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ABSTRACT

Essays on Empirical Political Economy

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This dissertation contains three essays. In the first essay, *The Role of Connections in Congressional Lawmaking*, I explore the role connections play in shaping the effectiveness of lawmakers. I do so by studying how legislators' deaths impact their peers' capacity to sponsor and advance bills in the U.S. House of Representatives. I focus on legislators who represent the same states as deceased legislators: these lawmakers collaborated with the deceased more closely, but are otherwise comparable to all other lawmakers. Following the death of a legislator from the same state, lawmakers suffer a 22% decrease in their effectiveness. Bills sponsored by these lawmakers are less likely to receive action in committee, and fewer of them become law as a result. The effects are especially large after the deaths of committee chairs.

In the second essay, *Political Consequences of Economic Hardship: State Economic Activity and Polarization in American Legislatures*, I analyze how the economy affects legislative polarization. Using recently available state legislator ideal point estimates, I find a strong negative relationship between state economic activity and political polarization. States that

fared worse economically have experienced greater increases in political polarization. I establish this correlation as causal by employing an instrumental variables strategy. The instrument isolates exogenous variation in state economic activity by exploiting time series variation in oil prices, which differentially affects individual states according to their economic dependence on oil production. The estimated polarization effects are stronger for Republicans. The findings have implications for understanding the interaction between the economy and political outcomes.

In the third essay, *Quid Pro Quo? Corporate Returns to Campaign Contributions* (joint work with Anthony Fowler and Jörg Spenkuch), we study whether corporations distort public policy and subvert the will of the electorate by donating to politicians. Well-publicized anecdotes notwithstanding, whether and how much corporations actually benefit from supporting political candidates remains unknown. To systematically address this question, we utilize two complementary empirical approaches that isolate the monetary benefits a company derives from a favored candidate winning office. First, we use a regression discontinuity design exploiting close congressional, gubernatorial, and state legislative elections. Second, we leverage within-campaign changes in market beliefs about the outcomes of U.S. Senate races. We find no evidence that corporations benefit from electing candidates supported by their PACs, and we can statistically reject effect sizes greater than 0.3 percent of firm value. Our results suggest that corporate campaign contributions do not buy significant political favor—at least not on average.

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CHAPTER 1

The Role of Connections in Congressional Lawmaking

1.1. Introduction

Lawmakers in Congress do not merely cast their votes on bills that reach the floor. They are also in charge of drafting the initial bills and deciding which bills should receive action in committees, advance to the floor, and ultimately become law. In the same way that legislators differ in their floor voting behavior, they also exhibit sizable differences in their legislative effectiveness—that is, their capacity to sponsor bills and ensure those bills advance through the lawmaking process.

Tom Lantos was a Hungarian Holocaust survivor who represented California in the U.S. House of Representatives from 1981 until his death in 2008. During his long tenure, this effective legislator sponsored numerous human rights bills on issues such as genocide prevention and sanctions for Serbia and Burma. Senator Amy Klobuchar (D-MN) is a persistent lawmaker who has sponsored numerous bills that became law, on various issues—such as human trafficking, drug disposal safety, adoption, and telecommunications quality in rural areas. She is currently one of the most effective senators, despite being in the minority party.¹ Both Lantos and Klobuchar formed vast networks of legislative connections during their tenure in Congress. Other legislators, by contrast, sponsor bills that hardly ever advance to law, or do not sponsor many bills in the first place.

Some lawmakers more effective because they have greater abilities, certain individual characteristics, and institutional positions to exercise their influence. But what role do their legislative connections play in enhancing their capacity to sponsor and advance substantive bills? How does the death in office of a prominent legislator, such as Lantos, impact the effectiveness of lawmakers connected to him?

¹<https://thelawmakers.org/>.

Understanding the factors that contribute to legislative effectiveness is a central aim in the congressional literature (Matthews, 1960; Frantzich, 1979; Hibbing, 1991; Anderson, Box-Steffensmeier and Sinclair-Chapman, 2003). Volden and Wiseman (2014) systematically measure lawmakers' effectiveness in the U.S. House of Representatives and study individual characteristics and institutional positions that might explain effectiveness. Such factors include seniority, membership in the majority, and committee chair positions. In addition, the literature on networks has documented a strong correlation between lawmakers' network size and legislative influence (Fowler, 2006a). Nevertheless, legislators endogenously form their networks, which poses a challenge to the identification of the causal effects of networks (Rogowski and Sinclair, 2012). To estimate how legislators' connections impact their effectiveness, researchers must contend with the fact that unobserved traits that affect the size and prominence of legislators' networks, such as innate ability or social skills, are also likely to affect their success in sponsoring substantive bills and advancing them through the lawmaking process. Failure to account for network endogeneity could lead to incorrect inferences about the causal effects of networks.

In this paper, I show that connections substantially affect lawmakers' effectiveness. I propose an empirical strategy that addresses the endogeneity of networks, estimates the value of connections for legislative effectiveness, and uncovers the mechanisms through which connections impact effectiveness. I identify the disruptions to legislative networks in the U.S. House of Representatives caused by deaths of legislators while in office, and study how these deaths impact the effectiveness of legislators who represent the same states as the deceased legislators. The rationale for studying legislators from the same states as deceased legislators, rather than legislators directly connected to deceased legislators based on bill

cosponsorship measures, is twofold. First, legislators who represent the same state are more likely to collaborate with each other than legislators who represent different states. Same-state legislators often belong to state organizations, are likely to know each other before they arrive in Congress, and tend to represent the interests of districts that have similar needs and characteristics. Second, legislators who represent the same state as a deceased legislator in a particular congress constitute a sample that is comparable to all other legislators in the House in that congress. By contrast, legislators who are connected to a deceased legislator are not comparable to non-connected legislators. Well-connected and powerful legislators, by virtue of being connected to many other legislators, are likely to have been connected to the deceased legislator. Crucially, well-connected lawmakers tend to be systematically more senior and more powerful than less connected lawmakers. If the research design does not properly account for this sample selection problem, it produces substantially incorrect estimates.

Legislators who represent the same state as a deceased legislator experience a sizable (22%) drop in their effectiveness the congress after the legislator passes away. They sponsor fewer bills (14% less) and fewer of their sponsored bills become law (22% less). This drop in the number of bills that become law is especially large (a 32% drop) for substantive and significant bills, which are landmark bills that received an end-of-the-year write-up in the Congressional Quarterly Almanac. The decrease in the number of bills that become law occurs in large part because bills that are sponsored by same-state legislators become much less likely to receive action in committee (24% less). These results highlight the team-based nature of lawmaking. Legislative connections play a key role during the initial stages since a bill is drafted until it is considered at the committee stage.

Placebo tests indicate that the estimated effects are not attributable to unobserved differences between legislators who were affected and unaffected by deaths of legislators from the same state. Consistent with the hypothesis that connections to deceased legislators drive the observed results on effectiveness, I find that the loss in effectiveness experienced by same-state legislators is larger when the deceased legislator was a committee chair. Chairs are powerful players in Congress (Berry and Fowler, 2018). They exercise both positive and negative agenda control, deciding which bills are considered by their committees and which bills are blocked or disregarded. This also gives them power and influence over the chairs and members of other committees. Consequently, when lawmakers lose an ally in the House as powerful as a committee chair, their capacity to draft and advance legislation experiences a sizable decrease.

When a lawmaker dies in office, both same-state legislators from the same and different parties as deceased legislators experience significant decreases in their effectiveness levels. In a Congress characterized by increasing levels of political polarization and party control of roll-call voting (McCarty, Poole and Rosenthal, 2016), this finding underscores the importance of weak ties and connections that cross the party lines (Harbridge, 2015; Kirkland, 2011). Weak ties formed between legislators that have strong fundamental differences—such as those formed by lawmakers from opposing parties—allow lawmakers to obtain crucial information and gather the support of legislators that would otherwise oppose a bill.

1.2. Related Literature and Theory

Effective lawmakers play a crucial role in legislative bodies. They devise solutions to pressing societal problems, gather coalitions to support these solutions, and fight for their

proposals throughout the lawmaking process. Eventually, some of these proposals become law and bring about meaningful policy change. Understanding what factors influence lawmakers' effectiveness is important to understand the lawmaking process itself. Hence, it is unsurprising that the study of legislators' effectiveness has a long history in the literature.

Matthews (1960) provides the first attempt at measuring lawmakers' capacity to advance their bills by creating an index of legislative effectiveness. Frantzich (1979) documents that seniority, majority-party status, and electoral safety are the most important determinants of legislative effectiveness. Hibbing (1991), Anderson, Box-Steffensmeier and Sinclair-Chapman (2003), Padró i Miquel and Snyder (2006), Cox and Terry (2008), and Volden and Wiseman (2014) also study which individual characteristics and institutional positions predict lawmakers' effectiveness in the U.S. House, Senate, and state legislatures.

Although one of lawmakers' most pressing goals is to be reelected (Mayhew, 1974), other key objectives include achieving institutional power, sponsoring substantive bills, and ensuring their passage (Fenno, 1973; 1978). Effective and productive lawmakers bring about policy change and draft bills that, if passed, would advance their policy goals. Legislative effectiveness is valued by voters or it is correlated with traits that voters value. Padró i Miquel and Snyder (2006), Volden and Wiseman (2014), and Fournaies and Hall (2018) show that lawmakers' effectiveness in the first term is positively correlated with longevity in the chamber.

In addition to individual characteristics and institutional positions, the literature on networks has shown that lawmakers who have many connections in the U.S. House and Senate tend to be relatively more effective (Fowler, 2006a; Kirkland, 2011).² Yet, legislative networks

²Tam Cho and Fowler (2010) show that Congress as a whole is more productive when legislators are more interconnected in the cosponsorship network.

are endogenously formed. It is likely that personal traits that predispose legislators to build large or influential networks also help these lawmakers to be effective. Thus, natural experiments that induce variation in networks are needed to credibly learn how connections impact legislators' effectiveness. However, recent papers that rely on natural experiments that induce variation in networks have estimated the effects of legislative connections on voting behavior, rather than effectiveness.

Masket (2008) and Harmon, Fisman and Kamenica (2018) show that deskmate pairs influence each other's voting behavior in the California Assembly and the European Parliament, respectively.³ Minozzi and Caldeira (2015) show that co-residence in boardinghouses predicts and causes voting agreement in the U.S. House of Representatives before the Civil War. Fong (2019) shows that lawmakers take cues for voting from expert peers. Cohen and Malloy (2014) show that senators who studied at the same university have higher agreement rates in roll-call votes and engage in logrolling. These papers have studied how networks affect legislators' voting behavior, but to date, little research has been done to estimate the causal effects of connections on legislative effectiveness through natural experiments. Battaglini, Sciabolazza and Patacchini (2019) show that network centrality improves legislative effectiveness. They account for the endogeneity of network centrality by exploiting the tendency of pairs of legislators who studied at the same university at similar times to collaborate.⁴ Network centrality measures the location of a lawmaker in the congressional network of collaborations. By contrast, I focus on direct connections between pairs of

³Rogowski and Sinclair (2012), however, find that office proximity—instrumented by the House office lottery—does not impact lawmakers' voting and cosponsorship behavior.

⁴Battaglini and Patacchini (2018) show that network centrality increases campaign contributions. Canen, Jackson and Trebbi (2019) develop a structural model that accounts for the endogenous formation of networks, and find that politicians' efforts in Congress are complements.

legislators. Whereas the centrality of a lawmaker contributes to bill success in the later law-making stages, I show instead that connections matter most in the committee consideration stage.

The notion that connections are important for effectiveness is theoretically intuitive. The lawmaking process in the U.S. House is tortuous. A bill that becomes law must first be introduced into the chamber, considered in committee, sent to the House floor, approved by both chambers, and signed into law by the president. Most bills receive no action in committee.⁵ In light of this lengthy process, legislators must cooperate with each other so that their bills can successfully navigate through the different lawmaking stages. One way in which connections can facilitate a bill's passage is by providing access to influential committee members and committee chairs. They can also promise to cosponsor each other's bills and mobilize resources. Evidence exists that vote trading occurs among connected legislators in Congress (Cohen and Malloy, 2014), and similar dynamics might operate in the lawmaking realm.⁶ Enlisting additional cosponsors could be valuable, as bills supported by a larger number of cosponsors are more likely to pass in the later stages of lawmaking (Box-Steffensmeier, Christenson and Craig, 2019). Lastly, in the same way that lawmakers take cues from their peers to decide their vote on bills for which they lack expertise (Matthews and Stimson, 1975; Fong, 2019), committee members and chairs might base their bill consideration decisions on whether influential lawmakers cosponsored them. Likewise, lawmakers might decide to cosponsor a bill if influential lawmakers cosponsored it early in the lawmaking process.

⁵Only 17% of all bills receive action in committee.

⁶Box-Steffensmeier, Campbell, Podob and Walker (2019) study another form of cooperation among legislators, namely the sharing, trading, or selling of donor and supporter lists between congressional campaigns. They show that this form of electoral collaboration is influenced by similar strategic considerations and dynamics.

Another channel through which connections can enhance effectiveness in Congress is information sharing. Information about relevant committee members, the specific content and cosponsors to include in a bill to maximize peer support, or the optimal timing to introduce specific legislation can help legislators to fine-tune their bills to increase their valence and improve their appeal, without making large concessions on their policy goals. Although information exchanges in Congress are particularly challenging to observe, researchers have devoted ample attention to analyzing how information is aggregated by individuals and committees (Krehbiel, 1992; Ainsworth and Akins, 1997; Victor and Ringe, 2009 on the informational role of caucuses).

1.3. Endogenous Connections

The death of a legislator in office constitutes an abrupt disruption to legislative networks, which should be felt especially by lawmakers connected to the deceased legislator.⁷ Accordingly, a natural first attempt to identify the effects of losing a legislative connection on effectiveness is to examine how the effectiveness of connected legislators changes—compared with non-connected legislators—after a legislator passes away. Several measures of connections between legislators have been used to date, all of which are based on cosponsorship patterns (Fowler, 2006a, 2006b; Battaglini, Sciabolazza and Patacchini, 2019).⁸

⁷Previous literature leveraged deaths as network disruptions to estimate the effects of connections on different outcomes. Azoulay, Graff-Zivin and Wang (2010) analyze how the death of academic superstars affects their collaborators' quality-adjusted publication rates. Fedaseyeu and Lvovskiy (2018) find that firms that contribute to a legislator from the U.S. House of Representatives that dies in office experience a decrease in equity value shortly after the legislator dies. Roberts (1990) leverages the death of Senator Henry "Scoop" Jackson (D-WA) in 1983 to investigate the relationship between seniority and federal benefits.

⁸Cosponsoring a bill is considered as an endorsement, a signal about its content (Wilson and Young, 1997; Koger, 2003; Gross and Shalizi, 2009; Bratton and Rouse, 2011), and a commitment of support (Bernhard and Sulkin, 2013). House bills can only have a single legislator as the sponsor, but there are no restrictions on the number of cosponsors.

However, the above approach is deeply flawed and leads to incorrect inferences about the value of connections. The fundamental problem is that well-connected legislators, by virtue of their many legislative connections, are likely to be connected to legislators who die in office. Likewise, well-connected legislators tend to be senior and powerful members of the House. The imbalance in the characteristics of lawmakers who are affected and unaffected by the deaths of their colleagues implies that the causal effects of networks on effectiveness cannot be estimated by comparing connected and non-connected legislators. Even in the absence of legislator deaths, more and less connected legislators tend to experience different changes in their legislative effectiveness over time.⁹ In the next section, I propose a research design that overcomes this sample selection problem and identifies the causal effects of connections on legislative effectiveness.

1.4. Same-State Legislators

As explained in the introduction, the endogenous formation of legislative connections poses an obstacle in assessing how connections impact the effectiveness of legislators in the U.S. House of Representatives. Individual characteristics that affect lawmakers' capacity to form legislative connections likely affect their effectiveness too. As a result, a positive correlation between legislators' connectedness and effectiveness does not imply that connections help lawmakers to be more effective. To estimate the causal effects of connections on effectiveness, I study the legislators who represent the same states as deceased legislators. These legislators collaborated with the deceased legislators more often than legislators from other states, but were otherwise comparable to other legislators. The identifying assumption is

⁹More connected legislators expand their connections and become more effective and more powerful over time. Less connected legislators, by contrast, wander around Congress crestfallen, cognizant of the dismal fate awaiting their bills and their slim prospects of ever achieving positions of power.

that state delegations are quasi-randomly affected by deaths in office: absent the deaths, the effectiveness of legislators from the same states as deceased legislators would have paralleled the effectiveness of other legislators in the House.

Deceased Legislators were more Connected to Same-State Legislators

First, I show that same-state legislators were more connected to deceased legislators than were legislators from other states. Prior work has established the importance of regional relationships in Congress (Pellegrini and Grant, 1999; Fowler, 2006a; Caldeira and Patterson, 1987 and Bratton and Rouse, 2011 for state legislatures). Politicians from contiguous districts or belonging to the same state or regional delegations are likely to know each other before they enter Congress and are likely to represent the interests of similar constituents. Some of the strongest connections in the House tend to occur between pairs of legislators who represent the same state or contiguous districts (Fowler, 2006a). Frequently, strong connections are formed even when the legislators belong to different parties. Table 1.1 shows the results of estimating the following model

$$(1.1) \quad \textit{Connection}_{i,t} = \alpha_i + \delta_t + \beta \textit{Same State}_{i,t} + \gamma X_{it} + \epsilon_{it}$$

where $\textit{Connection}_{i,t}$ denotes the number of cosponsorships received by legislator i in congress $t - 1$ from legislators deceased in congress t (Columns 1 and 2) and the number of reciprocal cosponsorships at $t - 1$ with legislators deceased in congress t (Columns 3 and 4).¹⁰ Both measures of connection are calculated for the congress before the legislator passed

¹⁰The reciprocal cosponsorship variable is calculated as $\min\{\textit{Cosp}_{ij}, \textit{Cosp}_{ji}\}$, where $\textit{Cosp}_{ij}$ and $\textit{Cosp}_{ji}$ denote the number of bills sponsored by legislator j and cosponsored by legislator i , and vice versa.

away.¹¹ $SameState_{i,t}$ is equal to 1 if a legislator from the same state as i dies in congress t . The terms α_i and δ_t denote legislator and congress fixed effects, and X_{it} denotes a set of legislator-level controls.¹² Standard errors are clustered at the legislator level to account for arbitrary within-legislator correlation. The sample spans the 1980 to 2010 election cycles and includes the deaths of 45 legislators.¹³

Table 1.1. Same-State Deaths and Legislative Connections

	Cosponsorships Received		Reciprocal Cosponsorships	
	(1)	(2)	(3)	(4)
Same State, t	0.544** (0.112)	0.633** (0.118)	0.265** (0.047)	0.294** (0.050)
Legislator FE	Yes	Yes	Yes	Yes
Congress FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Mean Dependent Variable	1.05	1.05	0.33	0.33
Observations	6800	6564	6800	6564

Note: The table reports the results of estimating equation 1.1. The dependent variable in Columns 1 and 2 is the number of cosponsorships received in congress $t - 1$ from legislators deceased in congress t . The dependent variable in Columns 3 and 4 is the number of reciprocal cosponsorships in congress $t - 1$ with legislators deceased in congress t . The unit of observation is a legislator-congress. Standard errors (in parentheses) are clustered at the legislator level. Significance levels: * 5%, ** 1%

The results show that the lawmakers that experienced the death of a same-state legislator were more connected to deceased legislators. Thus, representing the same states as deceased legislators constitutes a strong proxy for being connected to the deceased.

¹¹If legislator i was connected to more than one deceased legislator in a given congress, the connection variable is the sum of the connections to each deceased legislator.

¹²The control variables include seniority, seniority squared, majority membership, size of the state delegation, vote share in previous election, chair of a committee, chair of a subcommittee, and member of a powerful committee. I explain the rationale for including these control variables later in the "Main Results" section.

¹³The list of legislators that died in office during this period is in Table ??

Same-State Legislators are Comparable to Legislators from Other States

In this section, I present a balance test that analyzes whether legislators from the same states as deceased legislators are comparable to all other legislators. Ideally, there should be no systematic differences in the characteristics of treatment and control units. I estimate the following equation

$$(1.2) \quad Effectiveness_{it} = \delta_t + \beta SameState_{i,t+1} + \gamma X_{it} + \epsilon_{it}$$

which does not include legislator fixed effects. $SameState_{i,t+1}$ is equal to 1 if a legislator from the same state as i dies in congress $t + 1$. Equation 1.2 tests whether the effectiveness scores of treatment versus control legislators differed systematically a congress before the treatment. $Effectiveness_{it}$ denotes the legislative effectiveness scores of legislator i in congress t , which are obtained from the Legislative Effectiveness Project (Volden and Wiseman, 2014). The scores are calculated as weighted sums of the number of bills sponsored by a legislator in a congress, the importance of those bills, and how far they advanced through the lawmaking stages. The scores are normalized so that the mean score in each congress is equal to 1.

The results are shown in Table 1.2. The first column shows that legislators from the same states as deceased legislators were slightly more effective than all other legislators, but the difference is not statistically significant. Furthermore, Columns 2 and 3 show that after controlling for seniority and further legislator-level variables, the differences in effectiveness between the two groups are even smaller. The results in Table 1.2 indicate that legislators

Table 1.2. Same-State Balance Table

	Legislative Effectiveness		
	(1)	(2)	(3)
Same State, t+1	0.110 (0.076)	0.089 (0.070)	0.060 (0.055)
Seniority		0.169** (0.015)	0.084** (0.021)
Legislator FE	No	No	No
Congress FE	Yes	Yes	Yes
Controls	No	No	Yes
Observations	5616	5606	5445

Note: The table reports the results of a balance test. The unit of observation is a legislator-congress. Standard errors (in parentheses) are clustered at the legislator level. Significance levels: * 5%, ** 1%

from the same and different states as deceased legislators, respectively, constitute appropriate treatment and control groups to determine whether losing a connection impacts the lawmakers' effectiveness scores.

Main Results

In the previous sections I showed that legislators from the same states as deceased legislators were more connected to them, but were otherwise comparable to all other legislators. In this section, I examine how those lawmakers' effectiveness scores were affected by their same-state peers' deaths by estimating the following model

$$(1.3) \quad Effectiveness_{it} = \alpha_i + \delta_t + \sum_{s=-1}^1 \beta_{t+s} SameState_{i,t+s} + \gamma X_{it} + \epsilon_{it}$$

where $\text{Same State}_{i,t+s}$ is an indicator variable that is equal to 1 if a legislator from the same state as legislator i dies during congress $t+s$, and legislator i was in the House since—at least—congress $t+s-1$. The terms α_i and δ_t denote legislator and congress fixed effects and X_{it} denotes a set of legislator-level controls; standard errors are clustered at the legislator level.

The legislator fixed effects, α_i , are included to account for time-invariant, legislator-level traits that impact their effectiveness and connections. Thus, the estimation exploits within-legislator variation in exposure to the loss of a connection and effectiveness for identification. The congress fixed effects, δ_t , absorb yearly common shocks. Same State_{t+1} is added to test for differing pre-treatment trends between treatment and control legislators. Same State_t and Same State_{t-1} test for effects of legislator deaths on the effectiveness of same-state legislators in the congress in which the legislators died, and the next congress, respectively. The control variables, X_{it} , include seniority and its squared value to control for common non-linear effectiveness trajectories throughout legislators' careers; an indicator variable for majority membership; size of the state delegation;¹⁴ vote share in the previous general election; and indicator variables for whether the legislator was the chair of a committee, chair of a subcommittee, or member of a powerful committee (Rules, Appropriations, and Ways and Means). These last control variables account for changes in legislative effectiveness scores attributable to changes in the institutional positions of legislators. Majority status, seniority, electoral security, committee leadership positions, and membership in powerful committees are major determinants of effectiveness (Volden and Wiseman, 2014).

¹⁴The size of the state delegation is mostly constant over time, and thus absorbed by the legislator fixed effects. However, it sometimes changes after redistricting as a result of changes in population across states.

Table 1.3. Effect of Death of Same-State Legislator on Effectiveness

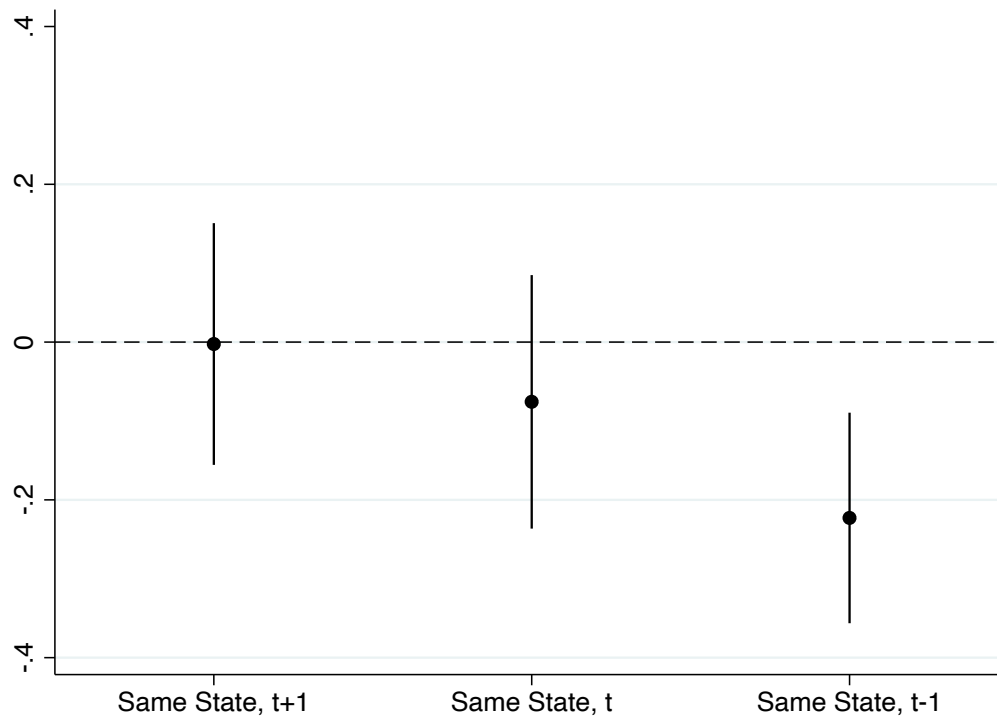
	Legislative Effectiveness				
	(1)	(2)	(3)	(4)	(5)
Same State, t+1			-0.022 (0.076)	-0.047 (0.119)	-0.002 (0.078)
Same State, t	-0.057 (0.048)			-0.153 (0.118)	-0.076 (0.082)
Same State, t-1		-0.156** (0.060)		-0.205* (0.088)	-0.223** (0.068)
Majority					0.888** (0.094)
Seniority					-0.013 (0.038)
Seniority ²					0.003 (0.003)
Size of State Delegation					-0.044 (0.038)
Vote Share					-0.001 (0.002)
Chair Committee					2.824** (0.316)
Chair Subcommittee					0.608** (0.100)
Power Committee					-0.243** (0.076)
Legislator FE	Yes	Yes	Yes	Yes	Yes
Congress FE	Yes	Yes	Yes	Yes	Yes
Mean Dependent Variable	1	1	1	1	1
Observations	6800	5423	5423	4284	4212

Note: The table reports the results of estimating equation 1.3. The unit of observation is a legislator-congress. Standard errors (in parentheses) are clustered at the legislator level. Significance levels: * 5%, ** 1%

Table 1.3 displays the results of estimating equation 1.3. Legislators from the same states as deceased legislators experience a significant drop in their effectiveness scores in the congress after the deaths. The results are robust to the inclusion of the control variables. The estimated decrease in effectiveness due to the loss of a same-state legislator is sizable: the coefficient for $Same\ State_{i,t-1}$ in Column 5 is -0.223. Figure 1.1 plots the results in Column 5. For comparison, the within-legislator standard deviation of effectiveness is 1.16, the mean effectiveness score in each congress is 1, and majority membership is associated with a 0.89 point increase in effectiveness. Furthermore, the set of same-state legislators likely includes several lawmakers who had negligible or no legislative connections with deceased legislators. At the same time, deceased legislators probably had many close connections to lawmakers who represented different states. Admittedly, deceased legislators were more senior and powerful than average legislators. The estimated effects might thus be larger than the effects of losing average legislators. That said, the estimated effects are substantial, especially as they pertain to the loss of a single lawmaker in a legislature composed of 435 members. Far from being able to replace the lost connection with new connections to lawmakers, the results show that the loss of a legislator exerts a lasting impact on the lawmaking capacities of representatives from the same states as the deceased.

The estimate for $Same\ State_{t+1}$ is very close to zero, indicating that same-state legislators display similar pre-treatment trends to all other legislators. This finding bolsters confidence in the identifying assumption that, absent the legislator deaths, same-state legislators' effectiveness scores would have followed parallel trends relative to all other legislators. Finally, the coefficient for $Same\ State_{i,t}$ is negative but not significant. Legislators who passed away in office worked in the House for at least some part of the congress during which they

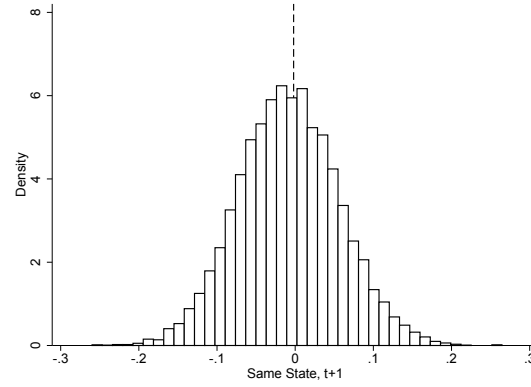
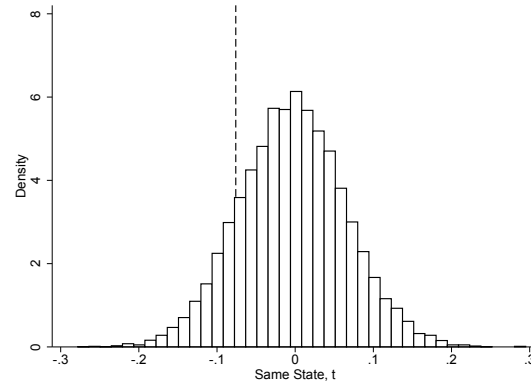
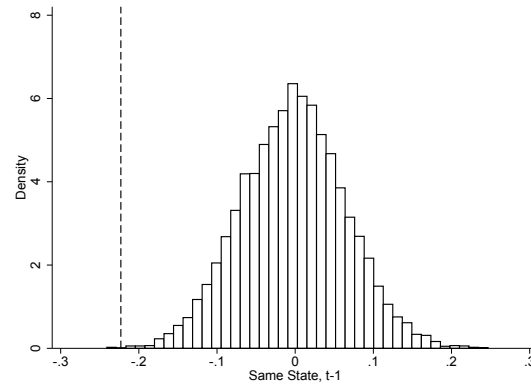
Figure 1.1. Plot of the Effect of the Death of a Same-State Legislator on Effectiveness



Note: This figure depicts the estimates of equation 1.3 (Column 5 of Table 1.3).

died, and most bills are introduced to the House early in the congressional term.¹⁵ It is thus not surprising that the effects for $Same\ State_{i,t}$ are small.

¹⁵When a congressional term ends, all bills which have not been signed into law are archived, regardless of their status. When Congress reconvenes, a bill has to be reintroduced as if it were a new bill.

Figure 1.2. Placebo Distributions of $\text{Same State}_{i,t+s}$ (a) Distribution of $\text{Same State}_{i,t+1}$ estimates(b) Distribution of $\text{Same State}_{i,t}$ estimates(c) Distribution of $\text{Same State}_{i,t-1}$ estimates

Note: These figures plot the point estimate distributions of the placebo exercises for $\text{Same State}_{i,t+s}$. The dashed lines denote the point estimates obtained with actual data. The distributions are based on 10,000 simulations.

Figure 1.2 plots the distributions of 10,000 placebo estimates for each β_{t+s} associated with $SameState_{i,t+s}$, $s = \{1, 0, -1\}$ and the point estimates obtained with actual data (dashed lines). As 45 deaths of legislators occurred in office during the period under study, each placebo estimate is obtained by randomly selecting a set of 45 legislators at certain congresses as deceased, and computing the resulting treatment and control units based on the states the randomly chosen legislators represented. The legislators who died in office were more senior than the average legislator in the House. Accordingly, in the placebo simulations, I randomly select sets of deceased legislators from the pool of legislators who were serving their fourth term in office, or a subsequent term. With this constraint, the seniority of the simulated sets of deceased legislators is similar to that of the actually deceased.

The placebo distributions reflect the sample variability of the β_{t+s} estimates and should have a mean value of 0. The sets of legislators considered as treated or not treated in each simulation are random; the effectiveness scores of the two groups should exhibit parallel trends, on average. Figure 1.2 shows that the placebo distributions for the three $SameState_{i,t+s}$, $s = \{1, 0, -1\}$ estimates are indeed centered around zero. In addition, the actual coefficient of -0.223 for $SameState_{i,t-1}$ stands out as an outlier among the simulation estimates; only eight of the 10,000 simulations yielded an estimate having a higher absolute value. In the absence of any effect, it would have been highly unlikely to obtain such an extreme estimate by chance.

Finally, the null hypothesis ($H_0 : \beta_{t+s} = 0$) is rejected in 3.82%, 3.57%, and 3.82% of the placebo simulations, respectively. These numbers are relatively close to the 5% theoretical size and indicate that the research design slightly under-rejects the null hypothesis no effects. Overall, the placebo exercise shows that the negative and significant estimate for

$Same\ State_{i,t-1}$ in Table 1.3 is not driven by unobserved differences between lawmakers affected and unaffected by deaths of same-state legislators.

1.5. Additional Results

Lawmaking Stages

A bill that becomes law needs to first go through several lawmaking stages. To identify the channels through which deceased legislators contributed to the lawmaking success of legislators who represent the same states, I conduct a stage-by-stage analysis of the main effects.

Table 1.4. Lawmaking Stages

	Sponsor	Action In Committee	Action Beyond Committee	Pass House	Law
	(1)	(2)	(3)	(4)	(5)
Same State, t+1	-0.708 (0.439)	-0.122 (0.158)	-0.041 (0.129)	-0.047 (0.114)	0.060 (0.066)
Same State, t	-1.314** (0.474)	-0.133 (0.175)	-0.074 (0.151)	-0.032 (0.128)	0.024 (0.072)
Same State, t-1	-1.952** (0.400)	-0.550** (0.144)	-0.414** (0.126)	-0.373** (0.104)	-0.151* (0.065)
Legislator FE	Yes	Yes	Yes	Yes	Yes
Congress FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Mean Dependent Variable	13.68	2.28	1.72	1.39	0.68
Observations	4212	4212	4212	4212	4212

Note: The dependent variables in Columns 1-5 are the number of bills that 1) were sponsored by the legislator, 2) were sponsored by the legislator and received action in committee, 3) received action beyond committee, 4) passed the House, and 5) became law. The unit of observation is a legislator-congress. Standard errors (in parentheses) are clustered at the legislator level. Significance levels: * 5%, ** 1%

Column 1 of Table 1.4 shows the estimates for the same specification as in equation 1.3, with the effectiveness scores in the dependent variable replaced by the number of bills sponsored. The results show that lawmakers sponsor relatively fewer bills in the congress during which a legislator from the same state died. The decrease, however, is largest in the congress after the deaths (-1.95 vs 13.7 bills sponsored by a legislator per congress, i.e. a 14% decrease). Column 5 shows that the number of sponsored bills that become law decreases only in the congress after a legislator from the same state died, and not in the congress in which the deaths occurred. Furthermore, the relative reduction in the number of bills that became law is more pronounced than the reduction in the number of bills sponsored.¹⁶ These two results show that the loss of a legislator from the same state not only affects the capacity of legislators to sponsor bills, but also markedly undermines their efforts to advance those bills through the lawmaking process until they become law.

Table 1.4 also shows that the loss of a connection exerts a great impact on the number of sponsored bills that receive action in committee (Column 2). While same-state legislators' number of sponsored bills drops by 14% relative to the mean number of sponsored bills (13.7 bills), the number of sponsored bills that receive action in committee decreases by 0.55, which is a 24% decrease relative to the mean number of sponsored bills that receive action in committee (2.3 bills). By contrast, conditional on a bill receiving action in committee, losing a connection does not notably affect the probability of it becoming law.

Therefore, the decomposition exercise indicates that the connection loss impacts lawmakers' capacity to draft bills and have them receive action in committee. The decrease in the number of bills sponsored could be explained by the fact that connected lawmakers

¹⁶0.15 vs 0.68 sponsored bills that become law per congress, i.e. a 22% decrease.

sometimes collaborate with each other to draft bills. Furthermore, lawmakers might anticipate that their bills will be less likely to be considered in committee after the death of an ally and might decide to exert lower effort in sponsoring bills as a result. The decrease in the number of bills sponsored by same-state legislators that receive action in committee illustrates the key role played by legislative allies in ensuring bills are considered by the relevant committees. Committee chairs are powerful gatekeepers (Berry and Fowler, 2018; Volden and Wiseman, 2014), and first- or second-degree legislative connections with committee members or committee chairs can facilitate a bill's journey to the relevant committee. Lorenz (2020) shows that agenda setters are more likely to allocate committee consideration to legislation that is supported by a diverse coalition. Connections to other lawmakers in the chamber can help sponsors mobilize a diverse coalition of interests in favor of a bill.

Bills, Committees, Money, and Reelection

Column 1 of Table 1.5 shows that the number of bills cosponsored by same-state legislators increases slightly in the congress after the death, but the variable is not statistically significant. The finding suggests that same-state legislators actively seek to establish new legislative connections or strengthen their current connections to make up for the loss. Such efforts result in a slightly higher number of cosponsors for their sponsored bills (Column 2).

The decreased effectiveness of lawmakers impacted by the death of a legislator from the same state cannot be attributed to same-state legislators being less likely than other legislators to become chairs of a committee or subcommittee (Column 3 of Table 1.5), nor to same-state legislators receiving significantly less money from campaign contributions by corporate political action committees (Column 4 of Table 1.5). These constitute two potential

Table 1.5. Bills, Committees and Money

	# Bills Cosponsored	Average # Cosponsors	Chair	PAC	Majority
	(1)	(2)	(3)	(4)	(5)
Same State, t+1	-1.827 (3.930)	0.802 (0.810)	0.021 (0.019)	8.677 (12.517)	-0.040 (0.023)
Same State, t	-4.344 (4.937)	1.384 (0.906)	-0.019 (0.021)	0.673 (13.536)	-0.027 (0.024)
Same State, t-1	5.816 (4.738)	1.270 (0.783)	-0.018 (0.017)	-5.124 (8.808)	-0.017 (0.019)
Legislator FE	Yes	Yes	Yes	Yes	Yes
Congress FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Mean Dependent Variable	190.55	15.42	0.27	289.30	0.57
Observations	4205	4205	4212	4192	4212

Note: The dependent variables in Columns 1-5 are 1) the number of bills cosponsored by the legislator, 2) the average number of cosponsors for bills sponsored by the legislator, 3) an indicator variable for committee or subcommittee chair, 4) total money received by PACs (in 1,000 dollars), and 5) an indicator variable for majority party status, respectively. The unit of observation is a legislator-congress. Standard errors (in parentheses) are clustered at the legislator level. Significance levels: * 5%, ** 1%

channels through which legislative connections can help lawmakers to become more effective.¹⁷ The null effect of losing a connection on PAC contributions is noteworthy, given that network centrality increases the amount of money lawmakers receive from PACs (Battaglini and Patacchini, 2018). A possible explanation is that connections might facilitate the initial contact between a lawmaker and a PAC, but once the relationship has been established, connections are no longer essential to ensure that money from the PAC continues to flow

¹⁷Table 1.3 shows that lawmakers' effectiveness significantly increases after they become chair of a committee or subcommittee (see also Berry and Fowler, 2018). Volden and Wiseman (2014) extensively study the crucial role played by committee chairs in advancing legislation.

to the lawmaker. Finally, Column 5 indicates that legislators from the same states as deceased legislators did not transition into or out of majority status in significantly different rates than all other legislators. This finding is not surprising, but given that majority status is a major driver of legislative effectiveness, it is important to ensure that treatment status does not correlate with changes in the partisan control of the House and concomitant changes in lawmakers' majority status.

Table 1.6. Incumbent Reelection

	Incumbent Reelection			
	(1)	(2)	(3)	(4)
Same State, t	0.000 (0.014)	-0.002 (0.014)		
Same State, t-1			0.025 (0.015)	0.022 (0.015)
State FE	Yes	Yes	Yes	Yes
Congress FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Mean Dependent Variable	0.85	0.85	0.85	0.85
Observations	7047	7032	5616	5616

Note: The table reports the results of regressing incumbent reelection on same-state death, with state and congress fixed effects. The control variables are the standard ones except vote share. The unit of observation is a legislator-congress. Standard errors (in parentheses) are clustered at the legislator level. Significance levels: * 5%, ** 1%

Likewise, in Table 1.6 I show that the decrease in effectiveness caused by the deaths of same-state legislators did not affect lawmakers' reelection probabilities. The dependent variable is an indicator that takes value 1 if the lawmaker is reelected at the end of a congressional term. The independent variable is $SameState_t$ (Columns 1 and 2) and $SameState_{t-1}$ (Columns 3 and 4). Columns 1 and 2 test whether the death of a same-state legislator affects

reelection rates in the end of the congress in which the deaths take place, and Columns 3 and 4 test reelection rates in the end of the congress after the deaths take place.¹⁸ If anything, reelection rates slightly increase in the end of the congress after the deaths. Either voters do not punish their representatives for their decrease in effectiveness, or same-state legislators shift resources away from lawmaking and into district activities and events to fend off any possible electoral penalty associated with a decreased congressional influence.¹⁹

Types of Bills

The legislative effectiveness scores developed by Volden and Wiseman (2014) assign different weights to bills depending on their importance. They classify bills as commemorative, substantive, or substantive and significant. Bills categorized as "commemorative" propose to rename post offices, commemorate important dates, or provide private relief to an individual. "Substantive and significant" bills refer to non-commemorative bills that received an end-of-the-year write-up in the Congressional Quarterly Almanac. "Substantive" bills consist of commemorative bills that received a CQ Almanac write-up and any bill not included in the other two categories.

In Table 1.7, I decompose the results for the number of sponsored bills and bills that become law according to the bill categories. The results show that the deaths of legislators from the same state has no effects on commemorative bills. For substantive bills, the number of sponsored bills decreases, both for the congress in which a legislator from the same

¹⁸The regression includes state and congress fixed effects. State fixed effects account for differences across states in the incumbency advantage and propensity to be impacted by same-state deaths. The control variables included in Columns 2 and 4 are the same as in the main specification, except for vote share.

¹⁹The null result on contributions received from PACs (Column 4 of Table 1.5) suggests lawmakers impacted by same-state deaths did not significantly increase their fund-raising efforts.

Table 1.7. Significant, Substantive, and Commemorative Bills

	Significant, Substantive, and Commemorative Bills					
	(1) Bills SS	(2) Law SS	(3) Bills S	(4) Law S	(5) Bills C	(6) Law C
Same State, t+1	-0.002 (0.107)	0.033 (0.046)	-0.786 (0.410)	0.015 (0.045)	0.081 (0.075)	0.012 (0.024)
Same State, t	-0.063 (0.105)	-0.025 (0.044)	-1.315** (0.439)	-0.002 (0.050)	0.064 (0.086)	0.051 (0.026)
Same State, t-1	-0.177 (0.097)	-0.073 (0.042)	-1.698** (0.358)	-0.051 (0.040)	-0.077 (0.085)	-0.027 (0.020)
Legislator FE	Yes	Yes	Yes	Yes	Yes	Yes
Congress FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dependent Variable	0.74	0.23	12.17	0.30	0.77	0.15
Observations	4212	4212	4212	4212	4212	4212

Note: The dependent variables in Columns 1-6 are the number of 1) substantive and significant (SS) bills sponsored by the legislator, 2) SS bills that became law, 3) substantive (S) bills sponsored, 4) S bills that became law, 5) commemorative (C) bills sponsored, and 6) C bills that became law. The unit of observation is a legislator-congress. Standard errors (in parentheses) are clustered at the legislator level. Significance levels: * 5%, ** 1%

state died and for the next congress. The number of substantive bills that become law also decreases for the congress after the death, although this effect is not statistically significant. For substantive and significant bills, the decreases for the congress after the death of a same-state legislator are large, relative to the mean, for both the number of sponsored bills (24% decrease) and bills that become law (32% decrease). Both decreases are significant at the 10% level. The decrease in the number of substantive and significant bills sponsored might indicate that the loss of a connection impacts the quality of sponsored bills. However, the decrease in the number of substantive and significant bills that become law is relatively larger. Lawmakers affected by the loss of a legislator from the same state find it more difficult to

advance their high-quality bills. Losing a connection critically impacts lawmakers' capacity to advance bills through the lawmaking stages, especially for landmark bills that propose far-reaching policy changes.

Chairs and Issue Areas

If the effectiveness decrease suffered by lawmakers impacted by the deaths of same-state legislators can be attributed to losing access to relevant committee members and influential lawmakers or to losing the signaling value of the cosponsorships and endorsements provided by deceased lawmakers, we should expect especially large effects when powerful lawmakers die. Powerful lawmakers have a wide network of connections whom they can persuade, and also have the strongest influence over their connections. I examine this hypothesis in Column 1 of Table 1.8 by testing whether the decreases in effectiveness experienced by same-state legislators were larger if deceased lawmakers held a chair of a legislative committee at some point during their careers.²⁰ The results support the hypothesis. Legislators from the same states as deceased committee chairs experience a significantly larger decrease in effectiveness in the congress after the death relative to the decrease experienced by legislators impacted by the death of a same-state non-chair legislator. Lawmakers' capacity to advance bills through the lawmaking process particularly suffers when they lose a powerful and influential ally in Congress. Box-Steffensmeier, Christenson and Craig (2019) argue that endorsements from well-connected interest groups serve as cues that help grow the list of bill cosponsors. Likewise, the endorsement and support of influential lawmakers—committee chairs—contributes to the success of bills in the early lawmaking stages.

²⁰I consider committee chair positions held during the 1973-2010 period.

Table 1.8. Comparative Statics

	LES			
	(1)	(2)	(3)	(4)
Same State, t+1	0.068 (0.090)	-0.028 (0.101)	0.052 (0.122)	0.094 (0.102)
Same State, t	0.009 (0.095)	-0.071 (0.107)	-0.066 (0.116)	-0.073 (0.120)
Same State, t-1	-0.118 (0.081)	-0.253** (0.091)	-0.188 (0.096)	-0.177 (0.093)
Same State, t+1 x Deceased Chair	-0.243 (0.165)			
Same State, t x Deceased Chair	-0.261 (0.184)			
Same State, t-1 x Deceased Chair	-0.364* (0.169)			
Same State, t+1 x Issue Overlap		0.054 (0.130)		
Same State, t+1 x Issue Overlap		-0.007 (0.137)		
Same State, t+1 x Issue Overlap		0.069 (0.113)		
Same State, t+1 x Copartisan			-0.096 (0.142)	
Same State, t x Copartisan			-0.020 (0.156)	
Same State, t-1 x Copartisan			-0.063 (0.120)	
Same State, t+1 x Post-1994				-0.214 (0.201)
Same State, t x Post-1994				0.011 (0.259)
Same State, t-1 x Post-1994				-0.128 (0.178)
Legislator FE	Yes	Yes	Yes	Yes
Congress FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	4212	4212	4212	4212

Note: The dependent variable is the legislative effectiveness score of legislator i in congress t . The unit of observation is a legislator-congress. Standard errors (in parentheses) are clus-

If connections help lawmakers become more effective because of the information they can provide on the specific content to include in bills, which lawmakers to contact for cosponsorship support, or about members of the relevant committee, we should expect the assistance provided by the connection to be especially valuable on issue areas in which the connection is an expert. In turn, an overlap in issue expertise between deceased and same-state legislators should predict larger adverse effects of deaths on lawmakers' effectiveness levels: lawmakers that specialize in similar issue areas would benefit the most from the mutual exchange of information and assistance. To test this hypothesis, I leverage data from the Congressional Bills Project (Adler and Wilkerson, 2015) to identify the issue areas in which the deceased and same-state legislators specialized. Each house bill is classified into one of 22 categories, such as agriculture, civil rights, energy, immigration, defense, and housing. I calculate the number of bills sponsored by each legislator during her career and consider the three issue areas in which she has sponsored most bills as her areas of issue expertise.²¹ Then, for each of the 45 lawmakers who died in office, I identify the same-state legislators that have at least one expert issue area in common. In Column 2 of Table 1.8 I differentially test whether same-state legislators who had greater overlap in issue expertise were more adversely impacted by the deaths. However, the results do not support this hypothesis, which suggests that the access and cue-taking channels are more prevalent than the information channel.

²¹I compute lawmakers' issue expertise using bills sponsored in the 1973-2010 period.

Political Polarization and the Value of Connections

Kirkland (2011) argues that weak legislative ties—ties formed by pairs of legislators who interact infrequently, such as those formed by legislators who represent opposing parties—are more valuable than strong ties in achieving legislative success. Column 3 of Table 1.8 shows that same-state legislators who represent the same party as deceased legislators experience similar decreases in their legislative effectiveness levels relative to same-state different-party legislators.²² As pairs of copartisan legislators from the same state tend to collaborate significantly more than do pairs of legislators from the same state but different parties, the result in Column 3 hints that cross-party, same-state connections are more valuable than same-party, same-state connections. In times of increasing political polarization between Democrats and Republicans, weak ties and cross-party connections can serve as bridges that build broad coalitions in support of substantive legislation.

In Column 4 of Table 1.8 I test whether the value of connections has increased or decreased after the 1994 revolution in which Republicans regained the control of the House and Senate. Newt Gingrich instituted 6-year term limits on committee chairs and gained control of the committee chair nomination process in an attempt to increase parties' influence in lawmaking at the expense of committee chairs.²³ At the same time, legislative polarization in the House of Representatives has steadily risen, a trend that was exacerbated by the nationalization of campaigns promoted by Gingrich in 1994. If anything, the evidence in

²²The coefficient is slightly larger in absolute value for copartisans, but the decrease is borderline significant (p-value: 0.051) for same-state, different-party legislators.

²³Berry and Fowler (2018) study whether the lawmaking power of committee chairs decreased after the 1994 Republican revolution. They find that becoming chair of a committee leads to a lower increase in contributions post-1994, but a larger increase in effectiveness.

Column 4 indicates that connections have become more valuable after 1994, although the difference between both periods is not statistically significant.

One potential factor that might explain why the value of connections has increased over time is that lawmakers have less time and resources to read the bills that are introduced in Congress. As a result of ever-increasing fundraising demands—either to secure reelection facing tough challenger competition or to bid for party leadership and prestige committee positions (Heberlig, 2003)—and a decrease in the number of professional staff in the House of Representatives,²⁴ lawmakers do not have time for policy analysis and increasingly rely on interest groups, committee reports, and cues from colleagues and party leadership to decide which bills they should endorse and support (Box-Steffensmeier, Christenson and Craig, 2019). In this environment, the success of a bill might increasingly hinge on the endorsement and support of influential connections at the expense of the content and intrinsic value of the policy proposal.

1.6. Conclusions

Legislative connections in the U.S. House of Representatives are crucial for the advancement of bills. Most bills introduced in the House are not considered in committee, and a network of connections who can pressure the relevant committee members and committee chairs is an invaluable asset for lawmakers. I show that the loss of a connection significantly decreases lawmakers' effectiveness: their bills become less likely to be considered in committee and are ultimately less likely to become law. The effects are stronger when the lost connection was a chair of a committee—a powerful and influential position.

²⁴In 1985 there were 11,537 professional staff workers. In 2015, the number decreased to 7,703 (Box-Steffensmeier, Christenson and Craig, 2019).

Collaborations in Congress arise endogenously, which constitutes a major challenge for identifying the causal effects of networks and connections on political outcomes (Rogowski and Sinclair, 2012). To learn how legislative connections impact voting behavior and law-making in Congress, studies that leverage quasi-random variation in connections are needed. The design in this paper draws inferences about the effects of connections by comparing the performance of otherwise balanced samples that are differentially exposed to disruptions in their legislative connections. Deaths in office of lawmakers result in abrupt and unexpected disruptions to collaboration networks in the legislature. Membership in the same state delegation induces differential exposure to such disruptions.

The effectiveness spillovers identified in this paper cross party lines. Lawmaking collaborations between legislators from different parties are valuable, even if these collaborations arise less frequently than copartisan collaborations. In times of increasing political polarization in Congress, cross-party legislative collaborations help to secure broader support for pressing policy reforms. Former Representative Beto O'Rourke of Texas recently shared an anecdote that illustrates the importance of cross-party collaborations. It also exemplifies how representing the same state can facilitate contact and spur legislative collaborations. On October 15th, 2019, in the Democratic presidential primary debate that took place in Westerville, OH, O'Rourke was asked about his most surprising friendship. He recalled the time he became friends with Representative Will Hurd of Texas, when both of them were in the San Antonio airport in March, 2017 and their flight to Washington, DC was canceled due to an East Coast blizzard. He asked Hurd if he would like to rent a car with him and drive to DC together. Hurd accepted the offer, and a strong friendship was formed as a result of the

long 25-hour journey. Their friendship became stronger over time, and resulted in legislative collaborations in, among other issues, a bipartisan immigration bill. The serendipitous story behind this example of bipartisan cooperation constitutes one of many ways in which representing the same state can lead to increased contact and subsequent collaboration, and emphasizes the value of cross-party friendships and cooperation in times of strong partisanship, polarization, and political hostility.

CHAPTER 2

**Political Consequences of Economic Hardship: State Economic Activity
and Polarization in American Legislatures**

2.1. Introduction

The dramatic rise of political polarization in Congress started in the 1970s. This trend is widely considered to be politically and economically harmful, but its roots are not fully understood. The increasing levels of gridlock and legislative delay facing Congress have been attributed to a widening ideological gap between Democrats and Republicans, which may hinder the ability of legislators to agree on compromises and forge bipartisan bills.¹ Scholarly interest in the polarization facing Congress is substantial, as shown by the proliferation of papers and books analyzing the possible causes and consequences of this trend (e.g. Poole and Rosenthal, 1984; Layman, Carsey and Horowitz, 2006; Nivola and Brady, 2006; Campbell, 2016).

Thanks to the recent availability and analysis of ideal point estimates for members of state legislatures (Shor and McCarty, 2011), we now know that state legislatures have mirrored the rise in polarization, at least since the mid 1990s. In this paper I study how state economic conditions affect political polarization in state legislatures. State legislatures provide an invaluable empirical laboratory to study how the economy affects polarization: states have experienced disparate economic ups and downs over time, and state legislatures have followed differing paths in their polarization levels. Employing a panel dataset of state lower and upper houses that covers the period from 1996 to 2014, I regress polarization on Gross

¹Early studies that examined the relationship between polarization and gridlock include Binder (1999) and Jones (2001).

State Product (henceforth GSP) per capita,² legislative chamber, and year fixed effects. Leveraging within-state and within-chamber variation in economic activity and polarization, respectively, I show that economic downturns are strongly associated with increases in political polarization.³

To establish this relationship as causal, I follow the instrumental variables approach. The estimation exploits yearly variation in oil prices, interacted with average oil production over GSP in each state, to isolate exogenous variation in economic activity. The intuition behind this strategy is that changes in international oil prices affect state economic conditions differentially according to the importance that oil production has in the economy of a state. The exclusion restriction requires that the oil instrument affects polarization solely through its effects on GSP per capita. Although this assumption is inherently untestable, I provide indirect evidence that suggests its validity, and additionally show that the main results are robust to sizable violations of the exclusion restriction. Furthermore, the instrumental variables approach yields point estimates that are quantitatively similar to the least squares specification. This outcome adds credence to the assertion that the evolution of political polarization in state legislatures is strongly affected by state economic conditions.

The effect of the economy on political polarization is primarily driven by Republicans, whose median ideology scores in state legislatures are highly sensitive to state economic conditions. Republican legislators elected to the legislative chambers for the first time are, on average, more conservative when the state is undergoing economic hardship. Consistent with previous literature (Fair, 1978; Lewis-Beck and Stegmaier, 2000; Margalit, 2011), I find

²GSP per capita is the variable used as a proxy for state economic activity.

³Polarization in a state legislative chamber is measured as the difference between the median Democratic ideological score and the median Republican score, as is customary in the literature.

that economic downturns have anti-incumbent effects: the control of a legislative chamber is more likely to flip in harsh economic times. A poor state economy, however, does not disproportionately favor either of the two main parties at the ballot box, nor does it significantly tilt legislatures' median ideology scores in any particular direction. Therefore, adverse economic shocks shift the ideological centers of gravity in the Democratic and Republican groups further away from each other, without significantly inducing a shift of the median legislator away from the center.

Lastly, state economic conditions affect politicians' ability to raise campaign funds. State legislators fund their campaigns with contributions from political action committees (PACs)—who disproportionately support incumbents and moderates—and direct contributions from individuals—who disproportionately support extreme candidates (Barber, 2016). I show that, during economic downturns, a higher share of incumbent state legislators' campaign funds come from individuals compared with times of economic bonanza. As extreme candidates enjoy a comparative advantage in obtaining direct donations from individual contributors, this factor can partly explain their increased electoral success during downturns.

2.2. Theoretical Considerations

The economy is arguably one of the most salient issues in American elections. A large body of literature—labeled as economic voting—has explored how economic circumstances affecting individual voters, and more broadly economic conditions in an area, influence voter behavior (Lewis-Beck and Stegmaier, 2000; Brunner, Ross and Washington, 2011; Margalit, 2011; Charles and Stevens, 2013; Burden and Wichowsky, 2014). An ailing economy decreases the odds of incumbent reelection (Margalit, 2011; Wolfers, 2002), and increases

voter turnout rates (Brunner, Ross and Washington, 2011; Charles and Stevens, 2013; Burden and Wichowsky, 2014). Under such circumstances, individuals displaced from the labor market become more politically informed (Charles and Stevens, 2013) and more likely to participate in the political process. Furthermore, personal economic experiences shift voters' policy preferences; Brunner et al. (2011) and Margalit (2013) show that personal experiences of economic hardship and job insecurity increase an individual's support for redistribution. In light of this stream of evidence, it is natural to ask whether—and how—local economic conditions affect political polarization within state legislatures. Changes in the politico-economic circumstances of a district can affect the composition and policy preferences of the electorate, which in turn can shift the odds of success among various candidate sets. In this paper I argue that economic conditions in a state affect the composition of the electorate, the success probabilities of moderate and extremist candidates, and ultimately the evolution of political polarization within state legislatures.

Politicians in the Democratic party tend to hold policy positions that favor redistribution increasingly as one moves left on the ideological spectrum. Thus, it is plausible to expect that economic downturns increase the electoral prospects of relatively liberal Democratic candidates: adverse economic shocks displace workers from the labor market and increase workers' perceptions of job insecurity, which could increase the electoral salience of, and support for, redistribution. However, the effects of economic downturns on polarization that I identify in the paper are mainly driven by the entry of relatively conservative candidates. Thus, as strongly conservative politicians are characterized by their staunch opposition to

redistribution and, more generally, to government intervention, the redistribution hypothesis alone falls short of explaining the causal link from local economic conditions to legislative polarization.

However, economic downturns also swell the number of people who need government transfers, which could trigger a backlash against redistribution.⁴ On the other hand, relatively extreme conservative candidates channel the economic resentment of disadvantaged white blue-collar workers into opposition against minorities receiving welfare. As they promote group identification along ethnic and racial lines (Abrajano and Hajnal, 2015), and vigorously lobby for cutting government transfers for low-income immigrants, economic hardship can increase the appeal of relatively extreme conservative candidates' political platforms (Skocpol and Williamson, 2012).⁵

This paper contributes to the large body of literature that explores factors behind political polarization. Previous studies have analyzed the role of income inequality (McCarty, Poole and Rosenthal, 1997 and 2016; Garand, 2010), noting that polarization levels in Congress have strongly comoved with income inequality. Voorheis, McCarty and Shor (2015) provide evidence that the relationship from inequality to legislative polarization is causal. Gerrymandering (McCarty, Poole and Rosenthal, 2009) and primary elections (Hirano, Snyder, Ansolabehere and Hansen, 2010; McGhee, Masket, Shor, Rogers and McCarty, 2014) were found not to have a significant impact on polarization. By contrast, laws that limit corporate campaign contributions (Barber, 2015) and term limits (Olson and Rogowski, forthcoming)

⁴The Tea Party's campaign against big government and social spending was key to Republicans' massive victory in the 2010 midterms (Margalit, 2013). The campaign ignited backlash against redistribution in the aftermath of the Great Recession.

⁵Economic anxiety—over stagnant wages and declining jobs in manufacturing—as well as fear of losing their status in society have been identified as important factors that contributed to Trump's success among white, blue-collar workers during the 2016 presidential elections.

do contribute to polarization in state legislatures. Autor, Dorn, Hanson and Majlesi (2017) find that districts exposed to large increases in import competition from China experienced an increase in polarization in congressional and presidential elections. The present paper contributes to this growing literature by showing that economic activity affects legislative polarization in state legislatures.

The result that economic activity affects polarization does not imply that this is the only factor that drives polarization. In fact, while income inequality and legislative polarization have experienced a parallel rise in the United States since the 1970s, the economy has not followed a parallel negative trend. However, the point of this paper is that economic conditions at the state level can explain a big part of the variation in political polarization in state legislatures. States that fared better in economic terms experienced, all else equal, smaller increases in polarization. Furthermore, the empirical evidence strongly indicates that the relationship is causal. Indeed, it can be argued that structural transformations affecting the economy in recent decades—such as increases in income inequality, uneven growth of wages across the income distribution, globalization, or skill-biased technological change—might have contributed to amplifying the effects of adverse economic shocks on voters' support of extreme policy positions. The reason is that such transformations have increased the fraction of voters affected by economic insecurity and grim future prospects. While the US economy has grown at a healthy 3 percent average yearly rate since the 1980s, the median hourly wage has only increased 6 percent in the entire 1979-2013 period.⁶ As suggestive evidence of this hypothesis, the empirical analysis indicates that the effects of state economic conditions on polarization have grown over time.

⁶<http://www.epi.org/publication/charting-wage-stagnation/>

2.3. Empirical Strategy

To estimate the effects of state economic activity on polarization I use the Shor-McCarty dataset (Shor and McCarty, 2011). The dataset contains estimated ideal points of legislators within the state houses and senates of 49 states⁷ for the period from 1996 to 2014.⁸ These ideal points are comparable over time and across states. Negative scores are assigned to legislators who have liberal voting records, and positive scores to conservative legislators.⁹ *Polarization* in a legislative chamber is calculated as the difference between the median ideological score of Republicans minus the median score of Democrats. One feature of the Shor-McCarty scores is that a single constant score is estimated for the entire political career of the legislator. Therefore, all the observed variation in polarization within a legislative chamber is attributable to legislator replacement. Ideally, time-varying scores would enable the analysis of changes in legislator ideology over time.¹⁰ However, an analysis of the DW-Nominate legislator scores for Congress—which are not fixed over time, but vary linearly—shows that the median absolute change in legislator ideology from one Congress to the next during the 1971-2013 period was equal to 0.008. This represents a meager 0.38 percent change on the liberal-conservative dimension.¹¹ Therefore, almost all the increase in political polarization experienced by Congress since the 1970s can be attributed to the entry of relatively extreme legislators and the exit of moderates, and not to the ideological

⁷Nebraska is the only state that has a unicameral body; it is also a nonpartisan legislature. Data from this state were excluded in this paper.

⁸Their dataset starts in 1993, but missing data is prevalent during the first three years due to unavailability of roll-call data. Thus, polarization data pertaining to 1993, 1994 and 1995 were excluded.

⁹More than 99 percent of observations are contained in the (-2,2) interval.

¹⁰The literature has analyzed whether legislators update their ideological positions in response to changing constituent demands, with mixed results. Fedaseyeu, Gilje and Strahan (2016), Lo (2013), and Poole (2007) show that they don't; Kousser, Lewis and Masket (2007) and Stratmann (2000) show that they do.

¹¹The mean absolute change equals 0.012, and the 90th percentile is equal to 0.028.

evolution of legislators over time. Unavailability of raw roll-call data pertaining to state legislatures precludes me from analyzing whether a similar pattern holds for these legislatures. If Congress is any guide, however, the lack of temporal variation of the Shor-McCarty legislator scores does not invalidate the research strategy pursued in this paper.¹²

Empirical Model: Panel Data with Legislative Chamber and Year Fixed Effects

The first econometric specification that I estimate to investigate the effects of state economic activity on polarization in state legislatures is given by

$$(2.1) \quad Polarization_{sct} = \alpha_{sc} + \eta_t + \beta \log(GSPpc)_{s,t-1} + \gamma X_{sc,t-1} + \epsilon_{sct}$$

where s stands for state, c for legislative chamber, and t for year, and X denotes the vector of controls. The unit of observation is a chamber-year. α_{sc} and η_t are chamber and year fixed effects, respectively, which are included in all the specifications throughout the paper. *Polarization* in a legislative chamber, as noted before, is calculated as the difference between the median ideological score of Republicans minus the median score of Democrats. $\log(GSPpc)$ is the log of real GSP per capita, deflated by the regional CPI (Consumer Price Index). Throughout the paper, standard errors are clustered at the state level to account for arbitrary within-state heteroskedasticity and serial correlation.¹³

¹²In fact, if economic downturns cause incumbent legislators' voting behavior to become more extreme over time, the estimates obtained in this paper will be biased towards zero.

¹³The GSP per capita variable without logs is right skewed, which justifies the preference for the specification using the logged variable. An additional important reason to favor the specification in logs is that it imposes homogeneity of effects—and sets the proper counterfactual—not in terms of an absolute increase in income per capita of X dollars, but in terms of income per capita growth.

Due to the panel structure of the data, it is possible to control for time-invariant, chamber-level omitted variables that affect polarization. It is also possible to control for yearly shocks that affect polarization in all states and legislatures. α_{sc} accounts for latent, fixed chamber-specific unobserved factors that influence polarization, and η_t flexibly controls for yearly common shocks—such as the strength of the national economy, or other unobserved shocks—that affect polarization in every state legislature. The relevant variation in income per capita that is exploited to estimate β is given by the residuals of a regression of $\log(GSPpc)$ on chamber and year fixed effects. Similarly, the variation in polarization exploited in the econometric analysis is given by the residuals of a regression of *Polarization* on chamber and year fixed effects.¹⁴

The vector X of control variables, included in some specifications, contains party controls and population. To the extent that these variables affect or are correlated with polarization in a time-varying manner, their inclusion enables the assessment of the sensitivity of the main estimates. In addition, these variables improve the precision of the estimation by removing noise. The party control variables include indicator variables for whether the governor of the state is a Democrat (*Democrat Governor*), whether the legislative chamber is controlled by Democrats (*Democrat Control*), and whether the state house and senate are controlled by different parties (*Split Legislature*), the absolute value of the difference between the proportion of seats held by Democrats versus Republicans (*Seat Share Difference*), and the degree of legislative professionalism of the chamber (*Professionalism Score*). Changes in the control of the state legislature, in the electoral competitiveness, or in the professionalism of a chamber can potentially correlate with polarization. This might occur if these changes

¹⁴This can be formalized using the Frisch-Waugh-Lovell theorem, and is pointed out in Mummolo and Peterson (2017).

differentially affect the marginal returns of running for office of moderates and extremists, or if they induce a shift in voter sentiment. Finally, *Population* controls for population dynamics. Inflows and outflows of migrants can alter the composition of voters or generate tensions among voters.

Regarding the timing of effects, both $\log(GSPpc)$ and the set of controls are lagged one period in equation 2.1. This choice is motivated by the fact that elections for state legislatures occur in November but new legislators do not enter their respective legislative bodies until January the following year. Thus, it is reasonable to expect polarization in year t to be affected by economic performance in year $t-1$, because voters cast their ballots around the end of year $t-1$. The same principle applies to party controls.

Panel Data Results

Table 2.1 reports the results of estimating equation 2.1.¹⁵ Column 1 shows that GSP per capita has a negative and highly significant effect on polarization. The coefficient of -0.465 is significantly different from zero at the 99% confidence level. A downgrade of 1 standard deviation (SD) in relative state economic performance is associated with a relative increase in polarization of 0.19 SD.¹⁶ Column 2 includes party controls, measured at the time of the election, into the regression. The main result obtained in Column 1 is robust to the inclusion of these party controls, and the estimated magnitude actually increases in absolute value, from

¹⁵The number of observations, 1746, is slightly lower than 1862 (98 chambers multiplied by 19 years) due to some missing observations for the polarization variable. The reason is that some states did not make roll-call data available for certain years. Therefore, the panel is unbalanced.

¹⁶As pointed out by Mummolo and Peterson (2017), the proper counterfactual shift in $\log(GSPpc)$ is the SD of the residuals of a regression of $\log(GSPpc)$ on chamber and year effects, as these residuals represent the variation that is used to estimate β . The same argument applies to the dependent variable, *Polarization*. Correspondingly, the SD of the residualized $\log(GSPpc)$ and the residualized *Polarization* are equal to 0.0548 and 0.1347, respectively.

-0.465 to -0.498. Finally, column 3 adds population as a control. Although the point estimate for GSP per capita decreases slightly to -0.383, it remains statistically significant at the 99 % confidence level.

Table 2.1. Least Squares

	Polarization		
	(1)	(2)	(3)
log(GSP pc), t-1	-0.465*** (0.169)	-0.498*** (0.165)	-0.383*** (0.126)
Chamber FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Party Controls	No	Yes	Yes
Population Control	No	No	Yes
Outcome Mean	1.42	1.42	1.42
R-squared	0.927	0.929	0.935
Observations	1746	1742	1742

Note: The table reports the results of estimating equation 2.1. The unit of observation is a chamber-year. Standard errors in parentheses are clustered at the state level. Significance levels: * 10%, ** 5%, *** 1%

The effects estimated by the OLS specification with chamber and year fixed effects are sizable and robust. The results strongly indicate that legislatures of states that were harder hit by adverse economic shocks, or did not grow as fast as other states, experienced greater increases in political polarization. Figure 2.1 illustrates the impacts of such polarization effects over the entire period of analysis. The figure plots the growth in polarization experienced by legislative chambers in the 1996-2014 period against the growth in real GSP per capita experienced by the respective states in the 1995-2013 period. A strong inverse relationship is evident between the growth of the state economy and the growth in polarization. All else equal, states that fared better in economic terms experienced smaller increases in

political polarization. A simple regression of polarization growth on GSP per capita growth yields a point estimate of -0.0053, which is statistically significant at the 99% confidence level.¹⁷ States that grew 16.72 percent (1 SD) more during the entire period of analysis experienced 0.084 units (0.5 SD) smaller growth in polarization in their state legislatures.¹⁸ This result implies that the health of a state's economy during the 1996-2014 period constituted a first-order determinant of polarization growth in the state's legislatures.

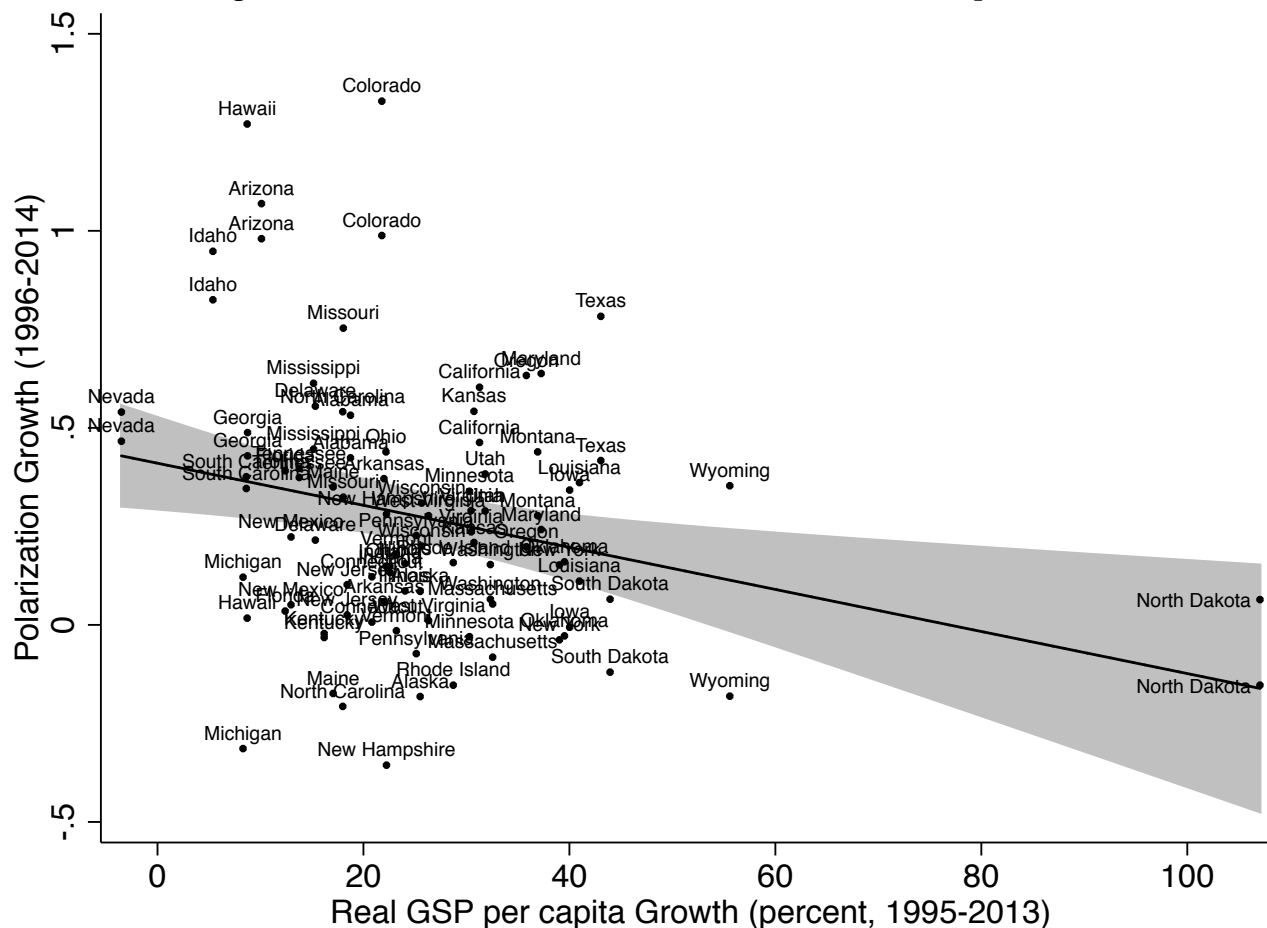
Possible Threats to Identification

The combined evidence provided by Table 2.1 and Figure 2.1 supports the hypothesis that economic downturns created upward pressure on political polarization in state legislatures. However, there are several reasons why the least squares estimates shown in Table 2.1 might not reflect a true causal effect. A potential source of bias is reverse causality: the level of political polarization in a state at a certain point in time might impact the subsequent evolution of GSP per capita, which in turn affects polarization. This type of cyclical dynamic is plausible if polarization leads to legislative gridlock. Already polarized state legislatures might become especially unfit to pass legislation aimed at weathering adverse economic circumstances or responding to new challenges posed by the digital economy, thus hindering the economy in the short term. The resulting economic downturn could potentially alienate voters and make them more likely to elect extreme legislators in the next election. In such a

¹⁷Although North Dakota is an outlier for the vigorous economic growth that the state experienced during the period of analysis, the inverse relationship estimated between growth and polarization does not hinge at all on the two North Dakota chamber observations. A regression that excludes North Dakota data yields a point estimate of -0.0068 (i.e. a stronger correlation), which is significantly different from zero at the 98.5 percent confidence level.

¹⁸The within-chamber SD of polarization is equal to 0.17.

Figure 2.1. Scatter Plot of Growth in Polarization and GSP pc



Note: This figure depicts a scatter plot where each observation pertains to a state legislative chamber. The horizontal axis plots the real GSP per capita percentage growth of the state during the 1995-2013 period. The vertical axis plots the absolute growth in the Shor-McCarty polarization score experienced by the legislative chamber in the 1996-2014 period. The figure contains two observations per state, and the marker labels contain the name of the state, but not of the specific legislative chamber. A linear regression of polarization growth on GSP pc growth yields a point estimate of -0.0053 and a standard error of 0.0019. The estimate is statistically significant at the 1% level.

scenario, the least squares estimation method would be ill-suited to precisely measure the causal effects of the economy on polarization.

In addition to the problem of potential reverse causality, least squares estimates might suffer from omitted variable bias. This would be the case if state economic activity was correlated time-varying factors that were unobserved, not included in the regression, and affected polarization. Finally, classical measurement error in the GSP per capita variable could potentially attenuate the estimated least squares coefficient towards zero.

A two-stage least squares strategy, in which GSP per capita is instrumented with time-series variation in oil prices interacted with states' average shares of oil production over GSP, offers several advantages. It overcomes the mentioned drawbacks of least squares, estimates the causal effects of state economic conditions on polarization (subject to the validity of the exclusion restriction), and sheds light on the possible direction of the bias pertaining to the least squares procedure. In the next section of the paper, I provide details about the construction of the instrument. The identifying assumptions under which the estimation procedure recovers the causal effects of economic activity on polarization are also carefully discussed.

2.4. Estimating Causal Effects

I define the oil instrument in the following way

$$(2.2) \quad Oil_{st} = \log(Oil\ Price)_t \cdot \theta_s$$

where θ_s is the 10-year average share of oil production over GSP in state s . It equals the average share from 1994 to 2004 for observations pertaining to this time period, and the average share from 2005 to 2014 for $t = 2005, \dots, 2014$. The reason oil prices are weighted by a

parameter that captures the intensity of oil production in a state is that the impact of international oil-price changes on state economies is larger for states whose economies are largely dependent on oil production. Likewise, calculating the oil intensity parameter by averaging over several years of oil production shares overcomes possible endogeneity concerns. Such concerns might otherwise arise if oil production in a state significantly responded to past international oil prices and this variation was systematically correlated with the political environment and past economic performance.

Acemoglu, Finkelstein, and Notowidigdo (2013) use a similar strategy to isolate exogenous variation in local area income in their study of the income elasticity of health spending. However, they use oil reserves to proxy for the importance of oil in an area. Brückner, Chong, and Gradstein (2012) also use a similar identification strategy to estimate the permanent income elasticity of government expenditures across countries.

Instrument Data

Data on crude oil production per state were obtained from the Energy Information Administration (EIA), whereas data on international oil prices were obtained from UNCTAD Commodity Statistics.¹⁹ The crude oil price that is used in the main specification is the average of UK Brent, Dubai, and Texas.²⁰

¹⁹The WTI Texas oil price index data were obtained from the Saint Louis Fed.

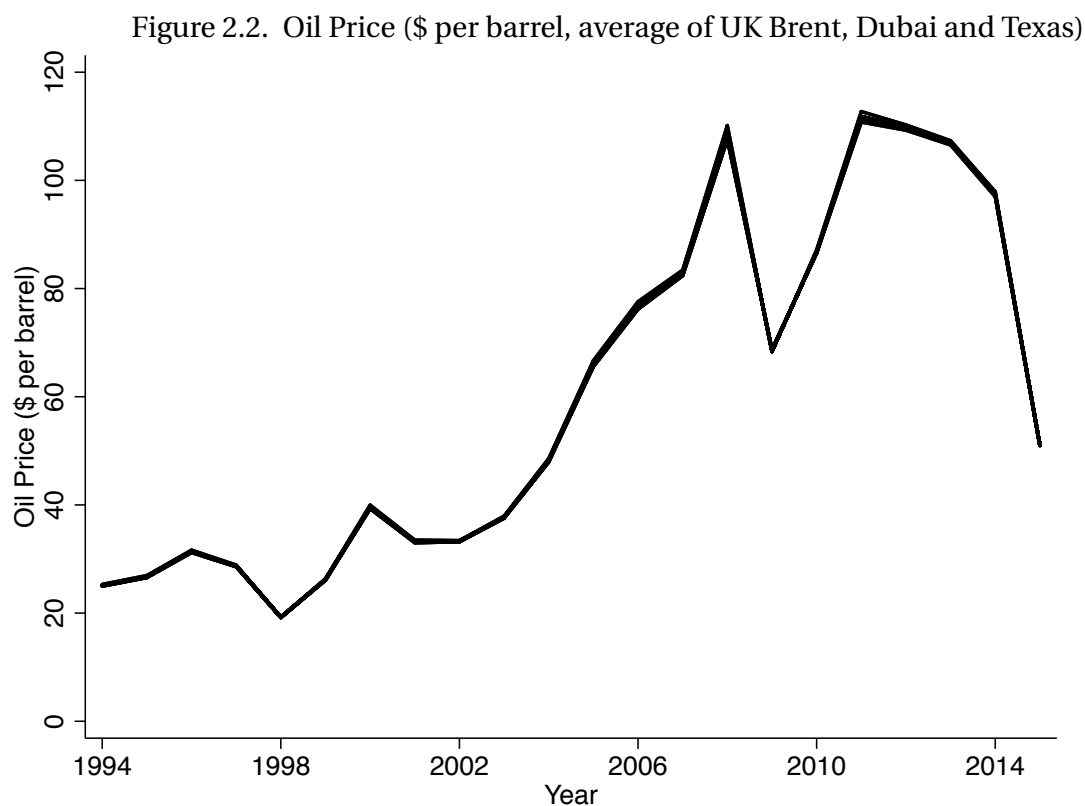
²⁰The three price indices are strongly correlated; the pairwise correlation coefficients are above 0.95 for the period of analysis. To the extent that US crude oil production is priced according to the Texas WTI index, using this price would result in a better first-stage fit. Nonetheless, employing the average of the three indices is more appropriate if the differential evolution of the Texas WTI oil price index is influenced by local production and demand conditions.

Figure 2.2 plots the time series of average annual oil prices from 1994 to 2014, and Figure 2.3 plots the quartiles of average oil production shares of GSP across US states. The estimation exploits the differential exposure among states to changes in oil prices. The economy of Wyoming, which depends heavily on oil production (oil accounted, on average, for 12.5% of the yearly GSP), will naturally be more affected by changes in the oil price than the economy of California would be; California is less dependent on oil (0.81% of yearly GSP). Instrumenting for GSP per capita using time-series variation in oil prices, interacted with an oil intensity parameter, isolates plausibly exogenous variation in GSP per capita across states and periods. This variation can then be exploited to estimate the causal effects of GSP per capita on polarization.

Empirical Strategy

A valid instrument must satisfy two conditions. The first is the relevance condition: the instrument needs to be a strong predictor of the endogenous variable. This condition is met in the present context, as will become clear from the results section. The second condition is the exclusion restriction. This identifying assumption requires that international oil prices interacted with state oil intensity have no effect on polarization, beyond the effect that they exert through GSP per capita. Although the exclusion restriction in the instrumental variables approach is inherently untestable, later in this section I provide indirect evidence to support the validity of the assumption. In addition, I establish that the main results are robust to sizable violations of the exclusion restriction.

The oil instrument has several advantages in the present context. First, international oil prices are exogenous to any single individual producer or state, as no single state has



Note: This figure plots the time series of average annual oil prices from 1994 to 2014. The average is taken over the Brent, Dubai and WTI Texas oil prices.

a sufficiently large share of the world market to affect the price determination by strategically modulating its oil production. Second, changes in oil prices are unpredictable; oil price time-series follow a random walk (Acemoglu et al., 2013). Third, the dependence of a state's economy on oil is strongly determined by the availability of reserves.²¹ Thus, both

²¹In the later period of my sample, oil production by hydraulic fracturing (fracking) constituted a large portion of all US oil production (43% in 2017). Concerns about potential environmental and health consequences of this geological procedure have generated political conflict between organizations opposed to fracking—who want it banned—and those in favor of fracking—who regard it as an opportunity to create jobs and revitalize local economies. Political conflicts notwithstanding, almost no state had banned drilling during the period of analysis for this paper. New York was the exception; this state banned drilling in 2012. Thus, availability of reserves constituted a crucial determinant of the dependence of a state's economy on oil.

$$(2.3) \quad \log(GSPpc)_{s,t} = \alpha_{sc} + \eta_t + \beta Oil_{s,t-1} + \gamma X_{sc,t-1} + \epsilon_{sct}$$

The oil instrument is lagged by a year, as this time-lag maximizes the explanatory power that the oil instrument has on GSP per capita. As in equation 2.1, η_t and α_{sc} denote year and chamber fixed effects, and the X vector denotes the set of controls.

The second stage regression is given by

$$(2.4) \quad Polarization_{sct} = \alpha_{sc} + \eta_t + \beta \widehat{\log(GSPpc)}_{s,t-1} + \gamma X_{sc,t-1} + \epsilon_{sct}$$

where $\widehat{\log(GSPpc)}_{s,t-1}$ denotes the fitted values estimated in equation 2.3.

The year fixed effects—in both the first and second stage—ensure that any common effects of oil price changes on state GSP per capita and political polarization that are orthogonal to the importance of oil in state economies (such as effects over oil consumption or costs of production) are controlled for. As for chamber fixed effects, they take care of any systematic time-invariant correlation between the importance of oil in a state and political polarization in any of the state's legislative chambers.

Instrumental Variables Results

As a prelude to the instrumental variables estimates, Table 2.2 reports the reduced form effects of the oil instrument on polarization. Column 1 controls for chamber and year fixed effects, column 2 additionally includes partisan control variables, and column 3 adds population. The timing of effects is such that oil at t-2 affects GSP per capita at t-1, which in turn

affects polarization at year t . Given this timing, the reduced form regression includes the 2-year lag of the oil instrument as the main regressor. Both the unconditional and conditional estimates of the effects of the oil instrument upon polarization are negative and strongly significant.

Table 2.2. Reduced Form

	Polarization		
	(1)	(2)	(3)
Oil, $t-2$	-0.159*** (0.051)	-0.161*** (0.051)	-0.143** (0.054)
Chamber FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Party Controls	No	Yes	Yes
Population Control	No	No	Yes
Outcome Mean	1.42	1.42	1.42
R-squared	0.926	0.927	0.934
Observations	1746	1742	1742

Note: The table reports the results of estimating the effects of the oil instrument on polarization by least squares. The unit of observation is a chamber-year. Standard errors in parentheses are clustered at the state level. Significance levels: * 10%, ** 5%, *** 1%

Although I argue below that the effects of oil on polarization occur via state economic activity, the reduced form results are, by themselves, novel and interesting. Whereas previous literature has documented how oil prices affect various political outcomes (Wolfers, 2002; Carreri and Dube, 2016; Fedaseyeu, Gilje and Strahan, 2016), this paper is the first to link oil prices and political polarization.

In Table 2.3 I report the first-stage effects of the oil instrument on GSP per capita. Columns 1, 2 and 3 all show that oil has a strong positive effect on GSP per capita, both unconditionally and conditionally,²² thus satisfying the relevance condition of the instrumental variables method. Importantly, the oil instrument is a good predictor of state economic activity, as it is built with the aim of capturing the intensity of treatment; changes in oil prices differentially impact the subsequent economic performance of states according to their economic dependence to the production of oil.

Table 2.3. First Stage

	log(GSP pc)		
	(1)	(2)	(3)
Oil, t-1	0.286*** (0.052)	0.289*** (0.050)	0.282*** (0.054)
Chamber FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Party Controls	No	Yes	Yes
Population Control	No	No	Yes
R-squared	0.933	0.936	0.938
Observations	1858	1848	1848

Note: The table reports the results of estimating equation 2.3. The unit of observation is a chamber-year. Standard errors in parentheses are clustered at the state level. Significance levels: * 10%, ** 5%, *** 1%

Table 2.4 presents the two-stage least squares estimates of the impact of GSP per capita on political polarization. For all three specifications, the estimated coefficients for GSP per capita are larger in absolute value relative to the least squares specification in Table 2.1 (-0.55 vs -0.465 in column 1); they do not vary substantially across specifications; and they

²²The set of controls included in this first stage is the same as in the least squares specification and the instrumental variables specification. By construction, the set of controls pertaining to the first and second stages of the instrumental variables procedure coincide.

Table 2.4. Second Stage IV

	Polarization		
	(1)	(2)	(3)
log(GSP pc), t-1	-0.550*** (0.192)	-0.556*** (0.181)	-0.499*** (0.163)
Endogenous Instrument	log(GSP pc), t-1 Oil, t-2	log(GSP pc), t-1 Oil, t-2	log(GSP pc), t-1 Oil, t-2
Chamber FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Party Controls	No	Yes	Yes
Population Control	No	No	Yes
First Stage F-statistic	24.29	26.62	23.58
Outcome Mean	1.42	1.42	1.42
Observations	1746	1742	1742

Note: The table reports the results of estimating equation 2.4. The unit of observation is a chamber-year. Standard errors in parentheses are clustered at the state level. Significance levels: * 10%, ** 5%, *** 1%

are statistically significant at the 99 % confidence level. On average, a downgrade of 1 SD in relative state economic performance causes a 0.22 SD relative increase in polarization. The first-stage F-statistic values denote the strength of the instrument.

Both the least squares approach and the instrumental variables approach yield similar results. The larger estimates obtained by the IV approach can be attributed to endogeneity and measurement error problems affecting the least squares approach.²³

²³Heterogeneous effects of economic activity on polarization can also generate quantitative differences between the least squares and instrumental variables specifications. The IV approach estimates a weighted average of effects, where bigger weights are assigned to states whose economies more heavily depend on oil production.

Mediation Analysis

Table 2.5 attempts to gauge the extent to which the effects of the oil instrument on polarization are mediated by GSP per capita. To this end, column 1 replicates the reduced form estimate from Table 2.2, which showed a strong unconditional effect of the oil instrument on polarization. Column 2 shows a horse race between the oil instrument and the log of GSP per capita.²⁴ The estimated coefficient for $\log(GSPpc)$ is very similar to the one obtained in previous least squares and instrumental variables specifications, and is significantly different from zero. Importantly, the estimated coefficient for the oil variable, compared with column 1, greatly attenuates towards zero and loses significance.²⁵ This result suggests that, once we control for state economic activity, the direct effects of the oil instrument on polarization are small. In fact, a Sobel-Goodman mediation test indicates that 82.93% of the effects of the oil instrument on political polarization are mediated by state economic activity.²⁶ Finally, column 3 replicates the specification in column 2 but adds party controls into the regression. The results strengthen the finding in column 2 that, conditional on lagged GSP per capita, the oil instrument is insignificant. Thus, the Sobel-Goodman mediation test and the regressions in Table 2.5 suggest that the effects of the oil instrument on polarization are largely mediated through the GSP per capita channel.

²⁴The oil instrument is lagged two periods, whereas the log of GSP per capita is lagged one period. This choice of lags is consistent with the postulated timing of the effects—the oil instrument in year $t-2$ affects state economic activity in year $t-1$, which in turn affects polarization in year t .

²⁵Although the coefficient for oil in column 2 might seem close to the coefficient for GSP per capita, the coefficients are not directly comparable because the variables are defined in different units.

²⁶The Sobel-Goodman method estimates the proportion of the effect of Z on Y that is mediated by X , by leveraging regressions of Y on Z (the reduced form in the current setup), X on Z (the first stage) and Y on X and Z (the horse race).

Table 2.5. Horse Race

	Polarization		
	(1)	(2)	(3)
Oil, t-2	-0.159*** (0.051)	-0.032 (0.061)	-0.044 (0.049)
log(GSP pc), t-1		-0.440** (0.191)	-0.347** (0.138)
Chamber FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Controls	No	No	Yes
Outcome Mean	1.42	1.42	1.42
R-squared	0.926	0.927	0.935
Observations	1746	1746	1742

Note: Column 1 is identical to column 1 of Table 2.2, which reports the reduced form effects of the oil instrument on polarization. Columns 2 and 3 reports the results of a horse race between the log of GSP per capita and the oil instrument. The unit of observation is a chamber-year. Standard errors in parentheses are clustered at the state level. Significance levels: * 10%, ** 5%, *** 1%

Heterogeneous Effects

Table 2.6 analyzes the extent to which the polarization effect can be attributed to Democrats versus Republicans. To this end, I present results of regressions analogous to Table 2.4, where GSP per capita is instrumented by the oil variable, but replace the chamber polarization dependent variable by the median Democratic ideology score and the median Republican ideology score, respectively. Columns 1, 2 and 3 show that economic downturns drive the median ideology score of Democrats to the left, but the point estimates are insignificant. Columns 4, 5 and 6, by contrast, show that the median Republican score significantly shifts to the right when the state economy takes a turn for the worse. The estimated effects of GSP per capita over the Republican median score are sizable: a 1 SD downgrade in relative

state economic performance causes a 0.23 SD relative increase in the median Republican score of the chamber.²⁷ This result, which shows that most of the effects of state economic conditions over legislature polarization can be attributed to Republicans, is consistent with the evidence for the federal level—most of the rise in political polarization in Congress has been attributed to Republicans’ shift to the right.

Table 2.6. Second Stage IV By Party

	Median Democrat			Median Republican		
	(1)	(2)	(3)	(4)	(5)	(6)
log(GSP pc), t-1	0.150 (0.181)	0.156 (0.173)	0.119 (0.142)	-0.400*** (0.107)	-0.400*** (0.110)	-0.380*** (0.126)
Chamber FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Party Controls	No	Yes	Yes	No	Yes	Yes
Population Control	No	No	Yes	No	No	Yes
First Stage F-statistic	24.29	26.62	23.58	24.29	26.62	23.58
Outcome Mean	-0.74	-0.74	-0.74	0.68	0.68	0.68
Observations	1746	1742	1742	1746	1742	1742

Note: Columns 1-3 report the results of estimating equation 2.4, replacing polarization with the median Democratic score of the chamber as dependent variable. Columns 4-6 replace polarization with the median Republican score. The oil variable is used as an instrument for the log of GSP per capita. The unit of observation is a chamber-year. Standard errors in parentheses are clustered at the state level. Significance levels: * 10%, ** 5%, *** 1%

2.5. Exploring Possible Channels

In Table 2.7 I analyze the effects of state economic activity on party control of the chamber. In Columns 1 and 2 I regress an indicator variable of change in party control of the

²⁷The SD of the residualized median Republican score is 0.0957, and is 0.091 for the residualized $\log(GSPpc)$.

chamber on the instrumented log of GSP per capita. I also flexibly control for the absolute seat share difference of the chamber. The results show that an ailing state economy significantly harms the incumbent party's chances of retaining control of the chamber. In Column 3 I analyze whether state economic activity impacts the overall ideological leaning of the chamber, which is proxied by its median ideology score. The results do not indicate that a poor state economy tilts the overall ideological leaning of legislatures in any particular direction. Thus, the anti-incumbent effects of economic downturns seem to affect both parties, and the resulting increased legislative turnover might be a factor that facilitates big swings in legislative polarization levels.

In Table 2.8 I analyze how state economic activity affects the average ideology scores of incoming and outgoing legislators. The goal is to establish a taxonomy of the kind of legislators who benefit or suffer when the state economy takes a turn for the worse. I separately report the instrumental variables results of estimating the effects of state economic activity on the average ideology score of incoming/outgoing Republican/Democrat legislators.²⁸ The analysis strongly suggests that the estimated polarization effects of economic activity operate via Republican entries. All else equal, state economic downturns give rise to a significantly more conservative pool of incoming Republican legislators.

Campaign finance is another dimension worth exploring. State legislators fund their campaigns with contributions from political action committees (PACs) and individual contributions. The former tend to support moderate candidates to a greater extent, whereas the latter disproportionately support relatively extreme candidates. Barber (2016) finds that

²⁸Each regression is weighted by the number of Republican/Democrat entries/exits pertaining to each chamber-year pair.

Table 2.7. Incumbency and Realignment Effects

	Change in Party Control		Median Chamber Score
	(1)	(2)	(3)
log(GSP pc), t-1	-0.349*** (0.095)	-0.453*** (0.081)	0.159 (0.316)
Abs(Seat Diff), t-1	-0.525*** (0.062)	-1.257*** (0.131)	
Abs(Seat Diff) ² , t-1		1.230*** (0.167)	
Endogenous	log(GSP pc), t-1	log(GSP pc), t-1	log(GSP pc), t-1
Instrument	Oil, t-2	Oil, t-2	Oil, t-2
Chamber FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
First Stage F-statistic	24.86	28.49	24.29
Outcome Mean	0.08	0.08	0.04
Observations	1862	1862	1746

Note: The table reports the results of estimating equation 2.4, replacing polarization with an indicator variable of change in party control of the chamber (columns 1 and 2) and the median ideological score of the chamber (column 3) as dependent variable. Additional controls include the absolute value of the seat share difference of the chamber (columns 1 and 2) and its square (column 2). The unit of observation is a chamber-year. All models include chamber and year fixed effects. Significance levels: * 10%, ** 5%, *** 1%

higher limits on the amounts corporate PACs can contribute to a single candidate favor moderate candidates. By contrast, higher limits on individual contributions lead to the selection of relatively extreme candidates. In Table 2.9 I show that incumbent state legislators obtain a higher share of their campaign funds from individuals—as opposed to PACs—during economic downturns.²⁹ As extreme candidates receive a bigger share of campaign funds from

²⁹Corporate PAC funds come from employees' voluntary contributions. Their higher elasticity of these contributions with respect to state economic conditions could be explained by PAC donors' lower attachment to politics and partisanship relative to individuals who directly contribute to political candidates. This hypothesis is consistent with the interpretation that campaign contributions constitute consumption goods (Ansolabehere, de Figueiredo and Snyder, 2003).

Table 2.8. Ideology Scores of Incoming and Outgoing Legislators

	Ideology Scores	
	(1) Dem	(2) Rep
<i>Panel A: Entries</i>		
log(GSP pc), t-1	-0.240 (0.384)	-0.398*** (0.118)
Endogenous	log(GSP pc), t-1	log(GSP pc), t-1
Instrument	Oil, t-2	Oil, t-2
Chamber FE	Yes	Yes
Year FE	Yes	Yes
First Stage F-statistic	17.97	28.74
Outcome Mean	-0.76	0.74
Observations	1060	1067
<i>Panel B: Exits</i>		
log(GSP pc), t	0.060 (0.202)	-0.276*** (0.076)
Endogenous	log(GSP pc), t	log(GSP pc), t
Instrument	Oil, t-1	Oil, t-1
Chamber FE	Yes	Yes
Year FE	Yes	Yes
First Stage F-statistic	34.13	39.68
Outcome Mean	-0.70	0.66
Observations	1078	1066

Note: The table reports the results of estimating equations 2.4, replacing polarization with the average ideological score of incoming (Panel A) and outgoing (Panel B) legislators as dependent variable. Columns 1 and 2 restrict the sample to Democratic and Republican legislators, respectively. The unit of observation is a chamber-year. Observations are weighted by the number of Dem/Rep entries/exits pertaining to the chamber-year. Standard errors in parentheses are clustered at the state level. Significance levels: * 10%, ** 5%, *** 1%

individuals, economic downturns improve their comparative ability to raise campaign funds relative to moderate rivals.³⁰ This factor can partly explain why entrant legislators are relatively extreme during economic downturns.

³⁰This effect could operate both in party primaries and general elections.

Table 2.9. Share of Campaign Contributions from Individuals

	% Individual Contributions		
	(1)	(2)	(3)
log(GSP pc), t	-0.211*** (0.059)	-0.204*** (0.052)	-0.190*** (0.059)
Chamber FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Party Controls	No	Yes	Yes
Population Control	No	No	Yes
Outcome Mean	0.42	0.42	0.42
R-squared	0.790	0.797	0.799
Observations	384	383	383

Note: The table reports the results of estimating equation 2.1, replacing polarization with the mean share of campaign contributions that incumbent state legislators receive from individuals. The unit of observation is a chamber-year. Standard errors in parentheses are clustered at the state level. Significance levels: * 10%, ** 5%, *** 1%

Next I analyze the effects of economic activity on turnout rates in state legislative elections.³¹ Table 2.10 shows that economic downturns increase turnout rates in state house elections. These results suggest that political mobilization increases during downturns. Marginal voters’—individuals who vote when the economy is struggling but stay at home otherwise—electoral behavior could also help to explain some of the observed polarization increases if they support relatively extreme policy positions during downturns.

³¹Previous researchers have reported disparate findings in this area. Basinger, Cann and Ensley (2012) contend that economic downturns discourage voter turnout. By contrast, Brunner et al. (2011), Charles and Stephens (2013) and Burden and Wichowsky (2014) argue that a poor economy increases turnout rates, possibly because constituents displaced from the labor market due to adverse economic shocks may become politically more informed and participate more in elections (Charles and Stephens, 2013).

Table 2.10. Turnout

	IV		LS	
	(1)	(2)	(3)	(4)
log(GSP pc), t	-18.876*** (6.196)	-22.004*** (6.685)	-20.089*** (4.287)	-20.511*** (4.026)
Chamber FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Population Control	No	Yes	No	Yes
Endogenous	log(GSP pc), t	log(GSP pc), t		
Instrument	Oil, t-1	Oil, t-1		
First Stage F-statistic	6.10	6.27		
Outcome Mean	37.82	37.82	37.82	37.82
R-squared	—	—	.911	.912
Observations	588	588	588	588

Note: The table reports the results of estimating equations 2.4 (columns 1 and 2) and 2.1 (columns 3 and 4), replacing polarization with the state legislative turnout rate as dependent variable. The turnout rate variable is defined from 0 to 100. The turnout data spans the 1996-2012 period, and pertains to 36 states. The unit of observation is a chamber-year. Standard errors in parentheses are clustered at the state level. Significance levels: * 10%, ** 5%, *** 1%

I also investigate whether the variation in state economic activity induced by oil I exploit to identify the effects of the economy on polarization also generates variation in state income inequality. If so, the estimation of the effects of mean income on polarization could lead to spurious results. In Table 2.11 I regress the state Gini index on the lagged oil instrument. The oil variable is statistically indistinguishable from zero, which implies that a possible confound can be ruled out. That is, the effects estimated in the instrumental variables specification are not generated by the combination of a causal link from income inequality to polarization and a correlation between changes in inequality and changes in GSP per capita.³² In fact, as argued in the introduction, it is plausible that the increasing income inequality in the U.S.—which is a driver of polarization as shown by Voorheis, Shor, and McCarty (2015)—can exacerbate the effects that an ailing economy has on voters' support for relatively extreme politicians.

³²Likewise, adding the state Gini variable as a control variable into the least squares and instrumental variables specifications of Table 2.1 and Table 2.4 has no effect on the main results.

Table 2.11. State Inequality

	Gini		
	(1) LS	(2) LS	(3) LS
Oil, t-1	-1.021 (0.771)	-1.235 (0.812)	-1.176 (0.831)
Chamber FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Party Controls	No	Yes	Yes
Population Control	No	No	Yes
Outcome Mean	59.21	59.21	59.21
R-squared	.762	.767	.767
Observations	1764	1754	1754

Note: Columns 1, 2 and 3 report the results of estimating equation 2.3, replacing the log of GSP per capita with the state Gini index. The state Gini index variable is defined from 0 to 100. The unit of observation is a chamber-year. Standard errors in parentheses are clustered at the state level. Significance levels: * 10%, ** 5%, *** 1%

To explore this last hypothesis—that structural transformations such as the increase in income inequality, the uneven growth of wages across the income distribution, globalization, or skill-biased technological change have exacerbated the effects of economic conditions on legislative polarization over time—in Table 2.12 I estimate equation 2.1, but allowing β to vary across the first and second decade of the dataset. The coefficient estimated for the second decade (2005-2014) is double in magnitude and significantly different from the coefficient for the first decade (1996-2004). Thus, these results offer some support for the hypothesis that the effects of the state economy on polarization have grown over time.

Table 2.12. Differential Effects By Decade

	Polarization		
	(1)	(2)	(3)
log(GSP pc), t-1	-0.263 (0.182)	-0.314* (0.182)	-0.171 (0.147)
log(GSP pc), t-1 x Decade	-0.274** (0.112)	-0.245** (0.117)	-0.277*** (0.102)
Chamber FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Outcome Mean	No	Yes	Yes
Party Controls	No	No	Yes
Population Control	1.42	1.42	1.42
R-squared	0.929	0.931	0.937
Observations	1746	1742	1742

Note: The unit of observation is a chamber-year. Standard errors in parentheses are clustered at the state level. Significance levels: * 10%, ** 5%, *** 1%

2.6. Concluding Remarks

In this paper I have examined the effects of state economic activity on political polarization. Analyzing state legislatures and leveraging within-state and within-chamber variation

in economic activity and political polarization, respectively, I find that economic downturns beget sizable increases in political polarization. To circumvent the inference problem posed by observational data—such as reverse causality from past polarization to current economic performance in the present setting—I leverage plausibly exogenous variation in state economic activity. Such variation is induced by time series variation in international oil prices interacted with a state economy's dependence on oil. The results strongly indicate that the effects of the economy on legislative polarization are causal. While economic downturns mildly shift the median ideology scores of Democrats in state legislatures towards the left, they strongly shift the Republican median score towards the right.

The paper adds to an active literature examining the impacts of economic conditions on polarization and far-right voting. This literature has not only focused on the US (see Autor et al., 2017), but also on Europe. Recent literature documents that after the 2008 global economic crisis voters' confidence in traditional political parties and political elites decreased considerably in Europe. In turn, support for populist or "outsider" parties increased. Guiso, Herrera, Morelli and Sonno (2017) argue that the rise of populist parties has its roots in voters' increased perception of economic insecurity and corresponding demand for short-term protection, as well as by their growing disillusion with existing political parties and institutions. In the same line, Becker, Fetzer and Novy (2017) show that economic insecurity was a major factor behind the success of the Brexit vote in the recent 2016 referendum in the UK. The Brexit vote was higher in areas with low education, low income, high unemployment and manufacturing tradition. The result that economic downturns improve the electoral appeal of relatively conservative candidates in American state legislatures is, thus, broadly

consistent with this literature. Voters affected by economic hardship and insecurity may become disillusioned by political elites and seek a remedy for their plight in relatively extreme candidates. These candidates capitalize on voters' resentment by promising to defend the interests of ordinary people against the establishment (Skocpol and Williamson, 2012).

CHAPTER 3

Quid Pro Quo? Corporate Returns to Campaign Contributions

(joint with Anthony Fowler and Jörg Spenkuch)

3.1. Introduction

In *Citizens United v. FEC*, U.S. Supreme Court Justice John Paul Stevens contends that "[c]orporations with a large war chest to deploy on electioneering may find democratically elected bodies becoming much more attuned to their interests" (2010, p. 65). Echoing this suspicion, legions of pundits and political reformers allege that corporate money corrupts politics. Corporate influence is, for instance, thought to alter election results in favor of pro-business candidates and away from the preferences of the public. In addition, corporate donations are often suspected to serve as a *quid pro quo*, affecting the policy choices of candidates, who, once elected, may feel indebted to their benefactors or hope to receive similar contributions in the future.

While the predominant view on the role of corporations in the electoral process is one of distress and disapproval, scholars remain starkly divided on the question of how much influence firms are actually able to exert through their donations. One influential school of thought notes that corporate campaign contributions are far too small to be reasonably expected to buy political favors (Tullock 1972; Milyo, Primo, and Groseclose 2000; Ansolabehere, de Figueiredo, and Snyder 2003). According to this view, firms do not derive meaningful benefits from meddling in elections or else they would do more of it—at least as much as is allowed under the law. Others however, have presented evidence that special interests give strategically (see, e.g., Barber 2016; Fourirnaies and Hall 2014, 2018; Powell and Grimmer 2016), even buying access (Kalla and Broockman 2016). In the words of Powell and Grimmer (2016, pp. 985), "business PAC contributions are consistent with a spot-market for short-term policy influence. [...] It appears that PACs and corporate interests use donations to solicit favors and to change policy." In this alternative view, firms give for nefarious reasons.

Yet, as the authors themselves acknowledge, the extant evidence creates only "an appearance of corruption—the key word being *appearance*" (p. 986). The extent to which corporations do, in fact, benefit from supporting political candidates remains unknown.

In this paper, we contribute to the literature on money in politics by asking whether and how much firms gain when a candidate they supported wins office. After all, if campaign contributions do indeed buy influence, then corporations ought to profit from their political investments. To measure and monetize the benefits a company derives from its favored candidate holding office, we utilize stock prices, which reflect the best available information about a firm's value, present and future. Thus, to the extent that corporations derive material advantages from contributing to political candidates, we expect these to manifest themselves in the value of the firm itself, i.e., its share price.

Our analysis begins by identifying donations from the political action committees (PACs) of publicly traded companies to candidates running for Congress, governor, or state legislator between 1980 and 2010. Corporations almost never support more than one candidate in any given race, allowing us to easily determine firms' preferred politicians. Causal inferences, however, are complicated by selection into "who gives to whom." Firms that are *ex ante* more successful are more likely to support winners, meaning that a naive comparison of firms that gave to winning and losing candidates would produce spurious results. To account for selection, we rely on two complementary empirical approaches. Our main analysis leverages the quasi-random outcomes of very close elections through a regression discontinuity (RD) design. Our second design uses *within-campaign* variation in market beliefs—as measured by betting odds—about the outcomes of U.S. Senate elections from 2004 to 2010. Identification

comes from high-frequency changes in the probability that a corporation's preferred candidate wins the race. Both research designs yield substantively identical results. An electoral victory of the supported candidate does *not* significantly benefit the typical firm.

Our results are not easily attributable to low statistical power. *Ex ante* power analyses suggest that we would have reliably detected any effect greater than about a quarter of a percent. *Ex post*, our confidence intervals allow us to statistically reject any purported effect greater than 0.3 percent of firm value. In addition, we find no sign that a genuine effect is masked by heterogeneity. In fact, we detect little variation across offices, time periods, the size of firms, the size of donations, or economic sectors. If companies benefit from donating to political candidates, then these effects must be small on average, and they are not detectable even in the settings where we would most expect them.

Since the corporations in our sample are extremely large relative to the typical donation, we cannot dismiss the possibility that campaign contributions are a worthwhile investment for the firm—albeit a modest one in absolute terms. We can rule out, however, that political donations are good investments for individual executives, and even the largest of our estimates are too small to support the jeremiads of pundits and reformers. In sum, our results suggest that, on average, contributing firms "give a little and get a little", as previously argued by Ansolabehere, de Figueiredo, and Snyder (2003; p. 126).

Some readers are likely to be surprised by the finding that companies do not derive significant benefits from a supported politician holding office. How can our results be so different from public perception and well-known journalistic accounts? One potential explanation is that our results are based on a broad sample of firms that supported many different politicians. If scholars and pundits focus on a few extreme cases in which corporations appear to

have benefited from political quid pro quos, they might conclude that the perverse consequences of money in politics are severe, even if they are negligible on average. By conducting a large-scale study that aggregates over thousands of different firms and elections, our findings speak to the typical effect of a supported candidate rising to office. To be clear, our contention is *not* that corporate money has no adverse effects; our point is that these effects appear to be small in most circumstances. Evidence on *both* averages and outliers is needed to sensibly discuss the consequences of corporate influence in American politics.

Before proceeding, we should clarify what we mean by *corporate campaign contributions*. In all federal elections and in most state elections, corporations are prohibited from donating directly to political candidates. However, firms can set up and fund the operation of PACs, which, in turn, raise money from individuals—often managers and shareholders of the firm—and give it to candidates. We use the term *corporate campaign contributions* as shorthand for contributions from corporate PACs to political candidates and their campaigns.¹ Since *Citizens United* and *SpeechNow v. FEC*, corporations can draw on their treasuries to give unlimited amounts to independent expenditure-only committees, which in turn advocate for or against particular candidates. Although these independent committees are officially prohibited from coordinating with campaigns, candidates are often alleged to solicit large sums for the groups that support them (Lee, Ferguson, and Earley 2014). On one hand, *Citizens United* makes the results of this paper all the more important because corporate electioneering expenditures are at an all-time high. On the other hand, one might worry

¹ Naturally, individual managers can also donate directly to political candidates. Previous scholars have argued that money channeled to candidates through corporate PACs is used by companies to seek access (e.g., Fournaies and Hall 2014; Barber 2016), while individual contributions may be due to various other motivations, even when carried out by CEOs (Bonica 2016, Barber, Canes-Wrone, and Thrower 2017). We, therefore, rely on the contribution patterns of corporate PACs rather than individual donations to identify firms' preferred candidates.

that recent changes in the political environment limit the generalizability of our results to the present era. Nonetheless, our limited data for the post-*Citizens United* period are consistent with our overall findings. Both before and after this landmark decision, an additional supported candidate winning office does not, on average, benefit the firm.

3.2. Related Literature

Political economists have extensively theorized about the role of corporate campaign contributions in the policy-making process. Many models conceptualize elections as a competitive market for private benefits, whereby firms or other special interests can give money to curry favor with politicians (e.g., Baron 1989; Denzau and Munger 1986; Grossman and Helpman 2001). Since elected officials have many levers through which to provide benefits to a firm, companies have an incentive to identify sympathetic politicians and increase their chances of being elected. A corporation might also use campaign contributions to ingratiate itself with a candidate who is likely to win anyway. Such a connection could be valuable because elected officials may propose grants and procurement contracts that can directly benefit the firm, change their roll-call vote on an important piece of legislation, alter the content of a bill through amendments or committee work, or pressure the bureaucracy to achieve favorable regulatory outcomes.

In light of these theoretical concerns, empiricists have actively looked for evidence on the perverse influence of political donations. The most common strategy involves regressing the roll-call votes of legislators on campaign contributions. One study, for instance, asks whether members of Congress who received contributions from dairy producers are more

likely to vote in favor of a dairy subsidy (Welch 1982). Even if the correlations between campaign contributions and subsequent legislator behavior were clear cut, such regressions do not necessarily uncover causal relationships. If corporations support politicians who already agree with them, then campaign contributions and roll-call votes will be correlated regardless of whether the former have any impact on the latter. Consistent with this account, the estimated effects of corporate contributions on roll-call votes tend to shrink dramatically with the inclusion of district or member fixed effects (Ansolabehere, de Figueiredo, and Snyder 2003; Wawro 2001). As a result, some dismiss the idea that campaign contributions affect legislators' votes. Others, however, dispute their conclusions. Stratmann's (2005) meta-analysis of the literature, for instance, rejects the null hypothesis of no effect. Similarly, Roscoe and Jenkins (2005) claim that about one in three roll-call votes show influence of campaign contributions.

Taking a contrarian stance, Tullock (1972), Milyo, Primo, and Groseclose (2000), and Ansolabehere, de Figueiredo, and Snyder (2003) note that there is not nearly as much corporate money in politics as one might expect if public policy were actually for sale. If dairy producers could really generate a billion dollars in price supports through a million dollars in campaign contributions, why would the dairy industry not give more? And why can members of Congress be bought so cheaply? Since the vast majority of corporate interests give less than the statutory limit on campaign contributions, legal restrictions on political donations are unlikely to be the answer to this puzzle.

Other scholars have looked beyond roll-call votes (e.g., Hall and Wayman 1990). Recently, Gordon and Hafer (2005) argue that firms contribute in order to signal their willingness to fight regulators and thereby achieve more favorable treatment from government agencies.

Gordon, Hafer, and Landa (2007) find that executives whose compensation is more closely tied to corporate earnings are more likely to contribute to political candidates, and they suggest that contributions are best understood as purchases of good will. Kalla and Broockman (2016) report that activists are more likely to secure a meeting with a senior staffer of a member of Congress when they reveal themselves to be donors rather than merely constituents.

Even if political donations do buy access, the ultimate consequences of such meetings remain unknown. If for-profit companies make campaign contributions to curry favor, and if these favors actually affect policy, then their value ought to be reflected in the financial worth of the firm. Thus, for publicly traded companies, we can utilize stock prices to gauge the pecuniary value of *quid pro quos*.

Outside the U.S., politically connected firms trade at a premium relative to unconnected ones (Faccio 2006; Fisman 2001). Within the U.S., Do, Lee, and Nguyen (2015) report that corporations profit from having a director who went to school with a governor, and Goldman, Rocholl, and So (2009) find benefits from appointing former politicians and high-ranking bureaucrats to the company's board. Similarly, Brown and Huang (2017) argue that corporate executives' meetings with key White House staff lead to positive abnormal returns. While political connections of this kind appear to be valuable—especially connections to the executive—it is *a priori* unclear whether campaign contributions can be used to forge similar bonds.

There also exists a sizable literature on the impact of presidential elections on different sectors of the economy as well as the stock market as a whole (e.g., Snowberg, Wolfers, and Zitzewitz 2007a; Wolfers and Zitzewitz 2016). Knight (2007), for instance, defines a group of politically sensitive firms and uses prediction market data to show that presidential policy

platforms are capitalized into equity prices. Our second empirical approach mirrors this research design, but we find no evidence that the election of senators affects the firms that supported them. One likely explanation for the discrepancy is that the president wields much more influence over policy than individual legislators.

The studies most closely related to our own are Boas, Hidalgo, and Richardson (2014), and Akey (2015). Using a regression discontinuity design, Boas et al. show that public-works firms in Brazil receive more government contracts when their favored candidates rise to office. Akey (2015) analyzes thirteen close U.S. congressional races and reports that the stock price of a firm increases by about three percent when a candidate to which it contributed narrowly wins. However, we use the author's own data to show that, given the small number of elections, his findings are highly sensitive specification. In particular, we show that the results reported in Akey (2015) are not robust—in terms of sign and magnitude—to using either reasonable alternative bandwidths or different-order polynomials in the running variable. In fact, eleven out of thirty-two combinations of bandwidth and polynomial yield negative point estimates. As a result, Akey's estimates fail one of the standard robustness checks recommended by Lee and Lemieux (2010).

In sum, whether corporations benefit from making campaign contributions in a mature democracy like the U.S. remains unknown. Our subsequent analyses provide systematic evidence on this question by drawing on data from thousands of closely contested elections for governor, Congress, and state legislatures.

3.3. Why Give?

There are at least six different reasons a corporation might contribute money to political campaigns, with each one corresponding to varying degrees of concern about the health of democracy.

First, corporations might give with the goal of altering election outcomes. If one candidate is more likely to support policies that would benefit a firm, that company has an incentive to contribute in order to help the aligned candidate win. Many studies show that campaign spending can influence vote shares, although the substantive size of these effects is typically small (e.g., Green and Gerber 2015). Experimental evidence from get-out-the-vote studies suggests that each additional vote costs between 100 and 200 dollars, so influencing the outcome of most large elections would be expensive. For example, consider the 2014 gubernatorial election in Illinois, which, *ex ante*, was thought to be an extremely competitive race. Ultimately, Bruce Rauner (R) won by over 142,000 votes. Had special interests wanted to tip the scale in favor of Rauner's opponent, Pat Quinn (D), they would have had to spend at least \$14 million, even if there was no equilibrium response from contributors on the other side. Since most campaign contributions are three or four orders of magnitude smaller and go to candidates in less competitive races, one may question whether influencing election outcomes is firms' primary motivation.

Second, companies might give to influence the behavior of a candidate who would have been elected regardless of their help. Perhaps, once in office, elected officials offer quid pro quos to companies that supported them, or they might dole out favors in order to secure similar contributions in future campaigns. This mechanism appears to be the most prominent one in the academic literature as well as the one that especially troubles observers.

Third, corporations might give with the goal of obtaining information from elected officials. Even if contributions affect neither elections nor public policy, they might create a connection between a firm and an official that benefits the company in other ways—say, by enabling it to better anticipate regulatory changes. Companies may be willing to pay for such information, even if they cannot directly affect policy choices.

Fourth, corporations might give to sitting incumbents in order to change their behavior *before* the next election. This mechanism is consistent with the observation that firms tend to target aligned incumbents. However, the fact that most contributions come toward the end of an official's term—when it is too late to enact meaningful policy change before the next election—casts doubt on such an explanation. Similarly, few corporations make contributions in support of incumbents that are expected to lose.

Fifth, corporations might give to signal their type. In the model of Gordon and Hafer (2005), firms have an incentive to signal their willingness to fight regulation. In the model of Schnakenberg and Turner (2016), corporations use campaign contributions to signal compliance with the law and thereby reduce their chances of being audited.

Sixth and last, corporations might give for consumption purposes without receiving any tangible benefits in return. This is the preferred explanation of Ansolabehere, de Figueiredo, and Snyder (2003) for individual contributions, and it is plausible the same argument applies to corporations. That is, individual managers may personally know candidates running for office, enjoy attending glamorous fundraisers, or derive utility from supporting their preferred candidates. Agency problems within large firms might allow the same executives to (mis)use some of the company's resources for political purposes. Given that the typical donation is minuscule relative to the operating budgets of even medium-sized corporations,

and as evidenced by the mixed results in the academic literature, it would be difficult for shareholders to determine whether campaign contributions are, in fact, a good investment.

To the extent that any of these mechanisms operate, our subsequent research designs estimate the *combined* impact of the first three. Put differently, the benefits a company derives from a supported candidate winning the election include the value of having someone hold office who would intrinsically pursue policies that benefit the firm, the value of any political quid pro quos, as well as the value of insider information. If any of these mechanisms are substantively important, the successful election of a favored candidate should increase a firm's stock price.

The last three mechanisms are not reflected in our estimates. The fourth mechanism produces immediate benefits, which do not directly depend on electoral outcomes. In the signaling theories, the value of campaign contributions is independent of election results. In fact, to signal their type, companies might rationally support the candidate who is *ex post* worse. And again, in the consumption account, who wins is irrelevant to the firm because there are no meaningful direct effects.

Our impression is that most of the worry about corporate influence in politics is due to the first two possibilities, i.e., its effects on election results and political quid pro quos. Since our empirical tests capture most of what observers find concerning about corporate contributions, our results directly contribute to the current normative debates about money in politics.

3.4. Data and Research Design

Our regression discontinuity (RD) analysis relies on general election results for the U.S. Senate, U.S. House, governor, and state legislatures from 1980 to 2010.² Information on campaign contributions over this period comes from the Database on Ideology, Money in Politics, and Elections (Bonica 2013). We identify corporate PACs in these data, match them to their publicly traded parent companies, and aggregate all contributions (within a particular election cycle) from the same corporation to the candidates in these elections. Supplemental data on firm size, profitability, sector, etc. come from the CRSP/Compustat Merged Database. Our final data set includes 2,939 corporations and 164,525 firm-candidate-election pairings—our unit of observation—across 16 two-year election cycles with 18,907 individual races.³

In our data, the median (mean) firm spends about \$4,000 (\$24,000) per cycle and supports 4 (21) candidates. A small number of companies, however, spend as much \$1.5 million and contribute to nearly 1,000 candidates. Corporate contributions skew toward Republicans but not dramatically so. Corporate PACs support Democratic candidates in about 40 percent of the firm-elections, and Republicans in the remaining 60 percent. Important for our purposes, most but by no means all donations go to ex post winners. Corporate contributions skew toward Republicans but not dramatically so. Corporate PACs support Democratic candidates in about 40 percent of the firm-elections, and Republicans in the remaining 60

² These data were kindly provided by Jim Snyder and represent an extended version of the data set used in Ansolabehere and Snyder (2002).

³ As we explain in the Appendix, we restrict our sample to firm-election pairs in which more than 90% of the contributions from a particular company went to one candidate. As a consequence, we discard 1.98% of observations.

percent. Important for our purposes, most but by no means all donations go to ex post winners.

To construct our outcome variable, we use daily stock returns from the Center for Research in Security Prices. Following standard practice in the finance literature, we rely on the market model to compute cumulative abnormal returns (CARs) for each firm (e.g., Campbell, Lo, and MacKinlay 1996). Intuitively, CARs adjust stock returns for the performance of the entire stock market over the same period. More precisely, the daily abnormal return of firm i on day t is defined as $AR_{i,t} = r_{i,t} - \hat{\alpha}_i - \hat{\beta}_i m_t$, where $r_{i,t}$ denotes the realized return of the company's stock on day t , and m_t is the market return on the same day. $\hat{\beta}_i$ and $\hat{\alpha}_i$, respectively, denote the sensitivity of the firm's stock to overall market movements and its usual risk-return performance.⁴ Residualizing stock returns in this fashion yields more precise estimates by accounting for market-wide forces that are out of companies' control as well as the fact that some firms are more responsive to overall market conditions than others. With the definition of $AR_{i,t}$ in hand, the cumulative abnormal return of a firm over a period of multiple consecutive days is given by $CAR_i(t_1, t_2) = (\prod_{t=t_1}^{t_2} [1 + AR_{i,t}]) - 1$. For our main analyses, we calculate CARs from the day before the election to the day after, i.e., $CAR(-1, 1)$, but we also test for longer-term effects. Reassuringly, sensible alternative ways of constructing our outcome variable result in qualitatively equivalent conclusions.

⁴ Following Acemoglu et al. (2016), we use a window from 230 to 30 trading days before the election to estimate $\hat{\beta}_i$ and $\hat{\alpha}_i$. Note, the efficient markets hypothesis implies that $\hat{\alpha}_i$ should be equal to zero. In our sample, enforcing this theoretical restriction produces a slightly tighter distribution of CARs and subsequently more precise estimates. The substantive difference, however, is negligible, which is why we have opted for the approach that more closely mirrors standard practice in the finance literature.

Our goal is to estimate the effect of electing a supported candidate on a firm's value. To do so, we would like to approximate the following hypothetical experiment. Suppose campaigns proceed as usual, but election results are secretly determined by a coin flip.⁵ If who wins the election is random, then we can estimate the effect of a firm's supported candidate (rather than her opponent) rising to office by simply comparing the mean stock return of firms that support winners with that of companies that support losers. Since such an experiment is not feasible, we attempt to replicate it as closely as possible by focusing on close elections in a regression discontinuity (RD) framework.

We implement our RD design by estimating the following equation:

$$(3.1) \quad CAR(-1, 1)_{i,j,t} = \beta Victory_{i,j,t} + \gamma X_{i,j,t} + \lambda X_{i,j,t} Victory_{i,j,t} + \epsilon_{i,j,t},$$

where $Victory_{i,j,t}$ is an indicator variable for whether firm i 's preferred candidate won election j , and $X_{i,j,t}$ denotes the candidate's vote margin. The coefficient of interest is β . Because many corporations given to multiple candidates in the same electoral cycle, β should be interpreted as the return to the firm from one additional supported candidate rising to office.

We cluster standard errors by election cycle to allow for almost arbitrary forms of correlation in the residuals across firms and elections. For our main results, we restrict attention to elections in which the two-party vote share fell between .45 and .55, and we present robustness checks to demonstrate that our conclusions are insensitive to alternative bandwidths.⁶

⁵The coin flip must be secret because candidates and firms need to behave normally, believing that their campaign efforts influence election results.

⁶We settled on our RD specification after conducting *ex ante* power simulations. In each simulation, we randomly select a new date for each election year to serve as a placebo Election Day. Taking election outcomes, the pattern of corporate contributions, as well as actual stock returns around the placebo date as given, we add in a hypothetical treatment effect of a known magnitude and implement our empirical strategy. The simulations suggest that if the true effect is 0, then our empirical strategy will only reject the null about 5 percent of the

Our primary specification uses CARs from the day before the election to the day after in order to maximize statistical precision. A drawback of this strategy is that it relies on the market to quickly internalize the effect of election results on the value of firms—even before the newly elected officials take office. There is an enormous literature on the efficiency of financial markets, which typically concludes that markets are close to efficient but not perfectly so (see, e.g., Fama 1970; Shiller 1981). We are hesitant to take a strong stand on how fast the market can internalize the impact of election results. Instead, we present a range of specifications, allowing for longer lags until prices accurately reflect all effects of elections. In particular, we present results for as much as 100 trading days after Election Day, when the winners have taken office and begun enacting their policy agendas. Although increasing standard errors make quantitative comparisons speculative, if anything, these specifications suggest that firms benefit even less from their preferred candidate’s electoral triumph than implied by our main results.

Our RD design serves several purposes. First, it directly addresses selection into who gives to whom. If the results of very close races are, indeed, quasi-random, then firms supporting the candidate who, *ex post*, barely won are, in expectation, identical to companies donating to the candidate who barely lost. Second, our research design ensures that the market is surprised by the outcomes that drive our inferences. For elections that are easily predictable, stock prices will already reflect the value of any potential quid pro quo *before* Election Day. Returns realized on or after Election Day would, therefore, be uninformative about the benefits accruing to the firm. In very close elections, however, both candidates

time. If, however, the true effect is at least 0.25 percent, then we would reject the null about 93 percent of the time.

should have an *ex ante* realistic chance of winning, which implies that the market receives new information when the votes are tallied.⁷

Our RD results are, of course, local to close elections. That is, we estimate the monetary value of the benefits accruing to a firm when its supported candidate narrowly wins rather than narrowly loses. If we are interested in the mechanism whereby firms benefit from campaign contributions by changing the result of an election, then this is exactly the quantity of interest. After all, close races are the ones in which firms could potentially affect the outcome. If, however, we are more interested in the quid pro quo or informational mechanisms, then our approach has both advantages and drawbacks. On one hand, the most powerful politicians might be electorally safe, which could give them more leeway to hand out favors relative to their counterparts who live under electoral threat. On the other hand, politicians may be more willing to engage in quid pro quos precisely when their (re)election prospects are uncertain—simply because campaign contributions are more valuable when the race is expected to be close. Although we believe there are good theoretical reasons to be interested in close races, we freely acknowledge that our RD results may not extrapolate to contributions to, say, members of the leadership, committee chairs, or other especially influential incumbents, all of whom are unlikely to be involved in tight reelection battles.

⁷One potential concern with our approach is that market participants may not have the opportunity to incorporate the effects of political contributions into their valuations of firms if they are unaware of who gave to whom. Fortunately for our purposes, PAC contributions are public record, and due to FEC reporting requirements, any contributions made by mid-October of the election year are publicly disclosed before Election Day. Traders can, therefore, know which firms gave to which candidates, and they can use this information if they deem it valuable. Another potential concern is that close elections are subject to recounts and court cases, meaning that there is lingering uncertainty about the outcome even after Election Day, which, in turn, would attenuate our estimates. Although recounts and court cases do occur, it is extremely rare for the initial vote tally to be reversed. Hence, the amount of residual uncertainty is likely small. Furthermore, this concern becomes less relevant as we examine longer time horizons, or when we conduct a donut RD design that ignores the closest of elections, which are the most likely to be affected by recounts and legal skirmishes.

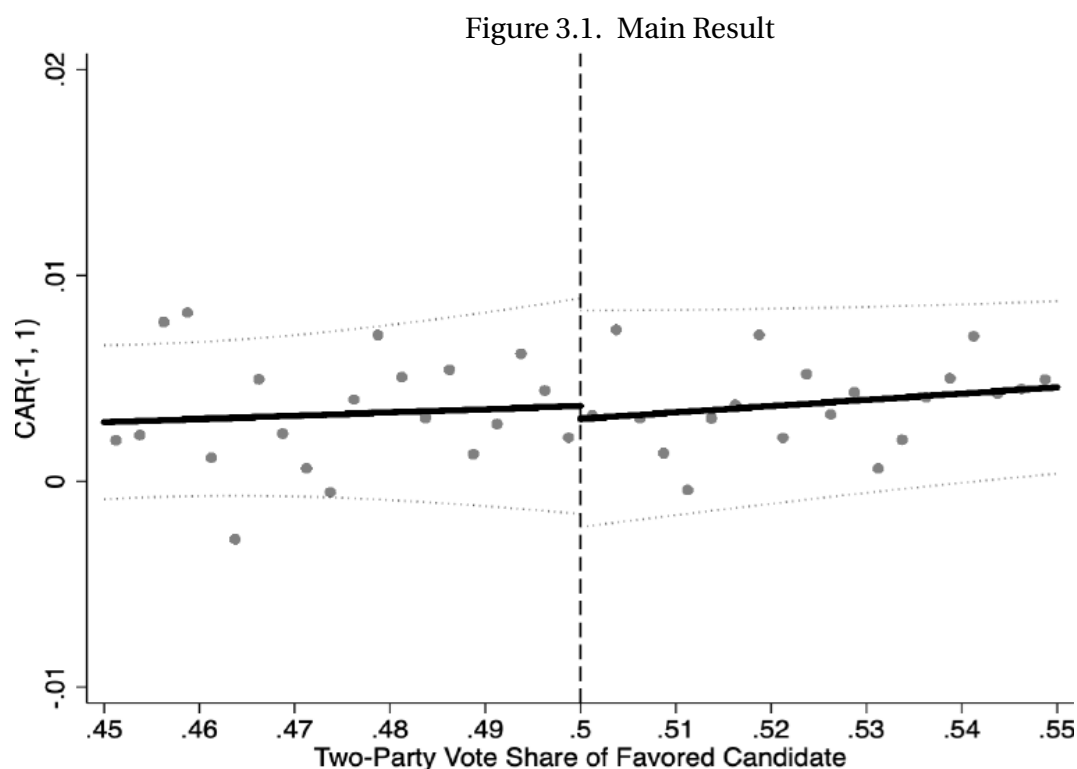
3.5. RD Results

We discuss and present several descriptive facts that help to motivate our empirical approach. First, elections that were *ex post* close were not *ex ante* predictable, meaning that the market received genuinely new information on Election Day. Second, almost no corporations give to both candidates in a given race. This means that we can easily identify a corporation's supported candidate in virtually every election in which they contribute. Third, although most firms give to winners, bigger firms especially give to winners. As a result, a naive, cross-sectional study might significantly overestimate the effects of corporate campaign contributions.

We present several tests assessing the validity of the continuity assumption necessary for our RD design. Even though corporations are overall more likely to contribute to winners—as mentioned above—there is little to no evidence to suggest that firms are systematically favoring bare winners over bare losers. Furthermore, we implement several placebo tests using pretreatment covariates as outcome variables. There appears to be no imbalance in cumulative abnormal returns leading up to Election Day, incumbency status of the supported candidate, or overall firm performance. There is, however, a difference in the number of other candidates that the firm supported, although this difference is not statistically significant when we correct for multiple-hypothesis testing.

Figure 3.1 presents our main result focusing on abnormal stock returns right around Election Day. For illustrative purposes, we average CARs across all observations within one-quarter-percentage-point-wide bins of the vote share, and we display fitted values based on the regression model in equation 3.1. The size of the estimated discontinuity is $-.0006$, which implies that a firm's value decreases by approximately 0.06 percent in response to a

supported politician winning office. Given that the 95% confidence interval associated with our point estimate ranges from $-.0044$ to $.0032$, we cannot reject the null hypothesis that the true effect equals zero. We can, however, statistically reject any purported impact greater than about 0.3 percent.



Note: Figure depicts fitted values and confidence intervals from estimating equation 3.1 by ordinary least squares. Confidence intervals account for clustering by election cycle. Each dot corresponds to the mean of the dependent variable over a 0.25 percentage-point-wide interval.

To interpret this finding, consider the following back-of-the-envelope calculations. The median firm in our data is valued at about \$1 billion. Taking the upper end of the 95% confidence interval at face value, this would mean that the median contributing firm is about three million dollars more valuable as a result of its favored candidate winning. While this

may seem like an insignificant amount relative to the size of the company, it comes at a low cost. The median firm spends only \$4,000 on campaign donations per cycle and supports three winners.⁸ Hence, there is a clear rationale for companies to contribute to political candidates from their own treasuries.

This does not mean, however, that a company's executives or shareholders have an economic incentive to give, and most PAC contributions are ultimately financed by individual managers. According to the best estimates in the literature, CEOs personally receive about \$1 for every \$100,000 of firm value that they create (see Murphy 1999). Thus, executives within the company would value the same benefits at only \$30.

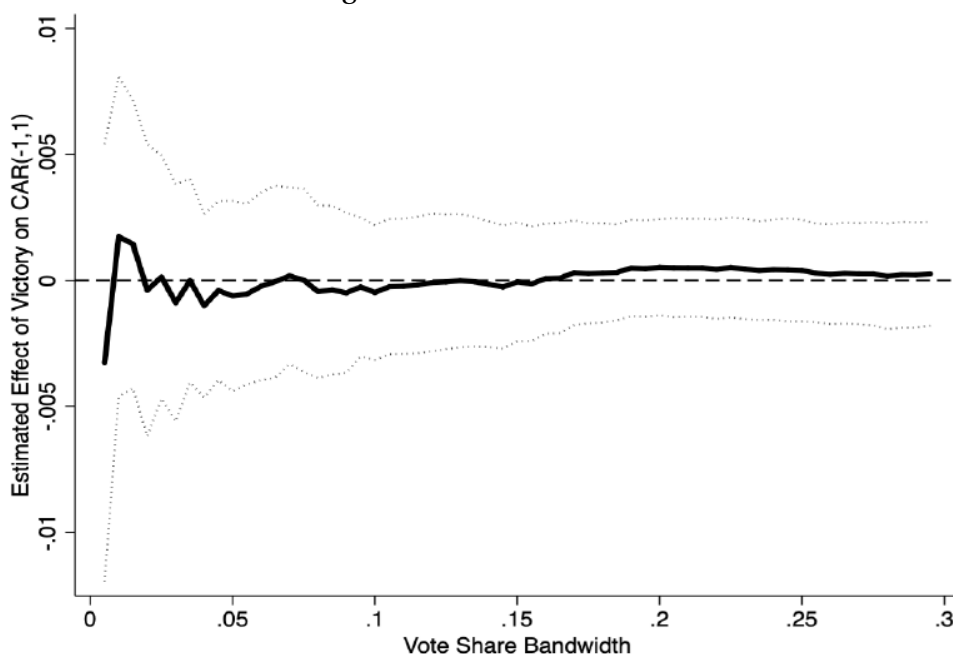
Based on these calculations, we cannot dismiss the possibility that small donations are a good investment for firms. Publicly traded companies are so large and the absolute size of most contributions is so small that it is difficult to envision a research design that could statistically rule out this possibility. We can, however, statistically reject the possibility that, on average, individual executives and managers benefit from their political donations through stock-price effects on their compensation. More importantly, the evidence does not suggest that, once elected, the average supported candidate engages in substantively meaningful *quid pro quos*.

As discussed, for our main RD analysis we rely on a bandwidth of .05. In Figure 3.2, we present estimates and confidence intervals for alternative choices ranging from .005 to .3. All of our RD estimates are substantively small—some negative—and for larger bandwidths

⁸Calculating the exact return is challenging because corporations typically spend a lot more money on electioneering beyond their direct contributions to candidates. For example, companies make independent expenditures, contribute to party committees, and sometimes try to mobilize and persuade their employees (see, e.g., Bombardini and Trebbi 2011). Many companies also donate to lawmakers' pet charities (Bertrand et al. 2018).

we can statistically reject even medium-sized effects. In Table 3.1, we present robustness checks for alternative regression models, including higher-order polynomials in the running variable, and race fixed effects. Reassuringly, we obtain qualitatively and quantitatively similar results.

Figure 3.2. Robustness across Bandwidths



Note: Figure plots estimates of β in equation 3.1 and the associated confidence intervals for different bandwidths around the electoral threshold. Confidence intervals account for clustering by election cycle.

Relying on our preferred specification in equation 3.1, Figure 3.3 presents estimates for longer time horizons. Even if financial markets are not perfectly efficient and cannot internalize the effects of close elections immediately, at some point we would expect the impact of election outcomes to be fully reflected in stock prices. We, therefore, present RD estimates for CARs ranging from the day after the election up to 100 trading days afterward—when the

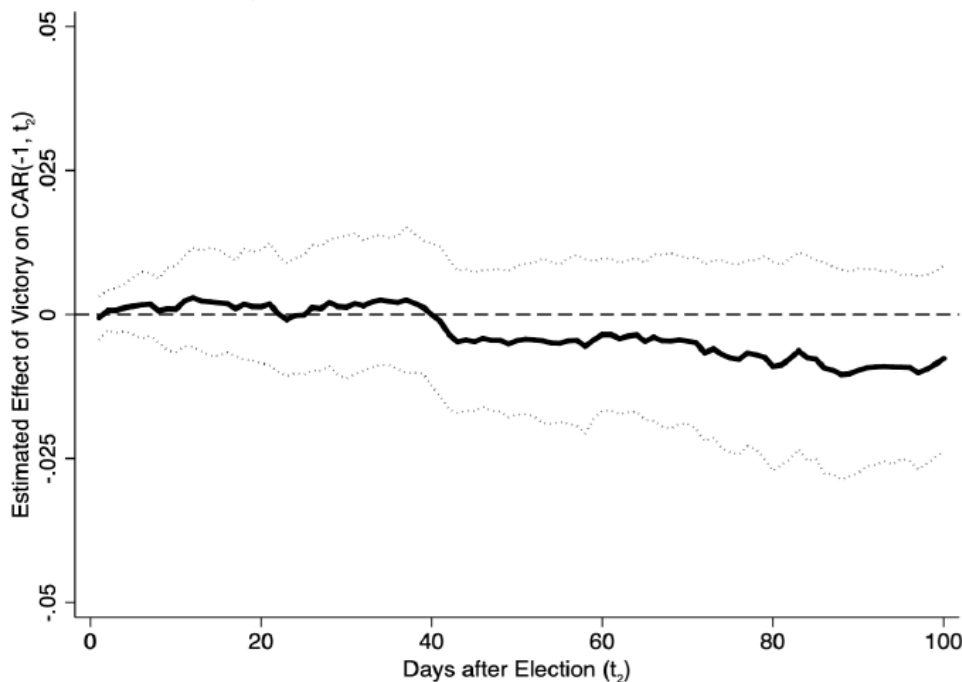
Table 3.1. Robustness Checks

<i>A. Standard RD Specification</i>						
Local Polynomial	Bandwidth (in p.p.)				IK-Optimal	CCT
	.5	1.25	2.5	5		
Constant	.0020 (.0023)	-.0003 (.0020)	-.0003 (.0012)	.0005 (.0009)	.0010 (.0008)	.0006 (.0007)
Linear	-.0033 (.0041)	.0019 (.0026)	.0001 (.0023)	-.0006 (.0018)	-.0005 (.0013)	-.0002 (.0013)
Quadratic	-.0037 (.0055)	-.0001 (.0038)	.0012 (.0031)	-.0002 (.0022)	.0000 (.0022)	-.0008 (.0017)
Cubic	-.0005 (.0079)	-.0032 (.0042)	.0021 (.0033)	.0010 (.0032)	-.0008 (.0021)	-.0006 (.0021)
Number of Observations	3,368	8,133	16,642	32,643	--	--
<i>B. RD Specification with Race Fixed Effects</i>						
Local Polynomial	Bandwidth (in p.p.)				IK-Optimal	CCT
	.5	1.25	2.5	5		
Constant	.0022 (.0024)	.0005 (.0023)	-.0002 (.0014)	-.0002 (.0007)	-.0002 (.0009)	-.0009 (.0007)
Linear	.0046 (.0021)*	.0042 (.0022)	.0015 (.0028)	-.0001 (.0025)	-.0008 (.0010)	-.0008 (.0011)
Quadratic	.0046 (.0020)*	.0042 (.0023)	.0014 (.0028)	-.0001 (.0025)	.0012 (.0025)	-.0007 (.0016)
Cubic	.0048 (.0044)	.0045 (.0022)	.0036 (.0023)	.0014 (.0027)	.0015 (.0028)	.0013 (.0027)
Number of Observations	3,368	8,133	16,642	32,643	--	--

Note: Entries are RD estimates and standard errors for the effect of a supported politician rising to office on $CAR(-1,1)$. The upper panel modifies the regression model in equation 3.1 by considering polynomials in the running variable, while the lower panel also controls for race fixed effects. Standard errors are clustered by election cycle. The optimal bandwidth in the fifth column is chosen according to the procedure of Imbens and Kalyanaraman (2011). The optimal bandwidth in the rightmost column is chosen according to the procedure of Calonico, Cattaneo and Titiunik (2014). ** and * denote statistical significance at the 1% and 5% levels, respectively.

electoral winners have taken office and begun implementing their policy agendas. Our estimates are never statistically distinguishable from zero, and after about 40 trading days they actually become negative. Thus, regardless of the time horizon, there is no indication that firms benefit from their preferred candidate being elected.

Figure 3.3. No Evidence of Positive Medium-Term Effects



Note: Figure depicts estimated effects and confidence intervals for time horizons of up to 100 trading days after the election. Point estimates correspond to β in equation (1) with $CAR(-1, t_2)$ as dependent variable. Confidence intervals account for clustering by election cycle. The sample is the same as in Figure 3.1.

We also explore the possibility of heterogeneous effects. To this end, Table 3.2 presents RD results for different subsamples. The first row shows our baseline estimate from Figure 3.1. The second row estimates the same regression model on a donut sample, i.e., a sample from which we removed all elections decided by less than 0.2 percentage points. These are the races for which one might be worried about sorting, fraud, or legal challenges, all of

which may lead to nonrandom outcomes. Excluding these races has virtually no impact on the RD estimate.

Next, we present results separately by office. *A priori*, one might have expected the largest impact for governors. After all, governors are politically important and operate as independent executives rather than one of many members of a legislature. While actual point estimate for governors is positive, it is substantively small and insignificant. In fact, and there is no statistically significant evidence of a positive effect for any of the offices we consider.

In addition, we study cases in which corporations donated relatively large sums. Even in races where a firm gave more than \$2,500—roughly the 90th percentile of donations—we obtain a small negative and statistically insignificant point estimate. If there were political quid pro quos, say in the form of a single grant or procurement project, we would expect to find a greater effect on firm value for small rather than large, diversified companies. We test this prediction by splitting our data according to firms' market capitalization. Again, it is not possible to reject the null of no quid pro quos. The same holds true when we separately analyze different economic sectors and when we consider elections before and after *Citizens United*. Although the sample size for the latter period is small, there is no evidence of an effect in either era.

Table 3.2 further examines heterogeneity across the number of *other* winners that a firm supported in the same election cycle. If there are diminishing marginal returns to political connections, an electoral victory should be most valuable when the company did not contribute to other elected officials. However, even in these cases, the RD estimate is small and

statistically insignificant. Moreover, our results do not depend on the overall number of candidates that the firm supported, suggesting that the sample imbalance with respect to this variable is inconsequential.

Since one might suspect that corporations giving to candidates on both sides of the aisle are especially access-oriented, we separately analyze firms that donated primarily to Republicans, Democrats, or both. Our results, however, yield no evidence of effects for any of these subgroups. One may also expect that quid pro quos are more likely to arise when only a few firms contribute to a candidate's campaign. Yet, we detect no meaningful variation across the number of firms supporting the winner. Lastly, for the legislative settings in our sample, we might expect greater effects when a company's favored legislator belongs to the majority party and thus stands a greater chance of influencing policy. But again, neither majority- nor minority-party winners have a meaningful impact on firm value.

Broadly summarizing, our RD results imply that, on average, firms do not derive significant benefits from the electoral victory of a supported candidate. In addition, we find little evidence of heterogeneity in effect size. Even in settings that are *a priori* most likely to yield evidence of political quid pro quos, there appear to be none.

3.6. Alternative Research Design

To ameliorate potential concerns with our RD analysis, we implement an alternative research design that relies on a different source of variation and, therefore, on a different set of identifying assumptions. Since both empirical approaches produce similar results, we conclude that our substantive results are neither driven by the assumptions underlying our RD design nor the unrepresentativeness of very close elections.

Our alternative approach uses *within-campaign* variation in market beliefs—as measured by betting odds—about the outcomes of U.S. Senate elections from 2004 to 2010. Instead of comparing returns across firms that contributed to different candidates, this approach holds firm-candidate pairs fixed. Identification comes from high-frequency changes in the probability that a corporation's preferred candidate ends up winning the race.

The sample for this analysis consists of 3,371 firm-candidate pairs across 120 Senate races that were listed on Intrade. The betting price provides the market's implied belief about the probability that a particular candidate will win (Wolfers and Zitzewitz 2004). For each firm-election, we focus on betting prices and stock returns in the 40 trading days leading up to Election Day. Restricting attention to a short period before the election ensures that the vast majority of corporations have already distributed their contributions. Furthermore, this is a period of intense campaigning, with often-significant swings in polls.

The within-campaign approach complements our RD design in a number of ways. First, since it *conditions* on "who gave to whom," the resulting estimates are not subject to the concern that firms contributing to winners may be systematically different from those supporting losers. Second, our within-campaign design leverages additional, high-frequency variation, resulting in more statistical power for any given election. Thus, if one is especially interested in recent Senate races, then this alternative approach yields more informative results than the RD estimates. Third, this design draws on *all* elections for which market beliefs fluctuated over the final weeks of the campaign. Since this is even the case for races involving prominent party figures, powerful committee members, and other influential incumbents,

the evidence is *not* limited to candidates who are electorally vulnerable. In sum, a within-campaign design helps to address reservations about the internal and external validity of our RD results.

An important limitation of the within-campaign approach is that rich betting market data are only available for a subsample of elections. Additionally, this design relies heavily on market efficiency. For our inferences to be valid, financial markets must accurately respond to high-frequency changes in candidates' electoral prospects, and betting markets have to be efficient enough for these changes to be incorporated into odds. Since betting markets are thinner than financial markets, the latter assumption may be problematic. If variation in betting prices is due to noise rather than genuine information, then our subsequent estimates would be attenuated. To speak to this issue, we present a case study of the 2006 Senate race in Virginia. At least within this particular setting, bettors are quite responsive to new information. In particular, most of the meaningful changes in betting odds are explained by gaffes, campaign events, and new polls. We also note that restricting attention to the most liquid and, therefore, least noisy contracts on Intrade has virtually no impact on the results below.

To implement the within-campaign design, we estimate the following equation:

$$(3.2) \quad AR_{i,\Delta t} = \alpha + \beta \Delta \text{Pr}(\textit{Favored Candidate})_{i,j,\Delta t} + \epsilon_{i,j,\Delta t},$$

where $\Delta \text{Pr}(\textit{Favored Candidate})_{i,j,\Delta t}$ is the change in the perceived winning probability of company i 's preferred candidate in election j over time period Δt , and $AR_{i,\Delta t}$ denotes the firm's abnormal return over the same time frame. The parameter of interest is β . It measures

the increase in market value that would result from an electoral victory of the firm's favored candidate, relative to a counterfactual loss.

By regressing returns (i.e., changes in stock prices) on changes in the electoral prospects of candidates, the regression model in equation 3.2 is akin to a first-differences design and therefore holds all firm- and election-specific factors constant.⁹ Since we work with abnormal rather than unadjusted returns, our results also control for overall market conditions.

The crucial assumption for estimates based on equation 3.2 to be unbiased is that changes in beliefs about the electoral prospects of a particular candidate are uncorrelated with changes in other, unobserved factors determining the value of the firms that contributed to her campaign. This assumption would be violated if, for instance, corporate scandals had spillover effects on the supported politicians, or if financially troubled companies could withdraw earlier donations.

Table 3.3 presents the results from estimating equation 3.2 by ordinary least squares. Columns (1)–(3) use daily observations, while columns (4)–(6) rely on weekly data. For the latter analysis, the dependent variable is the CAR from Friday to Friday, and the independent variable is the change in the betting market probability over the same period. If one is concerned that daily fluctuations in betting odds are noisy and only weakly related to changes in election fundamentals, then the weekly analysis will be more informative.

⁹To see why the model in 3.2 conditions on who gives to whom, consider the data generating process

$$\log(\text{stock price})_{i,t} = \mu_{i,t} + \beta \text{Pr}(\text{Favored Candidate})_{i,j,t} + \epsilon_{i,j,t},$$

where $\mu_{i,t}$ is a firm–candidate specific factor that is priced into the company's stock. Taking the difference between time t and t' gives

$$R_{i,\Delta t} = \beta \Delta \text{Pr}(\text{Favored Candidate})_{i,j,\Delta t} + \Delta \epsilon_{i,j,\Delta t},$$

with $R_{i,\Delta t}$ denoting the stock's return.

Our most inclusive regression models in columns (3) and (6) also include firm-election fixed effects. We, therefore, not only condition on "who gives to whom," but we also implicitly account for linear time trends in the electoral prospects of candidates and the performance of individual stocks. Identification in these specifications comes from temporary fluctuations around the respective trends. Arguably, the models in columns (3) and (6) rely on weaker identifying assumptions, but they require more faith in market efficiency.

Regardless of specification, all point estimates in Table 3.3 are statistically indistinguishable from zero. The coefficients in columns (4)–(6) even have the "wrong" sign. In sum, the evidence from this alternative research design is fully consistent with our RD estimates.

3.7. Discussion

In this paper, we provide systematic evidence on the impact of money in politics by studying corporate campaign contributions in over 18,000 elections for governor, Congress, and state legislature across three decades. Our research designs isolate the present value of the benefits a company derives if its favored candidate wins rather than loses the race. Surprisingly, we find no evidence that campaign contributions produce significant benefits for the firm, at least not on average. Our estimates are precise enough to statistically reject meaningfully large effect sizes as well as the possibility that campaign contributions are a good investment for individual executives.

We should emphasize that our results only speak to the impact of *one* additional supported candidate winning office. Previous research has convincingly shown that partisan majorities in Congress impact firm values through their (anticipated) effects on policy (see, e.g., Jayachandran 2006; Snowberg, Wolfers and Zitzewitz 2007b). Our results are not at odds

with this finding. The policy-making process is complicated, and electing one additional favored candidate may not be enough to shift the overall balance of power in a democracy with many checks and balances. In this sense, legislative systems with diffuse powers protect against the influence of special interests.

We should also emphasize that our findings complement rather than contradict the extant literature on the value of political connections. Previous work, for instance, demonstrates that political connections are quite valuable for firms in less-developed countries (e.g., Fisman 2001; Faccio 2006; Boas and Hidalgo 2014). Even in a mature democracy like the U.S., ties between corporations and important members of the executive branch appear to yield nontrivial benefits (Goldman, Rocholl, and So 2009; Do, Lee, and Nguyen 2015; Brown and Huang 2017), especially in times of crisis (Acemoglu et al. 2016). Our results differ, at least in part, because we study connections between firms and a *much* broader set of politicians, who are, on average, less powerful. Notably, our results are silent on whether companies profit from ingratiating themselves with members of the federal executive.

Taken together, the extant evidence suggests that context matters a great deal—not all connections between firms and candidates are created equal. Taking both our findings as well as those in the literature at face value, corporate campaign contributions may be cause for concern in (only) a limited number of circumstances. Delineating the conditions under which corporate electioneering is and is not damaging to the democratic process remains an important question for future work.¹⁰ For example, based on our RD results it appears that

¹⁰Fourinaies and Fowler (2018) take a first step in this direction by studying the influence of the insurance industry in U.S. state politics. Although this industry is a big player in state politics, and despite the fact that it is heavily regulated on the state level, Fourinaies and Fowler find no evidence that the ability to make corporate campaign contributions benefits insurance companies.

special interests are not particularly effective at influencing electorally constrained politicians through PAC donations. In any case, we need more information on the entire *distribution* of effects—be they good, bad, or null—in order to properly evaluate the overall impact of money in politics.

In addition, our null findings raise an interesting puzzle. Scholars have argued that special interests give strategically (Barber 2016; Fournaies and Hall 2014, 2018; Powell and Grimmer 2016), even buying access (Kalla and Broockman 2016). Yet, we find no evidence that contributions actually benefit the average firm. Is access not valuable? In order for contributions to consistently affect firm value through this channel, campaign contributions need to buy access, access has to change the behavior of elected officials, that behavior has to translate into policy, which in turn must help firms. Determining *why*, or at which point, this causal chain breaks down is also a fruitful question for future research.

If corporations do not meaningfully profit from one additional favored candidate winning office, why do they nonetheless contribute to so many political campaigns? Our results help to narrow down the set of possibilities. First, as in the signaling theories of Gordon and Hafer (2005) and Schnakenberg and Turner (2016), the benefits that accrue to the company may not depend on who wins the election. Second, companies might "give a little and get a little" (Ansolabehere, de Figueiredo, and Snyder 2003; p. 126). That is, the true benefits to the firm may be so small that they are not statistically detectable. Third, individual managers may derive consumption value from donating, and agency problems within the company may prevent shareholders from effectively monitoring the use of these funds.

The last explanation is consistent with Bonica's (2016) argument that even CEOs and other corporate elites donate largely for ideological reasons. Still, even spending driven by

consumption motives may alter election results in favor of the preferences of a small class of individuals; and our analysis is silent on the number of races that might have seen a different winner in the absence of such contributions. If the political leanings of high-ranking executives are not representative of the electorate as a whole, this might be reason enough to worry about giving by corporations and other wealthy donors.

Table 3.2. Analysis of Subsamples

	Estimate	Firm-Elections
Baseline	-.0006 (.0018)	32,643
Donut Sample	-.0006 (.0019)	31,980
<i>By Office:</i>		
Governors	.0013 (.0048)	1,949
U.S. Senate	-.0000 (.0034)	4,484
U.S. House	.0008 (.0018)	11,267
State Senate	-.0036 (.0035)	5,263
State House	-.0033 (.0026)	9,680
<i>By Donation Size:</i>		
≥ \$500	.0002 (.0022)	22,515
≥ \$100	.0005 (.0031)	14,426
≥ \$2,500	-.0027 (.0045)	4,385
<i>By Firm Value:</i>		
< 200 million	-.0101 (.0057)	1,748
200 million-1 billion	-.0007 (.0041)	4,014
1-5 billion	-.0032 (.0042)	6,858
> 5 billion	.0018 (.0014)	15,475
<i>By Sector:</i>		
Manufacturing	.0009 (.0015)	14,051
Transportation and Infrastructure	-.0019 (.0021)	7,775
Finance, Insurance, and Real Estate	-.0034 (.0050)	5,115
Other	-.0001 (.0024)	5,702
<i>Before vs After Citizens United:</i>		
Before	-.0006 (.0018)	31,468
After	.0002 (.0031)	1,175
<i>By Number of Other Winners Supported:</i>		
0	-.0002 (.0038)	886
1-5	-.0004 (.0053)	1,934
6-10	.0079 (.0048)	1,629
11-20	-.0106 (.0054)	3,035
21-100	-.0006 (.0025)	13,762
>100	.0002 (.0015)	11,397
<i>By Number of Candidates Supported:</i>		
0-5	-.0004 (.0052)	2,152
6-50	-.0018 (.0031)	10,450
> 50	-.0001 (.0020)	20,041
1st Quartile (<34)	-.0030 (.0023)	9,276
2nd Quartile (34-84)	-.0007 (.0035)	8,687
3rd Quartile (84-227)	.0006 (.0016)	7,766
4th Quartile (>227)	.0002 (.0014)	6,914
<i>By Share of Same-Cycle Donations Given to Democrats:</i>		
< 1/3	.0000 (.0010)	12,418
1/3-2/3	-.0009 (.0021)	18,399
≥ 2/3	-.0048 (.0075)	1,826
<i>By Number of Firms Supporting the Winner:</i>		
1	-.0045 (.0067)	2,071
2-4	-.0035 (.0044)	4,613
5-14	.0013 (.0025)	11,479
≥ 15	-.0009 (.0036)	10,595
<i>By Control of Chamber (Post Election):</i>		
Winner in Majority Party	.0017 (.0032)	15,279
Winner in Minority Party	-.0029 (.0025)	15,415
<i>By Incumbency Status of Supported Candidate:</i>		
Incumbents	-.0003 (.0016)	18,824
Challengers	-.0009 (.0021)	13,819

Note: Entries are point estimates and standard errors for β in equation 3.1, estimated on different subsamples of the data. Standard errors are reported in parentheses and account for clustering by election cycle. All specifications restrict attention to close elections, i.e., races with a two-party vote share between .45 and .55. The rightmost column indicates the number of observations in a particular sample. ** and * denote statistical significance at the 1% and 5% levels, respectively.

Table 3.3. Within-Campaign Results

<i>A. Daily Intervals</i>			
	Abnormal Returns		
	(1)	(2)	(3)
$\Delta\text{Pr}(\text{Favored Candidate})$	.0039 (.0076)	.0049 (.0076)	.0049 (.0076)
Firm–Cycle FE	No	Yes	No
Firm–Election FE	No	No	Yes
R-Squared	.000	.013	.013
Number of Observations	127,010	127,010	127,010
<i>B. Weekly Intervals</i>			
	Abnormal Returns		
	(4)	(5)	(6)
$\Delta\text{Pr}(\text{Favored Candidate})$	−.0082 (.0194)	−.0034 (.0139)	−.0045 (.0187)
Firm–Cycle FE	No	Yes	No
Firm–Election FE	No	No	Yes
R-Squared	.000	.108	.109
Number of Observations	22,924	22,924	22,924

Note: Entries are point estimates and standard errors for γ in equation 3.2. Standard errors are reported in parentheses and account for two-way clustering at the race and week level. All specifications restrict attention to the last 40 trading days prior to the Election Day. The upper panel uses daily data, whereas the lower one aggregates observations at the weekly level. ** and * denote statistical significance at the 1% and 5% levels, respectively.

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