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# Polarization and Corruption in America

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## Abstract

Using panel data from the US states, we document a robust negative relationship between state-level government corruption and ideological polarization. This finding is sustained when state polarization is instrumented using lagged state neighbor ideology. We argue that polarization increases the expected costs of engaging in corruption, especially deterring marginal low-level corruption. Consistent with this thesis federal prosecutorial effort falls and case quality increases with polarization. Tangible anti-corruption measures including the stringency of state ethics' laws and independent commissions for redistricting are also associated with increased state polarization.

Keywords: Corruption, Ideological Polarization, Media.

JEL: K4; H0

"The one thing that gnaws on me is the degree of continued polarization."

Barack Obama - 01/24/16

## 1 Introduction

Democracy, unfortunately, does not eliminate corruption. In international data Treisman (2000) finds it to be a rather weak constraint and Persson et al (2003) note that corruption, to varying extents, persists in mature democracies. Using cross-country data, Testa (2010) and Brown et al (2011) uncover the intriguing regularity that corruption falls with political polarization in democracies. This finding is intriguing because generally polarization is bemoaned in the political-economic literature as well as in public discourse, as illustrated by the above quote from the previous U.S. President.<sup>1</sup>

In theory polarization, defined as the ideological distance between parties, affects the incentives and opportunities for public officials to misuse their office for private gain. Following the evidence found in the cross-country setting the argument of this paper is that accountability increases. Ideological distance between parties increases the ideological consequences of government replacement (Testa, 2012). If incumbents are ideologically motivated and electorally punished for corruption, then the price of corruption increases and hence polarization increases accountability. Polarization also reduces the likelihood that parties collude in rent-seeking activities, or similarly strengthens the opposition's incentives to monitor the corruption of incumbents, as hypothesized in Brown et al (2011). Again the extent to which politicians are held accountable increases with polarization.

This paper tests the hypothesis that party polarization reduces corruption using panel data from the United States.<sup>2</sup> This testbed offers several major advantages over the international setting. Firstly, as Besley and Case (2003) observe, the common broad institutional and constitutional setting rules out many sources of unobserved heterogeneity, a major concern in the international context. Secondly, the data are considerably more extensive across time, covering the 48 contiguous states for the period 1976-2004. This permits using fixed (state) effects in the econometric analysis, hence time-invariant unobserved heterogeneity is controlled for. Third, as detailed below, the corruption data - taken from actual federal corruption convictions - are better measured than the corruption perceptions data used at the international level. Fourth, the data measuring political polarization are also superior, depending

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<sup>1</sup>The literature identifies adverse consequences, for instance, for policy efficiency (Schultz 2008; Azzimonti and Talbert 2014) and private investment (Azzimonti 2011).

<sup>2</sup>The substantial quantitative literature looking at corruption across the US states has pointed to various factors ranging from cultural diversity to political competition and divided government (Glaeser and Saks 2006; Alt and Lassen 2003, 2008), but it has not as yet investigated the effect of ideological polarization.

on actual voting behavior of elected representatives within a particular institutional framework. Fifth, the US context allows a deeper examination of how polarization may foster greater accountability. The mechanism proposed has implications for prosecution effort and case quality, for which data are available. Data are also available for actual anti-corruption measures.

We argue for causal inference by developing an instrumental variable (IV) for state-level ideological polarization at a particular point in time. As discussed below, there is broad evidence that both individual political attitudes and policy choices diffuse slowly across geographic space, from neighbor to neighbor and from state to state. Taking inspiration from this evidence we conjecture that liberal shifts in neighboring state average Democrat ideology, and conservative shifts in neighboring state Republican ideology, exogenously cause own-state polarization to increase with a lag. The data support this conjecture especially in the case of the Democrats. Under the identifying assumption that lagged political platforms in neighboring states are exogenous, the instrumental variable regression output can be understood to represent estimates of a causal relationship from polarization to corruption.<sup>3</sup>

The consistent finding is that lower corruption coexists with increased polarization. The estimated effect is sizeable. Under the identifying assumption that lagged neighbor democratic ideology only affects state corruption via its effect on state ideological polarization, an exogenous one standard deviation increase in polarization is estimated to cause nearly a half standard deviation reduction in corruption.

Furthermore when polarization increases, incumbent politicians have a greater incentive to reduce corruption *de jure* and pass visible anti-corruption measures so as to credibly signal positive valence. Consistent with this thesis we find that both the adoption likelihood and stringency of ethics laws are observed to increase with polarization. Similarly, more polarized states are also found to be more likely to adopt a politically independent commission for the redistricting process for state legislative districts, with the implication of reduced gerrymandering. In support of the accountability thesis policies that are widely recognized as effective at reducing corruption are thus increasingly enacted with state polarization.

State-level corruption convictions depend on prosecutorial effort and Alt and Lassen (2014) find some evidence that such resources are in part determined by political alignment between federal and state government. We therefore extend the empirical analysis to consider how the federal allocation of prosecutorial resources is related to state level polarization and document, novelly, a (somewhat weak) negative relationship in the data. This finding is consistent with our argument that polarization serves to increase political accountability, because case referrals will fall

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<sup>3</sup>Importantly, as discussed below, the IV results are especially strong for smaller states, for whom the potential reflection problem - that own ideology affects neighbors - is less likely to be pronounced.

when there is less actual corruption taking place. The *efficient* response at the federal level, whether aligned or opposed to state-level politics, is to reduce prosecutorial resources, simply because there is less corruption taking place. However, prosecutorial effort may alternatively be declining with polarization for *strategic* reasons, which in turn could explain the negative correlation between corruption convictions and polarization. Nonetheless under strategic prosecution, one would expect that the corruption-polarization relationship would change depending on whether or not state-level incumbent politicians are aligned with the President. Prosecutorial effort is hypothesized to be especially reduced with polarization under alignment, whilst it is hypothesized to increase with polarization when state-incumbents are not aligned with the President. We find that the relationship between prosecutorial resources and polarization holds irrespective of whether the State Governor is aligned with or opposed to the President and therefore conclude that the evidence is only consistent with the accountability thesis.

Relatedly, case-quality data taken from Gordon (2009) permits further comparison of strategic prosecution and accountability as competing explanations for the main finding. Gordon (2009) finds that the average case-quality, measured by subsequent punishment, for allies of the President is higher than that for the President’s political opponents. Strategically prosecutors turn a ‘blind eye’ to marginal corruption acts by allies. We document, again novelly, that case quality also increases with polarization, but comparably for both defendants politically aligned with the President and also for those without political affiliation. This suggests that strategic considerations are not the only driver of the relationship between case-quality and polarization. Moreover the evidence is consistent with our auxiliary hypothesis that greater polarization raises the expected costs of engaging in corruption, essentially the substance of increased accountability. When accountability is higher, only high-level corruption with higher potential rewards to those committing it is rationalizable, and hence case quality rises.

The next section develops a theoretical discussion of how polarization affects corruption. Section 3 presents the data. Section 4 discusses the empirical strategy. Section 5 presents the main results and the robustness checks. Section 6 examines how tangible de jure anti-corruption measures change with polarization and Section 7 investigates the relationship between polarization and the allocation of federal prosecutorial resources. Section 8 offers concluding remarks.

## 2 Theoretical Mechanisms

The literature suggests several avenues through which ideological polarization, defined as the ideological distance between candidates or parties, can affect the level of corrupt activity.

Testa (2010 and 2012) proposes that incumbents will reduce corruption with increased polarization because of a sharpened trade-off between current rent extraction and future policy. In this analysis incumbents care about future ideological policy and whilst corruption brings private benefits it also harms electoral prospects. The costs of election loss increase with greater ideological distance as the successor implements a platform far from the incumbent's preferences. Ideological polarization therefore helps to keep elected politicians accountable, lowering corruption.

Another possibility, advanced by Brown et al (2011), is that the capacity to collude in corruption is facilitated when parties are ideologically proximate. The likelihood of government coalition plausibly increases with ideological proximity (Laver and Schofield 1998). Parties may operate formally or informally in coalition, or similarly parties may anticipate greater likelihood of future coalition given ideological proximity. Given that rent-seeking opportunities are concentrated in the hands of incumbents, there is greater facility to collude, or for opposition politicians to turn a blind eye to incumbent corruption, when they are implementing ideologically consensual policy. Equivalently, if monitoring requires costly effort, then it must be justified by the benefits of expected office change. The consequences of office change are reduced when parties are ideologically proximate. Ideological proximity thus weakens the extent to which politicians are held accountable for acts of corruption.<sup>4</sup>

A corollary of the accountability hypothesis is that when polarization is high, incumbent politicians have a greater incentive to pass specific legislation designed to counter corruption. Because corruption *de facto* is lower, it becomes easier to pass visible legislation opposing corruption. Incumbent politicians can thus more easily signal their own anti-corrupt (or positive valence) credentials by proposing and passing such legislation. As a result we hypothesize that corruption '*de jure*' and corruption '*de facto*' will fall with polarization.

The underlying mechanisms can be characterized as situations where government officials consider the costs and benefits of engaging in actual corruption, and indeed the costs and benefits of passing anti-corruption legislation. In Testa (2010 and 2012), for a given probability of being detected and consequent loss of electoral support, the ideological costs of election loss are raised. In Brown et al (2011) one might think of detection probability being raised when opposition politicians are ideologically distant. In both cases the expected costs of corruption increase. As a consequence, as well as reducing the total amount of corruption, polarization should be especially potent at the margin: low-level corruption will no longer be rationalizable, whilst high-level corruption with potentially higher rewards may still

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<sup>4</sup>Elmelund-Præstekær (2010) provides evidence related to this mechanism if we consider "negative campaigning" as a particular case of monitoring of the incumbent. He finds that opposition parties with large proportions of party identifiers (i.e. who are partisan and ideologically distinct) in their membership are more likely to use negative campaigning, i.e. factual (or rhetorical) attacks against other parties, using data from Danish election campaigns.

be feasible. Case quality, as measured by sentencing severity, might therefore increase with polarization all else equal. Below we use data from Gordon (2009) to investigate this auxiliary hypothesis.

Brown et al (2011) also document mechanisms through which polarization may instead increase corruption. Suppose that candidates compete on both "position issues" (ideology) over which the distribution of voter preferences is defined on a left-right axis and "valence issues" as those candidate characteristics that all voters value in the same way (such as being against corruption) (Stokes 1963). If voters are ideologically concentrated, then candidates are also likely to be ideologically close, and hence contesting on the left-right dimension may not be very profitable, while even a limited valence advantage can yield significant electoral benefit. In this situation parties are strongly induced to compete on valence, for instance by committing less corruption and/or questioning the opponent's integrity. Conversely, increased ideological diffusion across voters and distance between parties arguably renders competition on valence less potent (for example if voters are habitual), thus reducing the cost of engaging in corruption and similarly reducing the incentives for the opposition to monitor corruption by the incumbent.<sup>5</sup>

### 3 Data

#### 3.1 Corruption Convictions

State-level corruption in a given year is measured as the number of federal convictions for corruption-related crime normalized by state population (following Glaeser and Saks 2006 and others). Corruption is defined as 'criminal abuses of public trust by government officials' and convictions data are reported annually by the Public Integrity Section of the US Department of Justice.

Cases are prosecuted at the federal level by the Public Integrity Section as well as by US Attorneys. Federal authorities have jurisdiction over robbery or extortion affecting interstate commerce, theft, and possible bribery where entities receive more than \$10,000 in federal funds, the mail fraud statute, conspiracies to defraud the federal government, and the RICO statute (Gordon 2009) - which cumulatively provide them with considerable capacity to pursue cases related to state and local governments. Prosecutions from state district attorneys or attorneys general are not available but are estimated to be only around 20% of the total (Corporate Crime Reporter 2004).

This measure aggregates convictions of state-, federal-, and local-level officials, plus "others involved." As pointed out by Campante and Do (2014), on the one hand,

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<sup>5</sup>Curini and Martelli (2010) provide related evidence that the ideological distance between the Communist Party (DCI/PDS) and the government in post-war Italy reduced the emphasis placed by that party on political corruption issues during the government investiture debates.

this adds noise to the extent that the empirical context is state governments. On the other hand, it does still contain relevant information, both because state officials represent a sizable fraction of total convictions at the state level, and because one would expect that a culture of corruption arising at that level would spill over to lower tiers of the state government.

As noted by Glaeser and Saks (2006) these data have a number of advantageous properties for use in testing theories of corruption. First, the data correspond to actual convictions. This contrasts with cross-national studies such as Testa (2012) and Brown et al (2011) that rely on subjective surveys of experts and firms. Second, because the convictions are determined through federal prosecution, the risk of collusion between prosecutors and officials is substantially lessened and in principle homogenized across states. Were the prosecutions made at the state level then potentially the more corrupt states could have reduced convictions due to corruption of the judicial process itself. Thus, the convictions data are considered to be comparable across states.

The sample extends from 1976 to 2004 (as our polarization data are available until 2004) for the 48 contiguous states, covering over 21,000 corruption convictions.<sup>6</sup> Figure 1 depicts the evolution of the average number of convictions normalized by state population across the US by year. The figure also depicts separate data for the ‘South’ and ‘North’ and shows similar patterns for the two regions.<sup>7</sup> Figure A.1 in the appendix depicts the evolution state by state. The data at the national level exhibit an upward trend with a peak of around 4.2 convictions per million inhabitants in 1990-91. Alt and Lassen (2014) explain this peak by increasing efforts allocated to corruption (e.g. number of Attorneys, number of investigators) in the 1980s, which first increased the detection and conviction of corruption but deterred corruption in the long-run. There is also considerable geographical variation, with state-averages depicted in Figure 2. Normalized convictions rates range from around 1 for Oregon and Washington to more than 6 for Mississippi and Louisiana.

\*\*\*Insert Figures 1 and 2\*\*\*

### 3.2 Ideological Polarization

To construct party polarization at the state level, we use the state government ideology measures produced by Berry et al (2010), which cover the period inclusive of 1976-2004.<sup>8</sup> They approximate the ideology of Democrats/Republicans in the

<sup>6</sup>There are a small number of missing observations in the convictions data. For these cases linear interpolation is used in order to maximize the size of the dataset.

<sup>7</sup>Besley, Persson and Sturm (2010) show that political competition was much lower in the US South, compared with the rest of the US, at least prior to the Voting Rights Act of 1965 which predates our sample.

<sup>8</sup>According to Berry et al (2010), a major advantage of this version of their government ideology measure is that the ideal points of the Congress members are comparable from one session to



state government by using the NOMINATE scores of US Federal Congress Democrat/Republican representatives from the same state and assign the average score to the state-level politicians.<sup>9</sup> This requires the assumption that the ideology of Democratic/Republican state officials mirror the ideology of the party's federal congressional delegation from this state.

The NOMINATE common space scores produced by Poole and Rosenthal (1997) are used to identify the ideal point of each member of the party's delegation based on actual voting (roll-call voting) in the Congress according to two dimensions. The primary dimension through most of American history has been "liberal" vs. "conservative". A second dimension picks up differences over slavery, currency, nativism, civil rights, and lifestyle issues during periods of American history. Berry et al (2010) focus on the first, economic, dimension based on the basic issue of the role of government in the economy. An example of roll-call voting used in this dimension is the House's vote on President Clinton's welfare reform package (the Personal Responsibility and Work Opportunity Act of 1996) that can be modelled through the economic liberal-conservative dimension. Another example is the Senate vote on the Balanced Budget Amendment of 1995.

Unidimensionality, in particular along the first economic dimension, is justified by Poole and Rosenthal (1997) who find that voting in Congress became almost purely one-dimensional following the passage of the civil rights laws in the 1960s. Indeed, in the wake of World War II the two dimensions were required: (1) the liberal-conservative dimension and (2) the conflict over race and civil rights. However, with the passage of the 1964 Civil Rights Act, the 1965 Voting Rights Act, and the 1967 Open Housing Act, (all of which pre-date the sample period under investigation,) the second dimension declined in importance and race related issues - affirmative action, welfare, Medicaid, etc. - became questions of redistribution and thus became part of the liberal-conservative dimension (Poole and Rosenthal, 1997). Thus using only the first dimension does not narrow the scope of the Berry's ideological polarization index for our observation period.

The Berry et al (2010) measures are thus unidimensional conservative-liberal ideology scores produced for each state at the level of the party and varies over time (as the party's congressional delegation changes). The data are denoted  $PARTYID\_RR_{i,t}$  and  $PARTYID\_DD_{i,t}$  for the Republican and the Democrat parties respectively in state  $i$  in year  $t$ , which in principle vary between 0 (extreme

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the next and between the House and the Senate, as opposed to their earlier measures based on interest-group ratings (Berry et al 1998).

<sup>9</sup>In some states, in some years, however, there are no members of Congress for one of the parties. This represents less than 7% of state-party-year observations. To estimate what the "mean ideology" score for these delegations would have been in these years, Berry et al (2010) used a regression-based method, relying on the ideology of the other party, as well as the ideology of the same party in similar states (based on geographic proximity as well as similarity in political culture). A more detailed explanation can be found in Berry et al (1998).

conservative) and 100 (extreme liberal).

Within the sample the Republican party ranges from Idaho in 1991 ( $PARTYID\_RR = 18.11$ ) to Massachusetts in 1976 ( $PARTYID\_RR = 57.35$ ), whilst the Democrat party were at their most conservative in Virginia in 1981 ( $PARTYID\_DD = 39.08$ ) and at their most liberal in South Dakota in 1976 ( $PARTYID\_DD = 86.59$ ). Over time the parties have, on average, diverged. Figure 3 plots the evolution of the average scores for the Democrats and Republicans across the US by year, as well as separately for the South and the North. The trend towards polarization is clear from the early 1980s onwards. While the Republicans have continuously become more conservative over time, the Democrats moved to the center of the political spectrum prior to the early 1980s since when they have on average become more liberal.

\*\*\*Insert Figure 3\*\*\*

Our baseline polarization measure ( $POL$ ) is the ideological distance between the Republican party and the Democrat party, corresponding to the polarization measure used in Garand (2010). Thus polarization ( $POL$ ) within a particular state-year is measured as:

$$POL_{i,t} = |PARTYID\_DD_{i,t} - PARTYID\_RR_{i,t}| \quad (1)$$

This series exhibits interesting variation across time and space. Figure 1 depicts average polarization across time and figure 3 depicts the annual average democrat and republican positions. In the early part of the sample both parties are measured to be moving rightwards, hence average polarization is somewhat static prior to the 1980s. However since that time the parties have diverged, and hence polarization has markedly increased. As with corruption there are similar patterns observed in the North and South of the US.

The mean value for  $POL$  is 33.38 and its standard deviation is 8.45. The least polarized state-year in the sample was Virginia in 1981 ( $POL = 3.50$ ), where the Republicans and Democrats had almost the same ideological score (35.58 and 39.08 respectively). The most polarized state-year was Arizona in 2004 ( $POL = 56.67$ ) - this latter case reflects the presence of one of the most conservative Republican party in our sample ( $PARTYID\_RR = 22.27$ ) together with one of the most liberal Democrat party ( $PARTYID\_DD = 78.94$ ), led by the Democrat governor, Janet Napolitano.

A key advantage of the polarization measure used in this paper is that it varies across time as well as across states. For instance, Idaho and Mississippi were respectively the most ( $POL = 55.43$ ) and the least ( $POL = 16.57$ ) polarized states in 1976. By 2004, their respective polarization scores were all but equal ( $POL =$

34.41 for Idaho and  $POL = 33.58$  for Mississippi). This heterogenous within-state variation enables the use of fixed effects in the regression analysis.

Shor and McCarty (2011) provide an alternative polarization measure, generated from roll call voting data within state legislatures. Compared to Berry et al, Shor and McCarty's data "cover a wide range of policy issues, including foreign policies, national security, international affairs, social issues, fiscal policy, environmentalism criminal justice." Unfortunately this series only starts in the mid-1990s and hence implies a significantly reduced dataset in the panel analysis. Nonetheless, Shor and McCarty's data permit a validation test of the  $POL$  measure used here. Following the above strategy we produce an alternative measure of ideological polarization using the absolute distance between the median ideologies of the Democrat and Republican parties for each state-year, thus producing data for the period 1995-2013. The correlation between the two polarization measures is 0.7, which makes us confident in the reliability of  $POL$ . Berry et al (2013) also found that the two separate measures of state government ideology converge and that both are valid measures of the underlying variables.

## 4 Empirical Strategy

### 4.1 Specification

The analysis first employs panel data drawing on the specification used in Alt and Lassen (2014). They analyze corruption convictions in the panel of US states over the period 1977-2003. Our study augments their specification with the polarization data ( $POL$ ) described above and extends the observation period to 1976-2004. The regression specification employs the same control variables used as standard by Alt and Lassen (2014): relative wages for public employees, male wage inequality, divided government (where the legislature and executive are controlled by different parties), average constant dollar income per capita, per capita constant dollar state government revenues or expenditures, the population share with high school education, state population, gubernatorial one-term limit legislation, gubernatorial two-term limit legislation, the state level of unemployment, Berry et al's (1998) measure of citizens' ideology, and the degree of urbanization on its own and interacted with state party control (measured as the Democrat share of the state senate).<sup>10</sup>

Whilst the data and specification both represent considerable improvements over cross-country studies, straightforward panel estimation using contemporaneous data would not by itself establish undeniable causality from polarization to corruption. State-level party positioning, and hence polarization, have their own driving forces,

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<sup>10</sup>The source of these detailed data is described in Alt and Lassen (2014) who generously made their data available.

which problematically also may independently drive corruption. The analysis goes some distance towards addressing this by controlling for the main candidate explanations for polarization in the US, in particular income inequality (McCarty, Poole and Rosenthal 2006), fixed state and year effects and a broad set of socio-economic and demographic characteristics. Our specification also controls for alternative mechanisms that could account for a negative empirical relationship between corruption and polarization. For example Lindqvist and Östling (2010) find that ideological polarization is associated with lower public spending in international data. Smaller government, in turn, arguably reduces the opportunity to divert funds. Our specification addresses this as the size of the state government is included as a control. Other alternative mechanisms are discussed in the robustness checks.

Moreover because in reality there are substantial lags between the time when a particular corrupt act is committed and when its perpetrator is convicted, in the regression analysis, polarization as well as the other independent variables are measured with a 5-year lag. This lag length corresponds to the average of the actual cases that we examined and for which information were available.<sup>11</sup> Taking a 5-year lag of the data helps to lessen concerns about endogeneity; the polarization measures now substantially predate the observations on corruption.

## 4.2 IV Strategy

In order to isolate exogenous movements in state-level ideological polarization, the analysis also employs an instrumental variable strategy. State polarization is instrumented with past political position-taking in geographically neighboring states. The idea is that when the Democrat parties of the neighboring states (weighted by population) move left, then the state Democrat position potentially moves to the left with a lag, and state polarization increases exogenously. Symmetrically, state polarization exogenously increases with a lag if the neighboring Republicans move right.

This strategy is based on an extensive literature in political science studying processes of policy and preference diffusion across the US states and internationally. This literature provides widespread empirical evidence that particular states are more likely to adopt particular laws or policies if its neighboring states have already done so. Regional diffusion of policy is documented in Berry and Berry (1990) and Mooney (2001). Seljan and Weller (2011) find that proposals to limit state tax and expenditure are strongly determined by policy in proximate states. Note that these policies typically enter the categories of ‘economic’ and ‘social’ issues congruent with the liberal-conservative axis of the NOMINATE scores. Mooney (2001)

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<sup>11</sup>For instance, P. Hamilton, a former member of the Virginia House of Delegates, was convicted for bribery in 2011 but prosecutors based their case on a series of emails that began in 2006.

and Boehmke and Witmer (2004) find that policy diffusion is due to a social learning mechanism. A social learning process also underpins the diffusion of ideology between states. The credibility of the IV strategy is revisited below, but there is strong evidence that ideas and ideology transmit geographically.

The argument that both Democrats and Republicans will symmetrically respond to given external influences is called into question by Grossman and Hopkins (2015). In this analysis the Republicans are characterized by ‘doctrinal purity’ in terms of conservative principles of belief in small government, that are essentially fixed across time and space. Moreover as a simple matter of definition, political conservatives are resistant to change. As such, it seems plausible that Republicans may be relatively impervious to the influence of neighbors, with adherence to universal conservative principles the dominant driver of ideological positioning. In contrast Democrats are characterized as a "coalition of social groups seeking concrete government action." Particular forms of activist government and specific interventionist policies are now the domain of the Democrats, and this form of position-taking is more plausibly subject to the ebbs and flows of position-taking in neighbor states.

This reasoning yields two instruments: the 5-year lagged weighted average ideology of the Democrats ( $PARTYID\_DD$ ) and the 5-year lagged weighted average ideology of the Republicans ( $PARTYID\_RR$ ) of the adjoining neighboring states. Neighboring states of a particular state  $i$  are defined as those that share a land border. Weights are determined by population, reflecting the fact that a populous neighbor state should be more influential than a smaller one.<sup>12</sup> Thus state polarization in  $t$  is instrumented with the weighted party positions of neighboring states in  $t - 5$ . Using *lagged* neighbor-state party positions reduces the risk that state  $i$  itself influences the ideology of its neighbors. Nonetheless the possible ‘reflection problem’ is addressed below.

## 5 Polarization and Corruption

### 5.1 Panel Estimation

We turn to panel data analysis drawing on the specification used in Alt and Lassen (2014) for the observation period 1976-2004. Results from applying OLS estimation with robust standard errors clustered at the state level are presented in Table 1. Column (1) presents results of a specification including state fixed effects but without time effects and controls, using annual data. In this specification the estimated coefficient of ideological polarization is negative, though not statistically significant. However, when augmenting this specification with year fixed effects (column 2) or

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<sup>12</sup>For example Maine polarization is instrumented by lagged New Hampshire data alone. On the other hand New Hampshire is instrumented with lagged data from Maine, Massachussets and Vermont where Massachussets is weighted more heavily due to its relatively larger population.

indeed just including the set of (time-varying) controls described above (column 3), the estimated coefficient on polarization increases in magnitude and becomes significant at the 5% level. Thus the positive raw correlation observed in Figure 1, reflecting the upward co-movement in the two series, is an artefact of other temporal factors. Column (4) contains results including both state and year fixed effects as well as the controls, hence corresponds to our benchmark specification. The estimated coefficient of polarization is negative and significant at the 5% level.

\*\*\*Insert Table 1\*\*\*

Column (5) shows that the result is robust to controlling for state specific time trends. Column (6) more extensively addresses the distinctive political history of the US South since (at least) the 1964 Civil Rights Act. To control for this in a non-parametric way, we follow Besley et al (2010) and include interactions between the year effects and an indicator variable for the 16 southern states as defined by the US Census. The estimation results are very similar in this alternative specification, hence the results are not being driven by some particular feature peculiar to the US South.

Column (7) presents results employing the same specification as column (4) but using 3-year moving averages for the dependent variable (from  $t-2$  to  $t$ ) (as in Alt and Lassen (2014)) while the independent variables are still measured in  $t-5$ . This specification helps to eliminate random variations in yearly data as a particular investigation can result in several convictions in one year. This specification again finds a negative and statistically significant coefficient estimate for polarization. The estimated relationship is fairly small, but not trivial: A one standard deviation increase in polarization (8.33) is statistically associated with a decrease in the (3-year moving average) number of corruption convictions per million inhabitants by 1.26, which is around 19% of one standard deviation. If interpreted as a causal mechanism, then this suggests that the 5.21 point increase in average polarization between 1976 and 2004 (from 31.25 to 36.46) reduced normalized corruption by 0.8 units, that is to say 12% of one standard deviation, holding all else constant. Moreover, there are potential reasons for suspecting that the OLS estimate is biased towards zero: for example corruption and polarization might be positively co-determined through separate mechanisms.

This finding is robust to controlling for various political covariates potentially correlated with polarization which could separately influence corruption. Firstly high polarization measures could reflect a situation where one party has a large majority and the opposition is composed of a small number of extremist representatives. Thus the baseline specification is augmented with the share of democrats in the lower and upper state houses. Second, polarization may be related to political competition, hence we also include political competition data as used in Besley

et al (2010). Furthermore, governors facing finite term limits and not eligible for re-election may act differentially hence a dummy variable for whether the governor is facing a term limit is also included.<sup>13</sup> Importantly, as documented by Alt and Lassen (2014), state-level convictions depend on federal prosecutorial efforts, in part politically determined and potentially related to state polarization. Thus we control for the Alt and Lassen’s prosecution measure with the number of Assistant US Attorneys per million population. This mechanism is discussed more extensively later. Column (8) of Table 1 shows that including these controls has no quantitative effect on the estimated impact of polarization.

## 5.2 2SLS Estimation

Table 2 contains the first stage results of our IV strategy, regressing contemporaneous state polarization on the 5-year lagged weighted average ideology of the Democrats and the 5-year lagged weighted average ideology of the Republicans of the neighboring states. Column (1) contains results when controlling only for state and year fixed effects. Interestingly, state polarization is found to be influenced by neighboring Democrat position-taking but impervious to the positioning of neighboring Republicans. We interpret these results as consistent with the asymmetry argument of Grossman and Hopkins (2015). When neighbor Democrats become more liberal, then own-state Democrats become more liberal with a lag, own-state Republicans do not change and polarization increases. On the other hand own-state Republicans are resistant to neighboring Republicans’ position-taking because of their stronger adherence to ‘doctrinal purity’. Because neighboring Republican ideology is evidently a weak instrument, it is excluded from the rest of the analysis. Column (2) includes the controls of the baseline specification following Alt and Lassen (2014), as described above, but excluding the citizens’ ideology variable. This particular variable is a ‘bad control’ because, by the same logic of diffusion of ideological positioning, it will also be likely affected by neighboring ideological position-taking.<sup>14</sup> Importantly the effect of neighboring Democrats holds when controlling for economic, demographic and political factors likely to impact polarization.

\*\*\*Insert Table 2\*\*\*

One issue with the identification strategy is that the mechanism of geographic ideology diffusion arguably better applies in the instance of small states with large neighbors. In particular the biggest states in terms of population are likely to

<sup>13</sup>These data are from Besley and Case (2003).

<sup>14</sup>A concern here is that dropping citizens’ ideology from the second stage could entail an omitted variable bias if this directly affects corruption. We are confident that such is not the case because when including it in the second stage, citizens’ ideology is never found to be a significant determinant of corruption.

be less susceptible to lagged neighboring ideology. For example position-taking in Vermont is likely to be affected, with a lag, by position-taking in New York. However politics in New York is unlikely to be symmetrically affected by lagged ideological shifts in Vermont. Therefore we investigate whether the predictive power of neighboring ideology increases when the biggest states are excluded from the sample. We first rank states by their population in 2004 and exclude the 8 states with a population over 10 million inhabitants. Column (3) contains estimation results using this small-state subsample. In support of the identification strategy susceptibility to neighboring ideology turns out to be stronger in small states.

Some states might have a small population, but still be immune to neighbors who may also have small populations. For instance in 2014, Maine is one the least populous states, ranking 40th in terms of state population. However it also has little in the way of neighbors. Defining ‘relative population’ as own-population divided by neighboring population (the sum of all the neighboring states), then Maine ranks 6th. Column (4) reports the estimation for a subsample excluding the 8 biggest states in terms of relative population. Again in line with the diffusion hypothesis at the heart of our identification strategy, the estimated influence of neighboring ideology is enhanced in this subsample.

An additional issue is that although neighbor-state ideological position-taking is measured with a lag, the estimated first stage relationship could pertain from diffusion in the opposite direction from the state ideology to neighboring states, and thus reflected back given enough persistence in the ideology data.<sup>15</sup> The reflection problem is especially likely in the case of the larger states. However the estimated effect of lagged neighbour-state ideology is higher when the larger states are excluded from the sample, suggesting that it is not reflection driving the results.

Tables A.1 in Appendix present a battery of robustness checks of the first stage estimations. A further test of the reflection problem is to control for the 5-year lagged own-state Democratic ideology (as under the reflection problem this is correlated with lagged neighbor states ideology.) Column (1) of Table A.1 contains results demonstrating that the effect of the Democrat neighbors survives, suggesting an influence of the lagged neighboring ideology independent of lagged own-state ideology. Moreover, whilst contemporaneous state polarization correlates with past neighboring states’ ideology, a placebo test presented in column (2) shows that it is not statistically related to future neighbor states’ ideology. Hence we infer that the correlation found is not symptomatic of reflection.

The first stage relationship could alternatively stem from the possibility that own-state polarization in  $t$  may be associated with neighboring ideology in  $t$ , which is itself correlated with past neighboring ideology measured in  $t-5$  because of persis-

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<sup>15</sup>If  $Y_t$  and  $X_t$  are both correlated, and also both strongly persistent and the true Data-Generating Process is that  $X_{t-1}$  causes  $Y_t$ , it will also be the case that  $Y_{t-1}$  and  $X_t$  are statistically correlated.



tence in ideology over 5 years. Thus columns (3) and (4) in Table A.1 further control for the Neighboring Democratic ideology in  $t$  in addition to its past value in  $t-5$ , for the total and the small sample respectively. Our instrument, lagged Neighboring Democrat ideology survives the inclusion of its contemporaneous value, especially in the appropriate smaller sample, where the lagged variable reaches the 5% significance level against only 10% for the contemporaneous variable. Otherwise stated, these estimation result support the argument that it is the variation in neighboring ideology between  $t-5$  and  $t$  that affects own-state polarization.

A further possibility is that the correlation between lagged Democratic neighbors and own-state polarization may simply reflect some other underlying trend in the Democratic party position taking and thus capture a common regional or national trend. To address this possibility, column (5) presents the results of a placebo test alternatively using weighted averages of lagged ‘non-neighbor’ states as the driver of polarization. In this instance the coefficient estimate is negative and statistically insignificant. This represents further evidence in favor of the diffusion hypothesis working through lagged neighboring Democrat position taking.

In further support of the proposed mechanism of geographic ideology diffusion working through Democrat position-taking, Table A.2 in the appendix reports estimation results where the contemporaneous state Democrat and Republican ideology measures are regressed against both the 5-year lagged weighted average ideology of the Democrats and of the Republicans of the neighboring states. These regressions provide further insights on the asymmetry between neighbor Democratic and Republican position taking on polarization observed in Table 2. Indeed, Table A.2 supports that own-state Democrats are consistently influenced by their Democratic neighbors, especially in relatively small states, but not by their Republican neighbors. On the contrary, own-state Republicans are immune to position taking of the neighbor parties. We interpret this as evidence in favor of Grossman and Hopkins’s (2015) argument that position taking by the Democrats and Republicans follow distinct and asymmetric processes.

Table 3 contains the instrumental variable estimation results. The upper part of the table contains the second stage of the 2SLS estimation results using the baseline specification including all controls except citizens’ ideology. The lower part contains the first stage results, reporting the estimated coefficients for the lagged Democratic ideology of neighboring states, as described above. Column (1) contains results for the full sample of states. Although the instrument works as expected, the weak instrument F statistic does not exceed the standard benchmark value of 10. As outlined above, one important reason why our instrument is found to be weak in the full data set is that certain states might be relatively immune to the ideology of their neighbors. Thus column (2) excludes the 8 biggest states with a population over 10 million inhabitants from the sample. The F-test for weak instruments now reaches

the value of 16 thus strongly rejecting the null hypothesis and, importantly, the coefficient estimate for polarization in the second stage is negative and significant at 5%. This conclusion holds when excluding the 8 biggest states in terms of relative population, as shown in column (3).

\*\*\*Insert Table 3\*\*\*

The coefficient estimates for the effect of polarization on corruption in the IV regressions are considerably larger in magnitude than the corresponding OLS estimation results. This is consistent with the argument made above, that the OLS estimate conflates mechanisms linking the two variables to each other, and is conceivably biased downwards as a result. Under the identifying assumption that lagged neighbor Democrat position-taking only affects state corruption through its effect on own-state Democrat position-taking (and hence polarization), the IV estimates can be interpreted as the estimated effect on corruption for exogenous changes in polarization - changes in polarization that are not co-determined with changes in corruption. Using the coefficient estimate from column (2) of Table 3, an exogenous one standard deviation increase in polarization is estimated to reduce corruption by 2.87 convictions per million population - which is about 43% of a standard deviation in this variable. Using the parameter estimates from column (3) a one standard deviation increase in polarization is estimated to reduce corruption by 48% of a standard deviation.

A potential criticism is that if neighboring states transmit their political attitudes to a state, they could also transmit their attitudes towards honesty and corruption. The exclusion restriction would be violated if ideology and tolerance of corruption were correlated. Nonetheless column (4) includes the 5-year lagged weighted average corruption level of the neighboring states (weighted by population as for the instrument). The results show that state-level corruption is unaffected by the past corruption of neighboring states, which gives further support to the exclusion restriction.

### 5.3 Robustness: Cross-Section Evidence

This sub-section presents results using cross-sectional data, facilitating robustness checks using alternative polarization and corruption measures. The basic specification follows Glaeser and Saks (2006).<sup>16</sup>

\*\*\*Insert Table 4\*\*\*

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<sup>16</sup>The control variables used are log income, log population, percent college educated, share of government employment, percent urban, census region dummies, as of 2000.

The mean of corruption convictions for the period 2001-2010 is first regressed on various alternative polarization measures and other control variables as of 2000 so that the explanatory variables pre-date the dependent variable. Column (1) of Table 4 employs the polarization measure based on Berry et al's data as used in the panel analysis above and confirms a negative relationship between corruption and polarization. Column (2) employs an alternative polarization measure using data from Shor and McCarty (2011), derived from actual state politicians' voting.<sup>17</sup> The coefficient estimate of polarization is now higher in magnitude and reaches the 1% significance level, consistent with Shor and McCarty's claim that their state-government ideology measure is more accurate than that of Berry et al. This specification also confirms our results when employing a broader polarization measure, i.e. based in part on non-economic votes. Thus, this is our preferred polarization measure in the context of the cross-sectional analysis. Column (3) shows that the relationship is robust when using ideology data from Bonica (2013) based on campaign finance.<sup>18</sup>

Column (4) instead uses a measure of state mass polarization using data from Garand (2010).<sup>19</sup> This time the estimated coefficient is still negative but is now far from being statistically significant. Taken at face value, this suggests that the negative relationship between corruption and polarization is due to 'supply side' factors related to parties' positions and not 'demand side' explanations related to voters' preferences. This is consistent with the theoretical mechanisms discussed above, which focus on accountability increasing with the ideological distance between parties rather than voters.

The estimation results are also robust to alternative corruption measures. In column (5), following Maxwell and Winters (2004), the number of convictions (in 2000) is instead normalized by the number of popularly elected state and local officials.<sup>20</sup> Column (6) alternatively uses a measure of corruption perceptions following Campante and Do (2014), and in turn Saiz and Simonsohn (2013) who built a measure from an online search, using the Exalead tool, for the term "corruption" close to the name of each state (performed in 2009). In both cases a significant and negative coefficient estimate is sustained.

<sup>17</sup>The data are re-scaled for comparability with the Berry et al index. Note also the Shor and McCarty data are not available for Nebraska.

<sup>18</sup>Bonica (2013) used over 100 million contribution records from state and federal elections to estimate ideal points for a number political actors at the federal and state levels. We calculated the mean ideology of Democrats and Republicans by state in 2000 by focusing on state legislators, governors, and other state officials.

<sup>19</sup>Garand (2010) used updated data from Erikson, Wright, and McIver (1993) who employed survey data from the CBS News and New York Times surveys to generate estimates of state partisanship and ideology. This mass polarization measure is positively correlated with Berry et al's measure (.24) and the Shor and McCarty measure (.59). the data are again re-scaled for comparability with the other polarization measures.

<sup>20</sup>2000 is the most recent year of their data. Their database is available at <http://www.dartmouth.edu/~rwinters/Datasets.html>

Following Campante and Do (2014) Table A.3 in the appendix contains a placebo test examining an alternative outcome related to crime and federal prosecutorial efforts, but distinct from corruption. In particular here the dependent variable is criminal cases in relation to drug offenses brought by prosecutors to federal courts for each state (as of 2011). The coefficient estimate for polarization based on the Shor and Mc Carty data is in this instance positive but statistically insignificant, as shown in column (1). Column (2) shows that Berry et al’s ideology data provides the same conclusion while Bonica’s data confirms the absence of relationship in column (3). Table A.4 in the appendix further confirms the absence of relationship by using panel data for the 1997-2011 period, during which the drug cases data are available.<sup>21</sup> This provides some support for the mechanism investigated in this paper in that polarization affects government corruption via enhanced accountability, but not crime in general.

The theoretical discussion is suggestive that polarization reduces corruption committed by elected politicians in particular.<sup>22</sup> The convictions data from the US DoJ used above aggregates over private citizens and non-political government employees as well as elected representatives. As a further alternative, we make use of Gordon’s (2009) detailed data on a sample of national priority public corruption prosecutions by U.S. Attorneys in the Bush Justice Department (2004-2006) and the Clinton Justice Department (1998-2000). This source contains data on 445 defendants for the Clinton (223) and Bush (222) administrations, of which 414 (i.e. 93%) were guilty verdicts (Bush: 92%; Clinton: 94%). These data distinguish between government officials (66% in total; Bush: 61%; Clinton: 70%) and private citizens (34%), and also give detail on whether defendants were publicly affiliated with one of the parties (41% in total; Bush: 48%; Clinton: 35%).

In order to examine whether polarization reduces the corruption of partisan officials in particular, individual cases are aggregated at the state level for the two separate periods to develop a state-level measure of convictions (guilty verdicts) normalized by state population for the different categories of defendants. Table A.5 in the appendix reports the regressions of these conviction measures on the 5-year average polarization from 1999 to 2004 and the controls as of 2000. Echoing previous results, polarization is negatively correlated (significant at 5%) when convictions are aggregated over all defendants. The significant and negative correlation holds (significant at 10%) when focusing on the sample of government officials (excluding private citizens) and also on defendants with an identifiable partisan orientation po-

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<sup>21</sup>The data come from: <https://www.uscourts.gov>.

<sup>22</sup>However the theoretical arguments do not preclude the argument that polarization serves to lower corruption generally. For example, it is plausible that partisan (elected) government officials lose electoral support when government at large is perceived to be corrupt. Thus one of the jobs of elected politicians is to police corruption. Increased political polarization could induce stronger anti-corruption efforts given the greater ideological costs of electoral loss.

tentially including private citizens. Focusing on the smaller sample of government officials with an identifiable partisan orientation displays a negative correlation coefficient, significant at even 5%. These data therefore are somewhat supportive of the hypothesis that polarization curbs the corruption of elected politicians in particular.

In the appendix, we use the cross-sectional data to argue against possible reverse causality from corruption to polarization.

## 6 Polarization and de jure Corruption

There are tangible policies that can be implemented to reduce corruption. In particular in recent decades many states introduced ethics commissions to monitor state politicians, whose investigations often lead to Federal and state public corruption prosecutions. This subsection examines evidence that polarization led to states adopting ethics laws and increasing their stringency, thus potentially decreasing corruption.

Rosenson (2000) provides time- and state-varying data on the stringency of ethics laws enacted by state policy makers for the period 1954-1996. These data lie on an 11 point-scale and are increasing in stringency.<sup>23</sup> Regarding the period from 1960 (the start of our polarization data) to 1996, this ethics laws stringency index has a mean value of 3.12 and a standard deviation of 2.87. The between-state standard deviation is 1.74 and the within-state deviation is 2.29. Most of the states have enacted ethics laws, and over time stringency has increased. For instance, in 1960 the mean value of this index is 0.23 as only 7 states enacted at least one ethics law. By 1996, the mean value is 6.16 ranging from 10.5 for Kentucky and 0 for Vermont and Wyoming, the only two states without ethics laws. The index is extended until 2002 as there was no change in the laws from 1996 to 2002.<sup>24</sup>

Table 5 contains estimation results investigating the hypothesis that state legislature polarization leads state legislators to increase self-monitoring by enacting ethics laws. Column (1) regresses the ethics laws index on polarization along with year and state fixed effects but without controls, for the period 1960-2002, for which both polarization and ethics laws data are available. Polarization has a statistically significant and positive effect on the stringency of ethics laws. However the intro-

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<sup>23</sup>The data are based on 6 categories of legislative ethics restrictions. The categories are: 1. existence of a basic ethics code (0, 1); 2. limits on honoraria (0, 1 or 2); 3. limits on gifts from lobbyists (0, 1, 1.5 or 2); 4. post-government employment restrictions (0, 1 or 2); 5. limits on representation of clients before state agencies (0, 1 or 2); 6. mandatory personal financial disclosure (0, 1, 1.5 or 2). For categories 2-6, they receive 1 point if state enacts a law on this specific issue (e.g. state requires disclosure of gifts) and 1.5 or 2 points depending on the stringency of the law (e.g. 1.5 points if state has a numerical limit on gifts and 2 points if state has a law banning legislators from taking "anything of value" from lobbyists or legislative agents).

<sup>24</sup>The first change in ethics laws after 1996 occurs in 2003 when Illinois adopted an ethics commission (Crider and Milyo 2013).

duction of more stringent ethics laws could also be directly driven by a phenomenon of "spatial diffusion" between neighbouring states. Therefore column (2) controls for the neighboring states' ethics laws weighted by the states population, constructed as for the instrument variable employed above. The effect of polarization is robust and we find that neighboring states' ethics laws do not affect the own state's ethics laws score.<sup>25</sup>

\*\*\*Insert Table 5 \*\*\*

The effect is also robust to including the political controls used in Besley et al (2010) available for this observation period (column 3), which are found to be important determinants of ethics laws in Rosenson (2003). Certain ethics laws were adopted by initiative (Arkansas 1990, California 1974, Florida 1976, Michigan 1975 and Montana 1995) and bypassed legislators. Thus column (4) includes a dummy coded 1 for the year-state corresponding to an adoption by initiative, which we constructed based on the information in Rosenson (2000). This variable has an expectedly positive impact on ethics laws but does not affect the impact of polarization.

As an alternative illustration of how polarization may lead legislators to reinforce self-monitoring, we look at the type of commissions adopted for the legislative redistricting process. Redistricting has become synonymous with gerrymandering, which in turn is widely held to be a soft form of corruption.<sup>26</sup> Confer (2003) discusses by exemplar the 2002 cycle in Kansas. The extent of partisan gerrymandering depends on the type of commissions in charge of redistricting and is in principle alleviated by the adoption of a nonpolitical commission (Confer 2003), while in most states, the state legislature has primary control of the redistricting process. In 2010, six states (Alaska, Arizona, California, Idaho, Montana, and Washington) adopted an independent commission having primary control of the redistricting process for state legislative districts. Members of these commissions were neither legislators nor public officials (Levitt 2016).<sup>27</sup>

Table 6 presents the results of a logit regression of a dummy for the state adoption of an independent commission in 2010 on state polarization as of 2008, the last year for which Shor and McCarthy provide data for 47 states. We estimate a specification without controls (column 1), one controlling especially for the number

<sup>25</sup>We have also tried the same test by measuring neighboring ethics laws with a 5-year lag but the result is unchanged.

<sup>26</sup>For example see the New York Times Editorial on November 11 2009. <http://www.nytimes.com/2009/11/12/opinion/12thu1.html>

<sup>27</sup>Other types of commissions are: (1) Legislative alone: the state legislature has primary control of the redistricting process; (2) Advisory Commission: to help advise the legislature about where the state legislative district lines should be drawn. (3) Backup Commission: have their influence afterward; (4) Politician Commission: some other entity than the legislature draws the lines (elected officials may serve as members) (Levitt 2016).

of neighbouring states having adopted an independent commission, as in Rosen-son (2003) (column 2), and one specification controlling for the state population, state area and democratic control of the legislature, which are possible candidates to explain the type of commission adopted (column 3). All specifications support the hypothesis that more polarized states are more likely to adopt an independent commission for the redistricting process for state legislative districts.

\*\*\*Insert Table 6\*\*\*

## 7 Polarization and Federal Prosecution

Corruption convictions are the outcome of prosecutorial effort, and the literature identifies this as endogenous rather than being evenly dispersed. Federal attorneys are appointed by the President, and thus to some extent may reflect a partisan political agenda.<sup>28</sup> Alt and Lassen (2014) found that under Republican Presidents prosecutorial resources are disproportionately allocated in states with smaller numbers of self-identified conservative voters. Gordon (2009) finds partisan bias manifest in an increased tendency to pursue weaker cases against opposition politicians. In broad terms in these analyses prosecution is, to an extent, somewhat strategic. The argument of this paper, that accountability increases with polarization, and hence that actual corruption falls also has implications for prosecutorial effort. If actual corruption falls, then all else equal case referrals should fall, and hence prosecutorial effort will fall as a simple matter of efficiency. It should be understood at the outset that strategic and efficiency explanations for differences in observed prosecutorial resources are not mutually exclusive.

### 7.1 Prosecutorial Resources

As Alt and Lassen (2014) observe the initial step in any prosecution is case referral to the US Attorney’s office, usually by the Federal Bureau of Investigations (FBI). Following this the Attorney General decides whether to pursue full prosecution. The capacity of the FBI to build a case will depend on the availability of information ‘on the ground’, which in turn will depend on the considerations outlined in the theoretical arguments made above. For example opposition politicians may be more likely or able to provide useful information to the FBI when incumbents are more ideologically distant. Under conditions characterized by ‘system capacity’ (in Alt and Lassen’s terminology) such a mechanism would imply that polarization would increase the number of referrals and thus convictions. However, given ‘deterrence’

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<sup>28</sup>As pointed out by Eisenstein (1978).

arguments, then the improved on-the-ground policing environment would deter actual corruption, which in turn would reduce referrals and eventual convictions.<sup>29</sup>

If state polarization increases, holding all else (in particular corruption levels) constant, the logic of strategic prosecution might suggest that incumbent Presidents (or more accurately officials who have been appointed by incumbent Presidents) may be increasingly motivated to prosecute political opponents as they become more ideologically distinct, and conversely increasingly reluctant to prosecute co-partisans. Hence with the fairly demanding caveat of holding the decision to commit a corrupt act constant, polarization may be hypothesized to increase the detection and conviction of the opponents' corruption while decreasing the detection and conviction of allies' corruption.

On the other hand if polarization serves to increase the costs of engaging in corruption, as in the deterrence argument above, then corruption in volume terms will fall. Case referrals will also fall and the US Attorney's office will rationally allocate fewer resources to states that are politically polarized.

Column (1) of Table 7 augments Alt and Lassen's first stage estimation results with our main polarization measure, *POL*. The dependent variable is the number of Assistant US Attorneys per million population assigned per state per year, and these prosecutorial resources are negatively correlated with state polarization. This finding is consistent both with a deterrence based argument - essentially that accountability increases with polarization and the 'supply' of actual corruption falls - or strategic prosecution - if local polarization acts as a deterrent to prosecute. In the latter mechanism it seems likely, as suggested above, that polarization might act differently depending on whether the state is aligned or not with the President. Polarization might increase prosecution of the opposition, and it might deter prosecution of politicians aligned with the President. To investigate this column (2) of Table 7 includes an interaction term between polarization and a dummy for whether the State Governor represents the same party as the President. As can be seen there the difference between the coefficient estimates for polarization in the two subsamples is statistically negligible.

\*\*\*Insert Table 7\*\*\*

The results relating to prosecution effort are complementary to those discussed above for corruption levels. Prosecution effort falls with polarization - a rational response to lower corruption levels, given the enhanced deterrence effect. Indeed if prosecution effort were to fall substantially for strategic reasons, then actual corruption would (rationally) increase whilst convictions would fall. At some level strategic prosecution subverts the convictions data as a good measure of corruption. Moreover

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<sup>29</sup>Alt and Lassen (2014) provide a full discussion of these mechanisms.



the symmetric estimated relationship between states with aligned and non-aligned governors is suggestive that strategic explanations do not account for the negative relationship between prosecution effort and polarization.

## 7.2 Case Quality

We also examine how the quality of the cases prosecuted change with political polarization. Gordon (2009) hypothesizes that the prosecutor’s relative enthusiasm for pursuing cases against political opponents will lead her to prosecute weaker cases than she would against her allies. Following similar reasoning, polarization might plausibly magnify the prosecutor’s bias such that increasingly weak cases are filed against opposition defendants and perhaps symmetrically increasingly only stronger cases against co-partisans. This yields the following testable extension of Gordon (2009): In more polarized states, sentences are weaker for political opponents and stronger for co-partisans.

Again this sort of strategic prosecution argument contrasts (though not mutually exclusively) with the theoretical arguments of this paper. Above we make the case that the greater accountability engendered by polarization raises the expected costs of engaging in corruption. The consequence of this is that marginal low-level corruption is especially reduced. Hence polarization will raise case quality along with reducing corruption in volume terms. Again the distinction between the strategic prosecution argument and the accountability argument is that the latter predicts symmetric effects for opponents and co-partisans.

Gordon (2009) analyzes 222 cases under President Bush and 223 cases under President Clinton. Under Bush 84 are classified as Democrats against only 23 as Republicans. Under Clinton, 49 are identified as Democrats and 28 as Republicans. Hence the majority of case are for non-affiliated defendants. Regarding the sentences, 130 defendants were sentenced to prison under Bush and 157 under Clinton. The length of incarceration varies substantially with the maximum of 488 months reached under Clinton. To test the hypothesis that case-quality varies with polarization, and that this relationship changes with partisan alignment we investigate three different case-quality measures: (1) the probability of imprisonment, (2) the length of incarceration for those sentenced to imprisonment and (3) the length of punishment considering both incarceration and probation, under the assumption that the most severe probationary sentence (60 months) is more lenient than the least severe incarcerate sentence (one month) following Gordon (2009).

Table 8 presents regression results separately for defendants who are aligned with the President (columns 1-3), defendants who are aligned with the opposition (columns 4-6) and non-affiliated defendants (columns 7-9). Note first that case quality (as measured by the three dependent variables) is positively related with

polarization in all 9 cases, consistent with the deterrence (accountability) argument. However there are some interesting differences across the subsamples. Firstly there is a very close correspondence between the results for the aligned defendants and the non-affiliated defendants. The coefficient estimates are not statistically different from each other for all three case quality measures. This is suggestive that strategic considerations are not affecting the relationship between case quality and polarization in the instance of aligned politicians. Moreover the estimated positive effect is consistent with the auxiliary hypothesis advanced above - that greater accountability will reduce total corruption, and in doing so raise the average severity of remaining corrupt activity.

\*\*\*Insert Table 8\*\*\*

However the relationship is statistically weaker in the case of opposition defendants. One possible interpretation here is that strategic prosecution is in this instance offsetting the accountability argument. The former mechanism will lead to a negative relationship between case quality and polarization as partisan bias is amplified and increasingly weak cases get prosecuted. The latter mechanism supports a positive relationship as polarization acts to especially limit low-level corruption.

## 8 Conclusion

By several different metrics polarization has been increasing in the US, and in many other countries around the world. Undoubtedly this trend is a cause for concern for many reasons already noted in the literature and beyond. In mitigation, following Brown (2011) and Testa (2012), polarization potentially increases politicians' accountability, thereby lowering corruption. Consistent with this hypothesis this paper finds a robust negative correlation between observed corruption levels and polarization using panel data from the US.

To establish causality, polarization is instrumented with lagged political position taking in geographically neighboring states. The widespread idea that policy, and ideas themselves, diffuse geographically is found to strongly apply in the instance of Democratic party position-taking. Under the identifying assumption that lagged neighbor democratic ideology only affects state corruption via its effect on state ideological polarization, an exogenous one standard deviation increase in polarization is estimated to cause nearly a half standard deviation reduction in corruption.

We also find that polarization is associated with tangible anti-corruption legislation, or 'corruption de jure'. The existence and strength of state ethics laws and also the existence of independent commissions for political redistricting are all positively related with polarization.

The paper also finds that prosecutorial resources fall, and case quality increases with polarization. Both findings are compatible with the accountability thesis. If political polarization raises the costs of committing a corrupt act, then less corruption will be committed, and in particular low-level corruption might be especially deterred. In response federal authorities invest less resources and case quality increases. In the case of prosecutorial resources we also find symmetry across subsamples where the State Governor is aligned and opposed to the President, which speaks in favor of the accountability thesis as opposed to a strategic response to polarization. Similarly for case-quality we find symmetry across subsamples of aligned and non-politically affiliated defendants. This evidence again is suggestive that it is not (just) strategic prosecution that explains the relationship between case-quality and polarization.

## Appendix: Reverse Causality

The main estimation results use lagged explanatory variables, and also an instrumental variable approach to identifying exogenous variation in state-polarization. This appendix explores the specific possibility of reverse causality between corruption and polarization. For example, corruption may cause voters to disengage from politics and thereby become less ideological and thus less polarized, which could account for the negative association between polarization and corruption.<sup>30</sup> We investigate the potential causal effect of corruption convictions on polarization using cross-sectional data, employing OLS and also using the degree of isolation of the state capital city as an instrument for state corruption as in Campante and Do (2014). The idea is that when the capital is more isolated, state officials are less accountable and thus more corrupt.

The results are reported in Table A.6. Using the politician polarization measures respectively based on the data of Berry et al (column 1) Shor and McCarty (column 3) in 2000 as the dependent variable, OLS estimation demonstrates the significant and negative association between convictions and polarization already established. However when state corruption is instrumented with the Campante and Do (2014) capital isolation measure, 2SLS regressions show that the association is not statistically significant anymore (columns 2 and 4).<sup>31</sup> This undermines the hypothesis of causality running from corruption to polarization and thus suggests that the significant relationship found in the OLS results is primarily driven by the causal effect of polarization on corruption. In the instance of Garand's measure of mass polarization (columns 5-6), neither the OLS nor the 2SLS estimation results find that it is statistically related to corruption, again suggesting that polarization in voter ideologies is not materially affected by corruption.

A possible concern with the IV strategy employed in Table A.6 could be violation of the exclusion restriction if capital isolation affects state polarization through channels other than corruption. To investigate this, columns (1), (3) and (5) in Tables A.7 and A.8 in Appendix report the reduced-form estimated effect of capital isolation on the three different polarization measures used in Table A.4. In the corresponding columns (2), (4) and (6), we check if the reduced-form effect survives the inclusion of the mediator variable, i.e. corruption convictions. Table A.7 includes the set of controls while Table A.8 does not. In all instances capital isolation never reaches statistical significance once corruption enters the regression. This is consistent with the exclusion restriction.

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<sup>30</sup>Richardson (2012) provides evidence that higher U.S. state rates of Federal corruption convictions were associated with significantly less citizen participation in activities associated with the campaigns and elections of 2008 and 2010. Olsson (2014) also documents a negative effect of perception of corruption on political participation using international data.

<sup>31</sup>As Campante and Do (2014) establish, this instrument is strong with an F-stat of around 15.

Tables A.9 and A.10 in the Appendix contain results directly investigating the hypothesis that government corruption affects various measures of citizen engagement and partisan affiliation, respectively using OLS and 2SLS where corruption is instrumented with the isolation of the capital. The dependent variables are: the 1991-2000 state-average of voter turnout for the highest office in the race from Besley and Case (2003) (column 1), the proportion of self-declared moderate voters (column 2), the proportion of voters declaring not to be strong liberal or conservative (column 3), absence of political information (column 4), distrust in government (column 5), disinterest in campaigns (column 6), and inattention to government and public affairs (column 7), with data from the 2000 American National Elections Studies (ANES) for columns (2)-(7). The analysis does not provide any evidence that government corruption leads voters to disengage from politics or changes their partisan affiliation.

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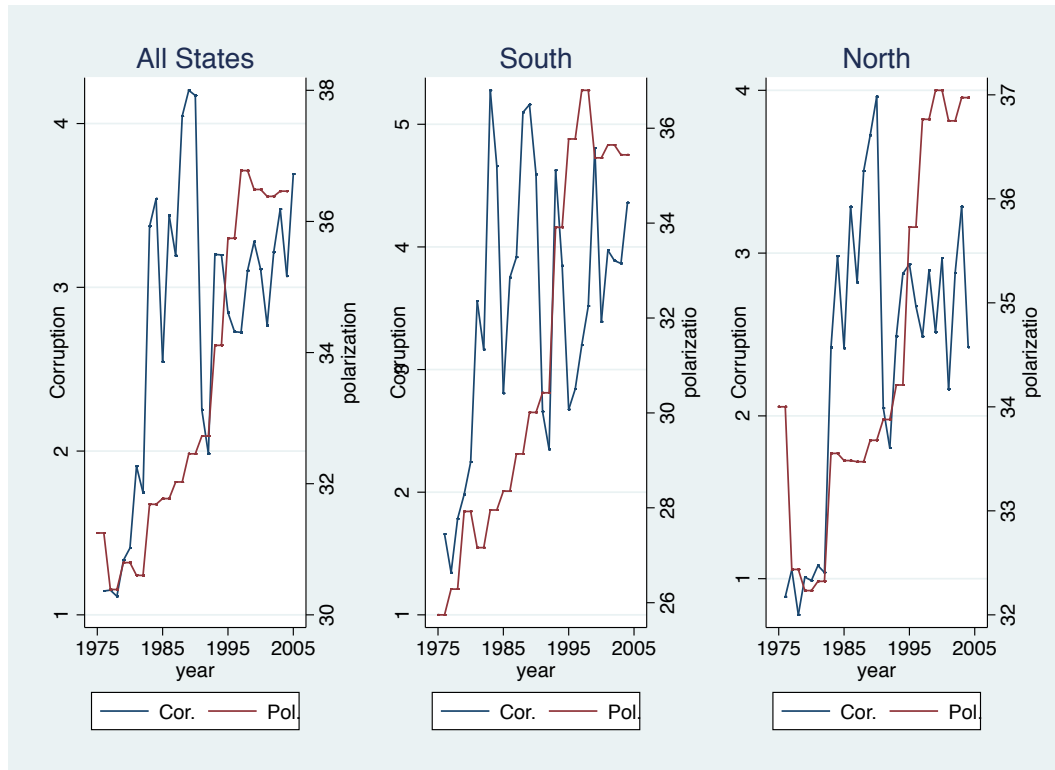
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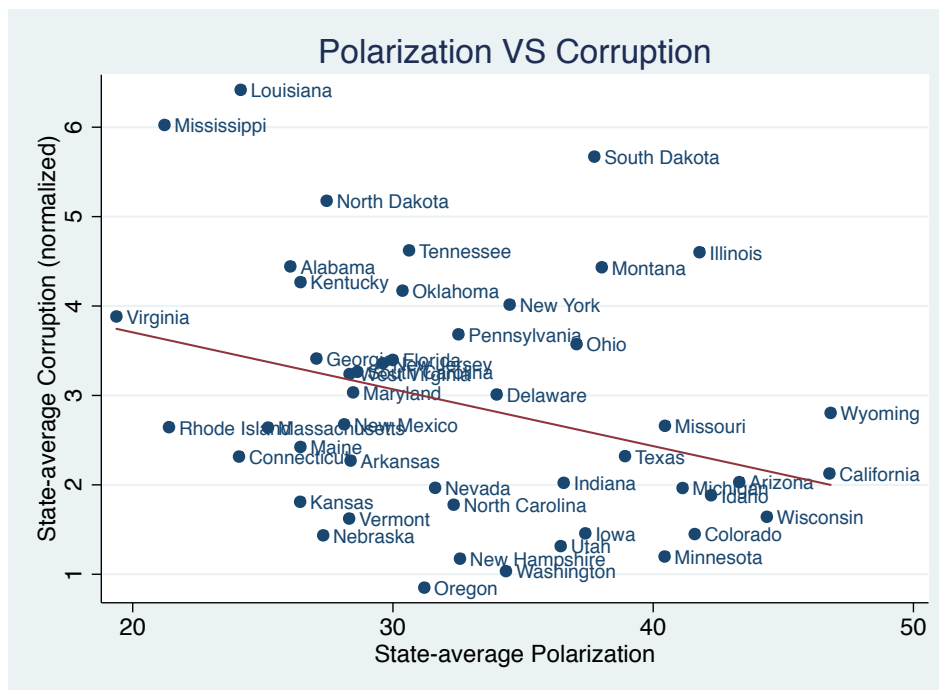
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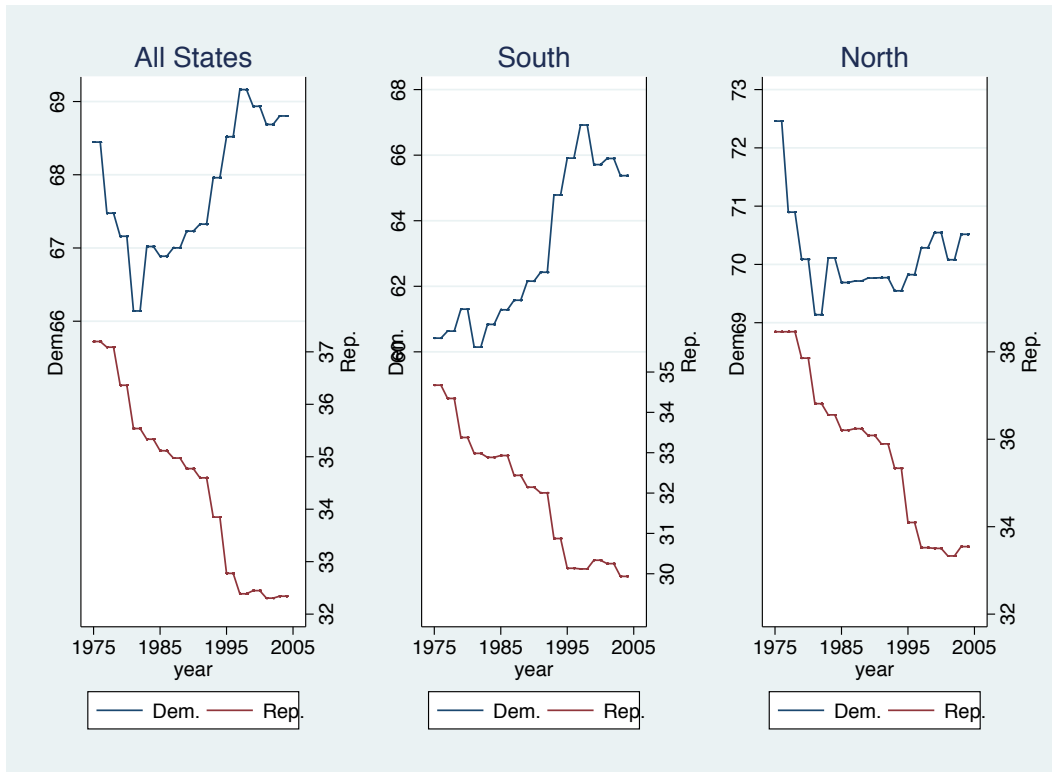
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**Figure 1.** Evolution of average federal corruption convictions per million population and average ideological Polarization, 1976-2004



**Figure 2.** Scatter plot of state-averages of federal corruption convictions per million population and state-averages of ideological Polarization, 1976-2004



**Figure 3.** Evolution of average ideology of the Democrats and the Republicans by year, 1976-2004

	Corruption							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Polarization	-0.0203 (0.0220)	-0.0472** (0.0201)	-0.0531** (0.0251)	-0.0578** (0.0268)	-0.0521** (0.0251)	-0.0524** (0.0236)	-0.152** (0.0697)	-0.0539** (0.0261)
% Democrats Low. House								-1.125 (1.848)
% Democrats Up. House								4.118 (6.132)
Political competition								-0.0204* (0.0120)
Lameduck Governor								-0.469 (0.592)
Prosecutorial Resources								6.95e-06 (0.0577)
Controls			X	X	X	X	X	X
State FE	X	X	X	X	X	X	X	X
Year FE		X		X	X	X	X	X
State trend					X			
South x year FE						X		
Obs.	1,384	1,384	1,126	1,126	1,126	1,126	1,124	1,093
R <sup>2</sup>	0.211	0.318	0.267	0.317	0.267	0.334	0.489	0.315

**Table 1.** Corruption and Polarization. Panel 1976-2004

*Notes:* Dependent variable: Federal corruption convictions per million population in  $t$  in every column except in column (7) and 3-year moving average in column (7). Independent variables measured in  $t-5$ . Regressions include state and year fixed effects and a set of unreported controls used in Alt and Lassen (2014), including relative government wages, wages inequality, divided government, real per capita income, real per capita government revenues, percent of high school graduates, log of population, binding one-term limit, binding two-term limit, unemployment, citizen ideology, percent living in urban areas, an interaction term between urbanization and share of democrats in state senate. Column (6) also includes interactions between the year dummies and an indicator variable for the 16 southern states as defined by the US Census. Column (8) includes additional controls: the share

of Democrats in the lower house, the share of Democrats in the upper house, political competition based on data from Besley and Case (2003), dummy for a governor not re-eligible (lameduck) and prosecutorial resources from Alt and Lassen (2014). Robust standard errors clustered at the state level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

	Polarization			
	(1)	(2)	(3)	(4)
Neighb. Democrat t-5	0.494*** (0.109)	0.347*** (0.116)	0.490*** (0.121)	0.494*** (0.129)
Neighb. Repub. t-5	0.0503 (0.141)			
Controls		X	X	X
State FE	X	X	X	X
Year FE	X	X	X	X
Sample	full	full	small	small 2
Obs	1,384	1,355	1,125	1,125
$R^2$	0.758	0.780	0.755	0.754

**Table 2.** Polarization and Neighboring States' Ideology. Panel 1976-2004

*Notes:* Dependent variable: State polarization in  $t$ . Independent variables measured in  $t$ . Independent variables: *Neighb. Democrat t-5* = 5-year lag of the neighboring states' Democrat Liberal score (weighted by the states population). *Neighb. Repub. t-5* = 5-year lag of the neighboring states' Republican Liberal score (weighted by the states population). *Democrat Ideology t-5* = 5-year lag of the State Democrat Liberal score. *Non neighb. t-5* = 5-year lag of the non-neighboring states' Democrat Liberal score (weighted by the states population). Regressions include state and year fixed effects and a set of unreported controls used in Alt and Lassen (2014). Column 3 and 5 exclude the 8 states above 10 million inhabitants in 2004: California, Texas, New York, Florida, Illinois, Pennsylvania, Ohio and Michigan. Column 4 excludes the 8 biggest states in terms of the ratio of own-population divided by neighboring population (sum of all neighboring states) in 2004: California, Texas, Florida, Washington, Michigan, Maine, New York, Illinois. Robust standard errors clustered at the state level in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10

	Convictions			
	(1)	(2)	(3)	(4)
Polarization	-0.299*	-0.344**	-0.386***	-0.461***
	(0.166)	(0.139)	(0.144)	(0.179)
Neighboring Corruption lag.				0.109 (0.0974)
Estimation method	2SLS	2SLS	2SLS	2SLS
Sample	full	small	small 2	small
Obs	1,355	1,125	1,125	908
$R^2$	0.195	0.128	0.081	0.051
<b>First stage</b>				
Neighboring Democrat lag.	0.346*** (0.121)	0.489*** (0.127)	0.493*** (0.136)	0.476*** (0.134)
$F$	8.950	16.487	14.700	14.916

**Table 3.** Corruption and Polarization. Panel 1976-2004. 2SLS

*Notes:* Dependent variable: Federal corruption convictions per million population in  $t$ . Independent variables measured in  $t$ . Regressions include state and year fixed effects and a set of unreported controls used in Alt and Lassen (2014). Column (4) also includes the neighboring states' corruption conviction ratio (weighted by the states population). IV is estimated by two-stage-least squares. The upper part of the Table provides the second stage of the 2SLS estimation and the lower part provides the first stage. The instrument is the 5-year lag of the neighboring states' Democrat ideology (weighted by the states population).  $F$  is an F-statistic for the statistical significance of the instrument in the first stage regression. Columns 2 and 4 exclude the 8 states above 10 million inhabitants in 2004: California, Texas, New York, Florida, Illinois, Pennsylvania, Ohio and Michigan. Column 3 excludes the 8 biggest states in terms of the ratio of own-population divided by neighboring population (sum of all neighboring states) in 2004: California, Texas, Florida, Washington, Michigan, Maine, New York, Illinois. Robust standard errors clustered at the state level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$

Polarization data	Convictions				Officials	Perception
	Berry	Shor-McCarty	Bonica	Garand	Berry	Shor-McCarty
	(1)	(2)	(3)	(4)	(5)	(6)
Polarization	-0.0874** (0.0430)	-0.0992*** (0.0361)	-0.4146** (0.1940)	-0.0112 (0.0439)	-0.0135** (0.00549)	-1.377** (0.634)
Controls	X	X	X	X	X	X
Obs.	48	47	48	48	48	47
R <sup>2</sup>	0.329	0.385	0.396	0.278	0.328	0.381

**Table 4.** Robustness. Cross-section

*Notes:* Dependent variables: *Convictions* = Average federal corruption convictions per million population 2001-2010. *Officials* = Convictions relative to the number of state and local popularly elected officials in millions as 2000, from Maxwell and Winters (2004). *Perception* = Number of search hits for “corruption” close to state name divided by number of search hits for state name, using Exalead search tool (in 2009); Polarization data: *Berry* = party polarization using Berry et al (2010), as of 2000 in column 1 and 1995 in column 5. *Shor-McCarty* = party polarization using Shor and McCarty (2011) (not available for Nebraska), as of 2000 in column 2 and 2008 in columns 6. *Bonica* = party polarization using Bonica (2013) as of 2000. *Garand* = mass polarization using Garand (2010) as of 2000. Control variables: log income, log population, percent college, share of government employment, percent urban, census region dummies, as of 2000. Robust standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10.



	Ethics Laws			
	(1)	(2)	(3)	(4)
Polarization	0.0399* (0.0201)	0.0388** (0.0188)	0.0404** (0.0198)	0.0387* (0.0193)
Neighboring ethics laws		-0.0328 (0.141)		
Initiative				0.936* (0.535)
Controls			X	X
state FE	X	X	X	X
year FE	X	X	X	X
Obs.	2,064	1,806	1,998	1,998
R <sup>2</sup>	0.809	0.811	0.809	0.811

**Table 5.** Ethics Laws and Polarization - Panel 1960-2002

*Notes:* Dependent variable: yearly score of stringency of state's ethics laws based on 6 categories of legislative ethics restrictions (Basic ethics code, honoraria limit, Gift limit, post-government employment limit, Representation limit, Personal financial disclosure) from Rosenson (2000). Regressions include state and year fixed effects. Independent variables: *Neighboring ethics laws* = neighboring states' score of of ethics laws (weighted by the states population). Column (3) and (4) also include a set of unreported political controls used in Besley, Persson and Sturm (2010), including a measure of political competition, a dummy for whether the Governor is democrat, the average Democratic vote share, a dummy for whether the Democrats control state house and senate, a dummy for whether Republicans control state house and senate, as well as a dummy if the legislative ethics commission is adopted by initiative in column (4). Robust standard errors clustered at the state level in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10.

	Independent Commission		
	(1)	(2)	(3)
Polarization	6.280** (2.759)	6.863** (3.287)	7.718** (3.522)
Neighboring independent commission		0.957 (0.713)	
Controls			X
Obs.	47	47	47

**Table 6.** Independent Commission for Redistricting and Polarization - 2010

*Notes:* Logit estimations. Dependent variable: Dummy for states - Alaska, Arizona, California, Idaho, Montana, and Washington - where an independent commission has primary control of the redistricting process for state legislative districts in 2010, with regulations limiting direct participation by elected officials. Members of these commissions are neither legislators nor public officials. Independent variable: *Polarization* = Polarization as of 2008 using Shor-McCarty. *Neighboring independent commission* = Number of contiguous states adopting an independent commission. Column (2) includes a set of unreported controls: state population, state area and democratic control of the legislature. Standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10.

	Prosecutorial resources	
	(1)	(2)
Polarization	-0.0447*	-0.0404
	(0.0262)	(0.0248)
Aligned States		-0.00201
		(0.896)
Interaction		-0.00839
		(0.0256)
Governor party	-0.125	-0.127
	(0.241)	(0.252)
Controls	X	X
state FE	X	X
year FE	X	X
Obs.	1,175	1,175
R <sup>2</sup>	0.903	0.904

**Table 7.** Prosecution and Polarization. Panel 1976-2004

*Notes:* Column 1 augments Alt and Lassen (2014)'s first stage estimation (Table 1, column 1) with our main polarization measure, *POL*, and *Governor Party*. Dependent variable: *Prosecution resources* = number of US Attorneys (per million population) prosecuting state corruption is regressed on the number of criminal investigators (per million population) from the US INS (Immigration and Naturalization Service). Regressions include state and year fixed effects and a set of unreported controls used in Alt and Lassen (2014). *Aligned States* = States in which the State Governor represents the same party as the President; *Governor party* = Dummy for Republican governor; Robust standard errors clustered at the state level in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10

	Aligned defendants			Non-aligned defendants			Non-affiliated defendants		
	Prison	Length	Punish.	Prison	Length	Punish.	Prison	Length	Punish.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Polarization	0.0157*	1.126**	1.286**	0.00162	0.250	0.223	5.22e-05	1.546**	1.526**
	(0.00921)	(0.542)	(0.580)	(0.00895)	(0.532)	(0.561)	(0.00569)	(0.712)	(0.726)
Controls	X	X	X	X	X	X	X	X	X
Obs.	70	70	70	112	112	112	251	251	251
R <sup>2</sup>	0.164	0.254	0.256	0.168	0.128	0.140	0.078	0.134	0.133

**Table 8.** Cases quality and Polarization - 1998-2004

*Notes:* The sample extracted from Gordon (2009) is composed of the defendants under Clinton administration (1998-2000) and Bush administration (2004-2006), who are politically aligned with the President in columns 1 to 3, aligned with the opposition in columns 4 to 6 and with no identifiable political affiliation in columns 7 to 9. Dependent variables: *Prison* = Incarceration; *Length* = Months of incarceration placing zero value on probation; *Punish.* = Sentences are calculated as 0.2 times the number of months of probation in the sentence if the sentence is solely probationary, and 12 plus the number of months of incarceration plus 0.2 times the number of months of probation if the sentence includes imprisonment (see Gordon (2009) for additional explanation). Independent variable: state polarization (POL) using Berry et al (2010). Regressions include a set of unreported controls: cases judged under Clinton administration, Democratic judge, defendant private citizen, and multiple defendants, accusation of violation of the Hobbs Act (robbery of extortion affecting interstate commerce), accusation of theft and bribery in entities receiving more then \$10,000 in federal funds, and state urbanization. Standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10.

## Appendix

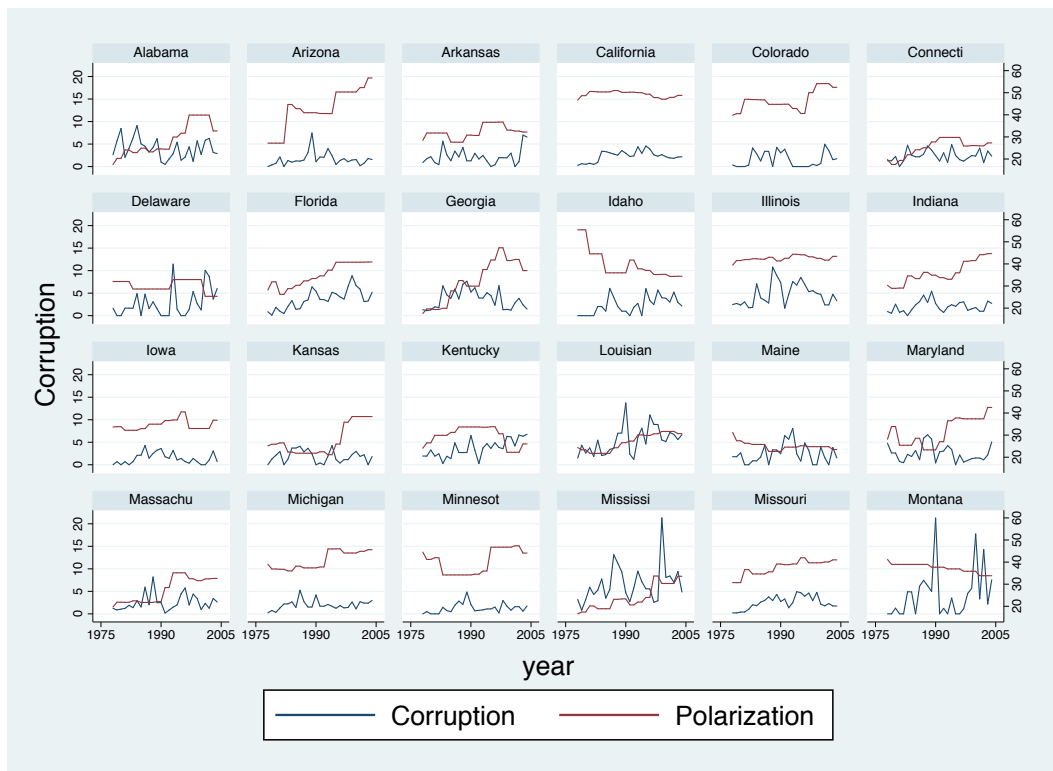


Figure A.1. Evolution of corruption convictions and Polarization, 1976-2004

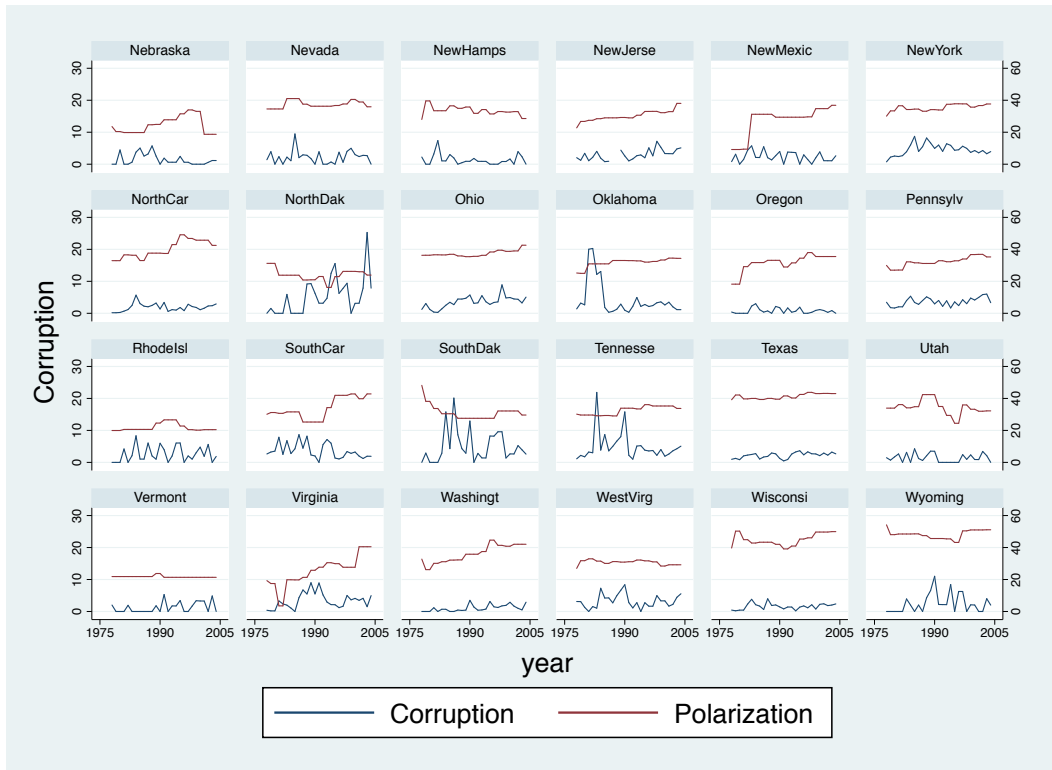


Figure A.1. (cont'd) Evolution of corruption convictions and Polarization, 1976-2004

	Polarization				
	(1)	(2)	(3)	(4)	(5)
Neighb. Democrat t-5	0.313** (0.119)		0.183* (0.0931)	0.252** (0.121)	
Democrat Ideology t-5	0.331*** (0.111)				
Neighb. Democrat t+5		0.0801 (0.191)			
Neighb. Democrat t			0.280* (0.143)	0.394* (0.195)	
Non neighb. t-5					-2.225 (1.592)
Controls	X	X	X	X	X
State FE	X	X	X	X	X
Year FE	X	X	X	X	X
Sample	small	full	full	small	full
Obs	1,125	1,120	1,355	1,125	1,355
$R^2$	0.773	0.798	0.782	0.759	0.774

**Table A.1.** Robustness. First-stage Estimations. Panel 1976-2004

*Notes:* Dependent variable: State polarization in  $t$ . Independent variables: *Neighb. Democrat t-5* = 5-year lag of the neighboring states' Democrat Liberal score (weighted by the states population). *Democrat Ideology t-5* = 5-year lag of the State Democrat Liberal score. *Non neighb. t-5* = 5-year lag of the non-neighboring states' Democrat Liberal score (weighted by the states population). Regressions include state and year fixed effects and a set of unreported controls used in Alt and Lassen (2014). Columns 1 and 4 exclude the 8 states above 10 million inhabitants in 2004. Robust standard errors clustered at the state level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$

	Democrat Ideology		Republican Ideology	
	(1)	(2)	(3)	(4)
Neighboring Democrats t-5	0.329*** (0.108)	0.448*** (0.125)	-0.0122 (0.0627)	-0.0299 (0.0775)
Neighboring Republicans t-5	0.0485 (0.0902)	0.0233 (0.101)	-0.0655 (0.0907)	-0.111 (0.0978)
Controls	X	X	X	X
State FE	X	X	X	X
Year FE	X	X	X	X
Sample	full	small	full	small
Obs.	1,355	1,125	1,355	1,125
R <sup>2</sup>	0.800	0.772	0.919	0.921

**Table A.2.** Democrat and Republican Ideology and Neighboring States' ideology. Panel 1976-2004

*Notes:* Dependent variable: Own-state Democrat liberal score in  $t$  in columns 1 and 2. Own-state Republican liberal score in  $t$  in columns 3 and 4. Independent variables: *Neighb. Democrat t-5* = 5-year lag of the neighboring states' Democrat Liberal score (weighted by the states population). *Neighb. Repub. t-5* = 5-year lag of the neighboring states' Republican Liberal score (weighted by the states population). Regressions include state and year fixed effects and a set of unreported controls used in Alt and Lassen (2014). Column 2 and 4 exclude the 8 states above 10 million inhabitants in 2004. Robust standard errors clustered at the state level in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$



Polarization data	Drug cases		
	Shor-McCarty	Berry	Bonica
	(1)	(2)	(3)
Polarization	0.132 (0.106)	0.142 (0.203)	-1.513 (2.513)
Controls	X	X	X
Obs.	47	48	48
R <sup>2</sup>	0.357	0.335	0.326

**Table A.3.** Placebo - Drug Cases and Polarization

*Notes:* Dependent variable: *Drug cases* = Criminal defendants commenced in federal courts, 2011. Polarization data: *Berry* = party polarization using Berry et al (2010), as of 2000. *Shor-McCarty* = party polarization using Shor and McCarty (2011) (not available for Nebraska), as of 2008. *Bonica* = party polarization using Bonica (2013) as of 2000. Control variables: log income, log population, percent college, share of government employment, percent urban, census region dummies, as of 2000. Robust standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10.

Polarization data	Drug cases			
	Shor-McCarty		Berry	
	1997-2011		1997-2004	
	(1)	(2)	(3)	(4)
Polarization	0.0277 (0.0255)	-0.0388 (0.0521)	-0.000114 (0.00143)	-0.000578 (0.00168)
state FE		X		X
year FE		X		X
Obs.	566	566	336	336
R <sup>2</sup>	0.027	0.840	0.000	0.902

**Table A.4.** Drug Cases and Polarization. Panel 1997-2011

*Notes:* Dependent variable: *Drug cases* = Criminal defendants commenced in federal courts in *t*. Polarization data: *Shor-McCarty* = party polarization using Shor and McCarty (2011); *Berry* = party polarization using Berry et al (2010). Columns 2 and 4 include year and state fixed effects. Robust standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10.

Defendants:	All	Officials	Partisan	Officials-Partisan
	(1)	(2)	(3)	(4)
Polarization	-0.00204** (0.00097)	-0.00129* (0.00076)	-0.00079* (0.00041)	-0.00058** (0.00027)
Controls	X	X	X	X
Obs.	47	47	47	47
R <sup>2</sup>	0.1878	0.1905	0.2428	0.2494

**Table A.5.** Conviction by types of defendants and Polarization. Cross-section

*Notes:* Dependent variables: State-average number of corruption convictions per million population for the periods 1998-2000 and 2004-2006 for 47 States. Convictions measures are based on Gordon (2009) data on a sample of national priority public corruption prosecutions by U.S. Attorneys in the Bush Justice Department (2004-2006) and the Clinton Justice Department (1998-2000). *All* = considering all guilty defendants; *Officials* = Only guilty Government Officials; *Partisan* = Only guilty partisan figures; *Officials-Partisan* = Only guilty Government Officials with a partisan affiliation. Dependent variables: Polarization = state-average polarization using Shor and McCarty (2011) from 1999 to 2004. Control variables: log income, log population, percent college, share of government employment, percent urban, census region dummies, as of 2000. Robust standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10

	Polarization					
	Berry		Shor-McCarty		Garand	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Convictions	-0.869** (0.403)	-0.346 (0.915)	-0.0751*** (0.0262)	-0.0284 (0.0605)	0.00636 (0.00713)	0.00338 (0.0114)
Controls	X	X	X	X	X	X
Obs.	48	48	47	47	48	48
R <sup>2</sup>	0.586	0.578	0.717	0.697	0.586	0.584
F-statistic		15.68		14.44		15.68

**Table A.6.** Polarization Regressions. Cross-section

*Notes:* Dependent variables: *Berry* = party polarization using Berry et al (2010), as of 2000; *Shor-McCarty* = party polarization using Shor and McCarty (2011), as of 2000; *Garand* = mass polarization using Garand (2010) as of 2000. Independent variables: *Convictions* = Average federal corruption convictions per million population between 1991 and 2000. Control variables: log income, log population, percent college, share of government employment, percent urban, census region dummies, as of 2000. IV: Isolation of the state capital city from Campante and Do (2014). F-statistic: the statistical significance of the instrument in the first stage regression. Robust standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10.

	Polarization					
	Berry		Shor-McCarty		Garand	
	(1)	(2)	(3)	(4)	(5)	(6)
Convictions		-1.047*		-0.0892***		0.00738
		(0.586)		(0.0296)		(0.00811)
Isolation	-3.848	7.793	-0.302	0.648	0.0376	-0.0445
	(11.31)	(15.57)	(0.754)	(0.746)	(0.141)	(0.160)
Controls	X	X	X	X	X	X
Obs.	48	48	47	47	48	48
R <sup>2</sup>	0.565	0.588	0.666	0.723	0.579	0.587

**Table A.7.** Polarization Regressions. Reduced form with controls - Cross-section

*Notes:* OLS estimations. Dependent variables: *Berry* = party polarization using Berry et al (2010), as of 2000; *Shor-McCarty* = party polarization using Shor and McCarty (2011), as of 2000; *Garand* = mass polarization using Garand (2010) as of 2000. Independent variables: *Convictions* = Average federal corruption convictions per million population between 1991 and 2000. *Isolation* = Isolation of the state capital city from Campante and Do (2014). Control variables: log income, log population, percent college, share of government employment, percent urban, census region dummies, as of 2000. Robust standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10.

	Polarization					
	Berry		Shor-McCarty		Garand	
	(1)	(2)	(3)	(4)	(5)	(6)
Convictions		-1.675***		-0.108***		-0.00738
		(0.622)		(0.0363)		(0.00927)
Isolation	-5.741	15.18	-1.448*	-0.143	-0.355**	-0.263
	(17.54)	(20.36)	(0.793)	(1.057)	(0.145)	(0.161)
Controls	NO	NO	NO	NO	NO	NO
Obs.	48	48	47	47	48	48
R <sup>2</sup>	0.003	0.083	0.059	0.171	0.071	0.081

**Table A.8.** Polarization Regressions. Reduced form without controls - Cross-section

*Notes:* See notes of Table A.4.

	Turnout	Moderate	No Strong	Uninformed	Distrust	No Interest	No Attention
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Convictions	0.000829 (0.00468)	0.0173 (0.0195)	0.0125 (0.0134)	-0.0868** (0.0358)	0.0209 (0.0163)	-0.0577 (0.0613)	-0.0617** (0.0301)
Controls	X	X	X	X	X	X	X
Obs.	48	48	47	48	48	48	48
R <sup>2</sup>	0.642	0.353	0.134	0.406	0.277	0.149	0.152

**Table A.9.** Corruption and Citizen Engagement. OLS - Cross-section

*Notes:* OLS estimations. Dependent variables: *Turnout* = 1991-2000 average of the voter turnout for the highest office in the race in the last even-year election, divided by the state's age-eligible voting population, using Besley and Case (2003). Other dependent variables are state measure of citizens' political engagement based on different variables of the 2000 ANES: *Moderate* = Proportion of moderate voters, based on V001370 "3-category lib-con summary". *No Strong* = Proportion of voters declaring not to be strong liberal or conservative, based on V000446 "Summary self plcmnt 7 points lib-con scale". *Uninformed* = Information level about politics, higher values meaning lower information, based on V001033 "R informed about politics ". *Distrust* = Distrust in government, higher values meaning lower trust, based on V001534: "How much can govt be trusted ?". *No Interest* = No interest in campaigns, higher values meaning lower interest, based on V001201 "R interest in campaigns?". *No Attention* = No attention to government and public affairs, higher values mean less attention, based on V001367 "R follows govt and public affairs?". Independent variables: *Convictions* = Average federal corruption convictions per million population between 1991 and 2000. Control variables: log income, log population, percent college, share of government employment, percent urban, census region dummies, as of 2000. Robust standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.10.

	Turnout	Moderate	No Strong	Uninformed	Distrust	No Interest	No Attention
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Convictions	-0.00387 (0.00867)	0.0216 (0.0382)	0.0290 (0.0234)	-0.0710 (0.0711)	0.0205 (0.0407)	-0.0389 (0.0680)	-0.0576 (0.0425)
Controls	X	X	X	X	X	X	X
Obs.	48	48	47	48	48	48	48
R <sup>2</sup>	0.634	0.352	0.077	0.404	0.277	0.145	0.152
F-statistic	15.68	15.68	15.68	15.68	15.68	15.68	15.68

**Table A.10.** Corruption and Citizen Engagement. 2SLS - Cross-section

*Notes:* 2SLS estimations. IV: Isolation of the state capital city from Campante and Do (2014). F-statistic: the statistical significance of the instrument in the first stage regression. See notes of Table A.6 for further information.