

Lobbying in Divided and Unified Government

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Abstract

Does divided or unified government have an effect on lobbying expenditures by special interests, and does the type of governmental regime – divided government, unified Democratic government, or unified Republican government – affect the lobbying efforts of special interests who tend to be aligned with one party over another? While empirical accounts of lobbying in Congress and at the state-level emphasize that lobbying activity is not primarily driven by characteristics of the political environment, there are theoretical accounts which predict that divided government should increase aggregate lobbying, and that the “friendliness” of a governmental regime towards certain types of special interests will affect lobbying efforts. To address whether the type of governmental regime affects lobbying expenditures, we employ lobbying expenditure data from 29 states and a regression discontinuity design (RDD) that accounts for the multiple elections that produce unified or divided government. Based on our preliminary estimates, we cannot reject the null hypothesis of no causal effect of divided government on overall or corporate lobbying expenditures. While some of the preliminary estimates for labor unions, corporations, and trade groups show a decrease under unified Republican control, the results call for the inclusion of additional data as well as robustness checks ahead of a substantive interpretation of these estimates.

1 Introduction

Does divided or unified government have an effect on lobbying expenditures by special interests, and does the type of governmental regime – divided government, unified Democratic government, or unified Republican government – affect the lobbying efforts of special interests whose preferences tend to be aligned with one party over another?

While observational studies of lobbying in Congress (Drutman 2015) and the American states (Gray and Lowery (1996) suggest that lobbying activity tends to be relatively unaffected by changes in the political environment, extant theoretical work suggests a number of mechanisms by which divided versus unified government could affect the incentives of special interests to lobby, and thereby alter institutional performance (e.g., Bannedsen and Feldman 2002b, 2006; Cameron and de Figueiredo 2015; Ehrlich 2007; Epstein and O’Halloran 1999, 133. At the same time, if lobbying mediates the effect of governmental regime type on legislative productivity, it may present a challenge to empirical analyses which try to distinguish between the effects of divided government and lobbying on legislative gridlock (e.g., Bowling and Ferguson 2001; Gray and Lowery 1995). Further, although not implicitly modeling the institution of governmental regime, seminal competing theories of lobbying suggest different implications about the effect of the “friendliness” of governmental regimes on the lobbying expenditures of different types of special interests (e.g., Grossman and Helpman 2001, Section 5.2; Hall and Deardorff 2006).

Causal inference in lobbying research is made difficult by the limited data availability, the stickiness of the decision to lobby, omitted-variable concerns and endogenous selection into lobbying (de Figueiredo and Richter 2014). To address these questions, we employ extensive expenditure data by lobbying clients in 29 states. Further, to estimate a causal effect of divided (vs. unified) government, and causal effects of unified Democratic or Republican government on the lobbying expenditures of aligned and non-aligned interests, we use a regression discontinuity design (RDD) that accounts for the multiple elections that produce unified or divided government in separation-of-powers systems (Kirkland and Phillips 2018).

Based on our preliminary estimates, we cannot reject the null hypothesis that there is no causal effect of divided government on overall or corporate lobbying expenditures. While

we cannot reject the null of no effect for the expenditures of different organizations under unified Democratic government, there is some preliminary evidence which indicates that aggregate lobbying expenditures by labor unions, as well as aggregate lobbying expenditures by corporations and trade associations decrease under unified Republican government.

The first result is in line with previous empirical research which finds that changes to the political environment do not have a strong effect on lobbying activities (e.g., Drutman 2015; Gray and Lowery 1996). It may also suggest that some theoretical accounts which focus on the difference between divided vs. unified government (e.g., Bennedsen and Feldman 2006; Ehrlich 2007) are incomplete. We hold off from a substantive interpretation of the results about partisan unified government until we include additional data in our analysis and conduct further robustness checks.

This analysis joins a small but growing literature which examines the empirical implications of theoretical models of lobbying (e.g., Austen-Smith and Wright 1994; Cameron and de Figueiredo 2009, 2014, 2015). Our analysis contributes to the growing field of studies which employ causal inference research design to study lobbying.¹ By combining extensive data with an empirical strategy for causal identification, the analysis provides additional evidence to the study of lobbying.

2 Background

Observational empirical studies on lobbying activity in Congress (Drutman 2015) and the American states (Gray and Lowery 1996) suggest that most lobbying activity is driven by an internal organizational logic and not affected by the pattern of interface to the government. This relative “stickiness” of lobbying has been identified as a hindrance to causal inference in observational analyses that study lobbying (de Figueiredo and Richter 2014).

At the same time, extant theoretical work suggests a number of mechanisms by which divided versus unified government could affect institutional performance by changing the incentives of special interests to lobby (e.g., Bennedsen and Feldman 2002a, 2002b, 2006;

¹See e.g., Blanes i Vidal et al. 2012; de Figueiredo and Silverman 2006; Fremeth *et al.* 2018; Huneus and Kim 2018; Payson 2018; You 2019).

Cameron and de Figueiredo 2015; Ehrlich 2007; Epstein and O'Halloran 1999). Further, although not implicitly modeling the institution of governmental regime, seminal competing theories of lobbying suggest different implications about the effect of the “friendliness” of governmental regimes on the lobbying expenditures of different types of special interests. If lobbying mediates the effect of governmental regime on legislative productivity, it would present a challenge to distinguishing between the effects of divided government and lobbying on legislative gridlock.

2.1 Lobbying in Divided and Unified Government

Existing theoretical research suggests a variety of mechanisms by which divided versus unified government could affect the incentives of special interests to lobby, and thereby change institutional performance. Generally, the theories tend to suggest that divided government increases lobbying efforts by special interests.

Few formal theories of lobbying examine the effects of lobbying under divided government (Mazza and van Winden 2008, 136).² Bennedsen and Feldman (2002a, 2002b) provide models in which divided government increases the incentives for lobbyists to provide information.³ Bennedsen and Feldman (2006) show that divided government offers more interest group influence in a model where a legislature decides how much to delegate decision-making authority to the bureaucracy.

Cameron and de Figueiredo (2015) develop a model of competitive endogenous-cost lobbying to influence a budget decision which incorporates important institutions, including governmental regime. The institutions include partisan control of a bicameral legislature and an executive, differential party control over proposal and veto power, and annual vs. biennial budgeting. Partisan bias is modeled as a preference for one of the two groups' preferred projects.

The model predicts that the out-group will spend more on lobbying than the in-group,

²Mazza and van Winden (2008) develop a model of multi-tier lobbying via contributions which is motivated by a divided government.

³They distinguish between divided and unified government in the sense that the latter includes a vote of confidence which reduces the ability to build coalitions. Lee (2016) suggests that insecure majority status can increase the salience of a government-vs-opposition cleavage which would make their unified government more relevant to the American context.

and that of all the governmental regimes simple divided government leads to the highest budget-related lobbying expenditures, which results in the selection of high quality projects where otherwise none would be selected.⁴ Cameron and de Figueiredo (2015) report empirical support for the main in- vs. outgroup hypothesis under unified government from a preliminary analysis using lobbying expenditures of groups that lobby in multiple states.

The model also predicts that expenditures for “in-group” lobbying on budgets will be lower under unified government than under divided or split-chamber government.⁵ Moreover, “in-group” lobbying on budgets under unified government will also be lower than “out-group” lobbying under unified, divided or split-chamber government.⁶ Assuming that labor unions are in-groups for Democrats (and out-groups for Republicans), and corporations and trade groups are in-groups for Republicans (e.g., Brunell 2005; Cameron and de Figueiredo 2009, 2014; Thieme n.d.; c.f., Bonica 2013), the model predicts that unions will spend less on budgetary lobbying under unified Democratic government than under other regimes. Similarly, corporations and trade associations are predicted to spend less on budgetary lobbying under unified Republican government than under other regimes.⁷

In the areas of business strategy and international political economy, Henisz and Zelner (2006) argue based on Henisz (2000) and Tsebelis (1995, 2003) that more “veto points” and lower preference congruence make political actors less susceptible to interest group pressure. This may complicate firms’ lobbying decisions (Macher and Mayo 2015), which may decrease their lobbying expenditures. On the other hand, Ehrlich (2007) and Macher *et al.* (2011) argue that divided governments provide more “access points” or “entry points” for special interests to establish, maintain, or enhance policy-making influence, which may increase lobbying by firms. Coming from a population ecology perspective, Gray and Lowery (1995) suggest that political competition increases uncertainty which increases interest group density for some interest group *guilds*. Moreover, divided government may increase the value of political intelligence provided by special interests about the viability of policy proposals

⁴Under unified government, lobbying prevents the selection of some low quality projects.

⁵In unified government, legislature and executive share a bias. In divided government, they have opposite biases. In split-chamber government, one chamber shares the executive’s bias.

⁶Under biennial budgeting, some of these comparisons again depend on the probability of a special session to revise the budget being sufficiently high.

⁷Since groups do not simply “burn money” to establish credibility, but invest in searching information, the predictions exclude expenditures on goodwill and outside lobbying.

(Lorenz n.d.). Preference divergence across branches of government may also increase interest group conflict, as opposed to interest group cooperation (Holyoke 2011).

Empirical analyses have found mixed evidence on whether and how divided government affects lobbying activity. Ehrlich (2007) finds mixed evidence for an “access point” theory, with greater party diversity in government having the expected positive effect on protectionism, but bicameralism having the opposite effect. Lowery and Gray (1995) in an examination of lobbyist registrations in the American states find that political certainty has a negative effect on the number of lobbying organizations in some sectors.⁸ In a cross-country survey of thousands of firms, Macher and Mayo (2015) find evidence that a higher number of “entry points” in countries with more independent branches and greater preference heterogeneity increase perceived influence of firms.⁹ Gray and Lowery, based on surveys of interest groups in six states, find that the structure of interest group communities is “more strongly determined by the internal needs of organized interests than by their patterns of interface with government” (1996, 108). Bullock and Padgett (2007) describe how divided government following a long period of unified government in Georgia spurred lobbying activity by special interests due to the need to hire lobbyists with ties to either party, the need to talk to more people, and the need to exert more effort to show the merits of bills.

2.2 Divided Government, Interest Groups, and Productivity

In the literature on legislative productivity, lobbying by special interests has been advanced as a – potentially competing, but generally independent – explanation for legislative gridlock (e.g., Berry 2002; Binder 2004, 30-31; Bowling and Ferguson 2001; Lowery and Gray 1995; c.f. Holyoke 2011, Ch. 7).¹⁰ Lowery and Gray (1995) find a negative effect for the number of organized groups at the state level on bill introduction and enactment, and no significant

⁸Certainty is measured with a folded Ranney index (1965), capturing the degree of unified government.

⁹In his analysis of lobbyist registrations and expenditures from LDA filings in Congress across time, Drutman (2015) does not examine divided government. However, he suggests that the decision to lobby is largely driven by a “stickiness” and a general growth over time.

¹⁰That is, a potentially competing, but generally independent explanation for legislative gridlock in addition to the governmental regime (e.g., Binder 1999, 2004) the preferences of institutional veto players (e.g., Krehbiel 1998), or the conjunction of institutional vetoes and parties (Chiou and Rothenberg 2003, 2009).

effect for the governmental regime.¹¹ Bowling and Ferguson (2001) find that the effect of more interest groups on gridlock depends on the policy area, and that divided government did decrease the likelihood of bill passage in the states. Holyoke (2011, Ch. 7) finds that inter-chamber preference heterogeneity affects lobbyist conflict, and that both reduce the likelihood that bills make it out of committee and get enacted in Congress.¹²

The above-mentioned analyses estimate effects for measures of governmental regime and interest group activity by including measures for both in a regression framework, implying that divided government has no effect on lobbying activity. However, if the amount of interest group activity is affected by the type of governmental regime (e.g., Cameron and de Figueiredo 2015; Gray and Lowery 2005a; Henisz and Zelner 2011; Macher *et al.* 2011), estimates from regressions that include measures of interest group activity and measures of governmental regime will suffer from post-treatment bias, with unknown magnitude and direction (e.g., Angrist and Pischke 2009, 64-66; King 2010; Rosenbaum 1984), leading to incorrect interpretations of the results (Samii 2016).¹³

At the same time, if interest groups have an effect on legislative productivity, and there is a non-causal correlation between the partisan composition of government and interest group activity, removing interest group activity from the above-mentioned regressions would result in omitted variable bias (Greene 2008). In estimating the effect of divided government on budgetary delay, Kirkland and Philips (2018) rely on “as-if random” assignment of divided or unified government which removes the issue of non-causal correlation between e.g., divided government and interest group activity.¹⁴

2.3 The “Friendliness” of Governmental Regimes and Lobbying

Existing explanations of which elected officials will be lobbied most in a policy-making setting have tended to abstract away from institutional features such as governmental regime.

¹¹They also find that interest group diversity, as measured by the proportion of registered non-economic interests, is associated with more enactments and a higher passage rate.

¹²Berry (2002) separately examines the effects of governmental regime and interest group testimonies on the chances of bill passage in three Congresses, finding no support for an effect of interest groups.

¹³Holyoke (2011, Chapter 7) recognizes the potential for endogeneity, but the proposed instrumental variable regression does not address post-treatment bias.

¹⁴This does not rule out the possibility that interest groups mediate the effect of divided government (e.g., Kirkland and Philips 2018, 196).

First, in models that emphasize exchange, campaign contributions may be given as bribes to affect the votes of marginal legislators or which policies are implemented (e.g. Hall and Wayman 1990; Snyder 1991). Alternatively, campaign contributions may buy the attention of legislators (e.g., Denzau and Munger 1986; Hall and Wayman 1990), or demobilize opponents (Hall and Wayman 1990). While these accounts focus on how campaign contributions are targeted, Hall and Wayman’s arguments imply that lobbying expenditures will be directed most towards “strong supporters” (1990, 814).

Second, in Hall and Deardorff’s (2006) decision-theoretic model lobbying takes the form of a “legislative subsidy”; costly information in the form of policy expertise and political “intelligence” that help legislators advance legislation (2006, 74). The model is “budget-centered”, as opposed to “preference-centered”, in that lobbyists primarily help to relax legislators’ budget constraints as opposed to trying to change legislators’ preferences (2006, 69). It predicts that lobbyists will lobby their “allies”, will lobby their strongest allies the most, and will not – or almost never – lobby their “enemies” (2006, 76). Similarly, Constantelos (2018) presents a decision-theoretic model where lobbying organizations are pushed to lobby at the federal level by “unfriendly” or divided state governments.¹⁵

Given the high level of polarization (Shor and McCarty 2011) and the low importance of filibusters in state politics (e.g., Reilly 2009), legislators from the majority party under unified government may not need to not rely on the minority party in enacting laws¹⁶, and minority party legislators may see their bills die in committee (e.g., Cox and McCubbins 2005). As a result, minority party legislators have few incentives to invest effort to craft legislation, and special interests allied to the minority party should doubt that their efforts to subsidize legislative allies are worth their while (Hall and Deardorff 2006, 72). Hence, the legislative subsidy model implies that lobbying expenditures on direct lobbying should be lower for out-groups under unified government compared to any other governmental regime.¹⁷

Third, there is an extensive literature on models of informational lobbying which empha-

¹⁵The hypotheses are based on the assumptions that an added veto and “unfriendly” governments reduce the ability to advance favorable legislation.

¹⁶Krehbiel (1998) argues that the unified vs. divided government distinction is mostly meaningless for lawmaking in Congress. However, without filibuster or with a low cloture threshold there is a closer link between governmental regime and the size of the gridlock interval (e.g., Chiou and Rothenberg 2008, 711).

¹⁷It is not necessarily clear whether legislators’ demand for policy expertise and political intelligence from in-groups should be higher under unified or divided government.

sizes persuasion. Endogenous-cost lobbying models (e.g., Cameron and de Figueiredo 2009, 2014, 2015; Potters and Van Winden 1992, Section 4.1; Grossman and Helpman 2001, Section 5.2) are most conducive to examining who is lobbied most, since they make predictions about the amount of costly effort to invest in a message to elected officials (Cameron and de Figueiredo 2009, 2014).¹⁸ These models imply that lobbyists will exert most effort in targeting legislators who are marginal or ex-ante opposed.

Although these models do not incorporate a separation of powers, they can be interpreted to have similar implications with respect to in- and out-groups under unified party government as Cameron and de Figueiredo (2015). Since the logic in the former models is one of “burning money” in order to convey credibility, the predictions hold for all budgetary lobbying expenditures, including expenditures related to goodwill and indirect lobbying. Table 1 summarizes some of the empirical implications.

There is mixed evidence on whom lobbyists will target and whom they will target most. On the one hand, Hojnacki and Kimball (1998) report survey results from groups lobbying Congress tend these tend to lobby their friends.¹⁹ By examining venue selection by financial interests lobbying Congress, Holyoke (2003) finds that organizations avoid venues with strong expected opposition and interpret this as consistent with organizations lobbying their allies more. Payson (2018), using both difference-in-differences design and an RDD, shows that municipalities are more likely to hire lobbyists when their districts elect non-co-partisan state representatives. Constantelos (2018) finds that groups lobby more at the federal level when faced with by “unfriendly” state governments.

On the other hand, Kollman (1997) suggests that whom lobbyists target is largely driven by a general agreement among groups and committees in the same issue areas rather than from deliberate choices by groups. Moreover, Austen-Smith and Wright (1994) provide evidence that groups lobby likely supporters and legislators who are predisposed to vote against their favored positions. Cameron and de Figueiredo (2009) analyze the lobbying expenditures of multi-state lobbying groups in 12 states using both a difference-in-differences and

¹⁸Models of non-costly signaling (e.g., Battaglini 2002; Schnakenberg 2017) or “exogenous-cost” lobbying (e.g. Austen-Smith and Wright 1992; Bennedsen and Feldman 2002a, 2002b; Grossman and Helpman 2001, Section 5.1; Potters and Van Winden 1992) are therefore less suitable.

¹⁹However, they appear not to examine corporations and the response rate is only 33%.

a multi-level regression framework. They provide evidence that the labor unions and corporations increase expenditures when faced with more distant governments – i.e., non-allied unified or divided, as opposed to allied unified governments. Analyzing the contacts of lobbyists for foreign governments, You (2019) shows that lobbyists of foreign governments contact likely opponents, supporters and “undecideds”, and that this depends on stage of the policy process. Thieme (Forthcoming) uses state-level disclosures to compare the position-taking and contribution behavior of lobbying organizations and shows results that are consistent with business interests following preference-centered strategies of exchange (e.g. Hall and Wayman 1990) or persuasion (e.g., Grossman and Helpman 2001, Section 5.2).

3 Data

3.1 Lobbying Expenditures

To measure the intensity of lobbying efforts across states and time, we collected lobbyist employer expenditure data from 29 states, in the period from 1993 through 2018. The intervals for which data were collected vary across states; 2005 being the median first year. In total, our sample contains 430 state-year observations, with total expenditures amounting to approx. \$25.0 billion (2010 dollars).²⁰ Since compensation for lobbyists tends to make up the vast majority of lobbying employers’ expenditures, we did not collect data from 28 states where lobbyists or employers are not required to disclose compensation data.²¹

There is some variation in what branches and activities are covered by the disclosure requirements across states (see e.g., King 2011). In general, the dollar amounts reported in the disclosures include expenditures for both legislative and executive lobbying. The data are therefore well suited for examining how expenditures vary across different governmental regimes.²² Generally, expenditures related to indirect communication – i.e., *outside* or *grassroots lobbying* – are included in the reported expenditures, although 14 states itemize at least some expenditures related to outside lobbying.

²⁰For an overview, see Appendix A.1. Data collection for additional years and New Hampshire is ongoing.

²¹For an overview, see King (2011). We exclude Louisiana and Nebraska due to their electoral systems.

²²Of the states in our sample, only Kentucky does not require disclosure of expenditures due to executive lobbying. Some states also include expenses from local or judicial lobbying.

Table 1: Summary of Empirical Implications

Authors	Empirical Implication
Bowling & Ferguson '01; Gray & Lowery '95b	Lobbying expenditures are not affected by the type of governmental regime
Cameron & de Figueiredo '15	More lobbying expenditures on budgetary lobbying (direct & no goodwill lobbying) under simple divided than unified government
Henisz & Zelner '11	Less lobbying expenditures by corporations under divided government (simple or split-chamber) than unified government
Macher <i>et al.</i> '11; Bennedsen & Feldman '02a, '02b, '06	More lobbying expenditures (by corporations, Macher <i>et al.</i> '11) under divided than unified government
Cameron & de Figueiredo '15	Less lobbying expenditures on budgetary lobbying by unions (direct, excluding goodwill lobbying) under unified Democratic government than other governmental regimes Less lobbying expenditures on budgetary lobbying by business interests (direct, excluding goodwill lobbying) under unified Republican government than other governmental regimes
Cameron & de Figueiredo '14; Grossman & Helpman '01, Potters & van Winden '92	Less lobbying expenditures on lobbying by unions (budgetary lobbying for Cameron and de Figueiredo '14) under unified Democratic government than other governmental regimes Less lobbying expenditures on budgetary lobbying by business interests (all expenditures) under unified Republican government than other governmental regimes
Constantelos '18; Hall and Deardorff '06	Less lobbying expenditures (direct & no goodwill lobbying) by unions under unified Republican government than other regimes Less lobbying expenditures (direct & no goodwill lobbying) by business interests under unified Democratic government than other regimes

Note: The table summarizes some of the empirical implications about how different types of lobbying expenditures are affected by the type of governmental regime. The first four implications do not vary depending on the preference similarity between governmental regime and type of lobbying organization.

3.2 Election Results Data

The key data for generating our forcing variables are historical state election results. For state assembly and state senate elections, we rely on Klarner’s (2016) “State Legislative Election Returns (1967-2016)” dataset. These data include candidates names, party affiliations, and vote counts by state legislative district. We supplement these with gubernatorial election returns from Congressional Quarterly’s (2003) “Voting and Elections Collection.” We rely on these gubernatorial and legislative election data, along with data from Dubin (2007), to generate the forcing variables for our RDD.

4 Empirical Strategy

4.1 Measures of Lobbying Activity

To measure lobbying activity across states and years, we aggregate expenditures by lobbyist employers, excluding any itemized campaign contributions (see Cameron and de Figueiredo 2009, 2014). Consistent with a perspective of “burning more money” to establish credibility (e.g., Cameron and de Figueiredo 2009, 2014; Grossman and Helpman 2001, Section 5.2; Potters and van Winden 1992, Section 4.1), and to test if overall lobbying expenditures vary by governmental regime, we include expenditures from “goodwill” and outside lobbying.²³

We adjust for inflation by converting the aggregated dollar amounts to 2010 dollars. Further, we address the skewness of the distribution by calculating the logarithm of the inflation-adjusted lobbying expenditures. While previous research has tended to use the number of lobbyist registrations as a measure of lobbying intensity, these have been shown to correlate strongly with lobbying expenditures Leech *et al.* (2005). Moreover, lobbying expenditures may reveal differences in the intensity of lobbying that are masked by relying

²³Goodwill lobbying includes meals, gifts, and similar expenditures to establish a good relationship between lobbyists and public officials. Next steps: Constructing measures that exclude expenditures related to fundraising events. In line with several models of lobbying (e.g., Cameron and de Figueiredo 2015; Hall and Deardorff 2006; Wolton 2018), we will also distinguish between direct lobbying, goodwill lobbying, and outside lobbying expenditures. Moreover, we will use lobbyist registration to focus on expenditures by organizations lobbying on budgets (e.g., Cameron and de Figueiredo 2014).

on the number of registered lobbyists.²⁴

4.2 Regression Discontinuity Design

A fundamental challenge in identifying the effect of divided (or unified) government on lobbying is that partisan control of state government is not randomly assigned. Some states may “select into” divided government more frequently, and states that routinely experience split party control may be systematically different than those which more often experience unified party control. The voter preferences that determine the partisan configuration of state government also are likely to be correlated with factors that shape the policy agenda. To address these endogeneity concerns, we adopt a regression discontinuity design (RDD).

One approach to causal inference with observational data, the RDD is a quasi-experimental design distinguished by its reliance on a forcing variable—i.e., a variable that determines treatment assignment. At a known value of the forcing variable, the probability of assignment to treatment changes discontinuously. For example, a candidate’s vote share captures the underlying probability of election victory with a sharp discontinuity at 50%. In close elections, there are a subset of candidates whose odds of winning are very close to 50-50. Intuitively, for observations that lie very near the threshold, units are, in expectation, nearly identical in whatever factors determine selection but differ in whether or not they are actually treated. The RDD leverages data close to the threshold in the forcing variable to mitigate the potential for selection bias and estimate a local average treatment effect.

Vote share commonly serves as the forcing variable in political science applications of RDD. The RDD has been used to estimate the effect of candidate ideology (Hall 2015), partisanship (Lee, Moretti, and Butler 2004; Gerber and Hopkins 2011), and race (Hopkins and McCabe 2012), as well as other candidate traits. Election results are crucial to our RDD, but assignment to divided (or unified) government is jointly determined by gubernatorial and legislative elections. Rather than relying on the customary vote share forcing variable, we require a measure that accounts for elections to the governorship, the state assembly, and

²⁴Although there is some evidence for incomplete disclosure at the federal level (e.g., Drutman 2015, 21; Edsall 2013a, 2013b; LaPira 2015), it is not clear to what extent similar trends exist on the state-level, and if this is affected by the type of governmental regime.

the state senate. At first glance, legislative seat share might seem like a potentially useful forcing variable, but this option proves untenable for two key reasons. First, a legislative chamber with a nearly even partisan distribution of seats is not necessarily one where the majority was up for grabs. Between gerrymandered districts and uncontested races, sometimes even narrow majorities in terms of seat share are electorally secure.²⁵ Second, there is no straightforward approach to combining gubernatorial vote shares and legislative seat shares because they measure distinct quantities of interest.

We modify the familiar RDD to accommodate these complications. We use a simulations-based approach introduced by Kirkland and Phillips (2018) to generate a single forcing variable that incorporates outcomes in elections for the governorship, the state assembly, and the state senate. In our simulations, electoral shocks of varying magnitudes are administered to actual district-level and gubernatorial election results. The simulations give us a sense of how close a state was, in a given election year, to experiencing a different outcome in terms of the partisan control of state government.

Each simulation unfolds in three steps. First, we determine the size of the state-level electoral shock (S_i), the value of which constrains the magnitude of district-level shocks (ΔV) that we apply to actual district election results. S_i is randomly drawn from a normal distribution with a mean and standard deviation that are equal to the mean and standard deviation of the actual distribution of historical aggregate election results for state i . The value of S_i can be either positive or negative, and smaller (larger) values of S_i produce smaller (larger) values of ΔV .

Next, we need to generate the district-level shock (ΔV). For each legislative district (j) in state (i), we take a new random draw (D) from a normal distribution with the mean and standard deviation of historical election shocks from each type of district in the state. For a state assembly seat in Pennsylvania, for example, D_{ij} will be drawn from a normal distribution with the mean of historical shocks across all state assembly districts in Pennsylvania. For gubernatorial elections, we simply take a random draw from a normal distribution with the mean and standard deviation of the distribution of historical gubernatorial election

²⁵In 1999, for example, both Texas and Tennessee had closely divided senates. However, neither state had a single senate race in which the winning margin was less than 10 percentage points, and in both states, nearly one-half of the seats up for election were uncontested.

shocks for that state. Incorporating (D) allows for random variation in the size of shocks across districts. Each ΔV_{ij} is a function of these two random draws:

$$\Delta V_{ij} = S_i + S_i * D_{ij} \tag{1}$$

In the third step of each simulation, we apply the electoral shocks and aggregate the results. In every legislative district election and gubernatorial election, we add ΔV_{ij} to the Democratic candidate’s vote share while subtracting ΔV_{ij} from the Republican’s vote share. Once we determine which candidate wins each district, we translate our simulated election results into legislative seat shares and combine these with the simulated gubernatorial election results to determine the partisan composition of state government.

We repeat this process 10,000 times, noting after each simulation whether it produces unified or divided government and whether this result differs from the actual observed outcome. We use these results to identify the smallest state-level vote shock (S) that produces the opposite outcome in terms of divided or unified government in the majority of simulations. For example, an observed distance to divided government value of -0.04 implies that a state actually experienced unified government but a shift of 4 percentage points in the aggregate vote share from one party to the other would have produced divided government. From these simulations, we generate three measures, the simulated electoral distance to divided government, the simulated electoral distance to unified Democratic government, and the simulated electoral distance to unified Republican government, which serve as the forcing variables in our RDD.

4.3 Estimation Strategy

After we generate the simulated distance to divided (unified) government, this measure becomes the forcing variable in the familiar sharp RDD. The aim of the RDD is to estimate a local average treatment effect, that is the change in the dependent variable at the threshold in the forcing variable—in our first case, the shift from just barely having unified government to just barely having divided government. We follow current best practices and use local linear regression models which incorporate only those observations that lie within a specified

window or bandwidth on either side of the cutpoint, weighting observations based on their proximity to the cutpoint (Gelman and Imbens 2014; Cattaneo, Idrobo, and Titiunik 2018; cf. Imbens and Lemieux 2008).

Determining the bandwidth involves a tradeoff between bias and variance. By restricting the analysis to observations very close to the threshold in the forcing variable, narrower bandwidths can produce unbiased estimates, but the small number of observations may produce noisier estimates. Conversely, wider bandwidths may reduce the variance of the estimates but introduce the potential for bias by including observations that lie far from the cutpoint. Determining the appropriate bandwidth is a crucial choice that can affect results. RDD guides generally suggest relying on data-driven techniques to calculate an optimal bandwidth, which helps to minimize researchers’ discretion (Imbens and Kalyanaraman 2012; Cattaneo, Idrobo, and Titiunik 2018). Following this advice, we use the algorithm formulated by Calonico, Cattaneo, and Titiunik (2014) to calculate what we refer to as the CCT optimal bandwidth. We present these results along with estimates from similar specifications using a bandwidth of 5%, which tends to be common in electoral RDDs.

5 Results

Before presenting our results, we briefly review the validity of our RDD. The key identifying assumption is that potential outcomes are smooth across the discontinuity in the forcing variable. We suspect that the “no sorting assumption” will be easily met since our forcing variable is composed of electoral results for multiple offices, making precise control over the forcing variable implausible. Nonetheless, we evaluate the validity of our design in several ways. We implement the McCrary (2008) test to assess the density of the forcing variable at the cutpoint. As expected, we fail to reject the null hypothesis of no sorting. Next, we conduct a series of placebo tests and check for imbalances in baseline covariates of observations that are near the threshold but differ in treatment assignment. The results of these placebo tests generally provide support for the validity of our design but do raise a few concerns about our distance to unified Republican government forcing variable. These results are included in the Appendix along with the details of the McCrary tests and figures

that show the distribution of the forcing variable.

5.1 Divided Government

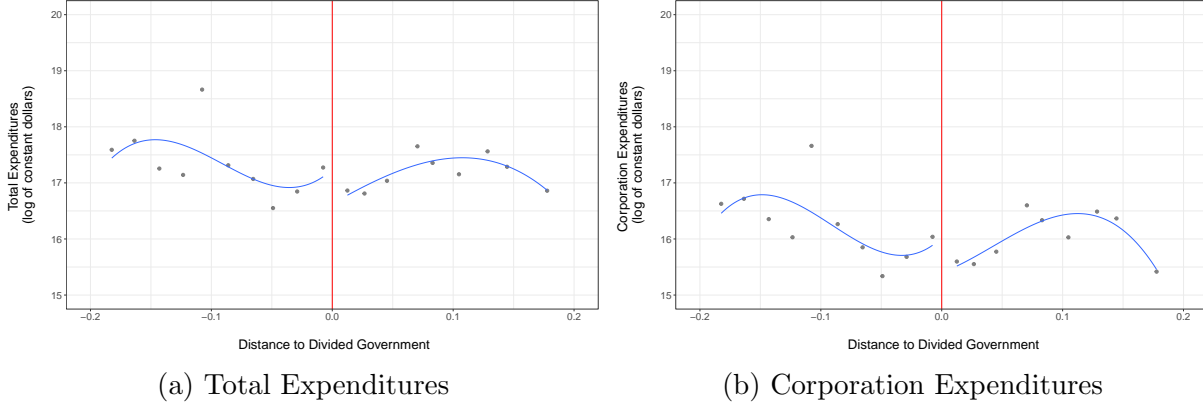
We begin by analyzing the effect of divided government on total lobbying expenditures. Figure 1a plots the forcing variable, distance to divided government, on the x-axis and total lobbying expenditures (log of constant dollars) on the y-axis.²⁶ Note that negative values of the forcing variable—to the left of the threshold at 0—indicate that one party has unified control of state government, while positive values of the forcing variable—to the right of the threshold—indicate that party control is divided. Using this plot we look for graphical evidence of a change (either a jump or dip) in the value of the outcome variable at the threshold in the forcing variable, indicated by the vertical line at 0 on the x-axis. In other words, we see whether going from barely having unified government to barely having divided government leads to an observable change in total lobbying expenditures. Observing a change at the cutpoint would provide preliminary evidence of a causal relationship. Lines (3rd order polynomials) on either side of the cutpoint plot the relationship between total expenditures and the distance to divided government. We observe little change in total expenditures at the cutpoint.

To more rigorously evaluate the effect of divided government on total lobbying expenditures, we estimate local linear regression models. The results are presented in Table 2. All models include two covariates, state population and state GDP, to increase the precision of our estimates. The estimates in Column 1 reflect a bandwidth of 0.05, while the results in Column 2 were generated using the CCT optimal bandwidth of 0.075. In both cases, the coefficients for divided government are negative but small in magnitude and fail to approach conventional levels of statistical significance.

Next, we consider the effect of divided government on lobbying expenditures by corporations. Figure 1b plots lobbying expenditures by corporations (log of constant dollars) on the y-axis against distance to divided government on the x-axis. At the threshold between unified and divided government, we find little evidence of a change in corporation lobbying

²⁶Because the distribution of lobbying expenditures are quite skewed, we operationalize all of our dependent variables as the log of constant 2010 dollars.

Figure 1: Lobbying and Divided Government



Note: In each plot, the x-axis is the distance to divided government centered at 0, and the y-axis is the change in the dependent variable (measured log of constant dollars). The points are averages of the change in surplus within 2% bins.

Table 2: Lobbying & Divided Government

	Dependent Variable: Lobbying Expenditures (log of 2010 dollars)			
	Total Expenditures (1)	Total Expenditures (2)	Corporation Expenditures (3)	Corporation Expenditures (4)
Divided government	-0.042 (0.309)	-0.172 (0.266)	-0.152 (0.388)	-0.232 (0.351)
Distance to divided gov't	-0.724 (7.238)	2.150 (4.234)	-1.343 (8.587)	1.492 (6.141)
Population	-0.762* (0.417)	-1.023** (0.335)	-0.839* (0.492)	-1.057** (0.451)
State GDP	1.575** (0.385)	1.815** (0.315)	1.700** (0.470)	1.888** (0.430)
Distance to divided gov't * Divided government	-1.272 (10.190)	0.003 (6.511)	1.570 (13.019)	0.303 (10.087)
Constant	9.447** (1.976)	10.542** (1.561)	7.882** (2.217)	8.958** (2.016)
Bandwidth	0.050	0.075	0.050	0.063
Observations within Bandwidth	116	167	119	146
Residual Std. Error	0.519 (df = 110)	0.495 (df = 161)	0.621 (df = 113)	0.613 (df = 140)
F Statistic	37.286** (df = 5; 110)	60.494** (df = 5; 161)	29.459** (df = 5; 113)	37.157** (df = 5; 140)

Note: Estimates from local linear regression models with robust standard errors. Dependent variables and state GDP (measured in inflation-adjusted constant dollars) as well as population transformed to logs. * $p < 0.1$; ** $p < 0.05$ (two-tailed test).

expenditures.

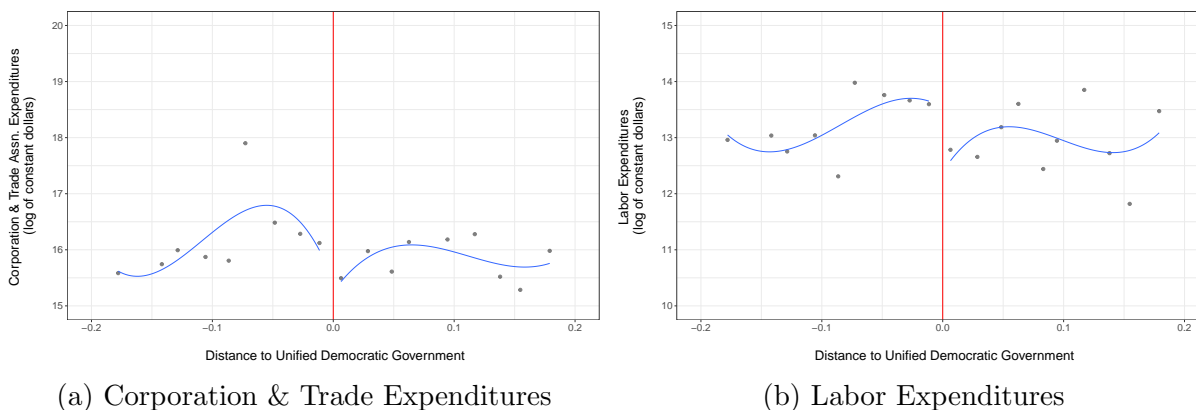
Table 2 shows the results of local linear regression models estimating of the effect of

divided government on lobbying expenditures by corporations. We again include two covariates, population and state GDP, to improve the precision of our estimates. The results in column 3 come from a model that uses a bandwidth of 0.05. Using the CCT optimal bandwidth of 0.063 generates the estimates in column 4. Again, the results of both models are quite small, negative, and not statistically distinguishable from zero.

5.2 Unified Party Control

To explore the possibility that firms and organizations will spend more lobbying politically friendly (or unfriendly) state governments, we extend our analyses to test for changes in lobbying expenditures when one party just barely wins unified control of state government. Specifically, we focus on expenditures by labor unions and by corporations and trade associations. Using the RDD, we examine whether and how these groups' lobbying expenditures change when either Democrats or Republicans control both the legislative and executive branches of state government.

Figure 2: Lobbying and Democratic Government



Note: In each plot, the x-axis is the distance to unified Democratic government centered at 0, and the y-axis is the change in the dependent variable (measured log of constant dollars). The points are averages of the change in surplus within 2% bins.

First, we investigate the effect of unified Democratic control of state government on lobbying expenditures. Figure 2a plots the relationship between lobbying expenditures by corporations and trade associations (log of constant 2010 dollars) on the y-axis and the

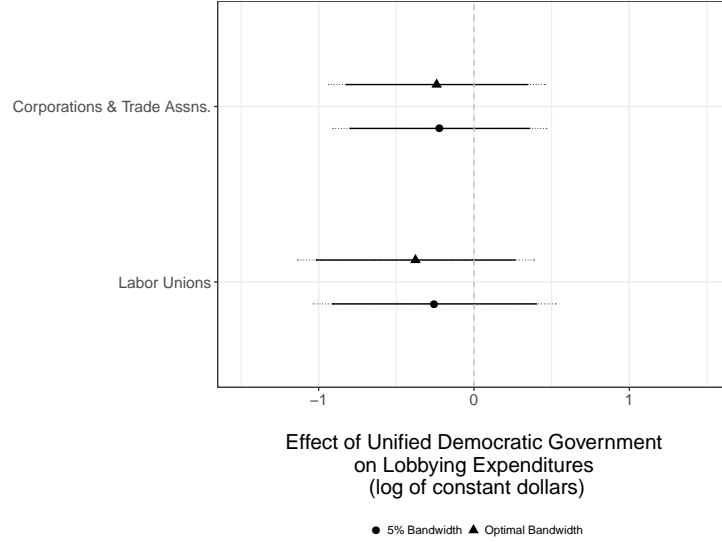
forcing variable on the x-axis. Here, the forcing variable is the distance to unified Democratic partisan control. Values to the left of the cutpoint at 0 (i.e., negative values of the forcing variable) are any observations where the Democrats do not control both the legislative and executive branch, and values to the right of the cutpoint (positive values) indicate unified Democratic control. At the threshold in the forcing variable, we observe a small dip in corporation and trade association lobbying expenditures.

Figure 3 presents the results of local linear regression models that estimate the effect of unified democratic control on corporation and trade association lobbying expenditures. Like the analyses of divided government above, we include two covariates, population and state GDP, to increase the precision of our estimates. The x-axis indicates the effect size (measured as the log of constant dollars) with a dashed vertical line at 0. Dots indicate results from models estimated with a bandwidth of 0.05, while triangles represent estimates from models relying on the CCT optimal bandwidth of 0.046. Solid black error bars indicate 90% confidence intervals, and the dotted lines extend the error bars to show 95% confidence intervals. Consistent with Figure 2a, the estimates are negative and quite small in magnitude, but neither is statistically significant. At a bandwidth of 0.05, the point estimate is -0.22 (SE = 0.35), and the slightly narrower CCT bandwidth produces very similar results with a point estimate of -0.24 (SE = 0.36).

Moving on to the effect of unified Democratic government on lobbying expenditures by labor unions, Figure 2b plots the relationship between labor lobbying expenditures on the y-axis and the distance to unified Democratic government on the x-axis. Compared to the corporate and trade association expenditures displayed Figure 2a, there appears to be some evidence of a slightly larger decrease in labor union lobbying expenditures at the threshold. However, when we estimate the effect of unified Democratic control, the results (presented in Figure 3) are again small in magnitude and fail to approach conventional levels of statistical significance. Local linear regression models with a bandwidth of 0.05 produce a point estimate of -0.26 (SE = 0.40), and the same specifications using the CCT optimal bandwidth of 0.08 generate a coefficient of -0.38 (SE = 0.39).

Finally, we turn to the effect of unified Republican government on lobbying expenditures. Our approach is nearly identical to the above analysis of Democratic party control, but our

Figure 3: Lobbying and Democratic Government

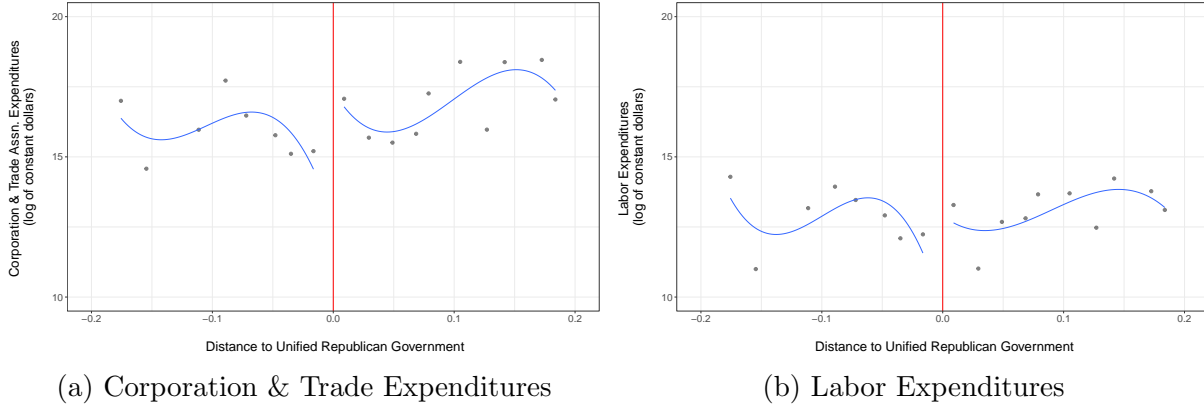


Note: The x-axis measures the effect of unified Democratic government on lobbying expenditures listed on the y-axis. Dots indicate point estimates from local linear regression models using a 5% bandwidth, and triangles indicate point estimates from similar specifications using the CCT optimal bandwidth. The error bars reflect two-tailed tests with solid black lines showing 90% confidence intervals and dotted lines indicating 95% confidence intervals.

forcing variable here is distance to unified Republican government. Figure 4a plots lobbying expenditures by corporations and trade associations on the y-axis against the distance to unified Republican government on the x-axis. Values of the forcing variable that lie to the right (left) of the threshold at 0 indicate observations where Republicans control (do not control) both the legislative and executive branch of state government. At the cutpoint, we see what appears to be an increase in corporation and trade association lobbying expenditures.

Despite the graphical evidence that unified Republican control may lead to an increase in lobbying expenditures by corporations and trade associations, the results of local linear regression models suggest that the effect could be negative. Recall that our specifications do include two covariates, state population and GDP. We present our results in Figure 5. With a bandwidth of 0.05, we produce a coefficient of -0.13 (SE = 0.74), but when we use the CCT optimal bandwidth of 0.03, the point estimate is -2.33 (SE = 0.37). This coefficient is relatively large compared to the wider bandwidth, and it is statistically significant. This result, however, is highly sensitive to choice of bandwidth, holding only at bandwidths of 0.02

Figure 4: Lobbying and Republican Government



Note: In each plot, the x-axis is the distance to unified Republican government centered at 0, and the y-axis is the change in the dependent variable (measured log of constant dollars). The points are averages of the change in surplus within 2% bins.

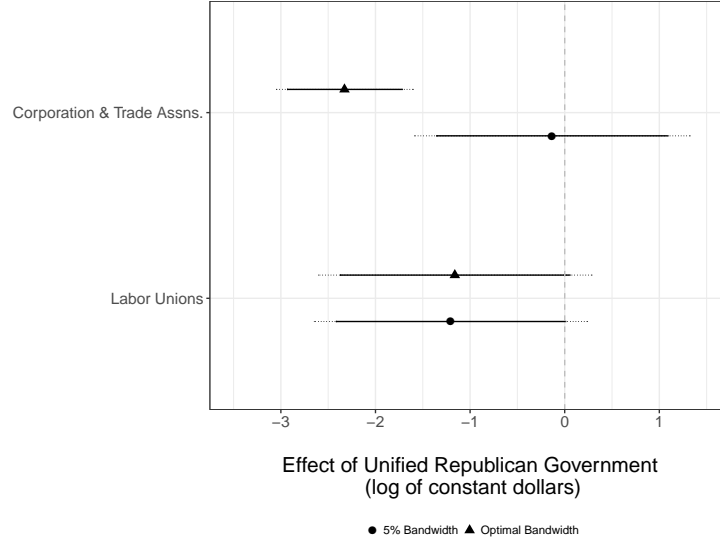
and 0.03, where the number of observations included in the RDD analysis is very small (29 observations within a bandwidth of 0.03 and 16 observations within a 0.02 window around the threshold).

Figure 4b plots lobbying expenditures by labor unions on the y-axis against the distance to unified Republican government on the x-axis. Here, we observe what appears to be a small increase in labor lobbying expenditures at the threshold in the forcing variable, suggesting that just barely having unified Republican control leads to a small increase in lobbying expenditures by labor unions. Again, however, results from local linear regression models suggest that, if anything, labor unions may decrease lobbying expenditures under unified Republican control of state government. The CCT bandwidth of 0.051 generates a point estimate of -1.16 (SE = 0.74), slightly smaller than the point estimate of -1.21 (SE = 0.74) from a model with a 0.05 bandwidth. These results, presented in Figure 5, just fail to reach conventional levels of statistical significance.

6 Discussion

Based on our preliminary estimates, we cannot reject the null hypothesis that there is no causal effect of divided government on overall or corporate lobbying expenditures. While

Figure 5: Lobbying and Republican Government



Note: The x-axis measures the effect of unified Republican government on lobbying expenditures listed on the y-axis. Dots indicate point estimates from local linear regression models using a 5% bandwidth, and triangles indicate point estimates from similar specifications using the CCT optimal bandwidth. The error bars reflect two-tailed tests with solid black lines showing 90% confidence intervals and dotted lines indicating 95% confidence intervals.

we cannot reject the null of no effect for the expenditures of different organizations under unified Democratic government, there is some preliminary evidence which indicates that aggregate lobbying expenditures by labor unions, as well as aggregate lobbying expenditures by corporations and trade associations decrease under unified Republican government.

The first result is in line with previous empirical research which finds that changes to the political environment do not have a strong effect on lobbying activities (e.g., Drutman 2015; Gray and Lowery 1996). It may also suggest that some theoretical accounts which focus on the difference between divided vs. unified government (e.g., Bennesen and Feldman 2006; Ehrlich 2007) are incomplete. We caution against strong interpretations, especially of the latter results, since they depend on the choice of the bandwidth, significance level and on whether covariates are included in the regression. This lack of robustness may be due to a still relatively small sample size.²⁷ Moreover, the expenditure data need to be dis-aggregated further, by whether they were related to direct lobbying of legislators - as opposed to outside

²⁷In addition, the results of the balance and placebo tests warrant further examination.

lobbying (e.g., Hall and Deardorff 2006; Wolton 2018) - by whether they were related to providing information or generating good will, and by whether they were made in relation to lobbying on the budget.²⁸

In addition to the above-mentioned next steps, we plan to examine whether the effect of divided government depends on unified or split-chamber control of the legislature (e.g., Binder 1999, 2003; Bowling and Ferguson 2001, Cameron and de Figueiredo 2015). Further, we will include measures of legislative professionalism in our analysis. In addition, we will consider different specifications of the dependent variable that account for population or GDP (Cameron and de Figueiredo 2009). Moreover, following coding of organizations by sector, we will be able to examine possible differences in expenditures across issue areas (e.g., Leech *et al.* 2005).

²⁸The expenditure data also require additional coding to further distinguish between expenditures of corporations, labor unions, trade associations, and other groups.

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Appendix A.1: Lobbyist Employer Expenditure Data

Table A.1: Lobbyist Employer Expenditure Data Overview

State	First Year	Last Year	Median	Min	Max.
Alaska	2004	2018	17.8	15.6	22.8
California	2002	2017	72.8	241.7	317.5
Colorado	2002	2017	24.5	19.3	28.9
Connecticut	2001	2018	47.1	37.8	170.7
Florida	2007	2018	192.5	178.5	211.5
Hawaii	2005	2018	4.8	4.2	5.9
Indiana	2007	2018	21.6	19.0	31.0
Iowa	2010	2018	18.0	16.1	19.1
Kentucky	1993	2018	13.5	2.3	19.1
Maine	2002	2018	4.2	2.4	5.9
Maryland	1993	2018	25.1	9.0	37.3
Massachusetts	2005	2018	73.6	53.0	83.9
Michigan	2001	2018	24.7	18.5	26.6
Minnesota	2002	2018	62.0	45.8	69.8
Mississippi	2012	2018	21.4	20.1	29.1
Montana	1999	2018	6.1	0.3	7.2
New Jersey	1996	2018	30.7	7.6	131.3
New York	2007	2018	205.7	190.7	225.4
North Carolina	2007	2009	23.1	18.0	24.1
Oregon	2007	2018	28.9	19.7	35.0
Pennsylvania	2007	2018	94.7	87.9	102.6
Rhode Island	2005	2016	11.7	8.5	34.8
South Carolina	2009	2018	17.4	16.0	18.8
Tennessee	2008	2017	44.4	39.5	51.1
Texas	1996	2018	205.8	145.3	260.3
Vermont	1994	2018	8.5	4.9	13.0
Virginia	2013	2015	14.8	12.2	21.9
Washington	2007	2018	46.2	41.4	53.2
Wisconsin	1997	2018	29.6	23.6	37.0

Note: The table provides an overview of the lobbying expenditure data from the 29 states in the sample. Median, minimum, and maximum expenditures are reported in millions of 2010 dollars (rounded to the first decimal). Florida and Texas only provide intervals of lobbyists' compensation by employer, while Tennessee reports separate intervals of employers' overall compensation of lobbyists as well as employers' expenditures. In each case, the median of the interval was used to calculate the amount (for Texas in years with no upper limit for highest category, assigned value based on distance between lower and upper limit from next lowest interval). Data from Indiana in 2010 are not comparable due to changes in filing deadlines which exclude several months. Disclosures of lobbying employers are incomplete in Wisconsin in 2003. Missing data from Maine in 2006 need to be collected. Kentucky does not require disclosure of executive lobbying. New York lobbying expenditures includes some compensation data from local lobbying expenditures. Disclosures from Connecticut from 2009 onward are by biennium. Based on previous trends, amounts equally divided between first and second years (individual reports to be collected will allow better distinctions) Since Maryland changed reporting threshold from 25,000 to 50,000 in 2004, a similar threshold was applied expenditures in the previous years.

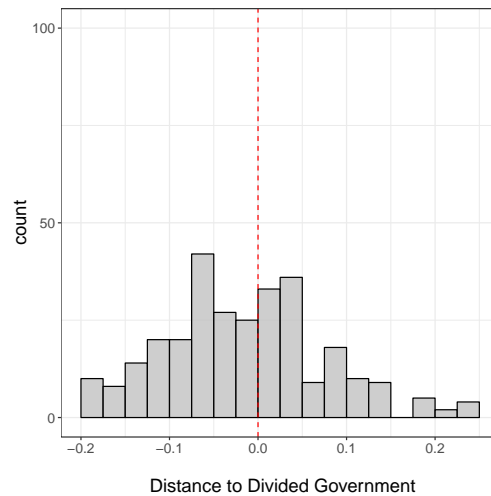
Appendix B.1: Validity of the RDD

The key identifying assumption of the RDD is that potential outcomes are smooth across the discontinuity in the forcing variable. We suspect that the “no-sorting” assumption will be easily met since our forcing variable is composed of electoral results for multiple offices, making precise control over the forcing variable implausible. Nonetheless, we evaluate the validity of our design in several ways and present the results for each forcing variable below. We provide a histogram to illustrate the distribution of the forcing variable. We implement the McCrary (2008) test to assess the density of the forcing variable at the cutpoint. In each case, we fail to reject the null hypothesis of no sorting. Finally, we conduct a series of placebo tests and check for imbalances in baseline covariates of observations that are near the threshold but differ in treatment assignment. Here, we use local linear regression models similar to those used throughout the paper to check for discontinuities in covariates and lagged values of the dependent variables. For the most part, these analyses support the validity of our design. We do uncover some concerns, however, in our placebo tests for the distance to unified Republican government forcing variable.

Distance to Divided Government

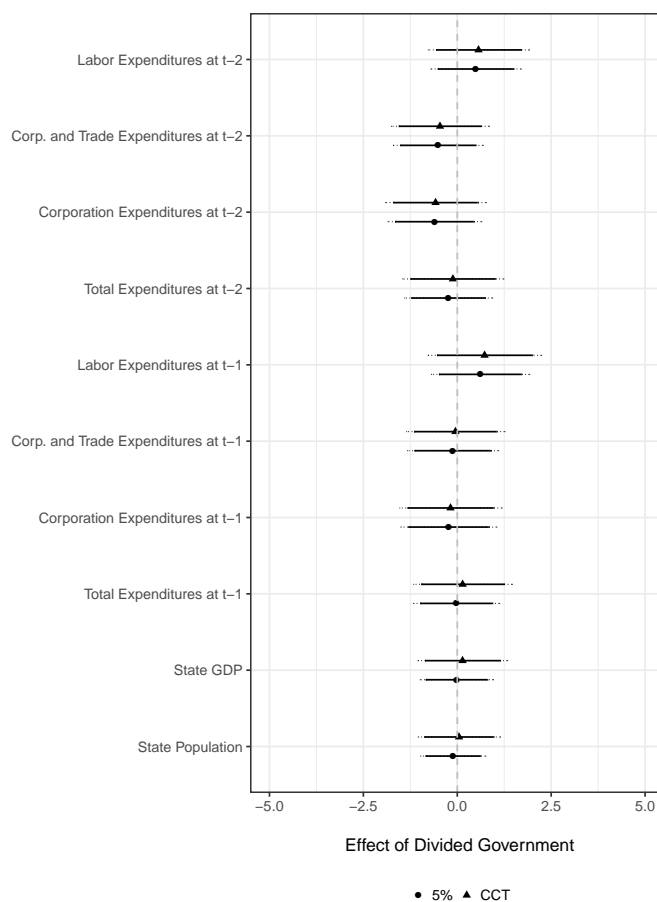
For the distance to divided government forcing variable, the McCrary (2008) density test returns a log difference in heights of 0.345 with a standard error of 0.269 and p-value of 0.2, so we fail to reject the null hypothesis of no sorting.

Figure 6: Distance to Divided Government



Note: The histogram displays the distribution of the forcing variable. Zero on the x-axis is the cutpoint. Observations to the right of the cutpoint (i.e., positive values) have divided government; observations to the left of the cutpoint (i.e., negative values) have unified government. The y-axis is a count of the number of state years that fall into each bin.

Figure 7: Distance to Divided Government

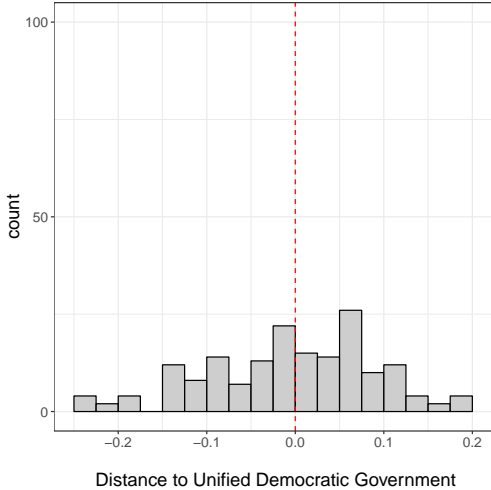


Note: The x-axis measures the effect of divided government on the variables listed on the y-axis. Dots indicate point estimates from local linear regression models using a 5% bandwidth, and triangles indicate point estimates from similar specifications using the CCT optimal bandwidth. The error bars reflect two-tailed tests with solid black lines showing 90% confidence intervals and dotted lines indicating 95% confidence intervals.

Distance to Unified Democratic Government

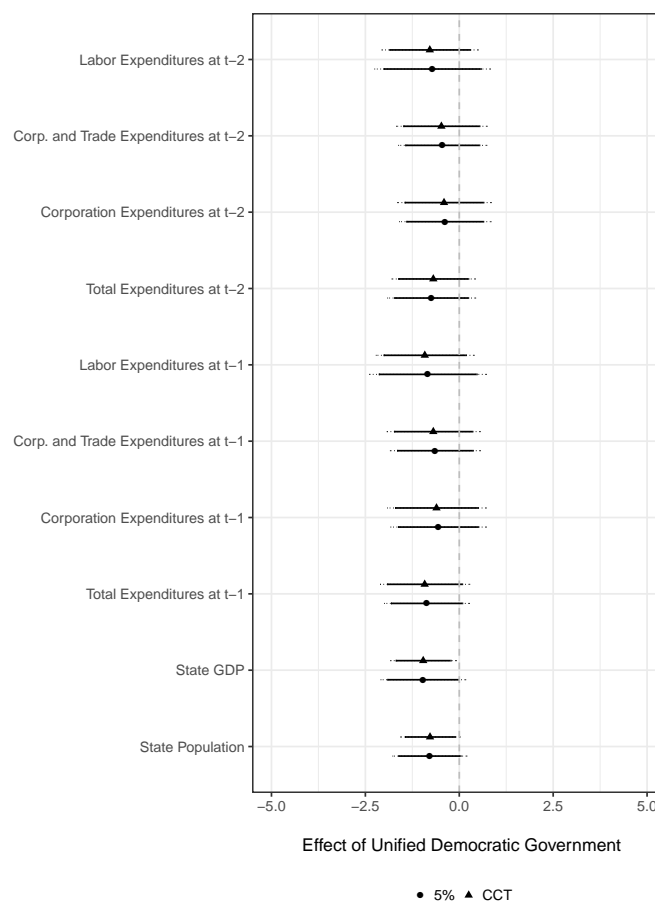
For the distance to unified Democratic government forcing variable, the McCrary (2008) density test returns a log difference in heights of -0.532 with a standard error of 0.390 and p-value of 0.172, so we fail to reject the null hypothesis of no sorting.

Figure 8: Distance to Unified Democratic Government



Note: The histogram displays the distribution of the forcing variable. Zero on the x-axis is the cutpoint. Observations to the right of the cutpoint (i.e., positive values) have unified Democratic government; observations to the left of the cutpoint (i.e., negative values) have divided or unified Republican government. The y-axis is a count of the number of state years that fall into each bin.

Figure 9: Distance to Unified Democratic Government

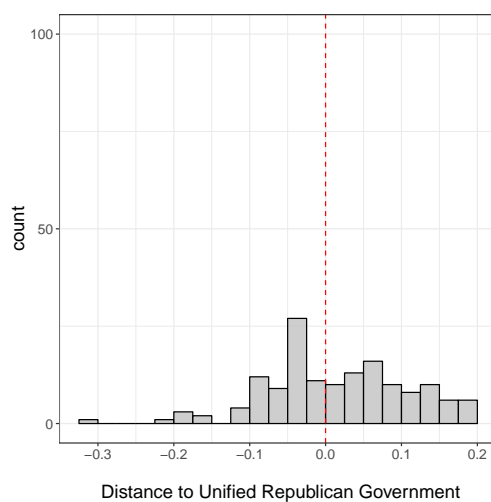


Note: The x-axis measures the effect of unified Democratic government on the variables listed on the y-axis. Dots indicate point estimates from local linear regression models using a 5% bandwidth, and triangles indicate point estimates from similar specifications using the CCT optimal bandwidth. The error bars reflect two-tailed tests with solid black lines showing 90% confidence intervals and dotted lines indicating 95% confidence intervals.

Distance to Unified Republican Government

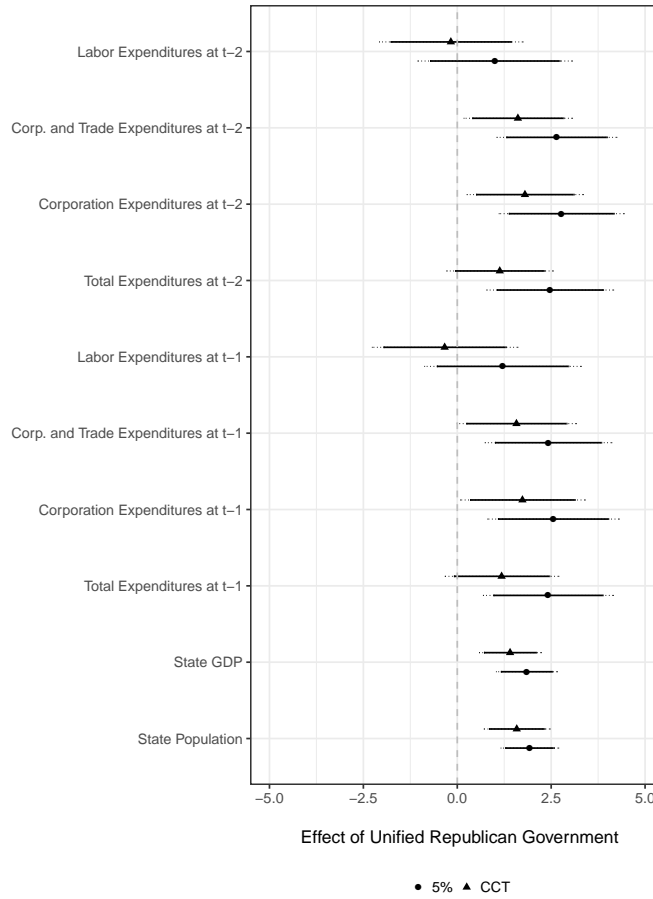
For the distance to unified Republican government forcing variable, the McCrary (2008) density test returns a log difference in heights of -0.532 with a standard error of 0.390 and p-value of 0.819, so we fail to reject the null hypothesis of no sorting.

Figure 10: Distance to Unified Republican Government



Note: The histogram displays the distribution of the forcing variable. Zero on the x-axis is the cutpoint. Observations to the right of the cutpoint (i.e., positive values) have unified Republican government; observations to the left of the cutpoint (i.e., negative values) have divided or unified Democratic government. The y-axis is a count of the number of state years that fall into each bin.

Figure 11: Distance to Unified Republican Government



Note: The x-axis measures the effect of unified Republican government on the variables listed on the y-axis. Dots indicate point estimates from local linear regression models using a 5% bandwidth, and triangles indicate point estimates from similar specifications using the CCT optimal bandwidth. The error bars reflect two-tailed tests with solid black lines showing 90% confidence intervals and dotted lines indicating 95% confidence intervals.