise prior to an election. Here, we test to see whether a similar pattern exists in the convictions data. As perhaps the "flashiest" stage of a corruption case, a conviction may be perceived to have greater political benefits (or costs) for one party in an election year. If the political returns to same-party and cross-party corruption convictions are higher in an election year, then we would expect the impact state-federal political alignment to be greater in those years as well.

To test the foregoing proposition, we interact *Same Party Governor* with *Election Year*, a dummy variable that equals one in a presidential election year. The interaction term is added to the baseline specification in column 1 of Table 6. If prosecutors successfully time cross-party convictions to coincide with election years, then the interaction term should be significant and negative. However, the interaction term is not significant. In column 2, we undertake a similar exercise using a dummy variable for even-numbered years, which coincide with congressional elections, with similar results.

We do not believe that our results necessarily are inconsistent with those reported by Nyhan and Rehavi (2017). The rise in cross-party indictments does not necessarily suggest a corresponding increase in convictions for three reasons. First, corruption cases may take months from start to finish. The average delay from referral to conviction is approximately 1.4 years for federal officials and 1.7 years for state and local officials (Cordis and Milyo 2016). Moreover, prosecutors may have limited control over the duration of a particular case. As a result, election year indictments may result in a conviction the following year. Second, if prosecutors do indeed pursue weaker cases against

Table 6 Time variation in partisan convictions

Variables	(1) Full sample Convictions	(2) Full sample Convictions	(3) Full sample Convictions	(4) Full sample Convictions	(5) Full sample Convictions	(6) Full sample Convictions
Same party governor	−0.110*** (−2.917)	−0.110** (−2.531)	−0.104 (−1.449)	−0.0837* (−1.700)	−0.0798 (−1.331)	−0.0516 (−1.014)
Same party governor x election year	0.0717 (1.070)					
Same party governor x even year		0.0400 (0.666)				
Same party governor x Tenure			0.00247 (0.206)			
Same party governor x high tenure				−0.0132 (−0.210)		
Same party governor x Tenure2					−0.0235 (−0.306)	
Same Party Governor x Tenure3					−0.00798 (−0.0992)	
Same party governor x honest services						−0.0670 (−1.026)
Republican governor	0.0123 (0.397)	0.0138 (0.445)	0.0125 (0.386)	0.0155 (0.493)	0.0144 (0.452)	0.0180 (0.576)
Republican Majority, US house	0.0640 (1.614)	0.0638 (1.609)	0.0636 (1.603)	0.0630 (1.589)	0.0630 (1.588)	0.0664* (1.670)
Republican majority, US senate	−0.0117 (−0.403)	−0.0126 (−0.436)	−0.0121 (−0.419)	−0.0117 (−0.405)	−0.0120 (−0.416)	−0.0102 (−0.351)
ln(Population)	0.579*** (12.02)	0.578*** (11.99)	0.578*** (11.99)	0.578*** (12.00)	0.578*** (11.99)	0.577*** (11.95)

Table 6 (continued)

Variables	(1) Full sample Convictions	(2) Full sample Convictions	(3) Full sample Convictions	(4) Full sample Convictions	(5) Full sample Convictions	(6) Full sample Convictions
ln(Income)	0.437	0.438	0.437	0.433	0.436	0.421
	(1.615)	(1.622)	(1.614)	(1.604)	(1.611)	(1.555)
College	−0.0352***	−0.0351***	−0.0353***	−0.0351***	−0.0353***	−0.0348***
	(−3.374)	(−3.362)	(−3.382)	(−3.367)	(−3.375)	(−3.335)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
	Yes	Yes	Yes	Yes	Yes	Yes
Constant	−12.16***	−12.16***	−12.14***	−5.766	−5.807	−11.99***
	(−5.143)	(−5.140)	(−5.127)	(−0.941)	(−0.947)	(−5.064)
Observations	1982	1982	1982	1982	1982	1982
Number of states	50	50	50	50	50	50

z-statistics in parentheses

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

political adversaries, as suggested by Gordon (2009), then those cases may be less likely to end in a conviction, and non-convictions do not show up in our dataset. Finally, the election year rise in cross-party indictments may be offset by a similar post-election rise in same-party indictments. That is, elections may impact the *timing* of same-party and cross-party indictments, but not the total number of such indictments when viewed over a full election cycle.

Next, we consider whether the duration of a party's control of the Justice Department increases the scope of politically motivated corruption convictions. Tenure in the executive branch might matter for politically motivated corruption convictions if, for example, it takes some time for political leaders to communicate political expectations to prosecutors or to weed out insufficiently partisan prosecutors, such as many observers believe happened under the George W. Bush administration in 2007.

To test that conjecture, we consider three specifications, interacting *Same Party Governor* with the variable *tenure*, which equals the number of years the president's party has occupied the White House, with *High Tenure*, a dummy variable for whether a party's presidential tenure exceeds the median of 4.5 years, and with *Tenure2* and *Tenure3*, dummy variables for party presidential tenure in the second and third terciles of the distribution. As seen in columns 3 through 5, none of these interaction terms is significant. Thus, our results provide no evidence that partisan prosecutorial bias increases over a party's presidential tenure.

Finally, we control for the potential effect of the presence and interpretation of *Honest Services* law on the scope for partisan behavior in pursuing federal corruption convictions. An important aspect of the legal framework supporting federal corruption charges is the statute related to honest service fraud. Honest service fraud is defined by the "honest services statute", 18 USC. § 1346, as a "scheme or artifice to deprive another of the intangible right of honest services". The law also has been interpreted to apply to private fiduciary relationships, and has been used to convict several high-profile defendants, including Washington lobbyist Jack Abramoff, former Enron CEO Jeffrey Skilling, and a host of public officials, such as former Illinois Governors George Ryan and Rod Blagojevich, former Alabama Governor Don Siegelman, and former Congressmen Duke Cunningham of California and Bob Ney of Ohio. This statute was added to the US Code in November of 1988; it was criticized as being overly broad. In June of 2010, the law was given a narrow interpretation by the US Supreme Court in *Skilling vs. United States*, so that it covered only bribes and kickback schemes. In the intervening years, 1989–2009, it is possible that, owing to its broad nature, the law expanded the scope for US Attorneys to pursue federal corruption cases in a partisan fashion. We define a dummy variable *honest*, which equals 1 for the years 1989 thru 2009, and zero otherwise, to coincide with the period in which the honest services statute was interpreted broadly.

To test whether the broad interpretation of the honest services statute led to more corruption convictions and a wider scope for partisan prosecutorial bias, we augment our baseline regression by entering an interaction term equal to the product of *Honest Services* and *Same Party Governor*. As seen in column 6, the coefficient on this interaction term is not significantly different from zero. Thus, our results are not consistent with the hypothesis that the honest services statute played an important role in facilitating the partisan pursuit of corruption cases.

Our investigations fail to provide evidence of systematic time variation in the intensity of partisan prosecutorial bias. In particular, we do not, however, find that the intensity of partisan prosecutorial bias varies significantly across election cycles, over a party's presidential tenure, or with changes in the interpretation of the honest services statute.

4.5 Evidence on total criminal convictions

The evidence presented above is consistent with our central hypothesis that federal prosecutors respond to political incentives to pursue politically sensitive public corruption cases more aggressively in states that are politically aligned with the federal government. However, it also is possible that the rise in public corruption convictions reflects a different, and plausibly more benign, mechanism. In particular, it may be that the decline in corruption convictions in politically aligned states reflects differences in either the sizes or productivities of the prosecutorial teams assigned to districts in politically aligned and non-aligned states. And, indeed, Alt and Lassen (2014) provide evidence in support of the second mechanism, finding that the number of a state's federal prosecutors is determined by politics.

While interstate differences in the number of federal prosecutors might reflect the unwanted influence of partisan considerations in the Justice Department's assignment decisions, they potentially are less troubling than partisan prosecutorial bias on the part of US Attorneys. Not only are resource differentials easier to detect and correct than partisan exercises of prosecutorial discretion, but because they occur one step removed from actual cases, it is harder to claim that partisan bias influenced the outcome of any particular case. Thus, it is important to determine whether our evidence reflects partisan allocation of prosecutorial resources *across states,* as Alt and Lassen (2014) find, or partisan allocation of prosecutorial resources *within states*, owing to the partisan exercise of prosecutorial discretion, as we argue here.

If the decline in corruption convictions in politically aligned states is a byproduct of differences in the sizes and productivities of the prosecutorial teams in politically aligned and non-aligned states, then state-federal political alignment should influence the number of non-corruption criminal convictions as well. To test whether that is the case, we assembled data on the total number of criminal convictions by state and year.[20] We use those data in two ways. First, we test for a statistically significant relationship between state-federal political alignment and total federal criminal convictions. To do so, we reconsider our baseline specification, but enter the total number of criminal convictions as the dependent variable. As seen in column 1 of Table 7, using federal criminal convictions as the dependent variable, the coefficient on *Same Party Governor* is slightly negative and not significantly different from zero. In column 2, we repeat that exercise, restricting the sample by omitting the first 2 years of a party's presidential tenure, with a nearly identical result. Thus, a statistically significant relationship between state-federal political alignment and the total number of federal criminal convictions is not evident.

Next, we test to see if a statistically significant relationship can be found between total criminal convictions and convictions for public corruption.[21] To do so, we return to our baseline specification with public corruption convictions as the dependent variable, but augment the regression by controlling for the number of federal criminal convictions. As seen in column 3, the coefficient on federal criminal convictions is not significant. Thus, no evidence is found of a statistically significant relationship between total criminal convictions and the number of federal corruption convictions. Moreover, controlling for criminal

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[20] These data are available from various years of the *United States Attorney's Statistical Fiscal Year Report,* which is published by the Justice Department.

[21] While total criminal convictions clearly not are exogenous, their inclusion helps us to rule out alternative explanations for our key finding.

Table 7 Corruption convictions and total criminal convictions

Variables	(1) Full sample Total cases	(2) Tenure > 2 Total cases	(3) Full sample Convictions	(4) Tenure > 2 Convictions
Same party governor	−0.0232	−0.0215	−0.0954***	−0.120***
	(−1.164)	(−0.954)	(−2.851)	(−3.227)
ln(Total Cases)			0.0149	0.0108
			(0.413)	(0.263)
Republican governor	−0.0354*	−0.0438**	0.0120	0.0317
	(−1.911)	(−2.006)	(0.384)	(0.878)
Republican majority, US house	0.0886***	0.112***	0.0685*	0.0831*
	(3.836)	(4.100)	(1.723)	(1.796)
Republican majority, US senate	−0.00954	−0.0171	−0.0130	−0.0234
	(−0.547)	(−0.834)	(−0.451)	(−0.703)
ln(Population)	0.534***	0.530***	0.570***	0.535***
	(16.97)	(14.23)	(10.43)	(8.469)
ln(Income)	0.0217	−0.0503	0.420	0.565*
	(0.127)	(−0.248)	(1.532)	(1.821)
College	−0.0266***	−0.0122*	−0.0348***	−0.0374***
	(−4.266)	(−1.665)	(−3.325)	(−3.208)
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Constant	−5.874***	−5.425***	−11.90***	−12.44***
	(−3.804)	(−2.882)	(−4.930)	(−4.396)
Observations	1950	1450	1932	1434
Number of states	50	50	50	50

z-statistics in parentheses

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

convictions, the coefficient on *Same Party Governor* remains negative and significant at the 1% level and, indeed, nearly is identical in size to our baseline estimate. We obtain similar results when we rely on the restricted sample, as seen in column 4.

In summary, our findings do not support the notion that the link between state-federal political alignment and public corruption convictions is a statistical artifact reflecting a general reduction in the sizes or productivities of the prosecutorial teams in politically aligned states. In particular, no evidence is found of a statistically significant relationship between state-federal political alignment and the total number of federal criminal convictions in a given state. In addition, state-federal political alignment is seen to reduce significantly public corruption convictions while controlling for the total number of federal criminal convictions in a state. That evidence is consistent with politically motivated prosecutorial bias, rather than the politically motivated allocation of prosecutorial resources across states. In particular, state-federal political alignment appears to affect the allocation of prosecutorial resources within a given state, tilting them away from cases involving public corruption, rather than the allocation of prosecutorial resources across states, which we would expect to increase criminal convictions more generally.

5 Conclusion

We present evidence that the number of federal corruption convictions falls—meaningfully and significantly—when partisan alignment exists between state governors and the White House. State-federal political alignment reduces the number of federal public corruption convictions by roughly 9% at the state level. That effect is robust to a variety of controls for state political environments, is more precisely estimated for Republican than Democratic administrations, and is not sensitive to electoral cycles, years of administration tenure, or changes in honest services law. Finally, evidence from the total number of federal criminal convictions in each state suggests that the observed impact of political alignment is driven by the politically motivated reallocation of prosecutorial resources within given states, rather than differences across states in the sizes or qualities of prosecutorial teams.

Given the number of elected and appointed officials involved in the investigation, indictment, and prosecution of public corruption, identifying the central reason for the effect we observe remains a challenge. However, our results are consistent with the hypothesis that US Attorneys, who have both the opportunity and incentive to do so, pursue corruption cases in a partisan manner. Appointed by presidents and confirmed by the US Senate, US Attorneys are powerful federal officials with career ambitions and political allegiances, and they are uniquely positioned to pursue corruption cases strategically—or not—as they see fit. It could be that US Attorneys act according to internal partisan biases that cannot be checked; perhaps Democrats work harder to target corruption in Republican states, for example, than in Democratic ones. Disentangling those competing motivations will require further research. It is clear, however, that our findings—robust to a large number of institutional, political and demographic considerations at the state level raise two important policy questions: how can (1) opportunities or (2) incentives for partisan prosecutorial bias be constrained?

Limiting partisan opportunities is a difficult challenge. Rooted in prosecutorial discretion, prosecutors have the discretion to make room for partisan bias in corruption cases because they have significant leeway in choosing which cases to pursue. In addition, convening grand juries—an important source of prosecutorial power—is required to secure indictments in federal cases. Finally, the challenge of developing an alternative arises. One reason that prosecutors retain so much power and discretion is because, as a result of the nature of their positions, they serve as a central point of information and decision-making power in federal legal proceedings. Outside federal agencies already serve an important investigatory role in building a case against would-be defendants, so it's difficult to imagine a system in which prosecutorial oversight somehow is restricted or decentralized. As long as prosecutors retain the power to decide which cases to pursue—and, later in a case, the severity of charges, sentence lengths, and the details of plea bargains—they will retain the opportunity selectively to pursue cases with strategic biases in mind. The question of whether prosecutors more frequently turn down cases recommended for indictment by referring agencies when partisanship comes into play will require further analysis using more detailed data.

Altering the existing incentive structure poses similar problems. Enforcing federal laws always will require federal officials to adjudicate criminal cases. No immediately obvious alternative to US Attorneys seems conceivable, aside from potential remanding all federal criminal cases to the state courts, and given the importance of state-level party control in our sample, that change wouldn't come without its own set of complications. Reducing the political payoff to partisan prosecutions is potentially a more feasible approach. One

method could be to require bipartisan consensus for the nomination of federal prosecutors and judges, but given the recent polarization in Congress, that approach seems unlikely to be feasible. Alternately, more media reporting of the partisan prosecutorial bias phenomenon could make the practice more politically costly by raising public awareness and, therefore, pressure on federal prosecutors. Although that method could create other problems for prosecutors, who would be subject to greater scrutiny that could neuter their authority, it also might prevent the abuse of power for partisan purposes.

One challenge to implementing the public pressure approach would be demonstrating the political cost of partisan prosecutorial bias. Existing research has reported evidence that corruption charges can have significant impacts on the accused in future elections (Peters and Welch 1980; Welch and Hibbing 1997). However, that work needs updating, and a broader question can be asked about party-level impacts. For instance, if Democrat-appointed prosecutors overwhelmingly target Republican elected officials in a given district, does that have the long-term effect of eroding public trust in Republicans public officials? Those are only a few of the questions raised by our investigation of partisan prosecutorial bias, and we hope to explore them in later work.

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